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Essays on behavioral economics: Nudges, food consumption and procedural fairness

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Contents

Introduction

This thesis examines the role of contextual factors for human decision making. In three self-contained papers, I analyze if aspects that are not directly related to the costs or benefits of an option matter for individuals' choices. The research questions underlying the papers are rooted in by now empirically well-tested assumptions of behavioral economics: Humans are not always fully selfish but might be other-regarding in the sense that they care about how others are doing (Cooper and Kagel, 2016), and human decision-making does not always conform to the standard model of carefully weighting the costs and benefits of all possible choices but might be influenced by 'supposedly irrelevant factors' (Thaler, 2015). Such supposedly irrelevant factors and their role for the environmental impact of consumption decisions are explored in papers 1 and 2 of this thesis. In two different field experiments, I examine how small changes in the decision environment affect food choices. As meat consumption is an important determinant of consumption-related greenhouse gas (GHG) emissions, I focus on the impact of the interventions on the choice between meat and vegetarian dishes. Paper 3 investigates one aspect potentially triggering other-regarding behavior, namely procedural fairness concerns, and its role for solving a coordination problem in a laboratory experiment. While laboratory experiments offer a high level of control over the variable of interest and allow for introducing institutional variation that might be hard to obtain outside the clean environment of the lab (Falk and Heckman, 2009), field experiments offer the advantage that subjects and their choices can be observed in their natural environments without them knowing that they are being observed, which is especially important in analyzing the effect of decision environments (Harrison and List, 2004). Both lab and field experimental methods have in common that they allow the identification of causal effects by the use of treatment and control groups, lending high internal validity to the results.

Chapters one and two: Can behavioral interventions help to reduce meat consumption?

In Sweden, around one fourth of GHG emissions emerging from consumption can be attributed to food, and meat consumption is responsible for approximately one third of these emissions (Naturvårdsverket, 2011). With numbers comparable in other western countries, changing dietary patterns towards a more plant-based diet is considered to hold significant potential for reducing consumption-related GHG emissions (Hedenus et al., 2014; Springmann et al., 2016; Tilman and Clark, 2014). However, despite the need of a reduction of meat consumption from the perspective of reaching the politically set climate targets (Bryngelsson et al., 2016), the trend in consumption patterns in Sweden is the opposite. In 2016, a record-high amount of 87.7 kg of meat were consumed per capita (Swedish Agricultural Board, 2017). At the same time, research has identified substantial co-benefits of a reduction in meat consumption in the area of public health. According to Tilman and Clark (2014), shifting from the current omnivorous diet to less meat-intensive diets can reduce the average risk of type 2 diabetes, cancer, mortality from coronary diseases, and all-cause mortality.

But why do people eat so much meat? The simplest answer is that individuals prefer meat to plant-based products. In addition, there is some evidence from the US that meat is associated with socially desirable attributes like strength and power (Rozin et al., 2012). Other factors likely to contribute to the increase in per-capita meat consumption observed in Swedes are that since 1990, both the price of meat has grown slower than average consumer prices, and that the median income has risen.

The traditional answer from an economic policy perspective would be that such a development should be reversed by introducing a Pigouvian tax to incorporate the environmental externalities into consumer prices. While some researchers consider carbon-based taxes on food as a promising tool for reducing meat consumption (Säll and Gren, 2015; Springmann et al., 2017), to date, no country has implemented such taxes and the political feasibility of such a tax is questionable. For example, in Denmark the national Council of Ethics suggested a meat tax in 2016, but the suggestion was immediately criticized and rejected by the governing coalition (Danmarks Radio, 2016).

Another strand of researchers advocates the use of behavioral interventions to reduce the environmental impact of consumption choices (Girod et al., 2014; Sunstein, 2015). Behavioral interventions build on the assumption that humans do not always make their choices in accordance with the model of the fully rational, fully informed homo economicus, but that they are limited in their attention and subject to cognitive biases, use decision heuristics instead of optimization in the classical sense, and act based on habits (see for example Kahneman, 2003; or Thaler, 2015 for an introduction to the underlying psychological assumptions behavioral interventions build on).

The nature of food-related decisions makes it likely that we use such 'mental shortcuts' when choosing what to eat: Although food expenses account for around 12% of household spending in

Sweden, individual purchasing occasions will usually involve rather low monetary stakes. On the other hand, the frequency of making a food-related choice is quite high: According to Wansink and Sobal (2007), we make around 200 decisions related to food each day (what to eat, when, where, how much and with whom), and many of those decisions we take un- or subconsciously. Marteau et al. (2012) argue that a low degree of deliberation makes decision-makers prone to react to environmental stimuli when choosing what and how much to eat. Such stimuli from the food environment emerge from to the way the food is presented or provided, and include amongst others the structure how it is presented, its salience, its packaging and how it is served (Wansink, 2004). Moreover, there is evidence that we base our consumption decisions not only on our own perceptions, but also on what we perceive that others do or think is appropriate to do. Such decision heuristics based on social comparisons have been shown to play a role in consumption domains relevant for the environment such as water and electricity consumption (Allcott and Rogers, 2014). In addition, present bias may also play a role in choosing which kind of food to consume. With present bias, people place disproportionate weight on immediate costs and benefits and undervalue delayed outcomes (Laibson, 1997). In the context of food choice, this could for example mean that immediate gains from the pleasure of consuming high-calorie meals are overvalued compared to the losses from unwanted future weight gain (Wisdom et al., 2010).

Interpreting food decisions from a behavioral perspective opens up new ways for interventions that aim to facilitate more sustainable food choices: First, by altering the environmental stimuli and targeting the automatic associative processes that govern behavior (Marteau et al., 2012), and second, by recognizing present-biased preferences in food choice. In economics, this approach has gained popularity as part of the ‘nudging’ concept and agenda by Thaler and Sunstein (2008, 2003). As they define it,

> “a nudge…is any aspect of the choice architecture that alters people’s behavior in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid. Nudges are not mandates. Putting junk food at eye level counts as a nudge. Banning junk food does not.” (Thaler and Sunstein, 2008, p. 6).

So far, behavioral interventions in the food domain based on nudging strategies mainly aim at increasing healthier food choices. An often-cited advantage of nudges (not only in the domain of health) compared to traditional policy instruments, is that they are quite cheap to design and to implement, at least in terms of costs for the regulator (Thaler and Sunstein, 2008). And at least in the health domain, research largely confirms the effectiveness of interventions that make use of the decision environment: Published studies in general do find them to succeed in their goal (see Arno and Thomas, 2016 for a meta-analysis of experimental studies). However, the evidence for the effectiveness of nudges to increase sustainable food consumption choices is still small (see Lehner et al., 2016 for a review).

A crucial question is thus if nudges proven successful in increasing healthy choices can also succeed in increasing the share of more sustainable food choices. Two factors that could reduce or even prevent the success of behavioral interventions in fostering sustainable choices are a smaller relevance of internal biases such as present bias for the trade-off between the vegetarian and the meat option, and the presence of strong preferences for a good, in this case for meat. Two studies that find no effect of nudges altering the food environment identify strong underlying preferences as the major driver of their result (Wansink and Just, 2016; Wijk et al., 2016). In one case, children opted out from the default option of apples as a side dish and chose French fries. In the other case, increasing the accessibility of whole grain bread compared to white bread did not increase its sales. Apparently, preferences for the goods that subjects were tried to be nudged away from were too strong.

This confirms that food decisions are not fully automatic, but that deliberation and preferences do play an important role. Thus, if preferences for meat are sufficiently strong in a population, a nudge changing the choice environment could have no effect. Moreover, if the trade-off between the meat and the alternative option is not governed so much by "internalities", such an overvaluation of the immediate benefits compared to the future costs, nudges trying to overcome such internalities by for example increasing immediate costs might fail as well. This could for example mean that an intervention that changes the convenience of an option and proved to be successful in the health domain (such as for example in Wisdom et al., 2010) does not show the same effectiveness in the environmental domain.

The first two papers of this dissertation examine the potential of nudging interventions to increase the consumption of vegetarian meals. **Paper 1**, "Nudging to reduce meat consumption: Immediate and persistent effects of an intervention at a university restaurant", reports results of a field experiment using an intervention targeting salience and order effects with the aim to increase consumption of a vegetarian dish. At the treated restaurant, the vegetarian dish was moved from the middle to the top of the menu, and the dish itself was located from behind the counter to a spot visible to the customers. The intervention was conducted in a university restaurant, while a comparable restaurant was used as a control in order to capture common changes in food consumption patterns over time. Using a difference-in-difference strategy to estimate the effects of the nudge shows that the change in the choice architecture significantly increased the share of vegetarian lunches sold by on average six percentage points during the three-month intervention period. When the original setup was reinstalled, the share of vegetarian lunches sold remained around four percentage points higher than prior to the intervention, meaning that the change in behavior due to the nudge was partly persistent. A back-of-the envelope calculation of the effects on GHG emissions from consumption at the restaurant shows that the increase in vegetarian dishes sold both during and after the nudge was in place decreased GHG emissions by around 4.5%. Thus, there is potential for nudging to reduce GHG emissions from food consumption. Moreover, the paper contributes to the debate on the long-term effects of nudges: The effect of the intervention did not only increase over the three-month intervention period, but partly persisted even after the food environment was reversed into its original state. This suggests that the average increase is not due to initial ordering mistakes or a one-off effect of trying vegetarian food. It rather seems like customers learn about the vegetarian option, and some incorporate it permanently into their choice set.

In **Paper 2**, “Nudging à la carte: A field experiment on food choice” (with Christina Gravert), we test how differences in convenience of ordering a vegetarian and a meat option affect consumer choice. In cooperation with an urban lunch restaurant, we design two different menus and distribute them simultaneously, but in different areas, at the restaurant. Across areas, we vary the convenience of ordering the vegetarian and the meat option out of three dishes offered. Rearranging the menu in favor of vegetarian food has a large and significant effect on the willingness to order a vegetarian dish instead of meat. However, this effect decreases over the three-week treatment period. We discuss potential channels through which our intervention might affect behavior and how our results can be interpreted with respect to those channels. Our results demonstrate that small, cheap interventions can be used towards decreasing carbon emissions from food consumption.

The two experiments show that nudging holds some potential for reducing meat consumption, but they also show that it is difficult to generalize the effects of nudges changing the food environment. As nudges using the food environment usually build on subtle manipulations of an existing choice architecture, one can think of many possible interventions. Although the underlying ideas might be similar, there will probably be no two real-world sites so similar that exactly the same nudge could be implemented. Hence, effects might vary in strength across contexts. The same goes for the target audience. Usually, restaurants or public catering sites do not serve a representative sample of the population, but guests select into the sites based on their characteristics. For example, the experiments in this thesis targeted mainly students and academic staff (paper 1) and mostly white-collar urban professionals (paper 2). While price and convenience of the location most likely play a major role for the students and academic staff in experiment 1, restaurant guests in experiment 2 most likely put a higher weight on the food that is offered. It is thus possible that the strength of preferences for meat differ across experiments, which, given previous findings in the literature, will most likely affect the effectiveness of the intervention. Moreover, it can also affect how the development of the effect over time. One hypothesis for future research, based on the results of the two experiments, could be that when preferences for meat are strong in a given population, initial effects of a nudge might not translate into long-term behavior change. However, testing any such hypotheses has to be left for further research.

Policy recommendations with respect to the effectiveness of nudging in reducing GHG emissions should also always point out two caveats that the current research cannot address: First, the possibility that individuals who were nudged into a meat-free dish compensate with additional meat consumption at a later point in time, which could reduce or neutralize GHG reductions. So far, no experimental research has addressed this question. Second, GHG reductions will depend on both the type of meat avoided by the nudge and the type of vegetarian substitutes that are consumed instead. As shown in Paper 1, different types of meat entail largely different GHG emissions per unit consumed, and the GHG emissions from non-meat substitutes vary widely. More research on the substitution patterns of the population targeted by a nudge is needed in order to make predictions that are more accurate on the GHG reduction potential.

Finally, given the seriousness of the challenge to reduce emissions related to food consumption, future research should explore different policy tools to address that challenge and evaluate how they perform in real life. Future research should aim at measuring the efficacy of nudging against other policy instruments and take the results into account when designing a policy toolbox for more sustainable food consumption.

Chapter three: Do procedural fairness concerns affect coordination?

The final chapter of this dissertation examines the role of procedural fairness concerns for solving a coordination problem. We define procedural fairness in relation to the expected monetary payoffs for the individuals involved in a strategic interaction. If such expected payoffs differ, a situation might be perceived as unfair and influence how people choose to behave compared with a fair setting.

The idea that individuals do not only care about how they do themselves but also how others are doing, has been studied widely in both laboratory and field settings (see for example Sobel, 2005 for a review). While people generally seem to care about being disadvantaged or advantaged in terms of outcomes and take such differences into account for their actions, there is less evidence on the role of procedural fairness for decision-making. However, economic interaction is often governed by formal and informal rules that shape expected allocative outcomes. Such "procedures" might be formalized, such as competition laws, or exist in the form of informal rules, social norms or recommendations. In a market setting, such rules are usually designed to create a "level playing field" for participants, but they can also help to overcome inefficiencies arising from coordination problems. Such problems are prominent in everyday interactions, spanning across dimensions such as public good provision, effort choice or volunteering, where optimal choices depend on the choices of others.

Experimental research on coordination problems has shown that coordination failure occurs frequently, but also that people can develop strategies such as the use of focal points in order to coordinate (see for example Camerer, 2003 for an overview). Another strand of research has shown that external mechanisms, such as action recommendations, can enhance coordination (Van Huyck et al., 1992). However, while it has been shown that procedural fairness matters in games of strategic interaction such as the ultimatum game (Bolton et al., 2005), the role of procedural fairness for the success of an external coordination mechanism has not been examined yet. Given that many coordination problems are governed by formal or informal social rules, it is important to know how individuals react to mechanisms that exhibit different degrees of fairness.

In **Paper 3**, “Fairness vs efficiency: How procedural fairness concerns affect coordination” (together with Kinga Posadzy and Andreas Orland) we examine the role of procedural fairness for the success of external recommendations as a coordination mechanism in a laboratory experiment. Using a two-player coordination game where no focal points or other endogenous coordination strategies are available, we provide subjects with a recommendation of taking one of two possible actions. One action is more costly to take than the other, but if all subjects follow their recommendations, coordination failure is avoided. We vary the fairness of the recommendation procedure by varying the ex-ante probability of receiving the costly action. We find that recommendations are always efficiency-enhancing compared to a situation where no external coordination mechanism is used. Even when some subjects receive the costly recommendation in nine out of ten cases, coordination failure is significantly reduced compared to the no recommendations case. We do not find statistically significant differences in terms of payoffs and behavior between the fair and the unfair recommendation mechanism, implying that individuals put a large weight on the efficiency gains from coordination. Although we do not find strong evidence for the relevance of procedural fairness concerns for coordination, this could be due to a feature of the experimental design we use: As subjects were randomized into being advantaged or disadvantaged, they might perceive the decision situation still as fair. Non-random assignment into roles with different expected payoffs is an important aspect for the study of procedural fairness that is left for further research.

