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

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First version: May 18, 2023
This version: August 28, 2023
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Behavioral interventions are popular tools to decrease the carbon footprint of consumers' choices, but not much is known about the channels through which they impact behavior. This project investigates one popular intervention in detail: carbon labels on canteen meals. In a series of lab-in-the-field and field experiments, I provide evidence that the labels mainly impact consumers by directing their attention towards carbon emissions. Increasing consumers' knowledge about carbon impact plays a secondary role. Using data from my experiments, I structurally estimate a model in which consumers prefer to avoid carbon emissions, but are influenced by attentional biases and misperceptions about carbon impact when making their purchase decisions. Combining this estimation with an experimental elicitation of consumers' willingness to pay to see or avoid carbon labels, I find that correcting these two frictions has an overall positive effect on consumer welfare. I directly experimentally estimate the effectiveness of the carbon labels in the student canteen as similar to that of a carbon tax of €120/ton.

*Part of this project was conducted in cooperation with the Studierendenwerk Bonn. I am very grateful for their support. Further, support by EPoS Collaborative Research Center Transregio 224 of the German Research Foundation (DFG) is gratefully acknowledged. I am indebted to Lorenz Götte, Sebastian Kube and Thomas Dohmen for their continuous support and helpful comments. Further, I am thankful to Peter Andre, Nava Ashraf, Sanchayan Banerjee, Simon Block, Stefano DellaVigna, Ximeng Fang, Holger Gerhardt, Michael Kosfeld, Matthew Levy, Julia Mink, Paul Schäfer, Ganga Schreedhar and her lab, Marie Claire Villeval, and audiences at the London School of Economics EEE hour, the University of Bonn Applied Micro Workshop, the briq workshop on climate change, the CRC young researchers' workshop, the Third winter workshop on the behavioral economics of food consumption, the 15th RGS doctoral conference in economics, the 2023 Mannheim conference on energy and environment, the IDOS research seminar, the 2023 AERE summer conference, the 2023 SABE-IAREP conference, and the 2023 M-BEPS conference for their comments and suggestions.

1 Introduction

Strong political action is likely necessary to limit global warming below 2°C (IPCC, 2023), but traditional policy tools for doing so do not enjoy broad support for all sectors. In the agricultural sector, for example, the introduction of carbon taxes on agricultural goods has so far been limited and remains an unpopular policy measure (Dechezleprêtre et al., 2022). The food system causes 26% - 34% of global greenhouse gas emissions (Poore and Nemecek, 2018; Crippa et al., 2021) and 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).

As a result, behavioral interventions such as carbon labels are increasingly discussed as a potential policy tool to reduce emissions in the food sector. They have received attention from academia[1], regulatory agencies[2], and private companies[3]. In a traditional economic framework, the consumer would be perfectly informed about the emissions caused by their consumption choice and such measures would not have an effect on consumption behavior. However, recent causal evidence (Bilén, 2022; Lohmann et al., 2022) shows that carbon labels do impact behavior. This paper adresses the question of why this is the case: Which behavioral channels can explain consumers' reaction to carbon labels?

Evidence from other consumption contexts has shown that consumer behavior is often influenced by behavioral frictions impeding consumers from behaving in a utility-maximizing manner (see e.g. Benartzi et al., 2017; Reisch and Zhao, 2017). In the presence of such frictions, behavioral interventions may affect consumption behavior by removing or weakening these frictions. However, it is unclear which friction(s) we predominantly expect carbon labels to address. One consequence is that we are also restricted in our understanding of whether and how such an intervention might inadvertently impose psychological costs on consumers. From a policy perspective, these psychological costs become especially relevant if an intervention is cheaper than alternative policy measures in monetary terms, but fares worse if the psychological impact on consumers is factored in. Furthermore, a better understanding of the behavioral channels through which consumption

1. (see Reisch et al., 2021, for an overview)
2. (e.g. the Obama administration issued an executive order on Behavioral Science and the European Commission includes carbon labels in its Farm to Fork Strategy; Obama, 2015; European Commission, 2023)
3. (e.g. Oatly, an oat milk producer, Just Salad, a restaurant chain, Panera Bread and Allbirds, a shoe brand; Wolfram, Jessica, 2021)

choices are impacted can help us quantify the effectiveness we can expect from such an intervention.

In a lab-in-the-field experiment in the student canteen context (N=444), I first provide reduced-form evidence that the labels mainly impact consumers by directing their attention towards carbon emissions, while improving consumers' knowledge about carbon impact plays a secondary role. The experiment elicits participants' meal valuations, prior beliefs of the carbon emissions caused by different meal options and participants' willingness to pay to receive or avoid carbon labels. I observe purchasing behavior in different treatment conditions: first, in the absence of any behavioral intervention, second, with a behavioral intervention increasing attention (asking consumers to guess emissions), third, with a behavioral intervention increasing attention and correcting misperceptions (carbon labels) and, finally, when carbon emissions are removed (carbon offsetting). I investigate in how far participants' reaction to carbon labels can be explained by an improvement of their knowledge about the carbon emissions caused. Participants on average underestimate the emissions caused by high-emission meals and overestimate the emissions caused by low-emission meals, similar to patterns found by Attari et al. (2010) for energy consuming appliances and Camilleri et al. (2019) for single food items. I find that correcting these misperceptions significantly impacts consumption choices: Consumers react to carbon labels with a stronger demand reduction if emissions were previously underestimated. However, a large part of the reduction in demand for high-emission meals resulting from the labels is independent of previous under- or overestimation. The results of a second intervention which only increases attention without correcting misperceptions suggests that a large part of the remaining effect can be explained by a direction of attention. I direct participants' attention towards the issue of carbon emissions by asking them to guess the carbon emissions caused by different meals, but I do not provide them with any information on actual impact. This produces a similar effect as the carbon labeling intervention, albeit effects are slightly smaller in magnitude.

To quantify the relevance of the two channels, I structurally estimate a model of consumption behavior in the presence of carbon emissions. My model deviates from the standard model in two ways. First, consumers may prefer lower carbon emissions, but may not be attentive of this at the moment of choice. Second, I allow for lack of knowledge of the carbon emissions caused by different meals, i.e. misperceptions on carbon impact, as a second potential source of non-optimal behavior. Behavioral interventions can increase attention and/or correct misperceptions and thus increase consumer welfare by improving consumption decisions. Further, my model allows for interventions to create a fixed costs or benefit to consumers independent of changes in consumption choices. This fixed term might reflect negative factors such as a general dislike of the behavioral intervention changing the consumption framing, or a feeling that the intervention is shifting attention away from other relevant factors. It might also reflect positive factors, such as contentedness with being informed and/or in control of carbon emissions caused.

My modeling choices are based on previous literature. I focus on attentional biases as an important factor impeding optimal decision making, as they have been identified as relevant in the tax salience and resource consumption salience contexts (Chetty, 2009; DellaVigna, 2009; Byrne et al., 2022), as well as in suggestive empirical evidence from the food consumption context (Lohmann et al., 2022). I focus on misperceptions of carbon impact as an important factor impeding optimal decision making based on suggestions in recent papers on carbon labeling (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022). I allow for behavioral interventions to create a fixed cost or benefit to consumers based on evidence from the resource consumption context (Allcott and Kessler, 2019).

The structural estimation quantifies the reduced-form findings. I estimate consumers' psychological cost of causing carbon emissions through food consumption choice at €110/ Ton of CO_2 emissions. However, attentiveness towards carbon emissions at the moment of choice is strikingly low and statistically indistinguishable from zero in the absence of any behavioral intervention. Based on the estimated model parameters, I approximate the effect on consumers' utility and on carbon emissions of solely removing attentional biases or solely correcting consumers' misperceptions in the student canteen context. The former is more than seven times as effective as the latter, both in increasing consumer utility and in decreasing carbon emissions. The combination of the two interventions (carbon labels) shows itself as most effective, and also as more effective than the sum of the two single interventions, suggesting considerable complementarities.

These results show that behavioral interventions that effectively target the frictions impeding consumers from making utility-maximizing choices can shift consumption towards less emission-intense options without decreasing consumers' utility. However, it is also important to understand how the effectiveness of removing these frictions compares to that of other policy tools. In a second lab-in-the-field experiment (N=289), I directly experimentally elicit an estimate, by quantifying the effectiveness of carbon labels relative to a carbon tax. In particular, I estimate how high of a carbon tax would produce similar changes in demand as is produced by the carbon labels. I do so by comparing the emissions caused by different meals with how willingness to pay for these meals on average changes when consumers are shown carbon labels. Since a decrease in willingness to pay for a meal should have the same effect on the total quantity purchased as an equivalent increase in meal price, this establishes a rough equivalence to a carbon tax. I estimate that a carbon tax of €120/Ton would produce a similar decrease in carbon emissions as carbon labels. This allows for a comparison with other policy tools and allows us to better understand the magnitude of the effect. €120/Ton is about four-fold the German carbon tax on petrol. At the same time, it is still slightly lower than estimates of the social cost of carbon (€160/Ton in Rennert et al., 2022) - This suggests that the labels are not inefficiently "over-correcting" behavior.

Using data from a large-scale field experiment (N>2,500,over 80.000 consumption choices), I show that the consumption reactions I observe to carbon labels in the lab-in-the-field context are reconcilable with behavior observed outside of a one-shot consumption setting. One of Bonn's university canteens was equipped with carbon labels for five weeks, while the two other canteens served as control restaurants, allowing for a causal difference-in-difference estimation of label effectiveness. The carbon labels I test are similar to those used in the lab-in-the-field experiment and include both an ordinal ranking (traffic light system) and a quantitative ranking (greenhouse gas emissions in kg). This has been identified as an effective combination in previous literature (Potter et al., 2021; Taufique et al., 2022). I estimate that the labels decrease consumption of the higher carbon option by 2 percentage points. The effect of the label persists in the three weeks following the intervention period, after which the university canteen closed for summer break.

Based on my model, I expect consumer welfare to be affected in two ways by a behavioral intervention that successfully raises attention and/ or corrects misperceptions about carbon impact. First, welfare is increased as it becomes easier for consumers to make the utility-maximizing choice. Second, the intervention might additionally cause fixed costs or benefits to consumers, with my model remaining agnostic as to the direction and extent of the fixed cost term.

The lab-in-the-field experiments are well-equipped to provide an estimate of the consumer welfare impact of carbon labels, as both experiments elicit participants' willingness to pay to see or avoid carbon labels in a direct and incentive-compatible manner. The reduced-form evidence points towards there being a net psychological benefit created by the carbon labels, with the vast majority (95%) of participants reporting a zero or positive willingness to pay to see carbon labels. The structural estimation allows me to separate consumer willingness to pay into the two components described in my model. The expected increase in consumer welfare attributable to consumers making more utility-maximizing choices is estimated at around €0.0016 per choice. The fixed costs and benefits accruing to consumers due to the carbon labels result in a net benefit estimated at around €0.20. This evidence speaks against carbon labels imposing disproportionate psychological costs on consumers and in favor of the interventions in fact correcting an internality (following the internality definition in Allcott, Mullainathan, and Taubinsky (2014)).

This is further supported by a post-intervention survey conducted after the field experiment in the student canteen (N=234). 73% of guests affected by the labels reported that they would like the labels to be installed permanently (18% did not know, 9% against). These survey results also speak in favor of carbon labels not imposing disproportionate psychological costs on consumers. A carbon tax, in contrast, was only favored by 60% of students, while 14% did not know and 26% were against. Carbon labels thus seem to enjoy greater support than carbon taxes, making an implementation more feasible.

My contributions to the literature are three-fold: First, I contribute to the literature on the role of attentional biases in consumption decisions. The finding that it is not only informational, but also attentional biases that lead to non-optimal decision making has been pointed out in other environmentally relevant consumption contexts, mainly energy and resource consumption (Allcott and Taubinsky, 2015; Taubinsky and Rees-Jones, 2018; Tiefenbeck et al., 2018). This project provides first evidence of attentional biases present in the food consumption context. Literature on carbon labels has so far mainly portrayed the labels as a tool for correcting consumer misperceptions (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022). My theoretical framework combining attentional biases and informational frictions as impedients to optimal decision making can shed light on the mixed findings in previous literature (Imai et al. (2022), Camilleri et al. (2019), Lohmann et al. (2022), Bilén (2022)). More generally, I provide evidence from a discrete choice context of how a behavioral intervention can correct attentional biases and thereby reduce externalities and increase consumer welfare.

Second, I contribute to the relatively young literature on the psychological costs or benefits of behavioral interventions. Literature in this direction so far has focused on the psychological effects of receiving social comparison information (Allcott and Kessler, 2019; Butera et al., 2022). Thunström (2019) is to my knowledge the only other paper assessing the psychological costs of a label. In a hypothetical choice experiment, she finds that calorie labels impose psychological costs on participants with low self control. I provide 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.

Finally, I contribute to the literature on the effectiveness of carbon labels on food consumption. The estimate most closely related to this study is Lohmann et al. (2022) who estimate that labels in a Cambridge student canteen causally decrease the probability of selecting a high-carbon meal by approximately 2.7 percentage points, using a difference-in-difference framework. Brunner et al. (2018) study a similar context, but only observe changes over time in a single restaurant. They find a decrease in sales of red labeled meat dishes by 2.4 percentage points. Bilén (2022) study the introduction of carbon labels in the grocery shopping context, and estimate a 2.5 percentage point reduction in carbon emissions caused by the carbon labels, employing a difference-in-difference estimation. Further correlational evidence (Spaargaren et al., 2013; Vlaeminck, Jiang, and Vranken, 2014; Visschers and Siegrist, 2015) and evidence from hypothetical decisions (Osman and Thornton, 2019; Banerjee et al., 2022) suggests carbon labels reduce carbon emissions. See Rondoni and Grasso (2021) for a review. Other studies examine consumer behavior in the lab, asking consumers to make a decision for consumption 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 the effectiveness of carbon labels relative to a carbon tax. Within-subject designs as used here and in other structural behavioral studies (Taubinsky and Rees-Jones, 2018) can easily be adapted to other experiment populations, consumption environments, or other behavioral interventions, making intervention effects comparable across various domains. The experimental design is further validated by my large-scale field experiment 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 (Jalil, Tasoff, and Bustamante, 2020) and carbon labels in the general food consumption context (e.g. Panzone et al. (2021) study the grocery shopping context).

The rest of this paper is structured as follows. Section 2 will outline a simple theoretical model describing possible behavioral biases influencing consumption behavior in the food consumption context, and the channels through which I expect a behavioral intervention such as carbon labels to impact behavior. Section 3 describes the lab-in-the-field experiment I conducted to examine mechanisms and quantify the parameters of the model, and discusses reduced-form evidence on the relevant behavioral channels. Section 4 structurally estimates the theoretical model using data from the lab-in-the-field experiment. Section 5 describes the second lab-in-the-field experiment which I conducted to quantify the effectiveness of carbon labels in reducing carbon emissions, and section 6 describes the design and results of the field experiment further supporting these results. Section 7 discusses the impact of behavioral interventions on consumer welfare, drawing on data from all experiments. Finally, section 8 discusses findings.