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Chapter I

Nudging to reduce meat consumption: Immediate and persistent effects of an intervention at a university restaurant*

Verena Kurz‡

Abstract

Changing dietary habits to reduce the consumption of meat is considered to have great potential to mitigate food-related greenhouse gas (GHG) emissions. To test if nudging can increase the consumption of vegetarian food, I conducted a field experiment with two university restaurants. At the treated restaurant, the salience of the vegetarian option was increased by changing the menu order, and by placing the dish at a spot visible to customers. The other restaurant served as a control. Daily sales data on the three main dishes sold were collected from September 2015 until June 2016. The experiment was divided into a baseline, an intervention, and a reversal period where the setup was returned to its original state. Results show that the nudge increased the share of vegetarian lunches sold by around 6 percentage points. The change in behavior is partly persistent, as the share of vegetarian lunches sold remained 4 percentage points higher than during the baseline period after the original setup was reinstated. The changes in consumption reduced GHG emissions from food sales around 4.5 percent.

Keywords: nudging, field experiment, meat consumption, climate change mitigation

JEL classification: D12, C93, Q50, D03

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1. Introduction

This paper presents results from a field experiment using a nudge with the aim of increasing the share of vegetarian lunches sold at a university restaurant. Changing diets to reduce consumption of meat and dairy is seen as an important part of mitigation efforts to reach a 2-degree climate target (Bryngelsson et al., 2016; Girod et al., 2014). The livestock sector contributes approximately 14.5 percent of global human-induced greenhouse gas (GHG) emissions yearly (Gerber et al., 2013), and meat consumption is causing about one-third of food-related GHG emissions emerging from consumption in Western countries such as Sweden and the United States (Jones and Kammen, 2011; Naturvårdsverket, 2011).[1] Reducing meat consumption is also seen as a way to protect biodiversity, land, and freshwater ecosystems (Machovina and Feeley, 2014; Pelletier and Tyedmers, 2010; Pimentel and Pimentel, 2003). Additionally, it is beneficial to human health, as current levels of meat consumption in most Western countries are higher than dietary recommendations, and high levels of meat consumption are connected with an increase in the risk of colorectal cancer, type 2 diabetes, and cardiovascular diseases (Swedish National Food Agency, 2015). Several recent studies conclude that a reduction in meat consumption can yield significant benefits for both public health and the environment (Springmann et al., 2016; Tilman and Clark, 2014; Westhoek et al., 2014).

However, reducing meat consumption will most likely not be an easy task. In Sweden, where this field experiment took place, per capita consumption has constantly risen since the 1990s to a record-high 87.7 kilograms (kg) per person in 2016 (Swedish Agricultural Board, 2017). Recently, behavioral interventions, mainly in the form of “nudges”, have been suggested as promising, cheap, and nondistortionary tools to initiate changes in consumer behavior toward less carbon-intensive consumption patterns (Girod et al., 2014; Lehner et al., 2016; Sunstein, 2015).[2] A nudge is commonly understood as a soft push toward behavior that is judged to be desirable by individuals or policy makers but that has not been adopted or is

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[1] In general, food is responsible for around one-fourth of the consumption-based emissions of an average US household. For Sweden, emissions from consumption are available not on a household but on an individual basis: approximately 8 tons of CO_2 equivalent (tCO_2e) per capita emerge from private consumption, of which 2 tCO_2e relate to food. Of those, 0.7 tCO_2e can be attributed to meat consumption (Naturvårdsverket, 2011).
[2] Alternative policy instruments such as food consumption taxes based on GHG emissions have been discussed by the scientific community, but implementation is not foreseeable yet (Säll and Gren, 2015; Wirsenius et al., 2011). Carbon labeling has also been discussed as a possibility to reduce meat consumption. See Shewmake et al. (2015) for a theoretical analysis and Visschers and Siegrist (2015) and Vlaeminck et al. (2014) for empirical tests.

adopted only to a limited extent. Such a soft push can be implemented through small changes in the decision environment, while prices and choice sets remain unchanged (Thaler and Sunstein, 2008). Lehner et al. (2016) identify four broad strategies that can be used to change the decision environment: simplifying and framing information, changing the physical environment, changing defaults, and using social norms. In this experiment, I test whether nudging can reduce meat consumption during lunch by altering two aspects of a restaurant's physical environment: the order in which the dishes are presented on the menu and the visibility of the vegetarian dish. Moreover, I analyze the effect of the nudge on consumption choices over time and test whether the nudge has any persistent effects after the intervention ends.

To date, few field experiments have actually tested the efficacy of nudging to reduce environmental impacts from consumption. Examples include the use of social norms to reduce household water and electricity consumption (Allcott, 2011; Ferraro and Price, 2013; Jaime Torres and Carlsson, 2016) and changing the default setting of office printers to duplex to reduce paper consumption (Egebark and Ekström, 2016). The evidence for nudging as a tool to facilitate healthier food choices is broader and mostly comes from experiments changing aspects of the physical environment, such as the menu order or the convenience of buying unhealthy items (Dayan and Bar-Hillel, 2011; Just, 2009; Rozin et al., 2011; Wisdom et al., 2010). However, there is scarce evidence as to whether nudging also works to induce more environmentally friendly food choices. Conceptually, nudging for the environment may be very different from nudging for health: while nudging healthy choices is often motivated by the idea of inconsistent individual preferences such as present bias, which causes people to choose unhealthy options in the present that they will regret in the future (see, for example, Wisdom et al., 2010), it is not clear that such cognitive biases exist with respect to the sustainability of food choices. With regard to the high observed levels of meat consumption, it could well be that people's preferences are coherent, reducing the potential of nudging as a strategy to change choices. However, there is some suggestive evidence that Swedes would like to reduce their meat consumption but fail to do so. In a representative World Wildlife Fund (WWF) survey, 37 percent of the respondents state that they will reduce their meat consumption in order to reduce their climate impact during the coming year, and 33 percent state that they have already done so during the previous year (WWF, 2016). At the same time, Swedish meat consumption rose to an all-time high in 2016 (Swedish Agricultural Board, 2017). Whether preferences for meat are simply too strong for nudging to help overcome the potential intentions-behavior gap is thus an important question to examine.

The experiment took place at two university restaurants in Gothenburg, Sweden, with one serving as the treated restaurant and the other as a control. Both are run by the same provider and serve three warm dishes during lunch, one vegetarian and two containing either meat or fish. Daily sales data on the number of each of the three main dishes sold were collected from September 2015 until June 2016, covering the whole academic year. The first nine weeks served as a baseline period, followed by an intervention period of 17 weeks at the treated restaurant, where the vegetarian option was moved from the middle to the top of the printed menu, and the dish was moved from behind the counter to a spot visible to customers at the point of decision-making. Thus both the menu order and the visibility of the vegetarian dish were changed simultaneously. However, we have some evidence for the effect of changing the menu order only, as the local chef changed the menu order for five nonconsecutive weeks during spring 2016 at the control restaurant. During the final 13 weeks of the year, the original setup was reinstated at the treated restaurant.

Previous experiments have focused on the immediate impacts of nudges on food consumption, but it is important to study longer time periods to evaluate their overall effect.[3] One concern with nudging is that it might have only short-term effects that quickly disappear once people gain experience with the good or the choice setting (Croson and Treich, 2014; Löfgren et al., 2012; Lusk, 2014). In the present experiment, this could be the case if customers were initially nudged to choose the vegetarian option but returned to their original choices as soon as they became accustomed to the new setting, either because they did not like the vegetarian option or because the nudge initially increased the number of ordering mistakes.[4] However, the effect of the nudge could also increase over time, such as if people recommend eating vegetarian to fellow students after trying it as a result of the nudge. A priori, it is not clear whether and how the impact of the nudge changes over time. Combining an intervention period of 17 weeks and a customer pool that can be assumed to be fairly constant throughout the

[3] Previous experiments on food nudges (for example, Dayan and Bar-Hillel , 2011; Just, 2009; Rozin et al., 2011; Wisdom et al., 2010) mainly were conducted in places that customers were not expected to visit repeatedly, such as diners or hotels; in other cases (for example, Policastro et al., 2015), the intervention was done too infrequently to analyze effects over time.

[4] This could occur if people simply point toward the dish that is most visible and assume it is the usual meat or fish dish served, or if they read off the first item on the menu. In an environment such as the University of Gothenburg, with a high share of international staff and students, this mistake is more likely than one might think. Many foreign employees and students do not speak Swedish, and simply reading off the first item on the menu could be a reasonable strategy if this had been successful in the past. Although an English menu is also provided, the Swedish menu is the one that features most prominently in the restaurants.

academic year, this is the first experiment that allows for studying the effects of a food nudge over time.

Another important question is whether the nudge affects choices only during the intervention period or has a persistent impact on behavior after it is removed. To date, no studies have looked at the habit-forming effects of nudges in the food domain.[5] If present utility of consuming a good depends on past levels of consumption, such as in the habit formation models of Becker and Murphy (1988) or Naik and Moore (1996), an initial increase of vegetarian lunches sold because of the nudge can lead to subsequent further increases. Empirical studies show habit formation for a range of foods (see Daunfeldt et al., 2011, for an overview), but experiments using incentives to increase healthier food choices show mixed results. Consumption of targeted items is usually somewhat higher immediately after the end of an intervention than prior to it (Just and Price, 2013; List and Samek, 2015, 2017), but while Loewenstein et al. (2016) find a persistent effect one and three months after the end of an incentive scheme, Just and Price (2013) and Belot et al. (2015) do not find any persistent effects of incentives in the medium run. Nudging could be a more promising approach to creating new habits than incentivizing choices, as it does not carry the risk of crowding out intrinsic motivation (Gneezy et al., 2011). On the other hand, habit formation could be even less pronounced when using nudging, as a subtle intervention targeting the decision environment might be less successful in causing behavior change in the first place. To examine if any effect of the nudge persisted after removing it, the original setup at the treated restaurant was reinstated for the last 13 weeks of the academic year.

Results show that when using a difference-in-differences approach to estimate the treatment effect, the combined nudge of changing visibility and menu order increased the average sales share of vegetarian lunches by around 6 percentage points during the intervention period. Analyzing the treatment effect over time shows that it increased over the course of the intervention, suggesting that the average increase is not due to initial ordering mistakes or a one-off effect of trying vegetarian food. Rather, it seems as if customers learn about the vegetarian option because of the nudge, and some then incorporate it permanently into their choice set. Support for this argument also comes from the postintervention period, when the original setup was reinstated and the share of vegetarian lunches sold persisted in being 4 percentage

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[5] Persistent effects of behavioral interventions on water and electricity consumption have been found by studies such as Ferraro et al. (2011) and Allcott and Rogers (2014). However, as Brandon et al. (2017) discuss, these long-term effects are most likely due to an adjustment of physical capital.

points higher than before the intervention. Back-of-the-envelope calculations of the effect of the intervention on GHG emissions show that the nudge decreased total emissions by around 5 percent.

The remainder of the paper is organized as follows: Section 2 provides some theoretical background on nudging and an overview of the previous literature. Section 3 describes the experiment, the data, and the empirical strategy. Results are presented in section 4, and calculations on GHG emissions can be found in section 5. Section 6 concludes.

2. Background and previous literature

Many nudges build on a dual process model of cognition, which departs from the classical economics assumption of perfect rationality, but instead models human behavior as governed by two modes of thinking and deciding (Kahneman, 2003, 2011). Decisions dominated by the first mode, also called system one, are characterized by an intuitive, fast, and automatic style of thinking where cognitive effort is usually low. In the second mode, or system two, slow, reflective, and controlled processes, which require more cognitive effort, dominate. Nudging often targets decisions dominated by system one, where cognitive effort is low and the decision environment is of high importance. Food choices are seen as classical examples of decisions governed by system one where the food environment, such as the salience of items, the structure of food assortments, or the packaging, matters (Cohen and Farley, 2007; Marteau et al., 2012; Wansink and Sobal, 2007).

The present experiment targets two aspects of the decision environment: the menu order and the visibility of the vegetarian dish. Changing the visibility can affect whether and how prominently a dish features in the consideration set (Wansink and Love, 2014). In our setup, customers might not even consider the vegetarian dish an option and routinely choose between the two meat dishes offered. Making the vegetarian dish visible can add it to the consideration set without changing what is offered. Enhanced visibility will also increase a dish's saliency—that is, how much it attracts attention and, as a result, how prominently it features in decision-making (Cohen and Farley, 2007; Wansink and Sobal, 2007). Previous experiments have shown, for example, that candy consumption increases when the candy is kept on top of the desk instead of in a drawer (Painter et al., 2002) or in transparent rather than opaque jars (Wansink et al., 2006). Wansink and Hanks (2013) also show that the order in which food is presented at a buffet line matters for how much of an item is selected. In their experiment, customers were randomized into two buffet lines where foods were arranged in an inverse order, one line featuring healthier items first, the other one featuring unhealthier items first.

They summarize their findings by "first foods most", as the first foods seen were the ones selected most. Moreover, changing which dish is visible at the point of purchasing can also change the customers' information about a dish. If vegetarian dishes are unknown by name to a majority of consumers, while they are familiar with the meat dishes offered, making the dish visible can help them evaluate the vegetarian option before making a choice.

The second part of the intervention, changing the order in which the three lunch options are presented on the menu, relies on findings from previous research that have shown that when people are choosing from a list, order effects can bias them toward selecting specific objects with a higher likelihood. "Primacy effects" increase the likelihood that they will choose items listed first. Such effects can arise if people exhibit a confirmatory bias, such as looking for reasons to choose an alternative rather than for reasons not to choose it, because of growing fatigue when reading through a list, or as a result of "satisficing" behavior, where options are evaluated as generally similar and reading through a whole list entails higher costs than benefits (Carney and Banaji, 2012; Mantonakis et al., 2009; Miller and Krosnick, 1998).

Experimental evidence for primacy effects in food choice is provided by Dayan and Bar-Hillel (2011), who study the influence of menu order on choices in a coffee shop. They find that placing an item at one of the extreme positions (top or bottom) increases its sales by approximately 20 percent compared with when the same item appears in the middle. Similarly, Policastro et al. (2015) manipulated the order of an ingredients list on an ordering form to study whether putting healthy items at the top of each ingredient category leads to healthier self-assembled sandwiches. In addition, they increased the healthy items' saliency by adding visual cues, such as stars and bold print. The manipulation increased selection of health-salient compared with unhealthy ingredients. Primacy effects are also found in contexts other than food choice. In a study of order effects in research paper lists, Feenberg et al. (2015) find a strong effect for hits, downloads, and citations for papers listed early, especially for the first paper on the list. Miller and Krosnick (1998) find a primacy effect when studying order effects in elections where people vote for candidates based on a list of names.

3. The experiment

3.1. Experimental design

The experiment was conducted at two restaurants at the University of Gothenburg during the academic year 2015–16. Gothenburg is the second largest city in Sweden, with a population around 550,000, and its university is the fourth largest in the country, with about 24,000 full-time students. The departments of the university are spread across the city, and the university buildings that hosted the experiment are approximately 2.5 kilometers (km) away from each other.

Both restaurants serve three warm alternatives during lunch: one vegetarian and two including either meat or fish (called "meat 1" and "meat 2" in the following).[6] The restaurants are subject to the same management, but the local chefs decide on the weekly menus, and hence they differ across restaurants. Prices, however, are the same: warm dishes cost 70 SEK (approximately €7.30 or US$7.80) and are accompanied by bread, salad, and water. Instead of a warm dish, customers can also opt for soup, various salads, or sandwiches, which are priced differently. At both restaurants, the menu for the whole week is posted at the entrance but only the daily menu is shown at the point of ordering. Many employees and students also subscribe to the restaurant's weekly menu by email.

Restaurant 1, the treated restaurant where the nudge was implemented, is in a building that houses the economics, business administration, and law faculty. Hence, students and faculty members eating there mainly belong to those disciplines. Restaurant 2, which serves as a control where no changes were undertaken, is in a building housing mostly institutions belonging to the humanities. To capture initial differences between the restaurants in the quantity of vegetarian food consumed, the academic year was divided into three experimental periods. Period 0, the baseline or control period, lasted from September 1 until November 8 (10 weeks). The intervention period (period 1) lasted from November 9 until March 6 (17 weeks including Christmas break). From March 7 until June 3 (13 weeks), the intervention ended at the treated restaurant and the original setup was restored (period 2, or reversal period).[7]

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[6] The two nonvegetarian dishes are called *dagens husman* ("traditional Swedish") and *gränslöst gott* ("limitless good"), indicating that the style of the dishes is different. However, a detailed analysis of the menus reveals that about one-third of the dishes served show up as both the "traditional Swedish" and "limitless good" dish.

[7] The final sample, as described in the data section, contains 10 weeks of data for the baseline period, 14 weeks of data for the intervention period, and 12 weeks of data for the reversal period.

The experimental design is summarized in Table 1. During the baseline period, the restaurants differed in terms of menu layout and visibility of the dishes at the point where customers made their decision about which dish to choose for lunch. Concerning the menu order, the vegetarian option was found in the second position at the treated restaurant, framed by the two meat options. In the control restaurant, the vegetarian dish was listed first. The restaurants also differed with respect to which dish was visible at the point of ordering. At the treated restaurant, only one of the three dishes can be kept before the counter and is visible to the customers when they place their order. Before the intervention, this was the dish that was also shown at the top of the menu, hence a meat or fish dish. At the control restaurant, customers place their orders, pay, and then proceed to a counter where they pick up their lunches. However, the counter is fully transparent, and all three dishes are equally visible. If a customer wants to see how a dish looks before placing an order, he or she can easily go and take a look. From comparing the setup at each of the two restaurants during the pre-experimental period, one can conclude that the control restaurant's food environment is more favorable for choosing the vegetarian option—if food environment matters.

Table 1. Summary of the food environment across restaurants and treatment periods

		Treated restaurant	Control restaurant
Menu order	Period 0 (Baseline period)	Position 1: Meat 1 Position 2: Vegetarian Position 3: Meat 2	Position 1: Vegetarian Position 2: Meat 1 Position 3: Meat 2
	Period 1 (Treatment period)	Position 1: Vegetarian Position 2: Meat 1 Position 3: Meat 2	Position 1: Vegetarian Position 2: Meat 1 Position 3: Meat 2
	Period 2 (Reversal period)	Position 1: Meat 1 Position 2: Vegetarian Position 3: Meat 2	Position 1: Vegetarian or Meat 1 Position 2: Vegetarian or Meat 1 Position 3: Meat 2
Visibility	Period 0	Meat 1 dish	All three equally visible
	Period 1	Vegetarian dish	All three equally visible
	Period 2	Meat 1 dish	All three equally visible

During the treatment period, the vegetarian dish was moved on both the weekly and the daily menus from position two to the top at the treated restaurant (see appendix Figure A.1 for examples of the menus during period 0 and period 1 at the treated restaurant). Moreover, it was made visible by placing it before the counter at the point of ordering, and consequently both meat dishes were placed behind the counter. At the control restaurant, no changes in menu order or visibility were made during the control or treatment period. However, on February 1, 2016 (14 weeks into period 1), the chefs changed at both restaurants. The chef of the control restaurant moved to the treated restaurant, and a new chef from outside the organization was employed at the control restaurant. Implications of this change in staff for identification of the treatment effect are discussed in the methodology section.

For the remaining 13 weeks of the semester, the setup at the treated restaurant was returned to its original state: the vegetarian dish was again placed in the middle of the menu, and a meat dish was put before the counter. At the control restaurant, the new chef independently introduced some changes in operations. Amongst others, he switched the menu order during five nonconsecutive weeks of period 2, moving the vegetarian dish from the top to the middle position. This small additional natural experiment will be used to analyze the effect of an isolated change in menu order without simultaneously changing the visibility of dishes.

3.2. Data

Sales data on the daily number of lunches sold by category (vegetarian, meat 1, and meat 2) and by restaurant were collected from September 1, 2015, to June 3, 2016, covering the whole Swedish academic year 2015–16. Data were collected via the electronic cash registers at the restaurants and delivered via Excel files for analysis.[8] The full dataset includes 181 days for the treated restaurant and 184 days for the control restaurant.[9] The analysis sample is restricted to days when all three options were offered for lunch, reducing the number of observations by three for the control restaurant.[10] Moreover, either sales or menu data are missing for one day from the control and ten days from the treated restaurant. For five days in spring 2016, a different lunch pricing scheme was applied at both restaurants, with one of the three dishes sold at a higher price. Data from these five days are excluded from the sample, as on four of those days the more expensive dish was a meat dish. The final sample used for the empirical analysis thus includes 175 days for the control restaurant and 166 days for the treated restaurant.

Descriptive statistics on the number of dishes sold overall and by dish type are shown in Table 2. The treated restaurant is slightly bigger than the control restaurant, selling on average 152 warm lunches a day throughout the year, while the control sells on average about 140 dishes. Total sales decrease at both restaurants throughout the year. The decrease is larger at the treated than at the control restaurant, which could be an unintended side effect of the

[8] The cash registers have three different buttons that were labeled with the Swedish category names of the dishes, *dagens husman* (meat 1), *gränslöst gott* (meat 2), and *grönt och gott* (vegetarian), minimizing the risk of mistakes in recording the type of dish correctly. An example picture of the registers is available on request.

[9] The number of days differs because two job fairs took place at the treated restaurant building, during which the restaurant was closed.

[10] During those days, which preceded public holidays, the control restaurant served only two warm dishes.

nudge. However, to evaluate the impact of the nudge on total sales, it would be necessary to compare changes during the experiment with changes from the previous year, which is not possible because of a price increase for the warm lunch in 2014–15. According to restaurant management, the observed decrease in total sales was no larger than in previous years. The overall decline in sales was attributed to students dropping out over the course of the year and the fact that as students' budgets tighten throughout the year, they increasingly substitute food brought from home for restaurant food. The relatively larger decline in sales at the treated restaurant in period 2 can be due to the fact that few new courses in business, economics, and law begin during that period, whereas during both period 0 and period 1, many new courses start, bringing in new students that partly compensate for those that drop out. At the campus where the control restaurant is located, however, many new courses also start during period 2.

Table 2. Number and share of different dish types sold across the three experimental periods

	All year		**Period 0 (September–November)**		**Period 1 (November–March)**		**Period 2 (March–June)**	
	Average no. sold / day	Share	Average no. sold / day	Share	Average no. sold / day	Share	Average no. sold / day	Share
				Treated Restaurant				
All dishes	152 (46.16)		176 (62.24)		157 (26.8)		125 (31.09)	
Vegetarian	26 (12.47)	0.175 (0.068)	24 (14.93)	0.139 (0.065)	31 (11.38)	0.201 (0.066)	22 (9.17)	0.176 (0.059)
Meat 1	70 (35.77)	0.454 (0.123)	95 (48.98)	0.529 (0.121)	66 (24.1)	0.421 (0.114)	53 (18.44)	0.43 (0.111)
Meat 2	55 (26.19)	0.371 (0.116)	57 (24.42)	0.332 (0.12)	59 (20.94)	0.378 (0.11)	50 (20.78)	0.394 (0.114)
No. of observations	166		47		63		56	
				Control Restaurant				
All dishes	141 (21.36)		151 (15.08)		142 (21.02)		129 (21.02)	
Vegetarian	36 (11.04)	0.258 (0.066)	40 (10.31)	0.265 (0.0543)	38 (10.75)	0.267 (0.062)	31 (9.99)	0.24 (0.078)
Meat 1	56 (16.07)	0.401 (0.105)	61 (14.77)	0.405 (0.0977)	53 (14.02)	0.375 (0.092)	55 (18.58)	0.43 (0.12)
Meat 2	48 (17.19)	0.341 (0.104)	50 (17.46)	0.329 (0.112)	51 (15.53)	0.358 (0.085)	43 (18.16)	0.329 (0.116)
No. of observations	175		49		72		54	

Note: Standard deviation in parentheses.

The share of vegetarian food consumed is consistently higher at the control restaurant, which might reflect differences in the customer population, as the restaurants are located at different faculties of the university. It might also reflect the more vegetarian-friendly decision environment described above.

In addition to the sales data, the restaurants' menus were collected to categorize each dish by its main component. This was done to analyze whether the menu composition at the restaurants changed over time and to control for different dish types in the empirical analysis. Meat dishes were categorized by type of meat: beef, chicken, pork, other meat (minced meat, sausages, game, lamb), and fish. An additional category was introduced for a soup that was served as the meat 1 dish on 35 days (30 at the treated and 5 at the control restaurant), as it could be customized to be vegetarian by omitting the bacon, without this being noticed by the cashier who recorded the alternatives (meat 1, meat 2, or vegetarian).[11] This soup is a traditional dish served on Thursdays throughout Sweden. The vegetarian dishes were categorized partly according to components included and partly by type of dish, resulting in the categories stew (such as a vegetarian curry), pasta, vegetables (for example, a vegetable gratin), patty (for example, a vegetarian burger), other vegetarian (for example, pies or omelets), vegetarian soup,[12] and world (such as vegetarian enchiladas, Asian noodles, and falafel). For some types of dishes, how often they occurred on the menu varied considerably. For example, vegetarian dishes belonging to the patty category were offered on 11 percent and 13 percent of all days during periods 0 and 1, respectively, but on 27 percent of all days during period 2. Appendix Table A.1 shows how the restaurants' menu compositions changed across experimental periods for both the vegetarian and the meat dishes.

Figure 1 shows that vegetarian dishes vary in popularity depending on the dish type. For example, during the pre-experimental period, sales shares ranged from 12 percent for vegetable dishes to 21 percent for world dishes at the treated restaurant. Overall, the popularity pat-

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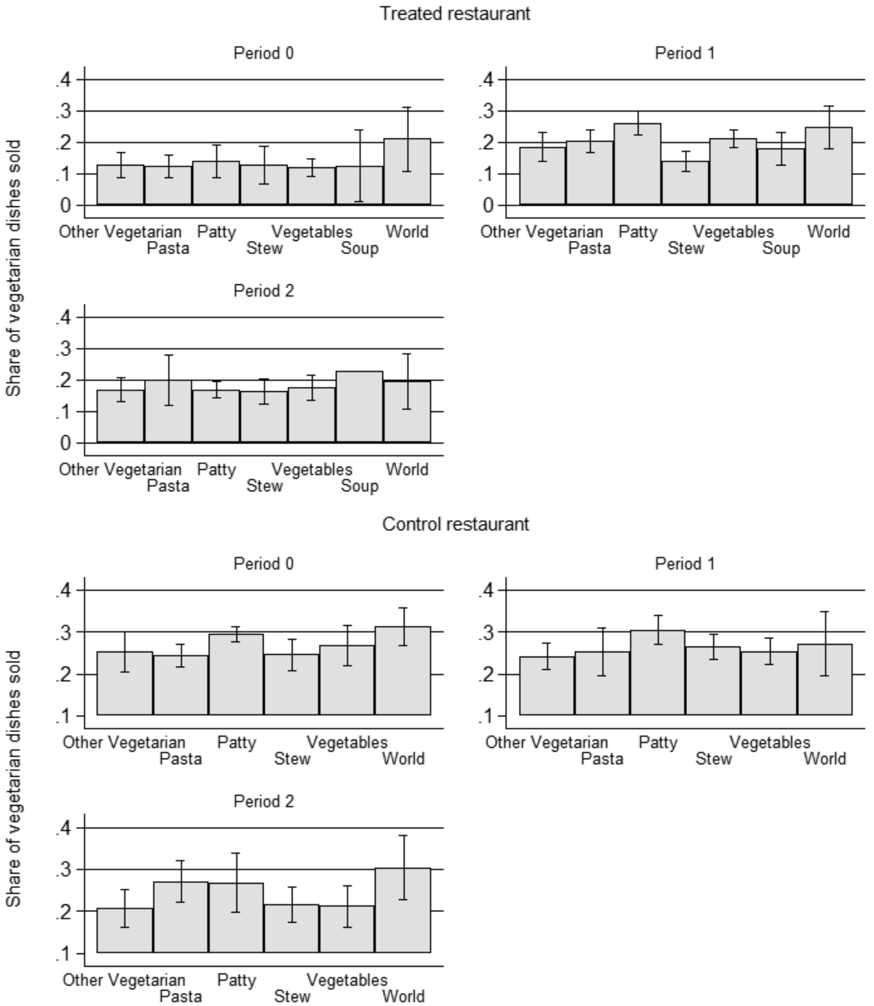
[11] As the soup could be customized to being vegetarian, the menu effectively contained two vegetarian and two meat dishes on the days it was served. This potentially creates measurement error in the share of vegetarian dishes sold. To minimize the impact of potential measurement error in the regressions, the soup was classified as its own category of meat dishes and entered as a control variable in the main regression specifications. Empirical results are robust to excluding the days where soup was served and are available on request.

[12] The vegetarian soup could also be customized to nonvegetarian by adding bacon, without this being noticed by the cashier. However, as it was served as the vegetarian dish, the menu effectively contained three meat dishes and one vegetarian dish on the days it was offered (12 days at the treated restaurant, 5 days at the control restaurant). Again, the share of vegetarian dishes sold as the variable of interest is most likely subject to measurement error on those days. Controlling for the type of dish should ameliorate the measurement error.

tern of dishes looks similar across restaurants, with patties and world dishes being most popular.

Figure 1. Share of vegetarian dishes sold by type of dish



Note: Error bars represent 0.95 confidence intervals around the mean for each type. Error bar for soup in period 2 is omitted, as there was only one observation this period.

3.3. Empirical strategy

3.3.1. Before-after analysis

Building on the experimental design, two identification strategies are used to estimate the effect of the nudge and its subsequent removal on the share of vegetarian dishes sold. The first approach is to compare the sales share at the treated restaurant across periods, controlling for additional factors:

$$V_t = \alpha_1 + \gamma_1 Period1 + \gamma_2 Period2 + Vegtype_t \times \theta + (Meattype1_t \times Meattype2_t) \times \mu + \lambda_{day} + \varepsilon_t \quad (1)$$

V_t is the share of vegetarian lunches sold at restaurant 1 on day t. $Period1$ and $Period2$ are dummy variables indicating whether an observation belongs to the treatment or the reversal period, respectively; γ_1 captures the effects of the combined nudge in period 1; and γ_2 captures any remaining effects of the nudge after its removal in period 2.