2 Theoretical Model

To fix ideas on how behavioral biases influence meal consumption decisions and consumer welfare, I present a simple model in this section. I describe how, according to the model, consumers' valuation for the same meal will differ depending on the setting: at baseline, with an intervention increasing attention and correcting misperceptions (carbon labels), with an intervention only increasing attention, and with an intervention removing environmental guilt. Further, I formulate how consumer welfare is expected to change when consumers are presented with carbon labels, according to the model. These variations make it possible to identify the parameters governing the model, and inspire the experiment conditions outlined in sections 5 and 3. The corresponding structural estimation is presented in section 4.

2.1 Consumers' valuation of meal options across consumption settings

Consumers evaluate a meal m relative to an outside option o. If the meal m is perceived to provide positive additional value relative to the outside option o, the consumer chooses meal m. This perceived additional value for meal m can be modeled as:

$$\hat{V}_m = \hat{v}_m - \hat{v}_o + \theta(g_m - g_o) \tag{1}$$

where g_m describes the consumers' valuation of the greenhouse gas emissions caused by the meal, and $\hat{v}_m$ describes the consumers' perceived valuation of meal attributes other than greenhouse gas emissions, and the analogous terms describe the outside option. Similar to Imai et al. (2022) I assume in this formulation that consumers' overall perceived valuation is additively separable in $\hat{v}_m$ and the psychological costs of greenhouse gas emissions g_m. Further, I assume that individuals only give weight $\theta \leq 1$ to their valuation of the greenhouse gas emissions caused, due to limited attention. I hereby use a similar formulation as used in the literature on attentiveness to taxes and resource consumption(

DellaVigna (2009), Chetty (2009), and Byrne et al. (2022)). A higher θ describes a higher degree of attention and a lower θ describes a lower degree of attention. I focus on greenhouse gas emissions and thus remain agnostic of whether and how consumers' valuation of other meal characteristics, $\hat{v}_m$, might be influenced by behavioral biases and limited attention.

I impose further structure on g_m by assuming that it is the product of two factors: First, the guilt the consumer perceives when causing one kg of carbon emissions, γ_m, and, second, the emissions the consumer believes to be caused in the production of the meal, $E[e_m]$. I assume this relationship to be linear, since every kg of emissions caused is approximately equivalent in the damages it causes. Consumer guilt over emissions should thus be approximately linear. Inserting this into the above equation for $\hat{V}$, I obtain the following formulation for consumers' perceived valuation of the meal at baseline, $\hat{V}_m^B$.

$$\hat{V}_m^B = (\hat{v}_m - \hat{v}_o) + \theta\gamma(E[e_{im}] - E[e_{io}]) \tag{2}$$

How does the perceived valuation change if we present consumers with well-visible carbon labels at the moment of choice? First, this makes consumers more attentive of carbon emissions. I assume it makes consumers fully attentive, such that $\theta = 1$. Second, consumers update beliefs on the carbon emissions caused by the meal when presented with the true emissions e_m. I allow for the individual's valuation of greenhouse gas emissions caused by the meal to not be immediately replaced with the true valuation as labels are shown. Specifically, I allow the consumer to update in a Non-Bayesian manner as described in Epstein, Noor, and Sandroni (2008): The updated belief is a linear combination of the Bayesian update (the true value, as indicated on the emission labels), weighted by $1-\kappa$, and the consumers' prior belief, weighted by κ. If $\kappa = 0$, the individual is completely Baysian in her updating process. If $\kappa = 1$, she does not update at all after seeing the carbon labels. This yields the following formulation for consumers' valuation of the meal in the presence of labels, $\hat{V}_m^L$:

$$\hat{V}_m^L = (\hat{v}_m - \hat{v}_o) + \gamma\Big((1-\kappa)(e_m - e_o) + \kappa(E[e_{im}] - E[e_{io}])\Big) \tag{3}$$

Now suppose we would only make consumers fully attentive of greenhouse gas emissions, without providing any information on the carbon emissions caused by the meal. This would set $\theta = 1$ without changing consumers' belief of the carbon emissions caused. This yields the following formulation for consumers' valuation of the meal when made attentive, $\hat{V}_m^A$:

$$\hat{V}_m^A = (\hat{v}_m - \hat{v}_o) + \gamma(E[e_{im}] - E[e_{io}]) \tag{4}$$

Finally, suppose consumers could make their consumption choice in the absence of any emissions considerations. Valuation in this condition, $\hat{V}_m^O$, would be described by:

$$\hat{V}_m^O = \hat{v}_m - \hat{v}_o \tag{5}$$

Comparing equations 2, 3, 4, 5, it becomes clear that consumers' perceived valuation will be governed by different parameters across consumption settings. This variation in perceived valuation yields my identification strategy for the structural estimation: The experiment conditions presented in section 3 will proxy the four decision settings above and consumers' meal valuations perceived in the different settings will serve as input to the structural estimation in section 4.

2.2 The effect of carbon labels on consumer welfare

In the subsection above, I describe how consumers perceive their valuation of a given meal in different consumption settings. In this section, I will focus on consumers true valuation, V_m, which will be relevant for the true utility the consumer realizes from her consumption choice. I model this true valuation as:

$$V_m = v_m - v_o + \gamma(e_m - e_o) \tag{6}$$

If the consumer makes a purchase, she reaps the increase in utility $\Delta U = V_m - p_m + p_o$ relative to purchasing the outside option. The consumers' true valuation of the emissions caused by the meal are not influenced by a lack of attention $\theta < 1$ or misperceptions of the carbon impact $E(e_m) \neq e_m$. Instead, the complete and true emissions caused by meal m enter into consumers' utility, weighted by the psychological guilt felt for each kg of emissions caused, γ. 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 guilt accordingly. This assumption is not too far-fetched if we think about how taking a flight was perceived as practically guilt-free in the early 2000s/2010s, but different activist and information campaigns changed the general public view, coining the term "flight shame". This likely not only changed how consumers perceive their current consumption behavior, but also how they look back at past consumption behavior, experiencing ex-post regret about certain consumption patterns. One could also view this assumption as modeling the carbon emissions caused by a meal as similar to its calories: Although the consumer may perceive the calories of a meal to be lower than they are, what he actually cares about are the true calories, and the consequences of a non-optimal choice will become visible at the latest when the consumer steps on the scale.

Consumers base their consumption choice at baseline on perceived and not true valuation, and thus purchase if $\hat{V}_m^B - (p_m + p_o) \geq 0$. This can lead to suboptimal choices in two cases: First, it may be the case that $\hat{V}_m^B \geq (p_m - p_o)$, but $V_m < (p_m - p_o)$. The consumer then purchases the meal although it does not maximize his true utility. Second, it may be the case that $\hat{V}_m^B < (p_m - p_o)$, but $V_m \geq (p_m - p_o)$. The consumer then does not purchase the meal although it maximizes his true utility.

Carbon labels decrease these suboptimal choices, as $\hat{V}_m^L$ is closer to V_m than $\hat{V}_m^B$. Equation 7 describes the expected change in consumer welfare when consumers are shown carbon labels. The first two terms of 7 describe an increase in consumer utility due to consumption choices closer to the optimum. The first term describes expected consumption utility when making choices with carbon labels: The consumer purchases if his perceived valuation for meal m is greater than the price difference to the outside option: $\mathbb{1}\left(\hat{V}_m^L \geq (p_m - p_o)\right)$. If he purchases the meal, he realizes utility $\left(V_m - E[p_m - p_o|\hat{V}_m^L \geq (p_m - p_o)]\right)$, where $E[p_m - p_o|\hat{V}_m^L \geq (p_m - p_o)]$ is the expectation of the relative price $p_m - p_o$ given that the price was low enough for the consumer to purchase. The second term describes expected consumption utility when making choices without carbon labels: The consumer purchases if his perceived valuation for meal m at baseline is greater than the price difference to the outside option. If he purchases, he realizes utility $\left(V_m - E[p_m - p_o|\hat{V}_m^B \geq (p_m - p_o)]\right)$, where $E[p_m - p_o|\hat{V}_m^B \geq (p_m - p_o)]$ is the expectation of the relative price $p_m - p_o$ given that the price was low enough for the consumer to purchase at baseline. The difference between the first and the second term in equation 7 is the additional utility the consumer reaps from making the more utility-maximizing choice in the presence of carbon labels.

Further, carbon labels might impose a fixed psychological cost or benefit F on consumers, independent of psychological costs or benefits corresponding to behavioral changes provoked by the carbon labels.

$$\begin{aligned} E(\Delta W_m) = {} & \mathbb{1}\left(\hat{V}_m^L \geq (p_m - p_o)\right)\left(V_m - E[p_m - p_o|\hat{V}_m^L \geq (p_m - p_o)]\right) \\ & - \mathbb{1}\left(\hat{V}_m^B \geq (p_m - p_o)\right)\left(V_m - E[p_m - p_o|\hat{V}_m^B \geq (p_m - p_o)]\right) + F \end{aligned} \tag{7}$$

2.3 Identification in experiment conditions

In section 3, I describe the experimental conditions I implement to proxy the decision settings described by 2, 3,4, and 5. I observe the left-hand-side of each of these equations, as experiment participants directly indicate their valuation for meals in each of the decision settings. Further, I elicit participants' guesses of the emissions caused by the meals and therefore also observe expected emissions $E[e_m]$ and $E[e_o]$. I also observe true emission values e_m and e_o. Finally, I directly elicit consumers' expected change in welfare upon seeing the labels, providing input for the left-hand-side of equation 7.

Based on these equations, I estimate four parameters in section 4:

- θ is the degree to which the consumer is attentive to greenhouse gas emissions in the absence of labels. The lower θ, the higher the potential for carbon labels to change consumer behavior merely by increasing attention.
- κ describes the extent to which the individual is non-Bayesian in her updating of beliefs of the emissions caused by meal m. The lower κ, the higher the potential for carbon labels to change consumer behavior by correcting misperceptions.
- γ describes how the emission of one kg of greenhouse gas emissions affects an individual's utility: it is the guilt felt per kg of emissions times the importance the individual places on carbon emissions in her decision.
- F describes the fixed psychological cost or benefit the consumer experiences from being shown the carbon labels. While carbon labels might impact consumer welfare through their impact on consumer decisions, they might also have an additional psychological impact on the consumer independent of any changes in consumption behavior provoked through the label. This is captured by F.

3 Lab-in-the-field experiment 1: Mechanisms

Experiment design

Overview. To structurally estimate the parameters of the model outlined in section 2, I need to observe how an individual's valuation for a given meal changes if she is shown carbon labels, if her attention towards carbon emissions is increased and if any considerations about carbon emissions are removed from the decision. I thus follow a within-subject approach, eliciting willingness to pay for a meal multiple times under different conditions. A within-subject approach ensures that nothing differs between the different elicitation except for the treatment.[4] Elic-

4. The mere act of eliciting willingness to pay twice does not have an effect: Lab-in-the-field experiment 2 described in section 5 follows a similar design and also includes a control condition simply repeating the baseline elicitation to check for any effects of eliciting participants willingness to pay for the same meal multiple times. This does not have a significant effect.

iting willingness to pay instead of merely eliciting consumption choices under a given set of prices allows for a precise estimation of consumer valuation. I summarize the most important design choices below and add details in the following subsections.

(1) I allocate participants to the Attent+Label, Attent or Attent+Offset condition: Participants in the Attent+Label condition first indicate their willingness to pay for four meals in the absence of carbon labels, then guess the emissions caused by each meal, and then indicate willingness to pay for the same four meals in the presence of carbon labels. Participants in the Attent condition also guess emissions after the baseline elicitation, but do not see any carbon labels in the second elicitation. For these participants, the change from baseline to second-round willingness to pay measures the effect of drawing participants' attention to carbon emissions, but not providing them with any information on emissions. Participants in the Attent+Offset condition also guess emissions after the baseline elicitation, and then indicate their willingness to pay under the premise that all emissions will be offset.

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

(3) Willingness to pay is elicited relative to an alternative lunch: In each of the 15 meal purchase decisions, participants first decide whether they prefer a given meal or a cheese sandwich. They then indicate how much they are willing to pay to receive the given meal rather than the cheese sandwich, and vice versa if they prefer receiving the cheese sandwich. Willingness to pay for a given meal is thus always measured relative to the cheese sandwich (reflecting the real-world fact that the alternative to not eating something is eating something else). The dependent variable of interest is the **change** in relative willingness to pay between the first and second elicitation.

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

to implement. Labels also state how long of a car drive (in km) would cause the same amount of CO_2 emissions.

(5) Willingness to pay to see or avoid carbon labels is also elicited: Before the final three meal purchase decisions (three new meals), participants indicate whether they would like to see carbon labels on these final decisions, and indicate their willingness to pay to enforce their choice. This elicitation is incentivized. I use these values to analyze effects on consumer surplus.

Experiment timeline. The experiment timeline is visualized in Figure 17. First, the elicitation of willingness to pay is explained to participants and they are shown how their payout and the meal they receive will depend on the choices they make throughout the experiment. They are then asked four comprehension questions, which they must answer correctly before proceeding. Any participant taking more than five attempts in doing so is excluded from the analysis, as pre-registered. Second, participants indicate their baseline willingness to pay for four meals (four questions). Third, participants guess the greenhouse gas emissions caused by different meals (see Figure 1). These include the four meals around which the meal purchasing decisions revolve, as well as six further meals (see Figure 8 for a list). Participants make each of the ten guessing decisions on separate screens, shown to participants in a random order. On each screen, they are always shown the emissions of a reference example meal (Red Thai Curry with pork and rice, causes 1.7 kg of CO_2). This reference meal is not included in any willingness to pay elicitations. An example is shown in Figure 2. The guessing questions are incentivized and timed.[5]

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

- In the 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 5.

5. 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.

- In the ATTENT+OFFSET condition, participants are informed that the emissions caused by their lunch choice will be offset.

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[6], 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 three rounds include four meal purchasing decisions each, constituting a total of 12 decisions. Additionally, three final purchase decisions revolve around three not previously seen meals. Before seeing these decisions, participants are asked whether they would like to see carbon labels for their final three purchase decisions, and indicate how much they are willing to pay such that their preferred display option is implemented. This elicitation is incentivized, as detailed below. Participants' willingness to pay to see or avoid labels is interpreted as the labels' effect on consumer surplus in the analysis, taking a similar approach as e.g. Allcott and Kessler (2019) and Butera et al. (2022). As this element of the experiment is shared with lab-in-the-field experiment 2, results are discussed jointly in section 7 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.

Details on the meal purchasing decisions. Participants make a total of 15 meal-purchasing decisions in the course of the experiment (4 baseline, 4 first-round, 4 second-round and 3 final decisions). Participants who indicate that they are vegetarian are shown only vegetarian meals. In the four repeating decisions, these are: Filled courgettes with potato croquettes, Italian vegetable ragout with pasta, Cheese "Spätzle" with mushrooms and stir-fried vegetables with rice. Participants who indicate that they are not vegetarian are shown two vegetarian and two meat meals for the four repeating decisions: Filled courgettes with potato croquettes, Italian vegetable ragout with pasta, Chicken Schnitzel with

6. In the analyses, I control for whether observations stem from a third-round elicitation. All the main results replicate including only data from the first two rounds.

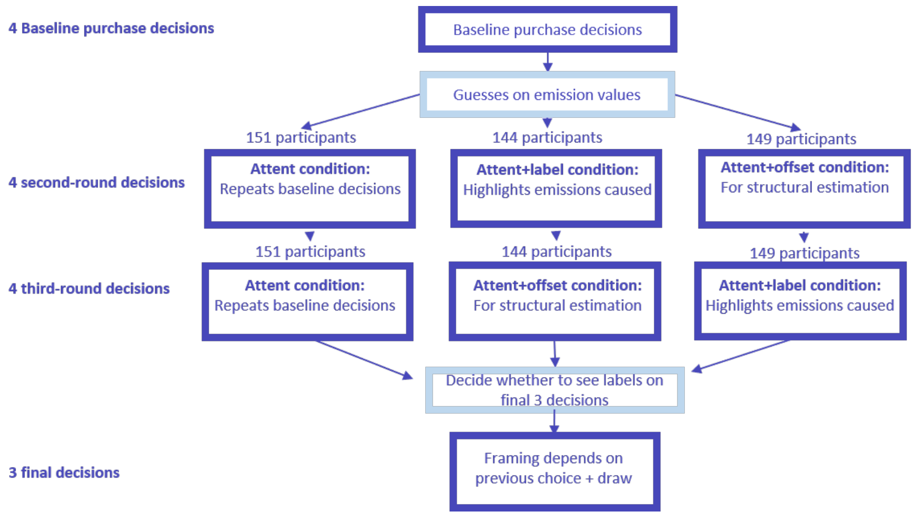

Figure 1. Experiment schedule and treatment groups

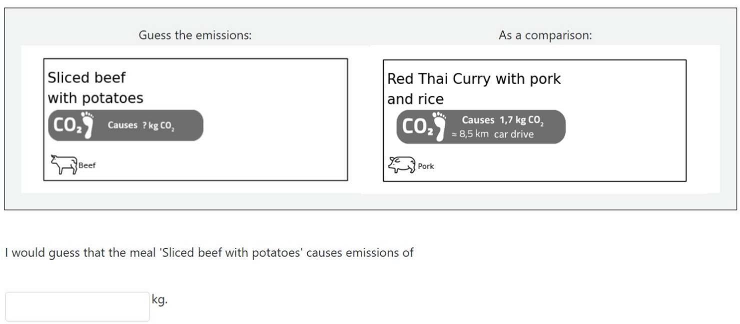

Figure 2. Example guessing questions

rice and beef ragout with potatoes.

The decision set-up

In each decision, participants first choose whether they prefer consuming a certain meal or a cheese sandwich. An example for a baseline decision is shown in Figure 3. 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

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.

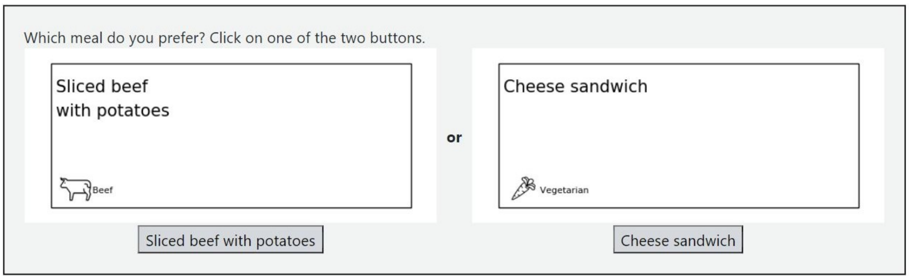

Figure 3. Meal purchase decision example: Step 1 of the purchasing decision

experiment payment they would at most be willing to forego to ensure their preference (see example in Figure 4 in which the participant indicated a preference for Sliced beef in the first step). If participants prefer the specific meal, they indicate how much they are willing to forego to ensure they receive this meal instead of the cheese sandwich. If participants prefer the cheese sandwich, they indicate how much they are willing to forego to ensure they receive the cheese sandwich instead of the specific meal. Any amount between €0.00 Euro and €3.00 can be indicated on a slider in five-cent intervals. I chose €3.00 as the maximum amount since this is the maximum price a student would pay to purchase any of the meals in the student canteen. A willingness to pay of €3.00 or -€3.00 was indicated in less than 3% of all observations.

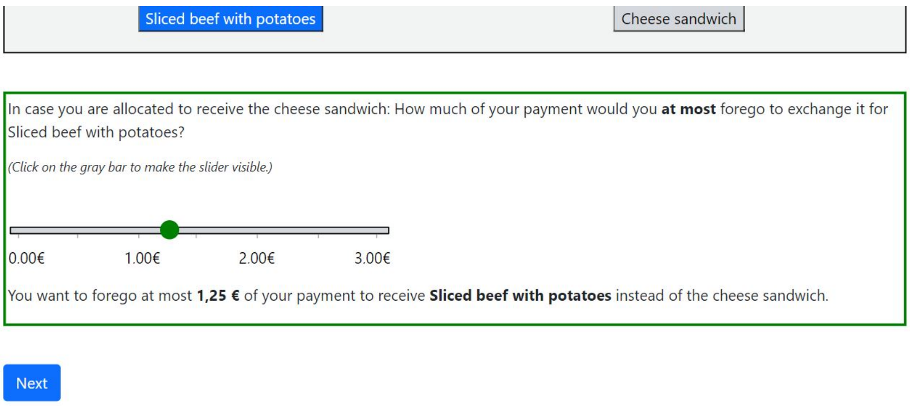

Figure 4. Meal purchase decision example: Step 2 of the purchasing decision

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

Decision framing differs across treatment conditions

In the four baseline decisions, participants do not see any carbon labels, but are merely shown the meal name and the meal's main ingredient (see Figure 3 for an example). The four second-round and four third-round decisions are very similar to the baseline decisions, with the exception that the framing of the decision changes for some of the participants. The four specific meals stay the same across rounds. For participants in the LABEL condition, emission values are added to the meal options. An example is shown in Figure 5. 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 6.[8]

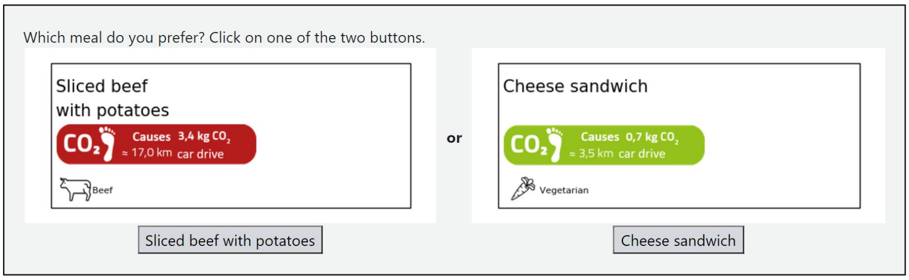

Figure 5. Meal purchase decision example: Decisions with labels

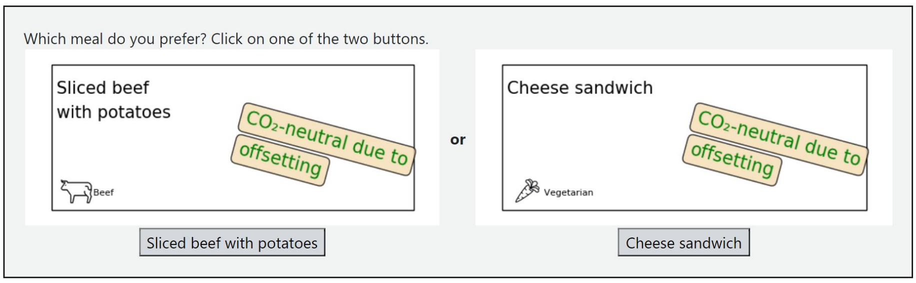

Figure 6. Meal purchase decision example: Decisions with carbon offsetting

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

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 3) or with labels (e.g. as in Figure 5).

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 six days between the 22nd of June and the 8th of July 2021 and was pre-registered ((Schulze Tilling (2021a))). 444 participants participated. Participants were informed in the experiment invitation that vegetarian participants were permitted, but not participants with stricter dietary requirements (vegan, gluten-intolerant, lactose-intolerant, or halal). Participants were informed that the experiment would be conducted online, but that they would be required to make their way to campus afterward to collect their payment in cash. They were also informed that they will be provided with a lunch in addition to their monetary payment and were not given any further information on the purpose of the experiment. The experiment was conducted using oTree software (Chen, Schonger, and Wickens (2016)).

When participants picked up their meal, it was warm, ready-to-eat, and could be consumed on the spot, as shown in Figure 7. Meal options were catered by the student canteen and were meals typically offered in the student canteen. The meal purchasing decisions participants made in the experiment thus closely mimicked real-life meal purchasing decisions at lunchtime. I calculated the emissions caused by each meal with the application Eaternity Institute (2020), using recipes provided by the student canteen.

Figure 7. Gazebo set up on University campus to provide participants with their payment in cash and the meal or sandwich corresponding to their choice, while adhering to Covid regulations.

Incentivization. The **meal purchasing decisions** are incentivized as follows: At the beginning of the experiment, participants are informed that one of the 15 decisions will be implemented. They are not told for which of these this will be the case. It is thus in their best interest to treat each decision as the relevant one. For the relevant decision, the willingness to pay elicitation is incentivized with an adapted BDM mechanism. There is a 50% probability that the specific meal and a 50% probability that the cheese sandwich is randomly drawn as the default meal. If the default meal and the preferred meal indicated in the first part of the decision (e.g. Figure 3) 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 4) is equal to or above the price drawn, the price is deducted from the participants' payment and participants are provided with the preferred option. If willingness to pay is below the price drawn, participants are provided with the less preferred option and no amount is deducted from participants' payment. The outcome lunch is provided to participants directly after the experiment, together with participants' payment in cash. For this purpose, experiment participants are required to travel to the university campus immediately after completing the experiment. Less than 4% did not pick up their cash payment and meal. The incentivization structure was explained to participants and they were required to pass an extensive comprehension check, which less than 4% of participants did not pass.

This **willingness to pay for seeing labels elicitation** is incentivized with a similar BDM mechanism. There is a 50% probability that the default option is that choices are displayed with, and a 50% probability that the default option is that choices are displayed without labels. If the default display option and the preferred display option coincide, the preferred display option is implemented at zero price. If the two do not coincide, a price is randomly drawn at which the display option can be changed. Each value between €0,00 and €3,00 Euro can be drawn with equal probability, in five-cent steps. If the willingness to pay indicated by the participant in the second part of the decision (similar to Figure 4, with display options instead of meals) is equal to or higher than the price drawn, the preferred display option is implemented. The price drawn is only deducted from participants' payment if one of the final three meals is relevant for pay-out. If the willingness to pay is lower than the price drawn, the less-preferred display option is implemented.

Data and results

Some observations were excluded as pre-registered [9]. 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 1. All characteristics are balanced across treatment assignments, i.e. I fail to reject significant differences between groups.

Table 1. Lab-in-the-field experiment 1 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 1 were asked to guess the emissions caused by different meals. Further, the 71 participants in the "Control, then Control" group in experiment 2 also estimated greenhouse gas emissions towards the end of the experiment (see section 5 for details). Figure 8 draws on both these data sources and displays how average guesses deviated for each of the meals. On average, participants rather underestimate emissions (green-colored dots) and overestimate emissions for some low-emission meals (red-colored dots).

In the next step of the analysis, I combine individual and meal-specific under- or overestimation of emissions with the corresponding treatment effects. If the correction of misperceptions was driving the effect of the carbon label and a given individual underestimated the emissions of a certain meal, one

9. Schulze Tilling (2021b)

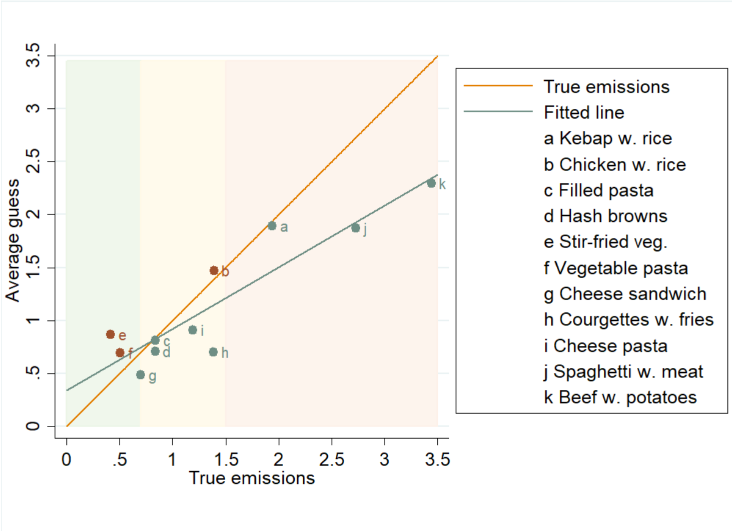

Figure 8. Average guess of the emissions caused by a given meal, plotted against true emissions. Values closer to the orange line were more precisely estimated. Meals corresponding to orange scatter points were on average overestimated in their emissions, while meals corresponding to green scatter plots were on average underestimated. The fitted line is described by y = 0.39 + 0.57 x, with both the intercept and the coefficient significant at $p < 0.01$. Values based on guesses made by the participants of the second lab-in-the-field experiment and the participants in the "Control, then Control" group of the first lab-in-the-field experiment. An exception is the meal "Spaghetti with meat" which is only included in the guessing questions of the first lab-in-the-field experiment. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. This leave a total of 4,261 observations made by 490 participants. The graph background is colored in green, yellow and red to show the label color assigned to the respective meals. Non-vegetarian participants make consumption decisions on the four meals "Chicken w. rice", "Vegetable pasta","Courgettes w. fries" and "Beef w. potatoes". Vegetarian participants made consumption decisions on the four meals "Stir-fried veg.", "Vegetable pasta","Courgettes w. fries" and "Cheese pasta"

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 9 shows how the effect of labels on willingness to pay differs depending on estimation. If emissions were underestimated, willingness to pay on average decreases by €0.19, while if emissions were overestimated, willingness to pay on average decreases by €0.07. Table 2, Spec. (1) shows that this difference in the decrease in willingness to pay is significant at the 1% level. Spec. (2) does not group observations by previous under- or overestimation but instead regresses the change in willingness to pay on the degree of underestimation (in kg). This specification suggests that seeing labels on average decreases willingness to pay by €0.16, with an additional decrease of €0.07 for each kg by which emissions were underestimated. This suggests that part of the effect of the labels can be explained through a correction in misperceptions on carbon impact: 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.

I replicate the analysis in Figure 9 including (a) only individuals who did an above-average job at guessing the relative emissions of at least three of the four meals correctly (Figure B.3 in the Appendix) and (b) only individuals who did an above-average job in guessing emission magnitudes (Figure B.5 in the Appendix). Patterns look similar to Figure 9.