$Vegtype_t$ is a vector of dummy variables characterizing the type of vegetarian dish served that day and is introduced to capture differences in popularity between dish types. $Meattype1_t \times Meattype2_t$ is a vector of all observed combinations of meat dishes offered.[13] It is introduced to control for the influence of the outside options on V_t. λ_{day} introduces day-of-the-week fixed effects. To estimate how the nudge affects the sales of the meat 1 and meat 2 dishes, equation (1) can also be specified with the share of meat 1 or meat 2 dishes sold as the dependent variable. While the intervention directly affected the visibility and menu position of the meat 1 dish such that one would expect the sales share to decrease, sales of the meat 2 dish could be also affected. Although the menu position and visibility of this dish were kept constant throughout the experiment, the nudge might change its salience relative to the meat 1 and vegetarian dishes.

The impact of the nudge over time can be analyzed by estimating equation (1) with a linear time trend and by dividing the period dummies further into subperiods and comparing their coefficients. This can also help elucidate whether the change of chefs at the treated restaurant had an additional impact on the share of vegetarian dishes sold.

[13] Alternatively, one could introduce each dish type separately in the regression. However, as the consumer is always faced with a combination of dishes, and the order in which they are presented on the menu might matter for decision-making, I control for each combination of meat types occurring in the data.

An alternative to looking separately at the share of each dish type sold as the dependent variable in a linear regression framework is to model the sales of all three dish types, vegetarian, meat 1, and meat 2, in a multinomial regression. This can serve as a robustness check for the ordinary least squares (OLS) results, taking into account that the share of vegetarian dishes sold results from customers facing three unordered options they can choose from, and has the advantage of simultaneously estimating the effect of the nudge on all three alternatives. I estimate the following conditional logit model with alternative-specific constants, modelling the probability p_{tj} that alternative j is chosen at day t:

$$p_{tj} = Prob[Y_t = j] = \frac{exp(\alpha_j + Dishtype_{tj} \times \rho + \gamma_{1j} Period1 + \gamma_{2j} Period2 + \lambda_{j,Day})}{\sum_{k=1}^{3} exp(\alpha_k + Dishtype_{tk} \times \rho + \gamma_{1k} Period1 + \gamma_{2k} Period2 + \lambda_{k,Day})} \quad (2)$$

where $j, k = 1, 2, 3$ denote the three alternatives (meat 1, meat 2, and vegetarian). Identification in the conditional logit model crucially depends on the assumption of independence of irrelevant alternatives (IIA), which excludes the presence of close substitute alternatives. As the meat 1 and meat 2 dishes are very similar, it is likely that consumers eating meat substitute between those two dishes to a greater extent than with the vegetarian dish. To relax the IIA assumption, I also estimate a partially degenerate nested logit model that partitions the choice set into one branch containing the meat alternatives and one branch containing the vegetarian alternative (see, for example, Hunt, 2000).

Estimating effects of the nudge on the share of vegetarian dishes sold by before-after analysis will give unbiased results only if factors external to the experiment that might drive changes in sales across the period can be excluded. Such external factors could, for example, be food trends, media reporting on food-related issues, or seasonal variation in consumption patterns. Given the long observation period, identification is especially sensitive to this (untestable) assumption. However, it can be relaxed by using data from restaurant 2 as a control, which should capture any exogenous changes that could affect the consumption of vegetarian food during the experiment, in a difference-in-differences analysis.

3.3.2 Difference-in-differences analysis

The following difference-in-differences (DiD) model is estimated to identify the effect of the nudge on the share of vegetarian dishes sold by comparing changes across periods 0 and 1 at the treated restaurant with changes at the control restaurant:

$$V_{it} = \alpha_0 + \beta_0 Restaurant + \gamma_0 Period1 + \delta_0 (Restaurant \times Period1) + Vegtype_{it} \times \rho + (Meattype1_{it} \times Meattype2_{it}) \times \tau + \lambda_{Day} + \lambda_{Holidays} + \lambda_{Month} + \varepsilon_{it} \quad (3)$$

where V_{it} is the share of vegetarian lunch dishes sold at restaurant i on day t. Initial differences in the share of vegetarian lunches sold are captured by the dummy variable $Restaurant$, which is 0 for the control restaurant and 1 for the treated restaurant. $Period1$ is a dummy variable taking the value 1 if an observation belongs to the treatment period and controls for changes in the popularity of vegetarian food across periods common to both restaurants. $Vegtype_{it}$ is again a vector of dummy variables characterizing the type of vegetarian dish served, and $Meattype1_{it} \times Meattype2_{it}$ controls for the combination of meat dishes served as outside options. $\lambda_{Day}, \lambda_{Holiday}$, and λ_{Month} are time fixed effects controlling for the day of the week, for the weeks around the Christmas holidays[14], and for the calendar month. Month fixed effects are especially important, as they capture any potential common effects of the chef change in February on the outcome variable. $Restaurant \times Period1$ indicates whether an observation belongs to the treated restaurant in the treatment period, and δ_0 captures the treatment effect.

DiD estimation is limited to the direct effect of the nudge (i.e., the effect in period 1), as it relies on two critical assumptions to deliver unbiased treatment effects. The first assumption is that the consumption of vegetarian food followed parallel trends at both restaurants before the introduction of the nudge. The second assumption is that restaurant 2 is a valid control in the sense that any exogenous events during the experiment affected consumers at both restaurants in a similar way. This assumption is weakened by the employment of a new chef toward the end of the treatment period at the control restaurant. Figure 2, which depicts weekly average sales of vegetarian dishes by restaurant, shows that from the week the new chef started, variability increased and sales shares slightly decreased at the control restaurant.[15] According to the restaurant's management, the higher variability in the share of vegetarian dishes sold was due to the fact that the new chef was not used to cooking vegetarian dishes and first had to acquire knowledge regarding the taste of his customers. Moreover, the menu order was changed for five weeks during the reversal period, such that the vegetarian dish was moved from the top to the middle of the menu. The change of chefs at restaurant 1 did not lead to a similar increase in variability, which is most likely because the new chef had worked there

[14] Potentially, more employees take holidays during these weeks, which could alter the customer composition.
[15] The spike in the last week of March coincides with the week before the Easter holidays, when the restaurant was open for only three days. This could have altered the composition of customers, as schools were closed that week, and some employees might have gone on holidays with their children. Most likely, both the lower number of observations and the composition effect contributed to the spike in the share of vegetarian dishes sold. The treated restaurant was open for four days that week.

before as a trainee of the old chef. Hence, he already knew the taste of the customers and was familiar with cooking vegetarian dishes. Limiting the DiD analysis to period 1 safeguards against overestimating persistent effects of the nudge in period 2. In addition, equation (3) is estimated with period 1 divided further into subperiods and with a linear time trend, which can provide some information about the impact of the chef change on the treatment effect.

Figure 2. Share of vegetarian meals sold per week over time, both restaurants

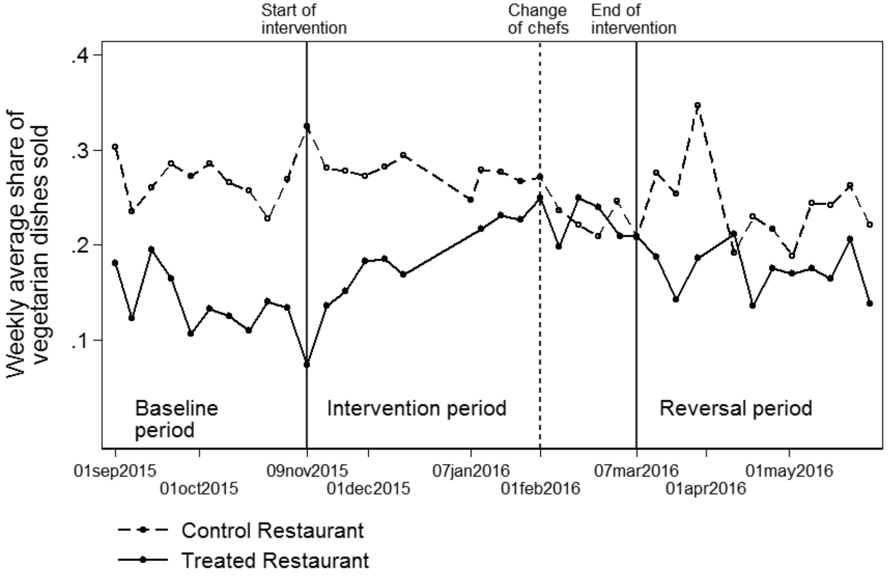


Figure 2 can also be used to examine the parallel trends assumption. A priori, this assumption is supported by several factors. Both restaurants are run by the same provider and subject to the same management, which minimizes the chance for management changes that affect only one restaurant. Moreover, both restaurants are located in the same city, and customers should be exposed to roughly the same media, weather conditions, and seasonal variation in food offered. Third, although the restaurants differ with respect to the customers to whom they cater, as they belong to different faculties, the populations are similar with respect to age structure and educational attainment, increasing the likelihood that they will react to exogenous events in a similar way.

Examining pretreatment trends in Figure 2 lends support to the assumption of parallel trends during the baseline period. At the control restaurant, the share of vegetarian dishes sold did not trend upward or downward until the new chef was employed. The apparent spike at the start of the intervention was most likely caused by the type of vegetarian dishes offered, which was a dish belonging to the most popular category (patties) during three out of five days. At the treated restaurant, the share of vegetarian dishes sold exhibits more variation but no clear trend during the preintervention period, and it increased steadily after the implementation of the nudge. The drop exactly at the start of the intervention was most likely caused by the fact that a job fair was taking place at the treated restaurant; only one day of sales data was delivered during that week. On that day, a vegetarian dish belonging to one of the least popular categories, stew, was sold. During the reversal period, the share of vegetarian lunches at the treated restaurant dropped compared with the intervention period but was still slightly higher than during the baseline period.

In addition to using a linear DiD model, sales in period 1 are also modelled by a conditional logit model and a nested model to relax the IIA assumption. The conditional logit model takes the following form, where p_{itj} denotes the probability that alternative j is chosen in restaurant i at day t, and R and $P1$ are dummies for restaurant 1 and period 1, respectively:

$$p_{tj} = Prob[Y_{ti} = j] = \frac{exp\left(\alpha_j+\beta_j R+\gamma_j P1+\delta_j(R\times P1)+Dishtype_{itj}\times\rho+\lambda_{j,Day}+\lambda_{j,Holid}+\lambda_{j,Month}\right)}{\sum_{k=1}^{3} exp\left(\alpha_k+\beta_k R+\gamma_k P1+\delta_k(R\times P1)+Dishtype_{itk}\times\rho+\lambda_{k,Day}+\lambda_{k,Holid}+\lambda_{k,Month}\right)} \quad (4)$$

4. Results

4.1. Preliminary analysis

Table 3 compares the average shares of vegetarian dishes sold across periods and restaurants. At the treated restaurant, the share significantly increased by 6 percentage points, from 14 to 20 percent, after implementing the nudge, while it remained stable at around 26 percent at the control restaurant. Comparing the changes in the share of vegetarian dishes sold at the treated restaurant between period 0 and period 1 to changes in sales share at the control restaurant across the same periods provides the unconditional DiD treatment effect. Without controlling for additional factors, the share of vegetarian dishes sold at the treated restaurant increased by 6 percentage points (column (4)).

In period 2, the share of vegetarian lunches sold dropped to around 18 percent and 24 percent at the treated and control restaurants, respectively. Comparing the shares in period 0 and period 2 at the treated restaurant shows that the sales share of vegetarian lunches was still 3.6 percentage points higher after the nudge was removed than during the baseline period. The DiD estimate is significantly higher but most likely confounded by the drop in sales shares in connection with the employment of the new chef at the control restaurant.

Table 3. Mean shares of vegetarian dishes sold across periods and restaurants

Share of vegetarian dishes sold	(1) Period 0	(2) Period 1	(3) Period 2	(4) Period 1–Period 0[a]	(5) Period 2–Period 0[a]	(6) Period 2–Period 1[a]
Treated restaurant	0.139	0.201	0.176	0.062***	0.036***	–0.025***
	(0.0038)	(0.0040)	(0.0046)	(0.0055)	(0.0059)	(.0060)
Control restaurant	0.264	0.267	0.240	–0.003	–0.025***	–0.026***
	(0.0051)	(0.0044)	(0.0051)	(0.0067)	(.0072)	(0.0067)
Difference-in-differences				0.060***	0.061***	0.001
treated – control[b]				(0.0166)	(0.0180)	(0.0170)

[a] z-test of proportions
[b] regression t-test
Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.2. Regression analysis: Immediate effects of the nudge

Table 4 presents the estimated effects of the nudge in period 1, using the before-after approach in columns (1) – (4) and the DiD approach in columns (5) – (8). Column 1 shows the raw before-after comparison; the share of vegetarian dishes sold significantly increases by 6.2 percentage points. Columns (2) and (3) add controls for the types of vegetarian and meat dishes sold each day. Although appendix Table A.1 shows that the menu composition varied across periods, controlling for it only marginally changes the treatment effect—to 6.4 percentage points when including the type of vegetarian dish and to 7.2 percentage points when including the types of meat dishes. Including weekday fixed effects (column (4)) increases the treatment effect further to 8.2 percentage points. Testing for pairwise differences reveals that treatment effects are not significantly different across specifications. Columns (5) – (8) show the results of the DiD estimation. DiD estimates of the treatment effect lie between 6 and 7.3 percentage points and are thus very close to the before-after estimates. Pairwise comparisons show no difference in the treatment effects across models. A minimum treatment effect of 6 percentage points, as found in the specification in column (5), represents a 43 percent increase in the share of vegetarian lunches sold, compared with the baseline period, as the result of the nudge.