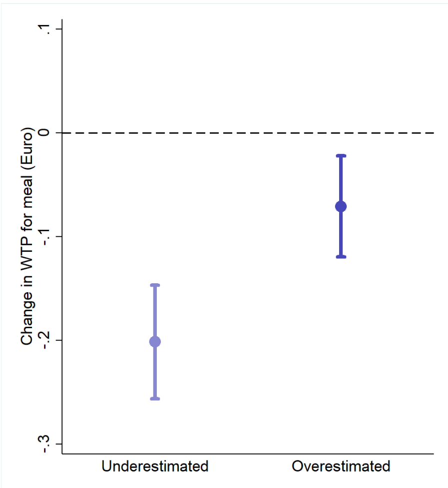

Figure 9. Within-subject change in willingness to pay for a specific meal when shown carbon labels, depending on whether the participant previously over- or underestimated the difference in emissions between the specific meal and the cheese sandwich. Participants are all in the "Attent+Label" condition. Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.13***	
	(0.04)	
Underestimation (in kg)		-0.07***
		(0.02)
Control for third round	0.05	0.07
	(0.05)	(0.05)
Constant	-0.10***	-0.16***
	(0.04)	(0.03)
Participants	293	270
Obs. underestimate	555	515
Obs. overestimate	562	494
Observations	1,117	1,009

Standard errors in parentheses

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

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

The effect of directing attention: Participants might in general be knowledgeable about the carbon emissions caused by different meals, but not be attentive to these at the moment of choice. In the ATTENT condition, participants' attention was drawn to the issue of carbon labels when participants guessed the emissions caused by each meal. However, they were not informed about the true emissions caused by the different meals 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 10. Simply increasing attention decreases willingness to pay for high-emission meals by €0.08, on average. Providing labels on top of increasing attention leads to an additional decrease of €0.10 for high-emission meals. The decrease in willingness to pay for high-emission meals in the ATTENT condition is driven by decisions for which participants had a relatively good idea of the emissions caused by the meal in question. This is visualized in Figures B.8 and B.9 in the Appendix.

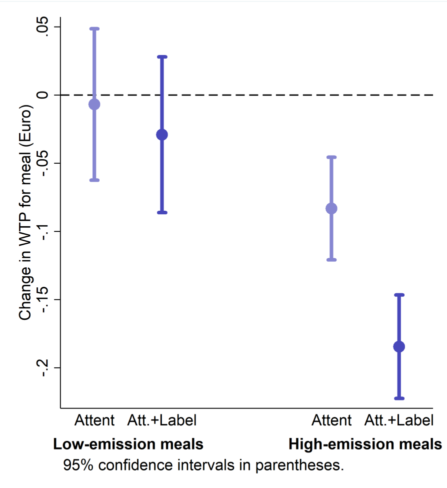

Figure 10. Within-subject change in willingness to pay for a specific meal, comparing participants in the "Attent" and "Attent+Label" condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

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

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

Table 3. Dependent variable: within-subject change in willingness to pay for a specific meal when made attent. Spec. (1) corresponds to Figure 10 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 Structural estimation

In this section, I will review the equations resulting from the theoretical model described in section 2, outline how the experimental conditions in lab-in-the-field experiment 1 described in section 3 relate, and discuss the structural estimation of the model and its results.

4.1 Consumers' valuation of meal options across consumption settings

Section 2 describes in equations 2, 3, 4 and 5 how consumers perceive their valuation for a meal m at baseline, with carbon labels, when made attentive of emissions and in the absence of any emissions considerations. Using data from the experiment described in section 3, I elicit each of these perceived valuations for each participant:

- Perceived valuation at baseline, $\hat{V}^B_{im}$, is the valuation for meal m relative to the outside option o (cheese sandwich) which participant i indicates during the baseline purchase decisions.
- Perceived valuation when shown carbon labels, $\hat{V}^L_{im}$, is the valuation for meal m relative to the outside option o which participant i indicates in the ATTENT+LABEL condition.
- Perceived valuation when made attentive of emissions, but not shown information, $\hat{V}^A_{im}$, is the valuation for meal m which participant i indicates in the ATTENT condition.
- Perceived valuation in the absence of carbon emissions, $\hat{V}^O_{im}$ is proxied by the valuation participant i in the ATTENT+OFFSET condition indicates for meal m relative to the outside option o.

For the purpose of the structural estimation, I rewrite equations 2, 3, 4 and 5 to yield:

$$\hat{V}^L_{im} - \hat{V}^B_{im} = \gamma(E[e_{im}] - E[e_{io}])(\kappa - \theta) + \gamma(e_{im} - e_{io})(1 - \kappa) \tag{8}$$

$$\hat{V}^A_{im} - \hat{V}^B_{im} = \gamma(E[e_{im}] - E[e_{io}])(1 - \theta) \tag{9}$$

$$\hat{V}^O_{im} - \hat{V}^L_{im} = -\gamma(e_{im} - e_{io})(1 - \kappa) - \gamma(E[e_{im}] - E[e_{io}])\kappa \tag{10}$$

I assume that the parameters γ, κ and θ are homogeneous across participants.

4.2 The effect of carbon labels on consumer welfare

The change in consumer welfare which consumers experience from carbon labels is described in equation 11 taken from section 2.

$$\begin{aligned} E(\Delta W_m) \;=\; & \mathbb{1}\Big(\hat{V}^L_m \;\geq\; (p_m - p_o)\Big)\Big(V_m - E[p_m - p_o|\hat{V}^L_m \;\geq\; (p_m - p_o)]\Big) \\ & -\mathbb{1}\Big(\hat{V}^B_m \;\geq\; (p_m - p_o)\Big)\Big(V_m - E[p_m - p_o|\hat{V}^B_m \;\geq\; (p_m - p_o)]\Big) + F \end{aligned} \tag{11}$$

In the experiment context, whether $\hat{V}^L_m \geq (p_m - p_o)$ will directly depend on the price draw for $p_m - p_o$, that is, the consumer will purchase the meal with probability $Prob\Big(\hat{V}^L_m \geq (p_m - p_o)\Big)$. I rewrite equation 11 accordingly:

$$\begin{aligned} E(\Delta W_m) \;=\; & Prob\Big(\hat{V}^L_m \;\geq\; (p_m - p_o)\Big)\Big(V_m - E[p_m - p_o|\hat{V}^L_m \;\geq\; (p_m - p_o)]\Big) \\ & -Prob\Big(\hat{V}^B_m \;\geq\; (p_m - p_o)\Big)\Big(V_m - E[p_m - p_o|\hat{V}^A_m \;\geq\; (p_m - p_o)]\Big) + F \end{aligned} \tag{12}$$

In the experiment described in section 3, I elicit subjects' willingness to pay to see carbon labels during their final three consumption decisions. This value will serve as a proxy for $E(\Delta W_m)$, the change in consumer welfare consumers expect from seeing carbon labels on their decisions. However, note that experiment participants are already attentive of carbon emissions (either due to previous treatment or some just through the mere act of asking for willingness to see or avoid the labels). [10] The changes in consumer welfare which participants expect through the carbon labels should thus depend on their perceived meal valuation seeing carbon labels $\hat{V}^L_m$ relative to their perceived meal valuation when attentive of emissions, $\hat{V}^A_m$.

I thus describe below $E(\Delta^{A-L} W_m)$, the change in consumer welfare a participant expects to incur from now making decisions in the presence carbon labels, relative to the previous state of making decisions merely attentive of carbon emissions.

$$\begin{aligned} E(\Delta^{A-L} W_m) \;=\; & Prob\Big(\hat{V}^L_m \;\geq\; (p_m - p_o)\Big)\Big(V_m - E[p_m - p_o|\hat{V}^L_m \;\geq\; (p_m - p_o)]\Big) \\ & -Prob\Big(\hat{V}^A_m \;\geq\; (p_m - p_o)\Big)\Big(V_m - E[p_m - p_o|\hat{V}^A_m \;\geq\; (p_m - p_o)]\Big) + \hat{F} \end{aligned} \tag{13}$$

In the experiment, relative meal prices $p_m - p_o$ are drawn from a uniform distribution, with each value between -3 and 3 being equally likely, in

10. Willingness to pay to see carbon labels does not significantly differ depending on previous treatments.

five-step intervals. Thus, $Prob(p \leq x) = (x+3)/6$. Similarly, $E[p|p \leq x] = (x-3)/2$

Inserting this above:

$$
\begin{aligned}
E(\Delta^{A-L} W_m) \ = \ & \Big((\hat{V}^L_m + 3)/6 \Big) \Big(V_m - (\hat{V}^L_m - 3)/2 \Big) \\
& - \Big((\hat{V}^A_m + 3)/6 \Big) \Big(V_m - (\hat{V}^A_m - 3)/2 \Big) + \hat{F}
\end{aligned}
\tag{14}
$$

Finally, I rewrite equation 14 using the definitions for V_m, $\hat{V}^L_m$ and $\hat{V}^A_m$ from section 2, such that equation 15 becomes a function entirely of participants' indicated willingness to pay to see the labels ($E(\Delta^{A-L} \hat{W}_m)$), emissions caused by meal m relative to outside option o ($e_m - e_o$), willingness to pay indicated for meal m at baseline ($\hat{V}^B_{im}$), emissions guessed for meal m ($E[e_{im}] - E[e_{io}]$), and the parameters γ, κ, θ and F.

$$
\begin{aligned}
E(\Delta^{A-L} \hat{W}_m) \ = \ & \frac{1}{6} \left(\hat{V}^B_{im} - \theta\gamma(E[e_{im}] - E[e_{io}]) + \gamma\big((1-\kappa)(e_m - e_o) + \kappa(E[e_{im}] - E[e_{io}])\big) + 3 \right) \\
\times & \Bigg(\hat{V}^B_{im} - \theta\gamma(E[e_{im}] - E[e_{io}]) + \gamma(e_m - e_o) - \frac{1}{2} \left(\hat{V}^B_{im} - \theta\gamma(E[e_{im}] - E[e_{io}]) + \gamma\big((1-\kappa)(e_m - e_o) + \kappa(E[e_{im}] - E[e_{io}])\big) - 3 \right) \Bigg) \\
& - \frac{1}{6} \left(\hat{V}^B_{im} - \theta\gamma(E[e_{im}] - E[e_{io}]) + \gamma(E[e_{im}] - E[e_{io}]) + 3 \right) \\
\times & \Bigg(\hat{V}^B_{im} - \theta\gamma(E[e_{im}] - E[e_{io}]) + \gamma(e_m - e_o) - \frac{1}{2} \left(\hat{V}^B_{im} - \theta\gamma(E[e_{im}] - E[e_{io}]) + \gamma(E[e_{im}] - E[e_{io}]) - 3 \right) \Bigg) + \hat{F}
\end{aligned}
\tag{15}
$$

Equations 9, 8, 10 and 15 yield four equations with four unknowns: θ, κ, γ and F. I estimate the four equations simultaneously by GMM using data from lab-in-the-field experiment 2. Observations from participants in the Attent condition inform equation 9 (N=151), observations from participants in the Attent+Label condition inform equation 8 (N=293), and observations from participants in the Attent+Offset condition inform equation 10 (N=293). Observations from all participants (N=444) inform equation 15.

4.3 Results

Results are shown in Table 4. Column 1 shows the results of estimating the model as described above. θ, the average attentiveness to greenhouse gas emissions in the absence of carbon labels, is estimated at 12% and insignificant. This suggests that individuals are relatively likely to be inattentive of carbon emissions when making consumption choices in the absence of labels. κ, the extent to which the average individual is non-Bayesian in her updating of beliefs following the labels, is estimated at 0.17 and insignificant. This suggests that individuals update beliefs in a relatively Bayesian manner when shown carbon labels, increasing the potential for labels to correct behavior towards lower emissions. γ describes how the emissions of one kg of greenhouse gas emissions affects an individual's utility. This is estimated as a decrease in monetized utility of Euro 0.11 per kg of emissions caused by the meal chosen. Finally, F is estimated at 0.20 - This implies that participants on average incur a psychological benefit of the value of Euro 0.20 of seeing the labels, independent of changes in consumer welfare attributable to behavioral change provoked by the label.

Columns (2)-(7) show that estimates are robust to alternative specifications of the model. In column (2), I re-estimate the model imposing that $\kappa = 0$, i.e. that individuals are completely Bayesian in updating their beliefs, and obtain similar results. In column (3), I re-estimate the model imposing that $\theta = 0$, i.e. that individuals are completely inattentive of carbon emissions in the absence of an intervention. In column (4), I impose $\theta = \kappa = 0$ and again obtain similar results for γ and F as in column 1. In column (5), I impose $\theta = 1$, assuming that consumers are fully attentive of carbon emissions, even in the absence of labels. In column (6), I impose that consumers are fully knowledgeable of carbon emissions in the absence of labels. Finally, in column (7) I assume that participants are indicating their willingness to pay to see labels assuming behavioral change through the labels will be relative to baseline behavior (and not behavior when attent). The estimate for F is numerically equivalent to the estimate in column 1.

4.4 Intervention comparison based on estimated parameters

As described in equation 14, the model implies that consumer welfare changes in two ways following the introduction of the behavioral intervention: First, the intervention changes consumers' consumption choices, changing consumption utility (first two terms of equation 14). Second, the intervention produces a fixed cost or benefit F, which I estimate at around €0.20. Since I assume that θ, γ, κ and F are homogenous across participants, this second term is homogenous by definition. However, the first term can still be heterogenous across participants and consumption choices, due to differences in the emissions caused by meals and differences in consumers' baseline valuation of the meals and consumers' perception of the emissions caused.

In the following, I use the experiment data to deduce how experiment participants would make typical student canteen choices in the absence of any intervention, as well as under different interventions. Based on the willingness to pay which participants indicated for each of the four meals at baseline, I can deduce how experiment participants would make their consumption choice in a typical canteen setting, i.e. with a meal offer and pricing structure typical at the university of Bonn. In a next step, I additionally make use of the results from Col. (1) of the structural estimation shown in Table 4, participants' emission guesses, and true emissions to estimate the consumption choices which participants would make if they were experiencing an intervention.

Table 4. Structural estimates of model parameters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theta	0.12	0.01				0.26*	0.12
	(0.20)	(0.19)				(0.15)	(0.20)
Gamma	-0.11***	-0.10***	-0.10***	-0.09***	-0.11***	-0.10***	-0.11***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Kappa	0.17		0.10		0.05		0.17
	(0.22)		(0.20)		(0.23)		(0.22)
F	0.20***	0.20***	0.20***	0.20***	0.20***	0.20***	0.20***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	3,216	3,216	3,216	3,216	3,216	3,216	3,216

Standard errors in parentheses

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

Note: Analysis is based on data from lab-in-the-field experiment 1. For each meal, the observations corresponding to the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. Regression does not include a constant, since the estimation follows the model outlined in section 2. Column (1) shows the main estimation, based on equations 9, 8, 10 and 15. Columns (2)-Column (7) each modify the model in Column (1) as follows: Column (2) imposes $\kappa = 0$. Column (3) imposes $\theta = 0$. Column (4) imposes $\theta = \kappa = 0$. Column (5) imposes $\theta = 1$. Column (6) imposes that $E[e_{im}] = e_m$ and $E[e_{io}] = e_o$, i.e. consumers are fully knowledgeable of emissions. Column (7) assumes that consumers base their expected welfare change on the difference between behavior with carbon labels and behavior at baseline.

I estimate the consumption choices participants would make on the following four typical student canteen days:

- Day 1: Canteen offers Filled courgettes with potato croquettes or Chicken Schnitzel with rice at a price of €3.05 each, as well as a cheese sandwich at a price of €1.50
- Day 2: Canteen offers Filled courgettes with potato croquettes or Beef ragout with potatoes at a price of €3.05 each, as well as a cheese sandwich at a price of €1.50
- Day 3: Canteen offers Italian vegetable ragout with pasta (€2.75) or Chicken Schnitzel with rice (€3.05), as well as a cheese sandwich at a price of €1.50
- Day 4: Canteen offers Italian vegetable ragout with pasta (€2.75) or Beef ragout with potatoes (€3.05), as well as a cheese sandwich at a price of €1.50

Figure 11 shows an estimation of how different types of behavioral interventions would impact consumer welfare. I compare an intervention only drawing consumers attention to the issue of carbon emissions (Attent) in food consumption with an intervention only correcting consumers' misperceptions (Know) and an intervention doing both (Labels). First, note that in 98% to 99% of consumption decision cases, the interventions do not impact consumer welfare at all. This intuitively makes sense: For such an intervention to move consumers away from a purchasing decision, it must be the case that consumers' valuation for the meal is sufficiently close to the price at baseline. In my sample, participants' valuation for the student canteen meals is, in over 70% of cases, lower than the student canteen price and participants would prefer a cheese sandwich to a classic student canteen meal. For these participants, there is little room for behavior to be changed through an intervention. Among regular student canteen guests, valuation for these meals would naturally be higher, leaving more room for behavioral change through interventions.

Of the three interventions, carbon labels lead to the largest increase in consumption utility. Increasing attention has a smaller effect, but the average effect is still nine-fold the effect of only correcting misperceptions, as shown in Table 5. Figure 11 shows that the change in utility achieved through making consumers solely attentive is more dispersed than with the combined intervention. In some instances utility change is even slightly negative. This is mainly attributable to meals for which participants on average overestimated emissions. In this case, increasing attention without providing information can make consumers avoid meals which are in fact low in emissions. Solely increasing knowledge can also decrease consumption utility. These negative effects are also attributable to meals for which participants overestimated emissions, but explicable with a different channel: In the absence of any behavioral intervention, the overestimation can partly compensate for particiants' lack of attention towards carbon emissions, and move participants more towards the optimal choice. When the misperception is removed, participants move further away from the optimal choice. There is also one case in which the carbon labels decreased consumption utility: This can be caused by special cases due to slow updating described by the κ parameter.

Figure 12 correspondingly shows the amount of emissions I expect to be avoided per meal under each of the interventions. On average, I estimate that carbon labels avoid 0.0338 kg of emissions per meal, while solely raising attention avoids 0.0267 kg and providing information avoids 0.0036 kg.

These results suggest two conclusions. First, increasing attention has a larger effect in shifting consumption choices than decreasing misperceptions. This can make the effect of merely increasing attention quite effective in reducing emissions. The effect of correcting misperceptions is, in comparison, much more modest. Second, results highlight the complementarities in increasing attention and correcting misperceptions simultaneously. The effect of providing carbon labels is larger than the sum of each of the two interventions in isolation. Providing both interventions prevents either of the two interventions from backfiring: Solely raising attention might lead to a decrease in consumer utility if meals are on average overestimated by consumers. Further, one can think of parameter combinations with very low consumer attention but high overestimation in which solely correcting misperceptions would also lead to a decrease in consumer utility.

Providing both interventions simultaneously can prevent both of these situations.

Table 5. Estimated effect of different policies in the student canteen

	# of choices for:			Utility change				GHGE change
Intervention	sandwich	veg.	meat	Average	SD	Min	Max	Average
None	73.1%	18.1%	8.8%					
Attention	74.4%	18.1%	7.4%	.0009	.0146	-.0790	.2253	-.0267
Knowledge	73.8%	18.1%	8.1%	.0001	.0041	-.0606	.0583	-.0036
Labels	74.1%	18.6%	7.3%	.0016	.0146	-.0016	.2253	-.0338
Carbon tax	74.4%	18.7%	7%	.0015	.0648	-.3130	.2423	-.0423
Meat ban	78.3%	21.7%		-.0358	.1741	-1.3929	.2253	-.1473
Beef ban	78.3%	20.4%	1.4%	-.0133	.1056	-1.3856	.2253	-.0800

Note: Estimated change in consumption choices, consumption utility and greenhouse gas emissions which would be caused by different types of interventions. Change in utility is in €per meal, and change in greenhouse gas emissions is in kg per meal.

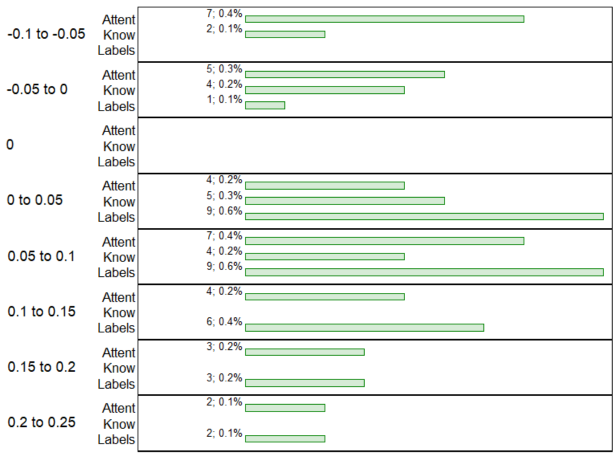

Figure 11. Estimated change in consumption utility per meal which would be caused by solely raising attention, solely correcting misperceptions or the combination of both (labels), in Euro. Figure shows utility changes for instances in which the interventions leads to behavioral change (otherwise change in utility is 0). For the intervention raising attention, this is 2% of instances, for the intervention increasing knowledge it is 0.9% of instances, and for the carbon labels it is 1.9% of instances.

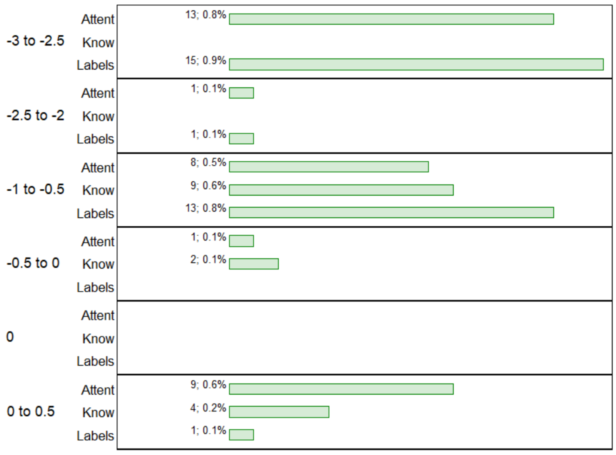

Figure 12. Estimated change in greenhouse gas emissions per meal which would be caused by solely raising attention, solely correcting misperceptions or the combination of both (labels), in kg. Estimation based on experiment data and student canteen prices and offer structure. For the intervention raising attention, this is 2% of instances, for the intervention increasing knowledge it is 0.9% of instances, and for the carbon labels it is 1.9% of instances.

In a next step, I compare the effects of carbon labels to three alternative policy instruments, as shown in Figures 13, 14, 15, 16 and Table 5.

First, I examine the effect of a ban on all meat meals. This has strong negative effects for a larger part of participants, with utility losses of over - €0.5 in over 2% of decisions, as visualized in Figure 13. I estimate emission savings of 0.1473 kg per meal. Second, I examine the effect of only banning the beef meal from the choice set. This produces average emission savings of 0.08 kg per meal, and leads to a much smaller decrease in welfare than the meat ban. Finally, I examine the effects of a carbon tax of €120/Ton. This amount is based on my conclusions from section 5. I assume that tax proceeds are paid back to all consumers in equal shares. This policy decreases consumption utility by a smaller amount for around 11% of decisions, as shown in Figure 15. The average impact on welfare is positive, with an average increase in consumption utility of €0.0014 per meal, slightly less than the carbon labels. The tax reduces average emissions per meal by 0.0423 kg, so it fares better than the carbon labels in this dimension.

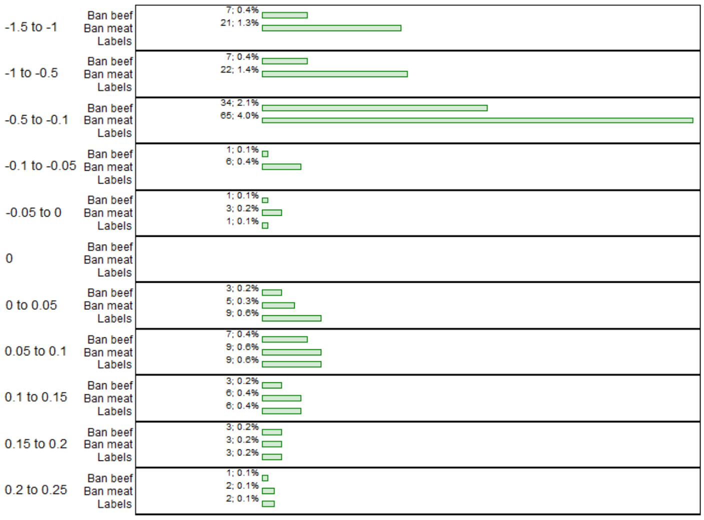

Figure 13. Estimated change in consumption utility which would be caused by a ban of beef or a ban of meat compared to carbon labels, in Euro. Estimation based on experiment data and student canteen prices and offer structure. Figure shows utility changes for instances in which the interventions lead to behavioral change (otherwise change in utility is 0). For the beef ban, this is 4.2% of instances, for the meat ban it is 8.8% of instances, and for the carbon labels it is 1.9% of instances.

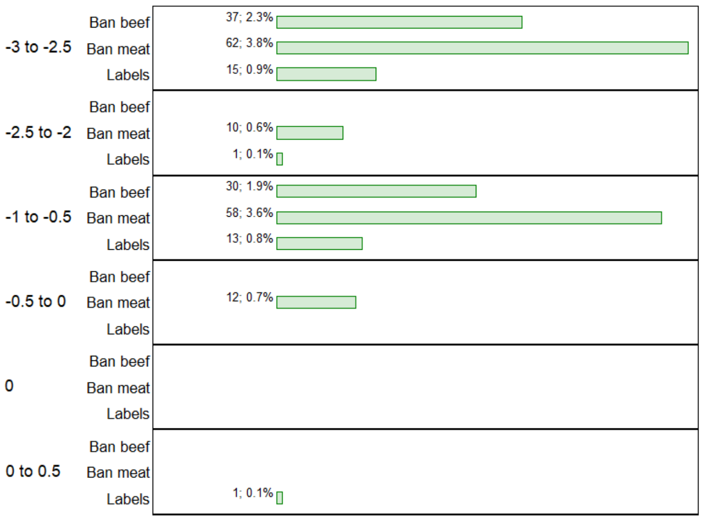

Figure 14. Estimated change in greenhouse gas emissions per meal which would be caused by a ban of beef or a ban of meat compared to carbon labels, in kg. Estimation based on experiment data and student canteen prices and offer structure. Figure shows emission changes for instances in which the interventions lead to behavioral change (otherwise change in emissions is 0). For the beef ban, this is 4.2% of instances, for the meat ban it is 8.8% of instances, and for the carbon labels it is 1.9% of instances.

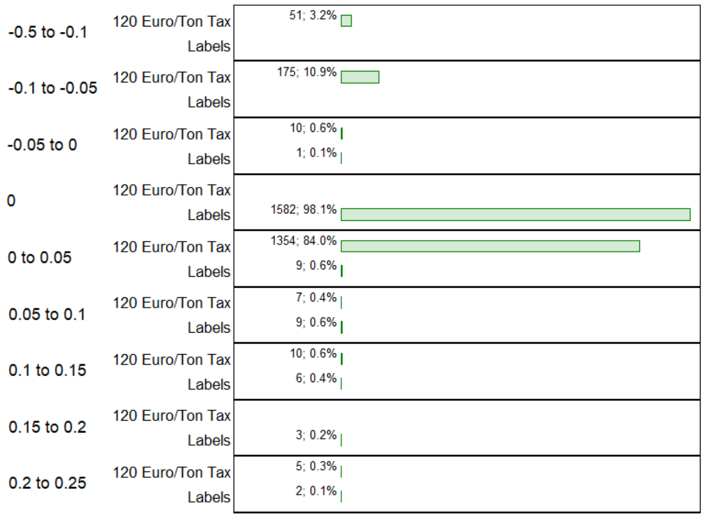

Figure 15. Estimated change in consumption utility which would be caused by a carbon tax of €120/ Ton compared to carbon labels, in Euro. Proceedings from the tax are re-distributed equally to all consumers. Estimation based on experiment data and student canteen prices and offer structure.

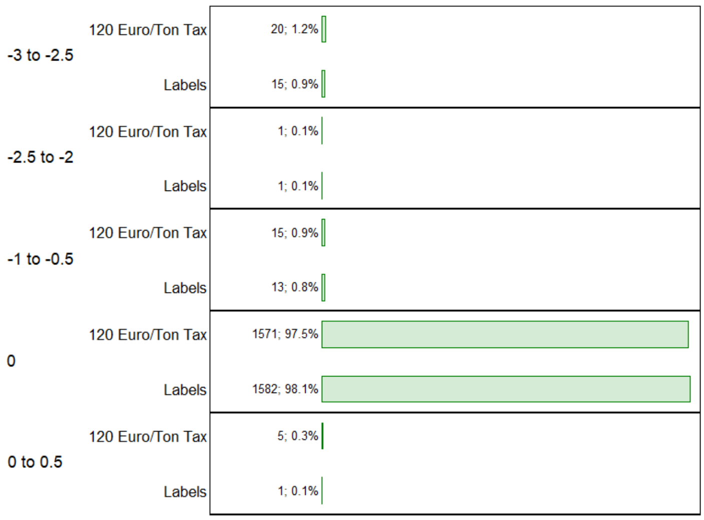

Figure 16. Estimated change in greenhouse gas emissions per meal which would be caused by a carbon tax of €120/ Ton compared to carbon labels, in kg. Proceedings from the tax are re-distributed equally to all consumers. Estimation based on experiment data and student canteen prices and offer structure.

5 Lab-in-the-field experiment 2: Quantifying the effectiveness of labels

The previous sections have identified attentional biases and a misperception of carbon emissions as the main drivers of non-optimal food consumption, and carbon labels as a promising policy tool to correct both of these biases. The following section will test this prediction experimentally, by providing a clean test of the effectiveness of carbon labels, and directly experimentally quantifying their effectiveness relative to a carbon tax. Section 6 will then provide evidence that the conclusions of this section are reconcilable with evidence from the field.

Experiment design

Overview. To cleanly identify the impact of carbon labels as a policy instrument, participants should not be influenced by any other experiment elements. I thus adapt lab-in-the-field experiment 1 by replacing the questions asking participants to guess carbon emissions with guessing questions on items completely unrelated to the consumption choice. Apart from this design choice, lab-in-the-field experiment 2 closely resembles lab-in-the-field experiment 1, with the main design choices being the following:

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

(2) Meal choices, carbon labels and general experiment set-up are the same as in lab-in-the-field experiment 1.

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 answer several incentivized and timed[11] guessing questions on issues unrelated to the consumption choice (e.g. how many bridges cross over the Rhine in the city of Bonn). This keeps the experiment timeline very similar to that of lab-in-the-field experiment 1 without drawing attention to the issue of carbon emissions.

The experiment then proceeds differently depending on the treatment group participants were assigned to by computer randomization. All participants are

11. 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.

again asked to indicate their willingness to pay for the four meals, but the framing of the decision and some characteristics of the decision depend on the treatment condition:

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

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 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.[12]
- 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.[13].