Table 4. Estimating the immediate effects of the nudge on the share of vegetarian dishes sold

Dependent variable: Share of vegetarian dishes/day	Before-after estimation				Difference-in-differences estimation			
	(1) No controls	(2) + Type of vegetarian dish	(3) + Type of meat dish	(4) + Day-of-week FE	(5) No controls	(6) + Type of vege-tarian dish	(7) + Type of meat dish	(8) + Time FE
Period 1	0.0617***	0.0642***	0.0723***	0.0820***	0.00130	0.000503	0.00120	–0.0149
	(0.0126)	(0.0115)	(0.0108)	(0.0121)	(0.0115)	(0.0108)	(0.0116)	(0.0243)
Treated restaurant					–0.126***	–0.125***	–0.134***	–0.138***
					0.00130	0.000503	0.00120	(0.0134)
Period 1 × Treated restaurant					0.0604***	0.0627***	0.0699***	0.0730***
					(0.0166)	(0.0156)	(0.0162)	(0.0166)
Constant	0.139***	0.124***	0.136***	0.138***	0.265***	0.248***	0.282***	0.320***
	(0.00956)	(0.0153)	(0.0231)	(0.0393)	(0.00887)	(0.0112)	(0.0221)	(0.0353)
Observations	110	110	110	110	231	231	231	231
Adjusted *R*-squared	0.173	0.328	0.452	0.433	0.393	0.479	0.522	0.526
Vegtype	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Meattype	No	No	Yes	Yes	No	No	Yes	Yes
Month FE	No	No	No	No	No	No	No	Yes
Holiday FE	No	No	No	No	No	No	No	Yes
Weekday FE	No	No	No	Yes	No	No	No	Yes

Note: Conventional standard errors are used, as the residuals exhibit very little heteroscedasticity and as they provide the most conservative confidence intervals in all specifications, even when compared with bias-corrected robust standard errors. See Angrist and Pischke (2008).
Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To analyze the development of the treatment effect over time, the intervention period is split into three subperiods: November–December, January, and February–March.[16] Results in columns (1) and (4) of Table 5 confirm the graphical analysis: the size of the treatment effect increases over time, from 4.1 to 11.3 percentage points when estimated by a before-after comparison, and from (insignificant) 1.2 to 13.5 percentage points when estimated by DiD.[17] The effect for February–March is most likely slightly overestimated in the DiD specification, as these are the months when the new chef was employed and the share of vegetarian dishes dropped slightly at the control restaurant (see Figure 2). However, the effect for February–March does not differ significantly between the before-after and the DiD specification.[18]

Estimating the treatment effect with a linear time trend (columns (2), (3), and (5)) shows that the nudge led to an increase of 0.8–0.9 percentage points in the share of vegetarian dishes sold each week. In column (2), a weekly linear trend is estimated using only data from the treated restaurant up to February 1, when the new chef started. Column (3) estimates the trend using all of period 1 but includes a dummy for the period February–March. The trend slightly decreases but is not significantly different from using only the data up to the chef change ($p = 0.10$), providing evidence that the change of chefs at the treated restaurant did not significantly change the impact of the nudge. Excluding the first week of the intervention, which represents a potential outlier at both restaurants (see Figure 2), as a robustness check for the linear time trend decreases the weekly trend to 0.75–0.84 percentage points, depending on the specification used.[19]

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[16] November and December were grouped because only about half of November was treated and the Christmas break started December 19. Similarly, February and March were grouped because only one week in March was treated.

[17] In model (1), pairwise comparison of treatment effects shows that the effects for January and February–March are not significantly different from each other. Both other pairwise comparisons show significant differences. In model (4), all pairwise comparisons show that monthly, the treatment effect increases over time.

[18] Treatment effects for November–December are significantly different at a 5% level in both the before-after and the DiD specifications. Effects for January and February–March are not significantly different ($p = 0.08$ and $p = 0.13$, respectively).

[19] Full results of the regressions excluding the first week of the intervention are available on request.

Table 5. Treatment effects over time

	Before-after estimation			DiD estimation	
	(1) Monthly treatment effects	(2) Linear time trend, before chef change	(3) Linear time trend, whole period	(4) Monthly treatment effects	(5) Linear time trend
Period 1					0.00788
					(0.0215)
Restaurant 1				-0.142***	-0.146***
				(0.0125)	(0.0111)
Period 1 × Restaurant 1					
Nov–Dec × Restaurant 1	0.0407***			0.0115	
	(0.0142)			(0.0197)	
Jan × Restaurant 1	0.0968***			0.0733***	
	(0.0159)			(0.0228)	
Feb–Mar × Restaurant 1	0.113***			0.135***	
	(0.0146)			(0.0199)	
Weekly trend × Period 1 × Restaurant 1		0.0090***	0.0083***		0.0085***
		(0.0014)	(0.0014)		(0.0012)
Constant	0.152***	0.158***	0.162***	0.319***	0.304***
	(0.0353)	(0.0347)	(0.0350)	(0.0337)	(0.0328)
Observations	110	87	110	231	231
Adjusted *R*-squared	0.547	0.521	0.539	0.591	0.587
Vegtype	Yes	Yes	Yes	Yes	Yes
Meattype	Yes	Yes	Yes	Yes	Yes
New chef	No	No	Yes	No	No
Month FE	No	No	No	Yes	Yes
Holiday FE	No	No	No	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes

Note: The baseline specifications shown in columns (1) and (4) correspond with columns (4) and (8) in table 4.
Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As individual-level data is not available, it is impossible to identify the mechanism behind the increasing treatment effect. One potential explanation is that an increasing number of individuals were exposed to the nudge over the course of the intervention. A guest survey conducted in May 2015 revealed that on average, customers eat at the restaurant about four times per month. If initially nudged customers subsequently increased their consumption of vegetarian food further, such as predicted by the models of Becker and Murphy (1988) or Naik and Moore (1996), this could result in an increasing treatment effect over time. Another explanation could be that any increase in the sales of vegetarian dishes as a result of the nudge increased sales further via network effects, such as if people recommended the dish to colleagues or if customers observed what others chose. Such effects could lead to increasing sales over time, even if the additional sales might not be directly attributable to the nudge.

4.3. Persistent effects of the nudge on the share of vegetarian dishes sold

Table 6 shows the results of the before-after regressions testing for persistent changes in the share of vegetarian dishes sold after removing the nudge. When estimating the full model, including controls for dish types and weekday fixed effects (column (4)), the share of vegetarian lunches is still around 4 percentage points higher than in the baseline period. Combining the results from the analysis of treatment effect over time and from the reversal of the intervention, the nudge seems to have led to a persistent shift in consumption toward more vegetarian food.[20] Apparently, the intervention led to a permanent expansion of the consideration set for at least some consumers.

Table 6. Estimating persistent effects of the nudge on the share of vegetarian dishes sold

Dependent variable: Share of vegetarian dishes sold per day	(1) No controls	(2) + Type of vegetarian dish	(3) + Type of meat dish	(4) + Day-of-week FE
Period 1	0.0617***	0.0642***	0.0703***	0.0703***
	(0.0122)	(0.0117)	(0.0118)	(0.0122)
Period 2	0.0365***	0.0367***	0.0404***	0.0409***
	(0.0125)	(0.0121)	(0.0124)	(0.0126)
Constant	0.139***	0.127***	0.148***	0.154***
	(0.00922)	(0.0136)	(0.0279)	(0.0315)
Observations	166	166	166	166
Adjusted *R*-squared	0.125	0.209	0.286	0.268
Vegtype	No	Yes	Yes	Yes
Meattype	No	No	Yes	Yes
Month FE	No	No	No	No
Holiday FE	No	No	No	No
Weekday FE	No	No	No	Yes

Note: Conventional standard errors are used as the residuals exhibit very little heteroscedasticity and as they provide the most conservative confidence intervals in all specifications, even when compared with bias-corrected robust standard errors. Standard errors in parentheses*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

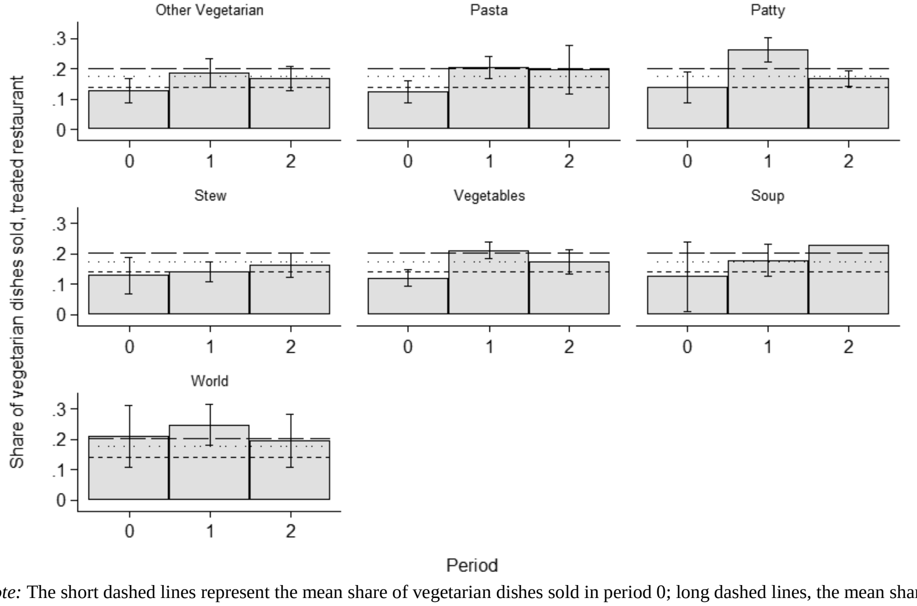
4.4. Heterogeneous effects: Type of vegetarian dish served

As Figure 1 shows, the sales share of vegetarian dishes varied considerably with the type of dish offered. This section analyzes whether the impact of the nudge varied across dish types. Changing the visibility of the dish can be expected to have a differential impact, depending on how appealing a dish looks and how this contrasts with the expectations formed by the customers when only reading the name of a dish.

[20] There is no evidence for a decline in the share of vegetarian dishes sold during the reversal period. When the reversal period is divided into two parts (March–April and May–June), treatment effects are 0.044 and 0.036, respectively, and not significantly different from each other. Results for the split reversal period are available on request.

Figure 3 shows how the sales share of vegetarian dishes changed across periods for each type of dish at the treated restaurant, according to the classification used in the regression analysis. It can be seen that the intervention increased the sales of all vegetarian dish types, but effects vary considerably across types. The nudge seems to work most effectively when a vegetarian patty is sold and least effectively when a stew is sold. One explanation could be that the appearance of patties, such as vegetarian burgers, appeals more to customers who usually consume meat or fish, as they resemble typical meat dishes. This explanation is corroborated by previous research finding that industry meat substitutes, such as soy burgers, are relatively popular meat substitutes for consumers adhering to flexible diets (i.e., those that are neither vegetarians nor heavy meat eaters) (Schösler et al., 2012). Stews, on the other hand, seem to attract only the core vegetarian customers even during the treatment condition, as their share hardly increased while the nudge was implemented. However, although the effects for patties and vegetables are large in absolute terms in period 1, none are statistically significant when tested in a regression (see appendix Table A.2), which could be due to the relatively low frequency with which each category was served.

Figure 3. Share of vegetarian dishes sold across periods, by type of vegetarian dish

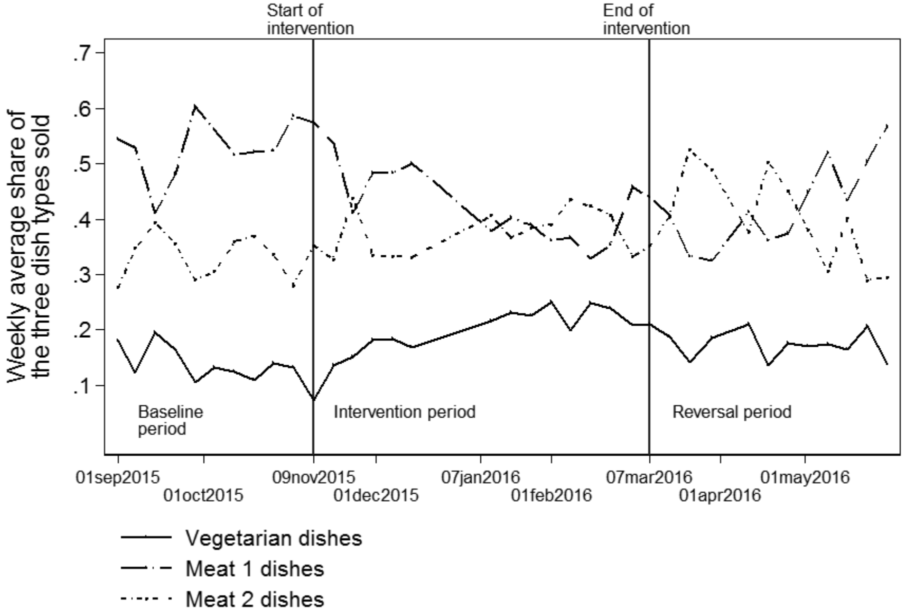


Note: The short dashed lines represent the mean share of vegetarian dishes sold in period 0; long dashed lines, the mean share sold in period 1; and dotted lines, mean shares sold in period 2. Error bars represent 0.95 confidence intervals around the mean for each type.

4.5. Alternative model specifications and substitution effects

Although the nudge changed only how the vegetarian and meat 1 dishes were presented, Figure 4 provides some indication that the sales share of the meat 2 dish was also affected by the intervention. Plotting the sales shares of all three dish types shows that the meat 1 and meat 2 dish were close substitutes, as their sales are highly negatively correlated (r = -0.84). It also shows an increase in the unconditional sales share of the meat 2 dish (not controlling for the type of dish or time effects) during period 1 and period 2 compared with the baseline period.

Figure 4. Development of sales shares of all three dish types over time, treated restaurant



Regression results confirm the graphical analysis. Table 7 shows treatment effects on all three dish types resulting from estimating OLS, using conditional logit and nested logit models, regressing observed frequencies of each alternative on an alternative-specific constant, the type of meat or vegetarian food served, period and restaurant dummies, and time. The coefficients shown are the marginal effects of the treatment on the sales shares.[21] Estimated treatment effects are largely similar, showing that the OLS results are robust to alternative model specifications.

Table 7. Treatment effects on all three dishes sold

	Before-after			Difference-in-differences		
Treatment effects	OLS[a]	Conditional logit[b]	Nested logit[c]	OLS[d]	Conditional logit[e]	Nested logit[f]
Outcome: Share of vegetarian dishes sold						
Period 1	0.0703***	0.0595***	0.0616***	0.0730***	0.0598***	0.0616***
	(0.0122)	(0.0057)	(0.0058)	(0.0166)	(0.0072)	(0.0071)
Period 2	0.0409***	0.0339***	0.0348***			
	(0.0126)	(0.006)	(0.006)			
Outcome: Share of meat 1 dishes sold						
Period 1	–0.0889***	–0.1050***	–0.1053***	–0.0528*	–0.0654***	–0.0629***
	(0.0247)	(0.0077)	(0.0081)	(0.0282)	(0.0113)	(0.0114)
Period 2	–0.0861***	–0.1015***	–0.0893***			
	(0.0269)	(0.0083)	(0.0087)			
Outcome: Share of meat 2 dishes sold						
Period 1	0.0227	0.0455***	0.0436***	–0.00447	0.0057	0.0013
	(0.0246)	(0.0073)	(0.0079)	(0.029)	(0.011)	(0.0111)
Period 2	0.0640**	0.0676***	0.0545***			
	(0.0257)	(0.008)	(0.0085)			
No. of observations	166	166	166	231	231	231
Vegtype	Yes	Yes	Yes	Yes	Yes	Yes
Meattype	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	No	Yes	Yes	Yes
Holiday FE	No	No	No	Yes	Yes	No
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes

[a] As specified in equation (1). Results are obtained from three separate OLS regressions with either the share of vegetarian dishes, meat 1, or meat 2 dishes sold as the dependent variable.
[b] As specified in equation (2).
[c] Nested multinomial logit model with the two meat alternatives specified as belonging to the same branch and the vegetarian alternative as a second branch.
[d] As specified in equation (3). Results are obtained from three separate OLS regressions with either the share of vegetarian dishes, meat 1, or meat 2 dishes sold as the dependent variable.
[e] As specified in equation (4).
[f] Nested multinomial logit model with the two meat alternatives belonging to the same branch and the vegetarian alternative as a second branch. The holiday fixed effect for the Christmas holidays is omitted because it kept the model from converging.