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

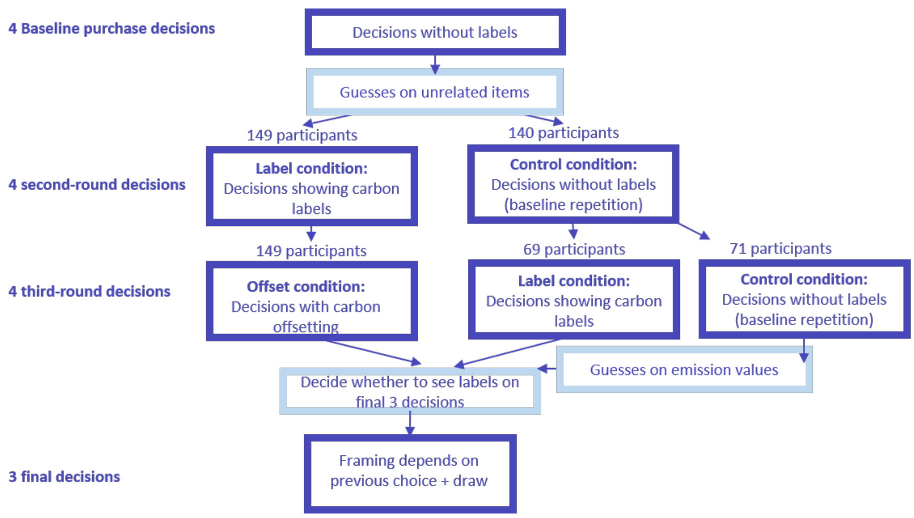

Figure 17. Experiment schedule and treatment groups

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.

12. The results of the OFFSET condition are not the focus of this paper.
13. This data is used for the analysis shown in Figure 8 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.

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)). Participant invitation and experiment set-up was as in the first lab-in-the-field experiment.

Data and results

Some observations were excluded as pre-registered [14]. 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 6. There is a higher proportion of meat-eaters in the group "Control, then Control" (significant at the 5% level). I also perform the main analysis seperately for vegetarian and non-vegetarian participants - These analyses should not be influenced by the higher proportion of meat-eaters in the control group. I find very similar results and thus do not believe that the higher proportion of meat-eaters in the "Control, then Control" group poses reason for concern.

Table 6. Lab-in-the-field experiment 2: 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 17).

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

14. Schulze Tilling (2021b)

compare: 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. In contrast, for meals with emissions higher than the cheese sandwich, emissions are reduced if consumers adjust their demand for these meals downward.

The results show that, for meals with lower emissions than the cheese sandwich, willingness to pay increases by €0.08 on average when participants are shown carbon labels. For meals with higher emissions than the cheese sandwich, willingness to pay in the LABEL condition decreases by €0.29. Changes in willingness to pay for participants in the CONTROL group are not significant and, coefficient-wise, move in opposite directions. Effects are visualized in Figure 18 and detailed in Table 7, specification (1). Specification (2) does not group the four meals into low-emission and high-emission meals but instead regresses the change in willingness to pay on the difference in emissions between the warm meal and cheese sandwich. This specification estimates that on average, willingness to pay decreases by €0.12 for every additional kg of emissions that the warm meal causes on top of the cheese sandwich. This suggests that carbon labels induce a demand effect similar to that of a carbon tax of €120/ Ton. This is four-fold the current German carbon tax on petrol (€30/ Ton).

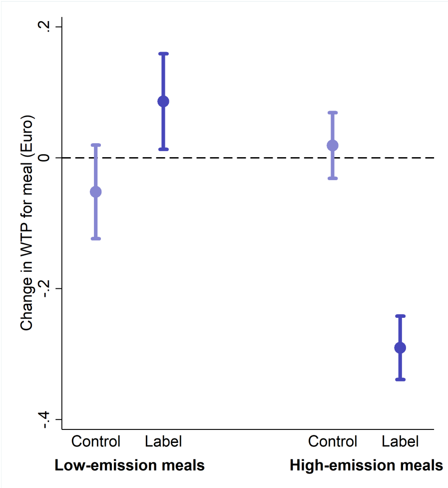

Figure 18. Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich)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

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

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

Table 7. Dependent variable: within-subject change in willingness to pay for a specific meal, compared to baseline. Spec. (1) corresponds to Figure 18 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.

6 Field experiment: Label effectiveness over longer time periods and post-intervention

The results above show that carbon labels reduce emissions and increase consumer welfare in a one-shot consumption setting. The following section investigates whether effects are similar if carbon labels are installed over longer time periods. For this purpose, I conducted a field experiment at the student canteens of the University of Bonn.

Design and setting

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

There are three student canteens in Bonn: The treatment student canteen, the first control restaurant (located 1.7 km from the treatment restaurant), and the second control restaurant (located 4.7 km from the treatment restaurant and frequented much less than the other two restaurants). Menu planning is centralized among the three student canteens, and there is thus a large overlap in the daily offering. All three student canteens offer two main meal components, which differ daily but are mostly the same across student canteens. In addition, each of the student canteens might offer additional options, which are student-restaurant-specific. The larger control restaurant sometimes offers pizza or pasta in addition, and all student canteens might serve 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. [15] Correspondingly, the dependent variable in my main regression is whether the main meal component a restaurant guest chooses contains meat or is vegetarian.

An average student canteen guest visited the student canteen 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 canteen at least 80% of the time. The student canteens 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 canteen 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 canteens would not exist. Switching between student canteens and other gastronomic offers is thus also not frequent. Figure B.10 in the Appendix includes an analysis based on the trackable personal card payments. I classify restaurant guests as "Treatment" or "Control" visitors based on their consumption behavior in the first two weeks. Around 3% of purchases made by "Control" visitors are made in the treated restaurant throughout the entire 14-week period. For "Treatment"

15. The main reason for this was that I wanted to test carbon labeling in a manner that was feasible for the student canteen to implement long-term. While main meal components are planned and known beforehand, sides and 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.

vistors, the percentage fluctuates between 8% and 11%, with no clear trend attributable to treatment. Figure B.11 further examines which percentage of these non-home visits involve consumption of a meat main component. There is no clear trend throughout the study period.

Further, an analysis of daily restaurant guests shows that the labeling intervention did not lead to a decrease in student canteen guests, relative to the control restaurant (see Figures B.13 and B.14). The introduction of carbon labels in the treatment restaurant was displayed as a measure taken by the student canteens themselves, with no connection presented to the University of Bonn or me specifically as the researcher. The introduction of the emission labels was explained on billboards and leaflets available inside the student canteen, as shown in Figure 20. 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 canteen. 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 canteen. The design of the carbon labels was proposed by the student canteen, based on what is technically feasible and possibly implementable as a long-run measure. Examples are shown in Figure 19. They were coded in a traffic-light system, with thresholds determined such that approximately a third of the main components offered by the student canteen during the study period would be classified as green, one-third as yellow, and one-third as red. This corresponded to thresholds of 0,7 kg and 1 kg. [16]

Data and Results

The student canteen provided me with purchase data from April 1st to July 29th, for the three student canteens. I exclude the following purchases from the main analysis[17]

16. Carbon emission labels for a given meal are calculated as the sum of the emissions caused by each of the ingredients. For each ingredient, emission values are calculated "from farm to gate". Hereby, it is assumed that the production process mirrors the average conventional production, e.g. I do not track the specific chicken breast bought by the student canteen, but assume average conventional production. Emissions caused by the student canteen cooling, freezing, and cooking ingredients on-site are not included. These calculation details are explained to students on the student canteen website and on leaflets lying out on-site in the student canteen.

17. These exclusions were not pre-registered, since I did not anticipate these events.

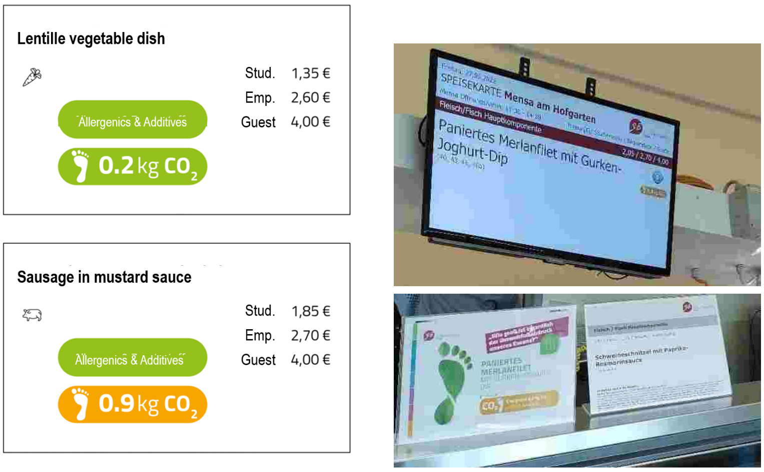

Figure 19. Labels online (left, menu translated from German) and in the student canteen (right)

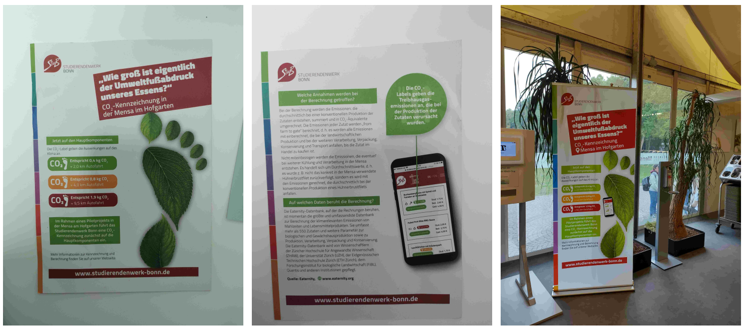

Figure 20. Explanation of the carbon labeling on flyers (left and center) and billboards in the entrance of the student canteen (right).

- During the first week of the label period, display was irregular, as the student canteen needed some "trial end error" to get the system running. On some days, the labels were only displayed in the student canteen or only online. Further, the student canteen had a special "Healthy Campus" week during the first week of May, during which it offered additional extraordinary meals which were also irregularly labeled[18]. It is thus not clear whether the decrease in meat consumption observed during this week was caused by the carbon labels (see Figure B.15 in the Appendix). I thus exclude this week from the main analysis.
- There are seven days on which the treatment restaurant and the larger control restaurant did not offer the same main meal components: 7th of April, 19th of April, 20th of April, 17 of May, 15th of May, 24th of June, 27th of June.

18. The student canteen only communicated this week to me shortly after pre-registration

This is because, although menu planning is centralized, one of the student canteens may not have been delivered an ingredient on time or may realize another ingredient is about to expire and independently adjust its meal offer. Any differences in the choice of the main meal component between treatment and control restaurant on these days is likely mainly influenced by differences in offer rather than by differences in label treatment. I thus exclude these days.

- Starting from week 9 of the treatment period, Ukrainian refugees received meals in the treated student canteen and the larger control restaurant, using specific student canteen cards. I thus identify these sales and exclude them from all analyses. For the treated restaurant, they make up 12% of total sales in week 9,25% in week 10 and between 14% and 18% for the rest of the observation period. For the control restaurant, they make up between 2% and 7% of total sales.

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

The final sample includes 121,071 observations, split between over 2,600 guests. The main analysis focuses on changes in the consumption of meat main meal components. Every day of the observation period, restaurant guests are offered the choice between a meat or a vegetarian main meal component. The emissions caused by the vegetarian main meal component are lower than or equal to the emissions of the meat meal offered on all observation days. The main specification is Col. (3) of Table 8. This specification includes controls for the number of vegetarian dishes, the price difference between vegetarian and meat meal, the total number of options, and the total daily sales, as included in Lohmann et al. (2022). Additionally, I include day-specific effects, which take up a large part of the variation in menu differences across days. I find that the labels decrease the likelihood of choosing the meat meal on offer by 2.1 percentage points, or 5.1% of the baseline likelihood. Figure 21 shows an event study of treatment effects, using the main specification. Col. (4) of Table 8 shows an intention to treat analysis at the individual level, including individual fixed effects. For this analysis, I restrict the sample to guests who payed with their individual student canteen card (this is 2/3 of the sample). This makes it possible for me to track the consumption behavior of a single individual across time. Further, I restrict the sample to guests who visited the student canteen at least ten times during the 13-week period (70% of the remaining sample), to enable a sufficiently powered estimation of individual fixed effects. Finally, for this analysis I classify individuals as belonging to the treated or control group based on their consumption behavior in the four-week pre-intervention period. Guests thus have to visit one of the student canteens at least once during this period and a "home" student canteen has to be clearly identifiable. I thus only include guests who visit the same student canteen at least 80% of the time during the pre-intervention period (85% of the remaining sample). This analysis identifies an intention-to-treat effect of 1.5 percentage points, significant at the 10% level.

All specifications in Table 8 identify strong post-intervention effects. Time trends are visualized in an event study graph in Figure 21. The effectiveness of the labels seems to increase throughout the labeling period. One explanation for this might be that perhaps restaurant guests do not notice the carbon labels immediately, but only at their second or third visit to the student canteen, e.g. when friends draw their attention to the labels. The magnitude of the identified post-intervention effects is comparable to the treatment effects estimated for the later weeks of the intervention period.

For an analysis of the impact on average greenhouse gas emissions per meal, I restrict the sample such that it only includes days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period: a day in the pre-intervention period where the same two main meal components were served. Further, I drop any sales not related to the two main components shared between treatment and control restaurants. The reason for this restriction is that the average emissions per meal vary a lot between days due to a changing offer. As vegetarian consumption is, at baseline, higher in the treated than in the control restaurants, a less restricted analysis might falsely attribute changes in meal offer to the label. [19] The restricted sample contains 33,711 observations. As shown in Table B.9 in the Appendix, I estimate that labels reduce average emissions per meal by 24 grams or around 3% of the emissions of a baseline meal.

Table B.7 in the Appendix examines treatment effects in different subsamples, using Spec. (3) of Table 8. Treatment effects are similar when restricting the sample to only employees (col. 2), to off-peak visit hours (col. 3), to purchases made with an individual payment card (col. 4) and to restaurant guests paying by individual card and visiting the student canteen rather frequently (at least ten times during the 13-week period, col. 5). Table B.8 shows analyses restricting the sample to guests who pay by individual payment card and for whom I have demographic information (around 1,411 guests). This analysis is merely

19. 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.

suggestive, as the number of observations is not sufficiently large to estimate treatment effects in such small subsamples.

Table 8. Field estimates of the effect of carbon labels on vegetarian consumption

	Likelihood of consuming meat (in per.)			
	(1) Meat meal	(2) Meat meal	(3) Meat meal	(4) Meat meal
Treatment restaurant x Label period	-2.09***	-2.07***	-2.07***	-1.53*
	(0.74)	(0.74)	(0.75)	(0.92)
Treatment restaurant x Post period	-6.89***	-6.82***	-5.27***	-3.15***
	(0.82)	(0.82)	(0.86)	(1.17)
Treatment restaurant	-10.09***	-9.98***	-13.44***	
	(0.59)	(0.59)	(0.76)	
Label period	0.55			
	(0.42)			
Post period	0.83*			
	(0.47)			
Second veg. main			-3.59***	-3.14***
			(0.51)	(0.64)
Price difference			-5.17***	-5.89**
			(1.90)	(2.44)
Number of meal options			-1.47***	0.99***
			(0.26)	(0.32)
Total daily sales			-0.91***	
			(0.06)	
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	6,936	6,936	6,936	1,949
Guests treated	2,821	2,821	2,821	680
Observations	121,071	121,071	121,071	49,921

Standard errors in parentheses

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

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

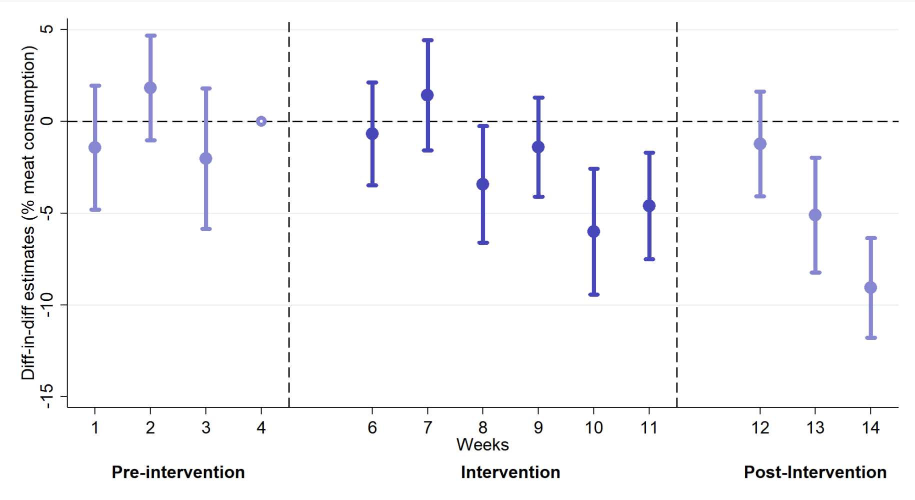

Figure 21. Event study: Difference in difference estimates of likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1-4 constitute the pre-intervention phase, while weeks 6-11 constitute the intervention phase, and weeks 12-14 the post-intervention phase. The regression specification closely follows specification (3) in Table 8, 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.

7 Effects on consumer surplus

The model in section 2 implies that consumer welfare changes in two ways following the introduction of the behavioral intervention. First, it impacts consumers' consumption utility as consumers make more utility-maximizing choices. Second, it produces a fixed cost or benefit to consumers, independent of the carbon labels impact on consumption choices. The following section will draw on evidence from all three experiments to further explore and explain this fixed term.

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

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 5 and serve as an indication of the effect labels have on consumer surplus. The frequency distribution of willingness to pay values is visualized in Figure 22. 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, as shown in Table 9.

Table 10 shows a correlation analysis between willingness to pay to see carbon labels and individual characteristics. Willingness to pay for seeing labels is strongly positively correlated with participants' approval of carbon labels being shown in the student canteen and participants' interest in using this information. It is also 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, Davis, and Dholakia (2016)) is very weakly correlated with willingness to pay to see emission values. Thunström (2019) find a similar, but much stronger relation between the experience of calorie labels and self control. Table 11 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.

7.2 Evidence from the field experiment

After the field experiment was completed, student canteen guests were asked in a follow-up survey whether they would like the labels to be installed permanently. This survey is described as the post-intervention survey in section A in the Appendix. I also describe there the measures I took to limit non-response bias. 73% of the 234 participants were in favor of installing the labels permanently, 18% were not sure and 9% against the measure.

7.3 Discussion of consumer welfare effects

Based on these results, one could hypothesize different possible channels: First, seeing the carbon labels might provide participants with a feeling of being informed and in control of their carbon emissions, which gives them a benefit additional to the change in consumption utility experienced. This idea would be supported by the strong correlation of willingness to pay with participants' self-reported willingness to use the information, and the strong correlation with participants' treatment effect.

Second, the fact that the experiments focus on carbon labels might signal to participants that the avoidance of carbon emissions is socially desirable, and participants are willing to pay to see carbon labels in order to adhere to the social norm. Since consumption decisions in the lab-in-the-field experiment are only observed by the experimenter, this could be described as an "experimenter demand" effect. Participants might then draw consumer welfare from being able to adhere to the social norm. Evidence in favor of this explanation is the positive correlation between participants' willingness to pay to see carbon labels and the perceived strength of social norms. Evidence not in favor of this explanation is the fact that a large proportion of survey participants in the field context were also in favor of a permanent installation of carbon labels in the student canteen. The effects of a carbon label installation would be - following this logic - two-fold: First, they would enable canteen guests to adhere to the social norm more easily, but second, they would also lead to a stronger social norm, making it more costly for canteen guests to deviate from the norm. Thus, an individual mostly concerned about adhering to the social norm rather than about environmental damage caused should have mixed feelings about such an intervention. This is difficult to reconcile with the very positive attitude towards carbon labels captured in the survey. I thus do not focus on social desirability in my model and interpretations. However, note that my model and results could also be interpreted through the lense of a social desirability effect: The factor driving treatment effects is then not that participants' attention is directed towards their feelings of environmental guilt, but attention is directed towards guilt from deviating from the social norm.

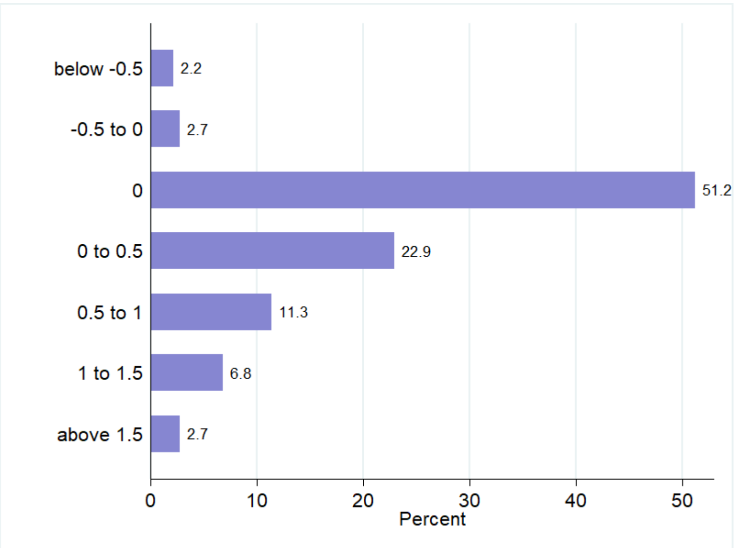

Figure 22. Average willingness to pay to see labels on the final three consumption decisions. Includes data from 733 participants from both lab-in-the-field experiments.

Table 9. 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 10. Correlations between willingness to pay for seeing carbon labels and individual characteristics

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

Standard errors in parentheses

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

Note: Dependent variable: Willingness to pay for seeing labels for the final three consumption decisions. "In favor of labels in student canteen" is measuring using approval of the statement "I would appreciate if the student canteen would introduce such a measure". "Self-reported willingness to use info" is measured using approval of the statement "I would include this information in my decision". "Self-reported confidence in own knowledge" is measured with two questions: (1) approval of the statement "I already know without labels which emissions are caused by different meals", and (2) "I think this information will partially surprise me." Perceived strength of social norms is measured using the procedure developed by Krupka and Weber (2013). Eating self-control is measured using the questions developed by Haws, Davis, and Dholakia (2016).

Table 11. 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.

8 Discussion

This paper has provided evidence that food consumption behavior is influenced by attentional biases and misperceptions about the carbon footprint of different options. While merely directing consumers' attention towards the issue of carbon emissions can already have a significant impact on consumption behavior, additionally correcting consumers' misperceptions complements the direction of attention and leads to more positive effects on consumer welfare and higher emission reductions. Carbon labels are an effective tool to achieve these goals. In the student canteen 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.

In the setting I study, the effects on consumer welfare of providing carbon labels are - both in their impact on consumption decisions and independently of these - overall positive. Further, the fact that a large part of the effectiveness of carbon labels seems to stem from their ability to direct consumers' attention suggests that it can be worthwhile to install carbon labels with relatively knowledgeable populations.

It is important to highlight that this paper does not argue in favor of installing carbon labels instead of implementing more traditional policy measures such as a carbon tax. While I find that carbon labels can be welfare-improving and emissions-reducing, I do not argue or provide any evidence that they are more so than a carbon tax. However, carbon labels seem to be a promising policy measure as long as carbon taxes are not politically feasible.

Further research would be beneficial to test the effectiveness and consumer welfare impact 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. Additionally, further research is needed to investigate how carbon labels interact with other policy interventions, such as nutritional labels.

Appendix A Details on experimental set-up

Survey accompanying field experiment

Pre-intervention survey

During the second week of April, I conducted a survey among student canteen guests at the treatment student canteen and the first, larger, control restaurant. The survey was advertised as an opportunity to voice one's opinion on the offer of the student canteen, took participants around five minutes, and motivated potential participants with the chance to win one of ten 50-Euro coupons for the student canteen. The survey was advertised through multiple channels. First, I put up posters advertising the survey in many faculties throughout the University of Bonn. Second, I distributed leaflets in front of the treatment restaurant and the larger control restaurant, together with research assistants (see Figure A.1). 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 A.1. Leaflet advertising participation in the survey, as distributed in front of the student canteen.

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

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

Appendix B Appendix A3: Additional tables and figures

B.1 Results of lab-in-the-field experiment 2 including only non-vegetarians

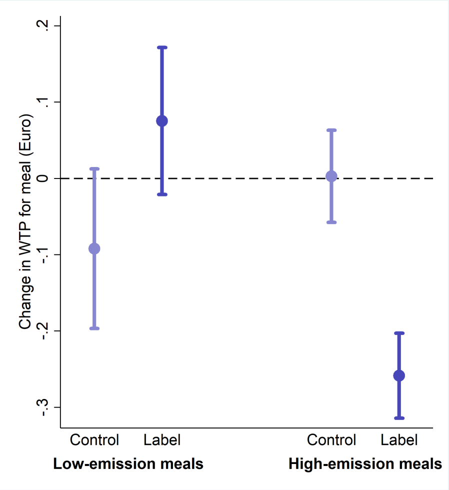

Figure B.1. Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) 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 B.1. Dependent variable: within-subject change in willingness to pay for a specific meal, compared to baseline. Spec. (1) corresponds to Figure 18 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.

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

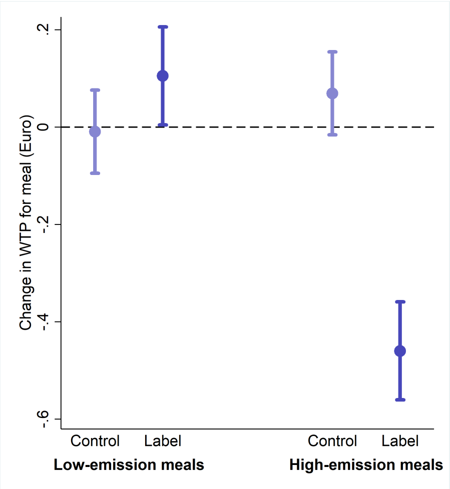

Figure B.2. Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.53***	
	(0.11)	
Low emission meal x Shown label	0.11	
	(0.07)	
High emission meal	0.06	
	(0.05)	
Low emission meal	-0.02	
	(0.04)	
Emissions(kg) x Shown label		-0.75***
		(0.18)
Emissions(kg)		0.08
		(0.08)
Shown label		-0.08
		(0.05)
Control for third round	0.04	0.04
	(0.04)	(0.04)
Constant		0.00
		(0.03)
Participants control	43	43
Participants treated	48	48
Observations	460	460

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

Table B.2. Dependent variable: within-subject change in willingness to pay for a specific meal, compared to baseline. Spec. (1) corresponds to Figure 18 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.

Table B.3. Lab-in-the-field experiment 1: Under- and overestimation of the emissions caused by the decision meals in the "Attention+Labels" group

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

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

Table B.4. Lab-in-the-field experiment 1: Number of under- and overestimations per participant

No. overestimated	0	1	2	3	4	Total
No. underestimated						
0	0	0	0	2	10	12
1	0	1	21	54	0	76
2	1	24	128	0	0	153
3	4	31	0	0	0	35
4	17	0	0	0	0	17
Total	22	56	149	56	10	293

Note: Relative emissions are emissions relative to the cheese sandwich (0.7 kg). I classify a participant as underestimating this amount if their guess for the meal's emissions minus their guess for the cheese sandwich is lower than the actual relative emissions. I classify a participant as overestimating this amount if their guess for the meal's emissions minus their guess for the cheese sandwich is higher than the actual relative emissions. Each cell shows the number of participants with the respective number of under- or overestimations.

B.3 Additional descriptives and robustness tests for under- and overestimation of emissions

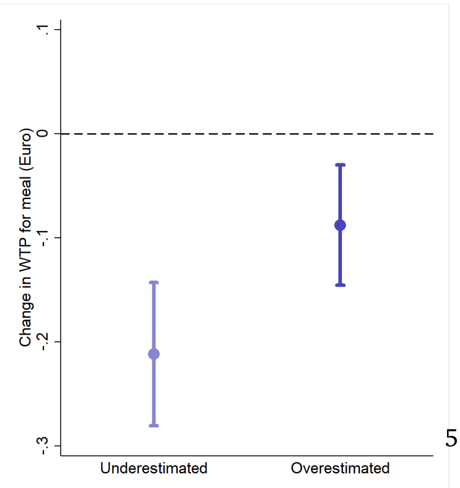

Figure B.3. Replication of Figure 9 including only individuals with at least three correct ranks (194 participants). Bars indicate 95% confidence intervals.

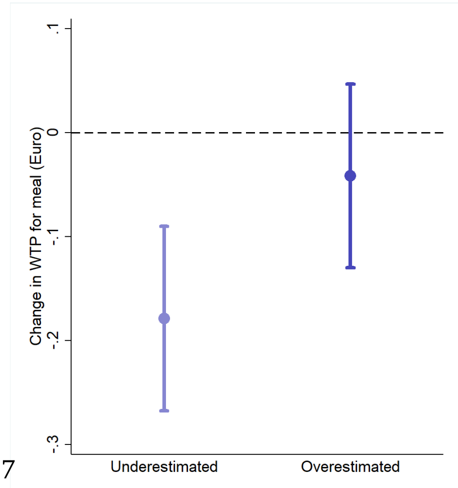

Figure B.4. Replication of Figure 9 including only individuals with at most two correct ranks (99 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

Table B.5. Lab-in-the-field experiment 1: Number of participants who correctly guessed how the four decision meals rank relative to each other

No. of correctly ranked meals	No. participants
0	11
2	88
3	188
4	6
Total	293

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

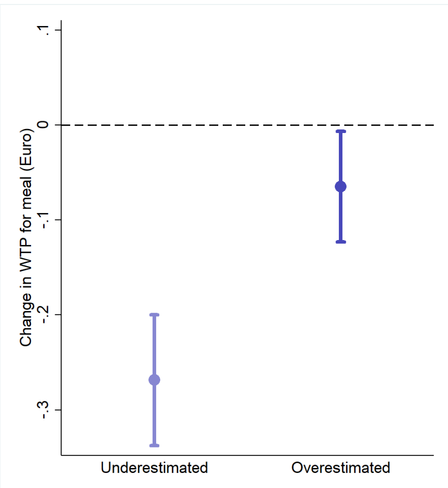

Figure B.5. Replication of Figure 9 including only individuals with at least three correctly guessed magnitudes (171 participants). Bars indicate 95% confidence intervals.