Results from the nested logit models, which are preferred over the conditional logit results because they are not sensitive to the IIA assumption,[22] show that the increase in the number of

[21] Puhani (2012) shows that the treatment effect in nonlinear difference-in-differences models is given by the incremental effect of the treatment indicator on the outcome variable: $\frac{\Delta Pr[y_{it}=j]}{\Delta Period1 \times Rest1} =$
$$\frac{exp\left(\alpha_j+\beta_j R+\gamma_j P1+\delta_j(R\times P1)+Dishtype_{itj}\times\rho+\lambda_{j,Day}+\lambda_{j,Holid}+\lambda_{j,Month}\right)}{\Sigma_{k=1}^{3} exp\left(\alpha_k+\beta_k R1+\gamma_k P1+\delta_k(R\times P1)+Dishtype_{itk}\times\rho+\lambda_{k,Day}+\lambda_{k,Holid}+\lambda_{k,Month}\right)} - \frac{exp\left(\alpha_j+\beta_j R+\gamma_j P1+Dishtype_{itj}\times\rho+\lambda_{j,Day}+\lambda_{j,Holid}+\lambda_{j,Month}\right)}{\Sigma_{k=1}^{3} exp\left(\alpha_k+\beta_k R+\gamma_k P1+Dishtype_{itk}\times\rho+\lambda_{k,Day}+\lambda_{k,Holid}+\lambda_{k,Month}\right)},$$
where $j,k = 1,2,3$ denote the three dish alternatives. The treatment effect is estimated as the marginal effect at data means. Standard errors are obtained by using the delta method.

vegetarian dishes sold was accompanied by a decrease in the meat 1 dish by around 10 percentage points and an increase in the meat 2 dish by around 4 percentage points. All effects persisted into period 2, when the nudge was removed. As the meat 2 dish constantly was placed at the bottom of the menu, an explanation for the increase in sales could be that the nudge changed its attractiveness relative to the meat dish in period 1. As Dayan and Bar-Hillel (2011) find, items placed at the end points of menus tend to be more attractive for customers than spots in the middle. An alternative explanation for the impact of the nudge on the meat 2 dish is that changing the menu order may have made customers pay more attention to the menu in general. For example, a changing menu order might have led customers to read the whole menu instead of only the first item, reducing primacy effects, which could also explain the persistence of the effect in period 2.

4.6. Changes in the menu order at the control restaurant

The experimental setup at the treated restaurant does not allow to disentangle which part of the nudge caused the increase in the share of vegetarian lunches sold: the change in the menu order or the increased visibility of the vegetarian dish. A priori, it seems more likely that it was the increased visibility that, via a change in saliency and additional information, caused most of the treatment effect. Order effects based on growing fatigue or satisficing behavior seem more apt to arise with longer lists, and previous studies included lists containing many more items than the menus in this study (for example, Dayan and Bar-Hillel, 2011; Feenberg et al., 2015; Policastro et al., 2015). Confirmatory bias, on the other hand, might cause a primacy effect even with such a short list.

Evidence that order effects play at least some role comes from the control restaurant. As mentioned earlier, the new chef changed the menu order during five nonconsecutive weeks, resulting in 22 days with a changed order out of the 79 days in the sample when the new chef was employed. Despite the small sample, when regressing daily sales of all three dish types on a dummy for changed menu order, type of meat or vegetarian dish, and weekday fixed effects in a nested logit model, there is a significant negative effect of –2.4 percentage points from listing the vegetarian dish in the middle instead of at the top (see Table 8). The effect of

[22] In the present case, a formal Hausman test of the IIA assumption did not provide valid results, as the covariance matrix for the difference between the restricted and unrestricted models was not positive definite. This is most likely a finite sample problem. Thus one cannot assume the IIA assumption to hold, and the nested logit model should be preferred.

being put at the top is larger; sales of the meat 1 dish increased by around 4 percentage points on the days when the menu order was changed. The reduction in sales of the meat 2 dish is not significant but still indicates that there was some substitution from the meat 2 to the meat 1 dish when the latter was placed at the top.[23]

Table 8. Effects of the change in the menu order at the control restaurant

	Nested logit model		
	Share of vegetarian dishes sold	Share of meat 1 dishes sold	Share of meat 2 dishes sold
Menu order changed	–0.0237**	0.0380***	–0.0143
	(0.0097)	(0.0123)	(0.0119)
Observations	79	79	79
Vegtype	Yes	Yes	Yes
Meattype	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes

Note: Only observations when the new chef was already employed were used, resulting in 17 weeks of data, of which the menu order was changed during five weeks. Treatment effects are calculated as the incremental effect of the treatment indicator on the outcome variable (Puhani, 2012) and as marginal effects at data means. Standard errors are obtained by using the delta method.
Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Analyzing the menu changes in the control restaurant shows that it is possible not only to be nudged into switching to vegetarian food, but also to be nudged away from it. Moreover, it confirms that the two meat dishes are close substitutes, such that increasing the attractiveness of the meat 1 relative to the meat 2 dish affects sales of the meat 2 dish slightly negatively, although this dish's position never changed throughout the period. Analyzing sales shares of the meat 2 dish at both the treated and control restaurants shows that nudging can have effects on alternatives that were not directly targeted. Taking such unintended effects into account is important when evaluating the overall effects of interventions.

5. Effects of the treatment on lunch GHG emissions

5.1. Substituted dishes

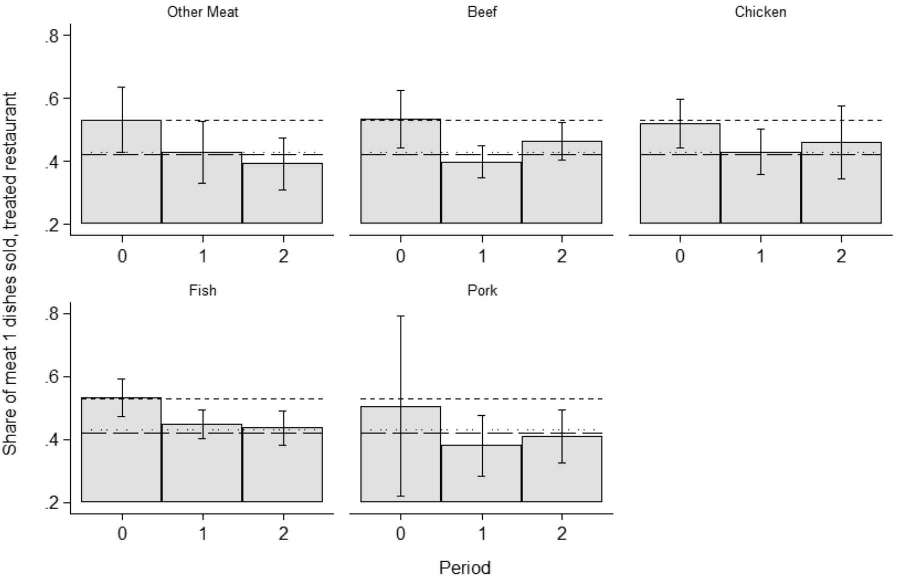
To evaulate the potential of nudging for decreasing food-related GHG emissions, it is necessary to study which type of meat customers substitute away from when being nudged into choosing a vegetarian meal, as different types of meat imply different emissions intensities per kg consumed. In terms of average CO_2 equivalents (CO_2e) emitted per kg of meat sold in Sweden, 1 kg of beef causes the highest emissions, followed by lamb, mixed meats (such as

[23] The results of changing the menu order at the control restaurant are robust to excluding the outlier identified in Figure 2. Excluding that week changes the effect on the vegetarian dish to –0.0216 but leaves significance unaffected. The effect on meat 1 slightly increases to 0.04062, while the effect on meat 2 remains insignificant.

minced meat), pork, chicken, and fish (Bryngelsson et al., 2016). Thus, if consumers substitute vegetarian meals for fish and chicken as a result of the nudge, emissions reductions will be lower than if they reduce their consumption of red meat.

Figure 5 shows that the reduction in sales shares is close to the average reduction for all types of meat served as the meat 1 dish.[24] Figure A.2 in the appendix shows a similar result for meat 2 dishes: the increase in sales of vegetarian dishes did not depend on the type of meat served.[25] For the calculation of climate impacts, it is thus assumed that the nudge affects dishes of the same type (meat 1, meat 2, vegetarian) uniformly.

Figure 5. Shares of meat 1 dishes sold by type of meat across periods, treated restaurant



Note: The short dashed lines represent the mean share of meat 1 dishes sold in period 0; long dashed lines, the mean share sold in period 1; and dotted lines, mean shares sold in period 2. Error bars represent 0.95 confidence intervals around the mean for each type.

[24] Soup was never served as a meat 1 dish in period 0 and was served only twice in period 1, so it was added to the pork dishes, as its regular version contains bacon.

[25] When testing for heterogeneous effects of the treatment depending on the type of meat served in an OLS regression, none of the effects are significant at a 5 percent level. The interaction effect for chicken is positive and significant at a 10 percent level in the regression modelling the sales share of meat 1 dishes as the dependent variable. Full regression results are available from the author on request.

5.2. Approximating the reduction in GHG emissions

How big is the impact of the intervention on GHG emissions of the restaurant? With the help of a few assumptions, a back-of-the-envelope calculation of the impact of the nudge on GHG emissions can be performed. The chef of the treated restaurant provided information on the standard quantities of meat, vegetables, carbohydrates, vegetarian substitutes, and sauces served, which can be found in appendix Table A.3.[26] Those standard portions were used as inputs into the calculations. Emissions of the raw inputs measured in CO_2e per kg were taken from Bryngelsson et al. (2016) and Röös (2014) and can be found in appendix Table A.4. As different types of meat differ in emissions per kg, it is important to account for the frequency of different types of meat served within a period. Similarly, vegetarian industry substitutes such as Quorn or soy products have considerably higher emissions than vegetables, eggs, legumes, or grains, which might otherwise be used to substitute for meat. The daily menus of the treated restaurant were used to identify the frequency of each type of meat or vegetarian substitute served during each period and can be found in appendix Table A.1.

Using the standard portions, the emissions values for the inputs, and the menus, emissions calculations were done in four steps: First, emissions of standard portions were calculated separately for dishes containing different kinds of meat and vegetarian substitutes.[27] The resulting emissions in kg CO_2e of each standard portion can be found in Table 9. Second, predicted sales shares for the three alternatives with and without treatment were taken from the nested logit regressions in section 4.5 and can be found in Table 10. Third, emissions for each period were calculated by using the average number of customers per day, the total length of the period, the number of times each type of meat or vegetarian substitute was served, and the predicted sales share of each dish. Finally, expected emissions for periods 1 and 2 were calculated as if there had been no treatment in period 1 using the same input values as in step three, but using the predicted customer shares without treatment.

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[26] As detailed recipes of all the dishes served at the treated restaurant could not be obtained, exact emissions calculations are impossible.

[27] Emissions values for carbohydrates, vegetables, and sauce were calculated by taking the average of all available values and do not differ between the meat and vegetarian dishes. With respect to the input of dairy products, the chef stated that the vegetarian meals contain less dairy than the meat dishes because he tries to keep many dishes vegan. As he could not quantify the difference, equal inputs of dairy for all dish types were assumed.

Table 9. CO_2e emissions of standard portions

	Standard portions containing meat or fish							Vegetarian standard portions	
Main component	Fish	Pork	Beef	Poultry	Other meat	Only high-emitting meats[a]	Only low-emitting meats[b]	Veg. substitute	No sub-stitute
Emissions (kg CO_2e)	1.06	1.56	7.46	0.97	4.16	7.06	1.20	0.99	0.65

[a] Includes only ruminant meats (beef and mutton).
[b] Includes only nonruminant meats (pork, poultry, and fish).

Results of the calculations are shown in Table 10. Scenario 1 compares emissions in period 1 (from step three) with expected emissions if the nudge had not been in place (from step four) using the point estimates of the treatment effect. Emissions with the nudge in place were 4.8 percent lower than they would have been without the intervention. During the reversal period, emissions were still 3.8 percent lower than they would have been if there had been no intervention in period 1. This relatively high reduction in emissions compared with the size of the treatment effect can be explained by the fact that during period 2, high-emitting meat such as beef was part of the meat 1 dish more often than during period 1, while the opposite was the case for the meat 2 dish. So although the share of meat dishes sold increased during the reversal period compared with the treatment period, emissions did not increase to the same extent, as customers substituted between the two meat dishes. This shows that not only demand, but also what is offered, drives emissions.

To explore the sensitivity of the emissions reductions with respect to the size of the treatment effect, scenarios 2 and 3 use the lower and upper bounds of the estimated effect as given by the 95 percent confidence interval. Total emissions are reduced by 0.7 and 8.5 percent, respectively, in those scenarios. The importance of the kind of meats served for GHG emissions is further explored in appendix Table A.5. Scenario 4 eliminates the impact of the changing menu on emissions by using the average menu composition across the whole year for calculating emissions in periods 1 and 2 (see the last column of appendix Table A.5). Using such an average menu results in a reduction of 5.4 percent of emissions in period 1 and 3 percent in period 2, showing that emissions reductions depend heavily on the types of meat offered. Scenario 5 assumes that all meat served is ruminant meat (beef and mutton), which is high in CO_2e emissions per kg. If the nudge were applied on such a menu (all other things equal), total emissions reductions would be larger (6.4 percent in period 1 and 3.6 percent in period 2). On the other hand, if the menu is already “climate-friendly” and only pork, poultry, and fish are served as the meat dishes, the emissions reduction potential of the nudge is smaller (2.9 and 1.9 percent).[28]

It should be noted that the relatively high reductions in GHG emissions found in the simulation are partly driven by the assumption that cheese was not substituted for meat. As Chen et al. (2016) show, substituting cheese for meat significantly decreases the reduction potential of vegetarian food and in some cases (if climate-friendly meats such as chicken or pork are substituted away from) might even lead to an increase in GHG emissions. In the present case, the chef stated that vegetarian dishes did not contain more cheese than in the meat dishes, but the GHG reduction potential of nudging toward vegetarian diets heavily depends on the kind of vegetarian food served at the restaurant in question.

Looking at the demand elasticities of meat can provide an idea about the price changes necessary to achieve reductions in demand comparable to those from the nudge, and hence about climate change mitigation costs. Estimated own-price elasticities in Sweden are –0.538 for beef, –0.370 for pork, and –0.363 for chicken (Säll and Gren, 2015). Thus a 6 percent reduction in demand would require price increases by 12 to 16 percent.

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[28] In terms of absolute reductions, scenario 1 shows that the nudge reduced emissions by a total of 1.77 tCO_2e. This corresponds approximately to the emissions of driving a car for 11,056 km in Sweden, based on the average CO_2 emissions of the car fleet (160g/km in 2015; see Swedish Traffic Agency, 2016).