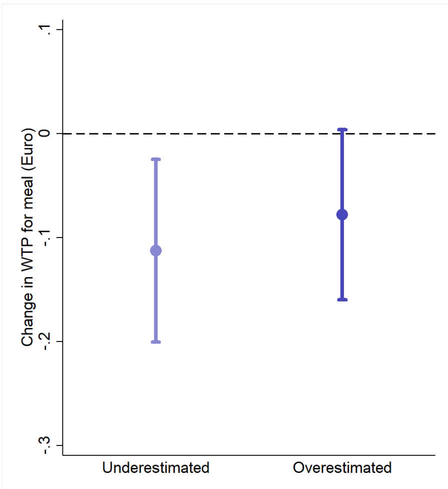

Figure B.6. Replication of Figure 9 including only individuals with at most two correctly guessed magnitudes (129 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

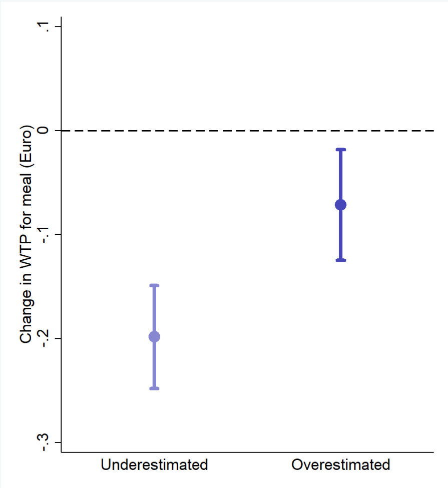

Figure B.7. Replication of Figure 9 based on under- or overestimation of the specific meal, instead of under- or overestimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.13***	
	(0.04)	
Underestimation (in kg)		-0.04
		(0.03)
Control for third round	0.05	0.06
	(0.05)	(0.05)
Constant	-0.09***	-0.18***
	(0.03)	(0.03)
Participants	293	267
Obs. underestimate	651	640
Obs. overestimate	471	376
Observations	1,122	1,016

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

Table B.6. Replication of Table 2 based on under- or overestimation of the specific meal, instead of under- or overestimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals. In specification (2), change in willingness to pay is regressed on underestimation in kg. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped.

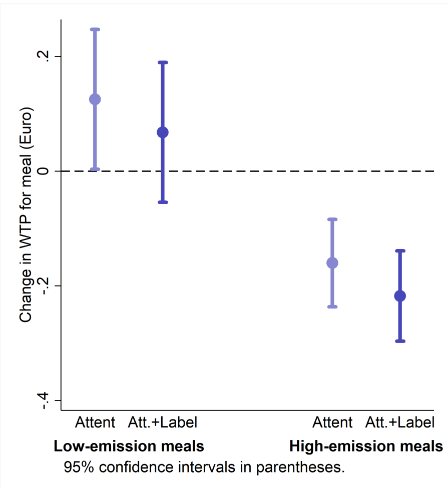

Figure B.8. Replication of Figure 10 including only participant-meal combinations where emissions were guessed accurately enough to receive a bonus payment (guess within 20% of true value, 543 observations). Bars indicate 95% confidence intervals.

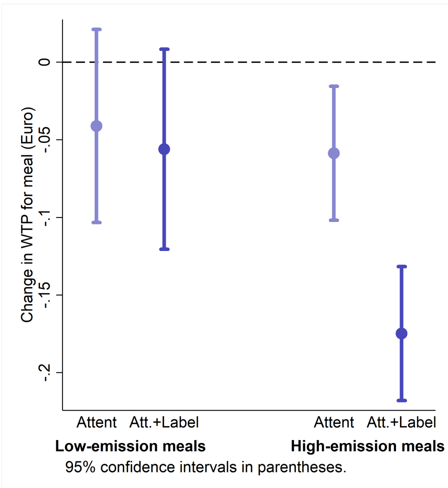

Figure B.9. Replication of Figure 10 including only participant-meal combinations where emissions were not guessed accurately enough to receive a bonus payment (guess not within 20% of true value, 1,837 observations)

Additional descriptives and results for the field experiment

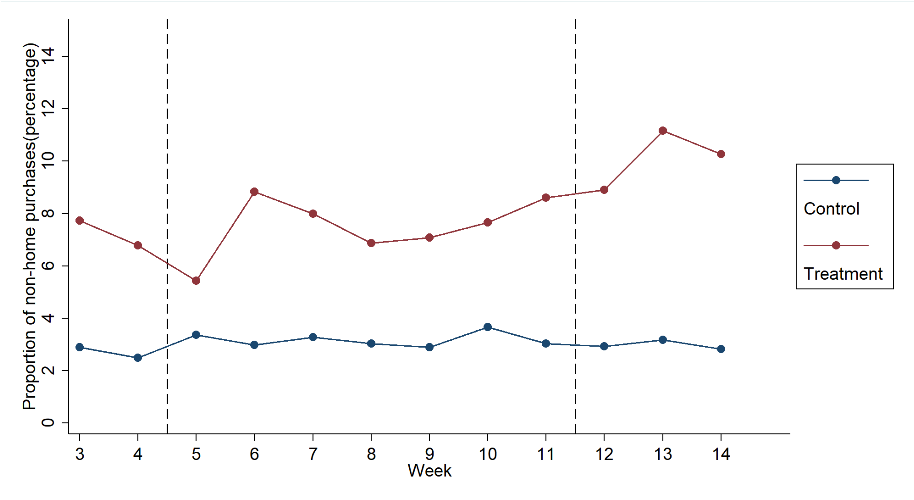

Figure B.10. Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. In accordance with the individual fixed effects sample, the sample includes only payments made by individual card, and only includes guests who visited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

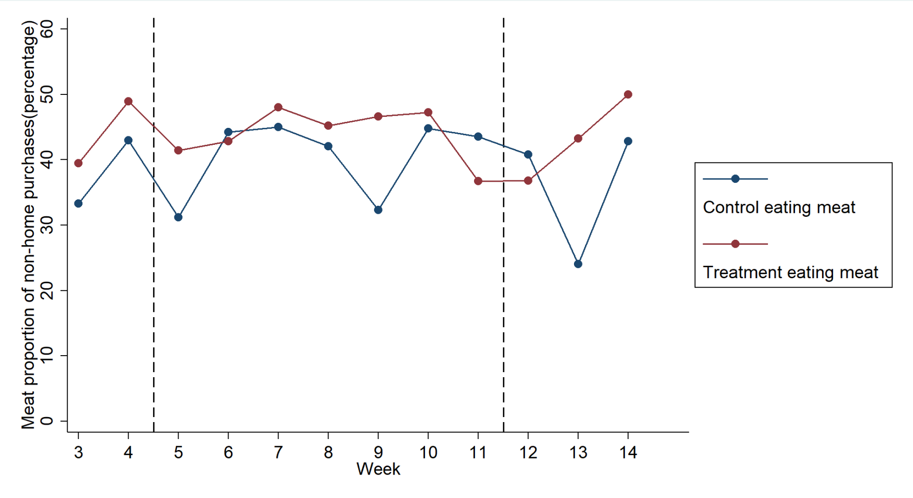

Figure B.11. Proportion of choices for the meat component among non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. In accordance with the individual fixed effects sample, the sample includes only payments made by individual card, and only includes guests who visited at least 10 times during the 12 week period, and at least once in the first two weeks. I further restrict the sample to those 93% of the remaining sample including only individuals who ate at the same restaurant in at least 80% of cases during those first two weeks.

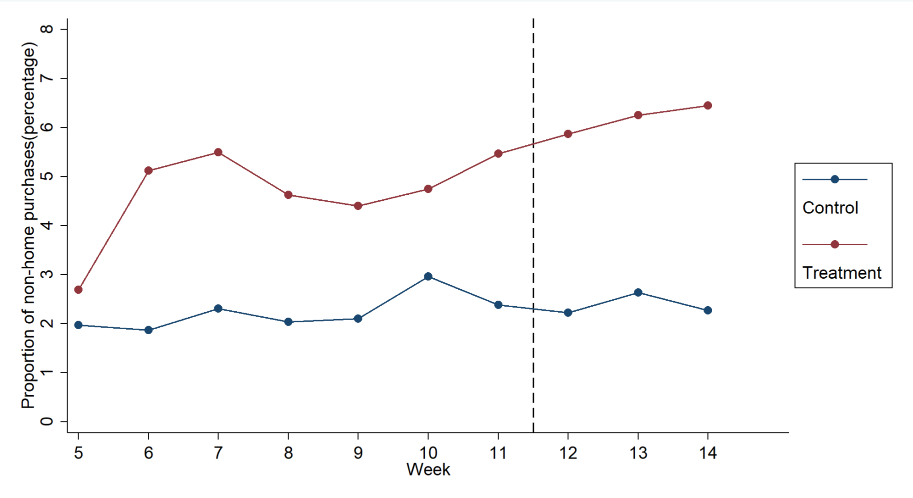

Figure B.12. Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first four weeks. In accordance with the individual fixed effects sample, the sample includes only payments made by individual card, and only includes guests who visited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

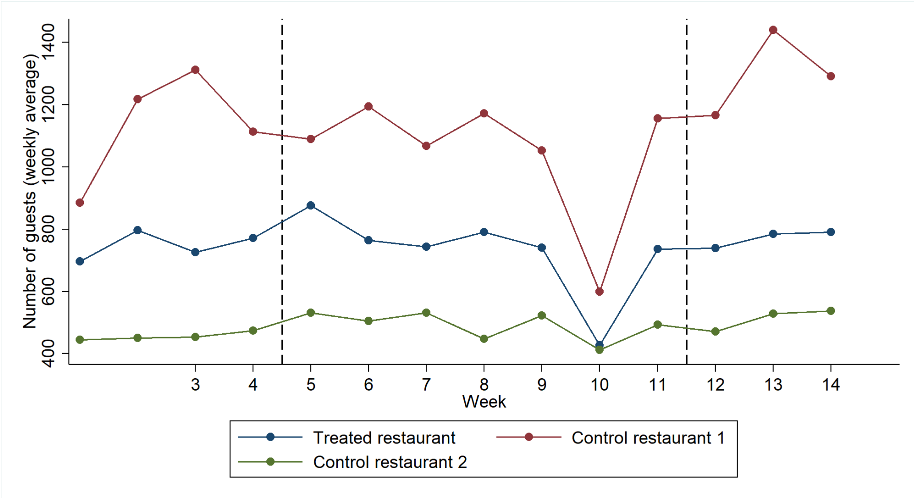

Figure B.13. Number of daily student canteen visitors, weekly average

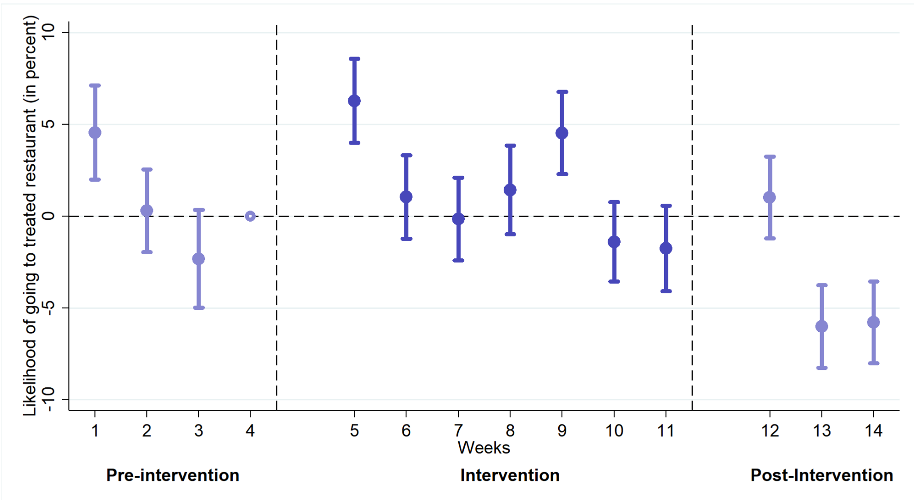

Figure B.14. Event study to check for trends in visiting the treated versus the control restaurant: Difference in difference estimates of likelihood of visiting the treated student canteens relative to the control restaurant (in percentage points) , using week 4 of the pre-intervention phase as a baseline.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

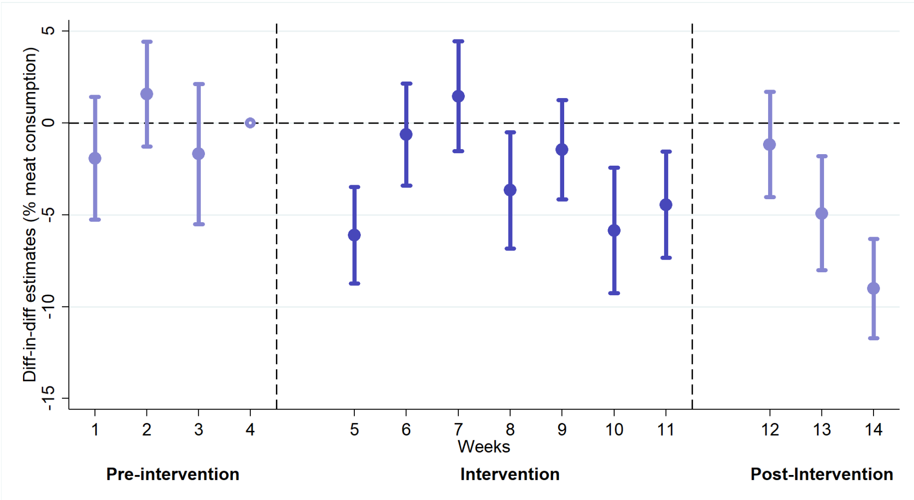

Figure B.15. Event study: Difference in difference estimates of likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. In contrast to the main analysis, this analysis includes week 5. During week 5, the treated restaurant promoted healthy eating. The strong decrease in meat consumption in week 5 is thus not cleanly attributable to the carbon labels. Weeks 1-4 constitute the pre-intervention phase, while weeks 6-11 constitute the intervention phase, and weeks 12-14 the post-intervention phase. The regression specification closely follows specification (3) in Table 8, controlling for the number of vegetarian dishes, the price difference between vegetarian and meat meal, the total number of meal options and total daily sales. It also includes daily fixed effects. Bars indicate 95% confidence intervals.

Table B.7. Effect of labels on vegetarian consumption, different subsamples

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

Standard errors in parentheses

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

Table B.8. Effect of labels on vegetarian consumption, different subsamples

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

Standard errors in parentheses

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

Table B.9. Effect of labels on average emissions per meal

	(1) GHGE (g)	(2) GHGE (g)	(3) GHGE (g)	(4) GHGE (g)
Treatment restaurant x Label period	-16.18	-24.78**	-23.52**	-51.40
	(11.21)	(10.22)	(10.19)	(37.28)
Treatment restaurant	-50.41***	-45.32***	-49.39***	
	(7.40)	(6.70)	(8.23)	
Label period	4.72			
	(6.25)			
Number of meal options			2.93	27.53**
			(3.37)	(13.25)
Total daily sales			-6.83***	
			(1.14)	
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	5,076	5,076	5,076	166
Guests treated	1,998	1,998	1,998	39
Observations	33,711	33,711	33,711	2,365

Standard errors in parentheses

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

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

Table B.10. Effect of labels on vegetarian consumption with restricted sample

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

Standard errors in parentheses

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

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

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