Table 10. Predicted sales shares and CO_2e emissions with and without treatment, periods 1 and 2, treated restaurant

		Period 1 (63 days, 157 customers/day)				Period 2 (56 days, 125 customers/day)				Both periods	
		Treatment effect[a]	Predicted customer shares[a]	Total emissions (kg CO_2e)	% change: treatment – no treatment	Treatment effect[b]	Predicted customer shares[b]	Total emissions (kg CO_2e)	% change: treatment – no treatment	Total emissions (kg CO_2e)	% change: treatment – no treatment
With treatment											
	Vegetarian dish		0.199	23,082			0.171	15,725		38,807	
	Meat 1		0.425				0.438				
	Meat 2		0.376				0.391				
Without treatment											
Scenario 1: Point estimates of the treatment effect	Vegetarian dish	0.062	0.137	24,253	**–4.8%**	0.035	0.136	16,354	**–3.8%**	40,607	**–4.4%**
	Meat 1	–0.063	0.488			–0.089	0.527				
	Meat 2	0.001	0.375			0.054	0.337				
Scenario 2: Lower bound of absolute treatment effect	Vegetarian dish	0.048	0.151	23,144	**–0.3%**	0.023	0.148	15,931	**–1.3%**	39,075	**–0.7%**
	Meat 1	–0.041	0.466			–0.072	0.510				
	Meat 2	–0.007	0.383			0.049	0.342				
Scenario 3: Upper bound of absolute treatment effect	Vegetarian dish	0.075	0.124	25,362	**–9.0%**	0.047	0.124	17,039	**–7.7%**	42,401	**–8.5%**
	Meat 1	–0.085	0.510			–0.106	0.544				
	Meat 2	0.010	0.366			0.059	0.332				

[a] Based on the estimation results in Table 7, column (6).
[b] Based on the estimation results in Table 7, column (3).

6. Conclusion

The results of the field experiment show that it is possible to nudge consumers into more climate-friendly diets. Making the vegetarian dish more salient by increasing its visibility and changing the menu order increased its sales share by around 6 percentage points, which constitutes an increase of around 40 percent of the sales share of the vegetarian dish compared with the baseline period. Although it is not possible to separate the impact of the two changes that were made simultaneously, the analysis of menu order changes at the control restaurant points toward both playing a role in the increase in sales shares. Analyzing the development of the effect over time and the effect of the restoration of the original setup shows that nudging can lead to persistent changes in behavior. This is the first study providing evidence that nudging can have a significant impact on what people eat well into the future, with the share of vegetarian dishes sold remaining about 4 percentage points higher after the intervention was ended. Testing for individual habit formation is not possible with the type of data collected from this experiment but is an important area for future research. The experiment also shows that the intervention affected consumption of all three options on the menu, although at both the treated and the control restaurants, only two dishes were subject to changes in the decision environment. The possibility of such unexpected consequences should be carefully considered when designing nudges, not only in the food domain. Although it is not possible to analyze the effect of the nudge on total sales, it should be mentioned that the restaurant permanently changed its setup to the nudge condition during the academic year 2016–17, indicating that it does not expect a negative impact on profits from changing the decision environment.

How does the nudge compare with other interventions targeted at reducing meat consumption? One such intervention is the establishment of mandatory vegetarian days in public catering. Lombardini and Lankoski (2013) study such a vegetarian day at public schools in Finland. They find that even though its introduction had positive spillover effects on parts of the population, such that they increased their consumption of vegetarian food even on other days, it also caused noncompliance in the form of less food taken and higher plate waste on the vegetarian days. The authors conclude that setting appropriate default choices might be a better option to increase the consumption of vegetarian food than restricting choices. Another type of intervention is environmental labelling of food. Although such labelling usually does not target meat consumption directly, meat products often do worse in environmental assessment. In a test of an environmental label in an experimental supermarket by Vlaeminck et al.

(2014), the share of meat products dropped by around 20 percent in their most effective treatment. Climate labelling of meals has been tested by Visschers and Siegrist (2015), who find that marking the two most climate-friendly choices out of four meals at a university restaurant led to a 20 percent higher share of climate-friendly choices.

With respect to policy recommendations, the external validity of the results has to be reflected. The restaurant where the nudge was implemented is frequented by a highly educated, rather young population consisting of university students and employees. Younger people might be more open to trying new types of food, and the nudge used here may have a smaller effect in a setting with an older population. On the other hand, data from the Swedish National Food Agency (2012) show that meat consumption is as high amongst people 18–30 years old as it is in the rest of the adult population, and that there are no differences in meat consumption with regard to educational level. Thus the potential for reducing meat consumption through nudging in the Swedish population seems to be equally high across ages and educational levels. Comparing Sweden with other countries with respect to willingness to reduce meat consumption is difficult, but according to a WWF (2016) survey, vegetarian food generally has a positive image in this country, as 30 percent of the respondents associate vegetarian food mainly with healthy eating. Such an association could increase the effectiveness of the nudge. Vetter and Kutzner (2016) do not find an interaction between environmental attitudes and the strength of green default effects in a hypothetical choice situation, but how attitudes and nudges for environmentally friendly food choices interact must be left for further research. Policy recommendations from the present experiment are therefore limited to the fact that it is possible to reduce meat consumption through effective and inexpensive behavioral interventions. Which specific intervention should be chosen depends on the characteristics of the restaurant. The case examined in this study offered good opportunities for nudging, as it was possible to change both the visibility of the vegetarian dish and the menu order simultaneously.

Finally, when considering nudging as a tool for changing consumption patterns, one should keep in mind possible ethical objections against nudging. Recent years have seen a lively and controversial debate on policy-making based on behavioral insights. Many feel that transparency is important for the use of nudging to be acceptable (see, for example, Hansen and Jespersen, 2013). Restaurants and policy-makers who want to use nudges to reduce the climate impact of food consumption should try to do so as transparently as possible. For example, if changes are made to the food environment in a faculty restaurant similar to the one in this study, transparency could be increased by informing customers via the weekly menu email that many subscribe to or by putting an appropriate note on the menus set up in the restaurant.

With respect to the environmental significance of the results, it should be pointed out that this paper studies just one of the many food decisions people make every day. Although lunch constitutes one of the larger meals of the day, it is possible that consumers compensate for having chosen vegetarian later during the day. Future research should address such compensation effects, which are important when evaluating not only food nudges, but also nudges for the environment in general. Back-of-the-envelope calculations on the climate impact of the nudge show that emissions from the restaurant's sales decreased by around 5 percent during the intervention period, compared with a scenario without the nudge being implemented. As the nudge was costless for the restaurant to implement and did not affect profits negatively, this paper shows that nudging is a promising tool to foster more climate-friendly food choices.

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Appendix

Figure A.1. Example menus from treated restaurant, period 0 and period 1

Måndag

Örtbakat kycklinglår med tzatziki samt ris
Provencalsk lökpaj med tomat och mozzarella
Pasta med rökt sidfläsk, kvibille ädelostås och örtrostade solroskärnor

Tisdag

Ugnsbakad sejfilé med vitvin- & gräslökssås
Curry- och kokoswokade nudlar med quorn och grönsaker
Lammfärsbiff med rostad blomkål och spiskummin samt gula ärtor med mejram och lök

Onsdag

Lättrimmad ugnsbakad lax med sojacremefraiche samt grönsaksris
Spaghetti med rostad paprikasås, råmarinerad zucchini samt grana padano
Stekt fläsk med löksås samt kokt potatis

Torsdag

Boeuf Bourguignon - långkokt högrev med svamp, smålök och med sidfläsk
Böngulasch med ris och crème fraiche
Pulled pork serveras med tomat- och mangosalsa, gräddfil och ris

Fredag

Boston och chiligratinerad falukorv med potatismos
Tagliatelle med quornfärssås samt Grana Padano
Stekt kycklingbröst med mango- och grönsaksyoghurt samt ris

Lunch med salladsbuffé, dressing, bröd & smör 70 kr | Komplett lunch inkl. kaffe 75 kr

Soppa – inklusive salladsbuffé, bröd & smör 50:- Endast soppa 35:-

Goda sallader från 30 kr

Måndag

Tagliatelle med grönärtpesto och mynta
Rosmarinstekt kycklingbröst med äpple, sherrysås samt ris
Basilikabakad laxfilé med tomatgräddsås samt kokt potatis

Tisdag

Spenatlasagne med getost och rostade solroskärnor
Katalansk högrevsgryta med chorizo, oliver och tomat samt kokt potatis
Fläskköttswok med thaibasilika, sweet chiliyoghurt och basmatiris

Onsdag

Cannelloni med matvete och champinjoner samt spenatsallad toppad med solroskärnor
Stekt pannbiff med rödlök- och balsamicosmör, löksky samt kokt potatis
Smörstekt sej med syrlig gurksallad och pepparrotsyoghurt serveras med kokt potatis

Torsdag

Ärtsoppa med pannkakor samt sylt och grädde
Baconlindad chorizo med kryddig paprikarelish och potatismos
Vitlöksmarinerad kycklingklubba med currycoleslaw samt rostade rotsaker

Fredag

Kikärtscurry med kokos och koriander samt ris
Sprödbakad torsk med remouladsås och kokt potatis samt citron
Timjan & fänkålsstekt fläskkarré med plommon och balsamicosås samt rostad klyft potatis

Lunch med sallad 70 kr | lunch inkl. kaffe/te 75 kr
Sallader från 30 kr | Soppa 35 kr

Note: The vegetarian option is highlighted by a frame, which was added by the author and not part of the original menu.

Table A.1. Menu composition of the two restaurants across periods

Type of dish	**Period 0** # of days served	**Period 0** share of days	**Period 1** # of days served	**Period 1** share of days	**Period 2** # of days served	**Period 2** share of days	**All year** # of days served	**All year** share of days
				Treated Restaurant				
Vegetarian								
Other vegetarian	8	0.17	10	0.16	10	0.18	28	0.17
Pasta	8	0.17	11	0.175	8	0.14	27	0.16
Patty	5	0.11	9	0.14	15	0.27	29	0.17
Stew	7	0.15	11	0.175	9	0.16	27	0.16
Vegetables	9	0.19	7	0.11	8	0.14	24	0.14
World	7	0.15	7	0.11	5	0.09	19	0.11
Soup	3	0.06	8	0.13	1	0.02	12	0.07
Total	*47*	*1.00*	*63*	*1.00*	*56*	*1.00*	*166*	*1.00*
Processed substitute (Quorn, tofu, soy)[a]	4	0.09	3	0.05	4	0.07	11	0.06
Other substitute[a]	43	0.91	60	0.95	52	0.93	155	0.94
Meat 1 dish								
Beef	13	0.28	10	0.16	9	0.16	32	0.19
Chicken	9	0.19	12	0.19	7	0.12	28	0.17
Fish	12	0.26	20	0.32	19	0.34	51	0.31
Other meat	10	0.21	9	0.14	10	0.18	29	0.17
Pork	3	0.06	9	0.14	9	0.16	21	0.13
Soup	0	0	3	0.05	2	0.04	5	0.03
Total	*47*	*1.00*	*63*	*1.00*	*56*	*1.00*	*166*	*1.00*
Meat 2 dish								
Beef	6	0.13	10	0.16	5	0.09	21	0.13
Chicken	8	0.17	7	0.11	18	0.32	33	0.20
Fish	12	0.26	17	0.27	12	0.22	41	0.25
Other meat	11	0.23	15	0.24	13	0.23	39	0.23
Pork	10	0.21	14	0.22	8	0.14	32	0.19
Total	*47*	*1.00*	*63*	*1.00*	*56*	*1.00*	*166*	*1.00*
				Control Restaurant				
Vegetarian								
Other vegetarian	12	0.25	11	0.16	6	0.11	29	0.16
Pasta	7	0.14	8	0.11	8	0.15	23	0.13
Patty	10	0.20	16	0.22	9	0.17	35	0.20
Stew	11	0.23	16	0.22	13	0.24	40	0.23
Vegetables	5	0.08	15	0.21	12	0.12	31	0.18
World	5	0.10	6	0.08	6	0.11	17	0.10
Total	*49*	*1.00*	*72*	*1.00*	*54*	*1.00*	*175*	*1.00*
Meat 1 dish								
Beef	5	0.10	5	0.07	5	0.09	15	0.09
Chicken	1	0.02	5	0.07	10	0.18	16	0.09
Fish	17	0.35	22	0.31	8	0.15	47	0.27
Other meat	6	0.12	13	0.18	16	0.30	35	0.20
Pork	10	0.21	13	0.18	9	0.17	32	0.18
Soup	10	0.20	14	0.19	6	0.11	30	0.17
Total	*49*	*1.00*	*72*	*1.00*	*54*	*1.00*	*175*	*1.00*
Meat 2 dish								
Beef	11	0.23	4	0.06	6	0.11	21	0.12
Chicken	13	0.27	19	0.26	6	0.11	38	0.22
Fish	6	0.12	21	0.29	21	0.39	48	0.275
Other meat	10	0.20	18	0.25	13	0.24	41	0.235
Pork	9	0.18	10	0.14	8	0.15	27	0.15
Total	*49*	*1.00*	*72*	*1.00*	*54*	*1.00*	*175*	*1.00*

[a] Used for calculating the climate impact of the intervention but not used in the regressions.

Table A.2: Heterogeneous effects: Type of vegetarian dish, period 0 and 1

Dependent variable: Share of vegetarian dishes served	Before-after + type	Heterogenous effects	Basic DiD + type	+ Heterogenous effects
Period 1	0.0703***	0.0658**	–0.0149	–0.00882
	(0.0122)	(0.0281)	(0.0243)	(0.0341)
Period 2	0.0409***	0.0466		
	(0.0126)	(0.0296)		
Restaurant 1			–0.138***	–0.140***
			(0.0134)	(0.0274)
Rest 1 × Period 1			0.0730***	0.0776**
			(0.0166)	(0.0378)
Period 1 × Pasta		0.0234		
		(0.0397)		
Period 1 × Patty		0.0502		
		(0.0440)		
Period 1 × Stew		–0.0389		
		(0.0413)		
Period 1 × Vegetables		0.0318		
		(0.0430)		
Period 1 × Soup		0.0140		
		(0.0496)		
Period 1 × World		–0.0270		
		0.0234		
Period 2 × Pasta		0.00567		
		(0.0438)		–0.00136
Period 2 × Patty		–0.0127		(0.0554)
		(0.0425)		0.0402
Period 2 × Stew		0.00252		(0.0540)
		(0.0429)		–0.0585
Period 2 × Vegetables		0.0196		(0.0534)
		(0.0431)		0.0717
Period 2 × Soup		0.110		(0.0599)
		(0.0766)		–0.0159
Period 2 × World		–0.0796*		(0.0608)
		(0.0470)		–0.00136
				(0.0554)
Constant	0.154***	0.156***	0.320***	0.301***
	(0.0315)	(0.0336)	(0.0353)	(0.0382)
Observations	166	166	231	231
Adj *R*-squared	0.268	0.297	0.526	0.524
Vegtype	Yes	Yes	Yes	Yes
Meattype	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	Yes
Holiday FE	No	No	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes

Note: Including all weeks in period 0 and 1. The baseline category for the type of vegetarian dish is other vegetarian. The specifications in columns 1 and 3 correspond to the specifications in column 4 of Table 6 and column 8 of Table 4, respectively, and are shown for comparison reasons.
Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3. Summary of the input values used for emissions calculations

Input category	Input quantity		Assumptions regarding CO_2e emissions
	Meat dishes	Vegetarian dish	
Meat or fish (raw input): • Beef • Pork • Chicken • Fish • Other meat	160 g		Individual values for beef, chicken, pork and fish[b] If category "other meat": average of the values for beef, poultry, mutton, and pork
Vegetarian substitute: • Processed product (Quorn, or processed soy product such as tofu) • Combination of legumes, potatoes, vegetables, and eggs		110 g	Processed products (Quorn, soy, tofu): emissions values from Röös (2014) Unspecified substitutes: average of the value for vegetables, legumes, flour, and potatoes
Vegetables • Combination of cabbage, onions, potatoes, roots, and green vegetables	50 g	100 g	Average of all values for vegetables
Carbohydrates/starch (cooked) • Combination of rice, potatoes, pasta, other grains, and flour	200 g	210 g	Average of the values for rice, potatoes, and pasta
Sauce/dairy products • Combination of liquid dairy, cheese, butter, and water	100 ml[a]	100 ml[a]	Average of the values for liquid dairy, butter, and cheese
Total weight	510 g	510 g	

[a] On average, 50 ml is water.
[b] For beef, it was assumed the origin was Western Europe. For fish, the emissions value of wild fish was used and not the (higher) value for farmed fish, as it is impossible to determine from the menus whether wild or farmed fish was used.

Table A.4. Emissions values used for climate impact calculation

	Emissions values in CO_2e/kg fresh product
Dairy products and eggs	
Butter[a,d]	11
Cheese[a]	11
Liquid dairy[a]	1.2
Average dairy[c,d]	7.73
Eggs[a]	0.97
Vegetables	
Green vegetables[a]	0.7
Cabbage, onions[a]	0.23
Potatoes, roots[a]	0.14
Average vegetables[c,e]	0.36
Meat, fish	
Fish[a]	3
Beef[a]	43
Mutton[a]	38
Pork[a]	6.1
Poultry[a]	2.4
Average meat (average of beef, mutton, pork, poultry)[c,f]	22.38
Average ruminant meat (beef and mutton)[c,j]	40.5
Average pork, poulty, fish[c,k]	3.83
Carbohydrates	
Rice[a]	1.8
Pasta[a]	0.57
Other grains, flour[a]	0.3
Average cereals[c]	0.89
Average carbohydrates (cereals + potatoes)[c,g]	0.70
Vegetarian substitute products	
Vegetable protein from legumes[a]	0.54
Average vegetarian substitutes (legumes, carbohydrates, vegetables, eggs) except processed substitutes [c,h]	0.48
Soy meat substitute (tofu, soy sausage, etc)[b]	3
Quorn[b]	4
Average processed vegetarian substitutes[c,i]	3.5

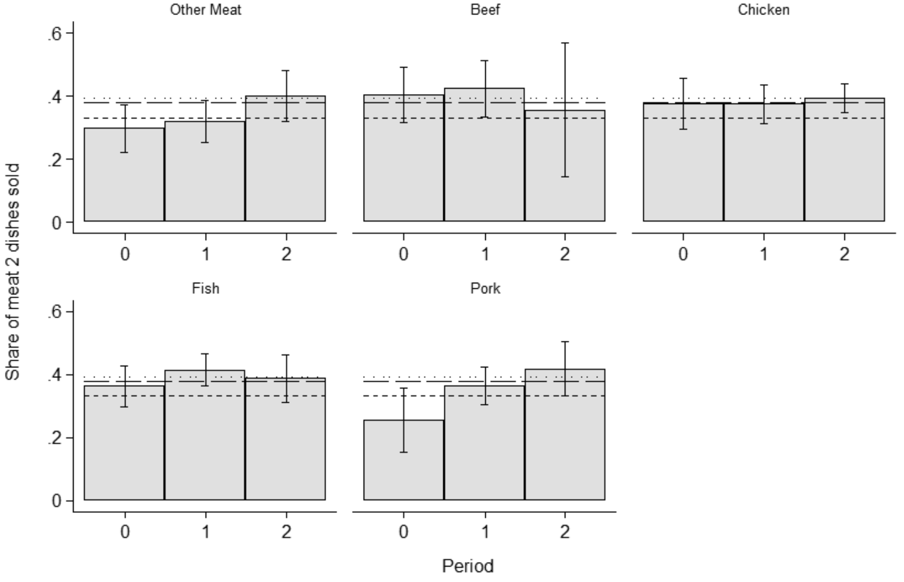
[a] *Source*: Bryngelsson et al. (2016).
[b] *Source*: Röös (2014).
[c] Own calculations, arithmetic mean of the raw input values.
[d] Used as input value for dairy products.
[e] Used as input value for vegetables.
[f] Used as input value for other meat.
[g] Used as input value for carbohydrates.
[h] Used as input value for vegetarian substitute.
[i] Used as input value for processed vegetarian substitute.
[j] Used as input value for high-emitting meats.
[k] Used as input value for low-emitting meats.

Table A.5. Sensitivity of the emissions simulations with respect to the type of meat served

	Period 1 (63 days, 157 customers/day)			Period 2 (56 days, 125 customers/day)			Both periods		
	kg CO_2e treated	kg CO_2e not treated	% change	kg CO_2e treated	kg CO_2e not treated	% change	kg CO_2e treated	kg CO2e not treated	% change
Actually observed composition of dishes each period (scenario 1)	23,082	24,253	–4.8%	15,725	16,354	–3.8%	38,807	40,607	–4.4%
Scenario 4: Average composition of dishes across the whole year[a]	23,359	24,699	–5.4%	17,446	17,980	–3.0%	40,805	42,679	–4.4%
Scenario 5: Only high-emitting meats served[b]	57,281	61,174	–6.4%	41,772	43,328	–3.6%	99,053	104,502	–5.2%
Scenario 6: Only low-emitting meats served[c]	10,799	11,117	–2.9%	8,610	8,774	–1.9%	19,409	19,891	–2.4%

[a] Instead of using the menu composition each period, the average menu composition was used as given in the last column of appendix Table A.1).
[b] Assumption: Only ruminant meats (beef and mutton) are served as part of the meat 1 and meat 2 dishes.
[c] Assumption. Only pork, fish, and poultry are served as part of the meat 1 and meat 2 dishes.

Figure A.2. Share of meat 2 dishes sold across period by type of meat offered, treated restaurant



Note: The short dashed lines represent the mean share of meat 2 dishes sold in period 0; long dashed lines, the mean share sold in period 1; and dotted lines, mean shares sold in period 2. Error bars represent 0.95 confidence intervals around the mean for each type.

Chapter II

Nudging à la carte: A field experiment on food choice[1]

Christina Gravert[a] and Verena Kurz[b]

Abstract

We tested the effect of framing of a menu on the choice of ordering climate-friendly dishes in a randomized controlled experiment. We varied the convenience of either the vegetarian or the meat option out of three dishes offered. Rearranging the menu in favor of vegetarian food had a large and significant effect on the willingness to order a vegetarian dish instead of meat. However, this effect decreased over the three-week treatment period. We discuss potential channels through which our intervention might affect behavior and how our results can be interpreted with respect to those channels. Our results demonstrate that small, inexpensive interventions can be used toward decreasing carbon emissions from food consumption.

JEL classification: D12, Q50, C93

Keywords: nudging, field experiment, decision heuristics, food choice

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1. Introduction

The assumption of stable individual preferences is still one of the cornerstones of consumer theory. When given a choice among a number of options, consumers should choose the option that maximizes their utility regardless of how the options are presented and the context they are put in, as long as the prices stay constant. However, a large amount of work in psychology and economics shows that this assumption is often violated (Slovic, 1995; Tversky and Simonson, 1993; Thaler and Sunstein 2008).

In this paper, we examine how stable individual preferences are in one of the most frequent decisions individuals make: deciding what to eat for lunch. We conducted a field experiment with a restaurant to test whether decreasing the convenience of ordering a meat dish and simultaneously increasing the convenience of ordering a vegetarian dish would decrease the sales of the meat option in favor of the vegetarian dish. Over the course of three weeks, customers entering the restaurant were randomly presented with one of two menus. One menu offered a meat dish and a fish dish, with a note that a vegetarian option was available on request. The other menu offered a vegetarian dish and a fish dish, with a note that a meat dish was available on request. The results show how a small change in the framing of different options can have a substantial impact on the choices individuals make. The vegetarian and fish menu resulted in 25 percent lower sales of the meat dish than the meat and fish menu.

As meat is one of the major contributors of greenhouse gases (GHG) emerging from consumption, our results are of relevance for both public and private actors in the food sector wanting to mitigate GHG emissions of their operations. Our experiment contributes to the discussion on how to reduce climate emissions from food consumption. While food production was responsible for about 16 percent of global greenhouse gas (GHG) emissions in the period 2005–7 (Springmann et al., 2016), GHG emissions per calorie vary widely among types of foods. Diets rich in meat and dairy products entail higher CO_2 emissions than plant-based diets. Tilman and Clark (2014) estimate that omnivorous diets are approximately four times higher in carbon intensity per calorie consumed than comparably nutritious vegetarian diets. Although climate benefits from reducing meat consumption are estimated to be large (Bryngelsson et al., 2016; Springmann et al., 2016; Westhoek et al., 2014), there are currently no policy instruments in place that target meat consumption directly. Meat taxes have been discussed in the scientific community (Säll and Gren, 2015; Wirsenius et al., 2011) but not implemented in any country yet. Initiatives to encourage individuals to reduce meat consumption, such as meat-free days, are limited in their outreach and probably also in their effectiveness. Forced choice restrictions such as mandatory vegetarian days in school and cafeterias entail the risk of causing psychological reactance and, ultimately, backlash (Lombardini and Lankoski, 2013).

Increasingly, researchers and policy-makers call for nudges (Thaler and Sunstein, 2008), behavioral interventions that neither change prices, choices, or the information that is given, to promote sustainable consumption choices in the food domain (Girod et al., 2014; Lehner et al., 2015). While there is evidence that nudging can push people toward making healthier food choices under some circumstances (Ellison et al., 2014; Just, 2009; Wansink, 2004; Wansink and Hanks, 2013; Wisdom et al., 2010), the evidence on the effectiveness of nudges for promoting sustainable food choices is limited. Our study is one of the first to test whether a behavioral intervention can be used to increase the consumption of vegetarian food. Ideally, the effectiveness of such an intervention would be evaluated by comparing its costs and benefits with those of other policy instruments with a similar aim. However, as no such policy instruments are currently in place, we will define an effective intervention as one that succeeds in significantly reducing meat consumption.

Our intervention is closely related to an experiment conducted by Wisdom et al. (2010) regarding sandwich choices in a fast-food restaurant. In their experiment, a set of unhealthy sandwiches was made less convenient to order either by putting them on a menu that was placed in a sealed folder or by listing them on a separate page from a set of "featured" sandwiches serving as an implicit default. The authors find that both interventions affected sandwich choice, with the first one (sealing parts of the menu) having a larger effect. The benefit of our study compared with that of Wisdom and colleagues is that in our experiment, customers were not aware that they were taking part in a study. We can thus be certain that the choices observed were not affected by experimenter demand effects. Moreover, we explicitly test whether such a convenience intervention can also be used to reduce meat consumption.

We find that a small decrease in the convenience of ordering the meat option, by making it necessary to ask the waiter to describe the dish, resulted in a significant decrease in the share of dishes containing meat sold at lunch and an increase in the shares of both vegetarian and fish dishes sold. The share of meat dishes sold decreased from an average of 47 percent before the intervention to around 21 percent in the treatment condition, where it was not directly displayed on the menu. This indicates that there is potential for restaurants to decrease the meat intensity of their dishes offered without banning meat items altogether or changing prices. Our results demonstrate both to policy-makers and to actors in the food service sector that small, inexpensive interventions can significantly decrease carbon emissions from food consumption. However, as we discuss in more detail in section 3, we are not able to identify the exact channel through which our intervention works. Depending on which of the potential channels dominates, our intervention can be classified either as a nudge, working mainly through decision heuristics, or as an intervention changing the (perceived) cost-benefit ratio of the lunch options.

The paper continues as follows. Section 2 presents the experimental design. In section 3, we discuss possible channels through which the intervention might influence behavior. Section 4 presents the data and the experimental results. Section 5 concludes.

2. Experimental design

The experiment was conducted for three weeks in May 2016 at a restaurant located in Gothenburg, Sweden. Food items are served à la carte during the evening and on weekends, but on weekdays, a daily changing lunch menu is available for two hours. Each day, the kitchen prepares two dishes for lunch: one containing meat and one with fish. On request, the kitchen also will prepare a vegetarian meal. All dishes include salad and bread and cost 110 SEK (approximately US$13), which puts it in the medium-priced category for Gothenburg restaurants according to the TripAdvisor website. On the restaurant's website, the food is described as "modern with tastes from around the world". The meat and fish options were approximately equally popular before our experiment started. The restaurant is frequented mainly by white-collar employees who work in the service sector and the arts, as the restaurant is located in the city center close to a major museum, a concert hall, and a library. It has 52 seats and space for a handful of people at the bar. Our experimental treatments make use of two specific features of the restaurant setup: the architecture of the restaurant and the design of the lunch menu.

Regarding architecture, the restaurant has two areas, which are separated partly by a wall and partly by a bar acting almost as a physical border (see appendix Figure A.1). The front part, where customers enter, has 30 seats. The back area has 22 seats. The lunch menu is printed each week on A3 coated paperboard and lists the options for the whole week. Proceedings during our experiment were as follows: On arrival, customers were seated by a waiter. Regular customers were seated at their regular tables as much as possible. Nonregulars were seated according to the size of the group. If there were several free tables, the waiter pointed out one possibility in the front and one in the back from which the customers could choose.[2] Once a customer or group of customers was seated, the waiter handed out the menus.

No menus are set up at the wall, at the entrance, or outside the restaurant. Our treatments built on this by letting the waiters hand out different menus to customers seated in the front area versus customers seated in the back. If customers wanted to have a look at the menu before deciding whether to eat at the restaurant, a waiter would give them a menu sheet from the bar. During the experiment, this was always the vegetarian and fish menu. Consequently, customers who wanted to have a look at the menu first were seated in the (slightly bigger) front area. We can rule out that any customers self-selected out of the experiment, as the waiters assured us that no guests left the restaurant after having looked at the menu.

Before the start of our experiment, the weekly lunch menu listed the two main options, one containing meat and the other containing fish. A vegetarian dish was available on request and could be customized to a vegan version. Nowhere on the original menu, which was distributed throughout the whole restaurant, was it stated that a vegetarian or vegan dish was available. We collected weekly sales data on the number of vegetarian, meat, and fish dishes sold at lunch for four weeks before our intervention.[3]

During the intervention, the waiters handed out two different menus at the restaurant. One menu contained, as before, the daily meat and fish options for the whole week, but it had an additional sentence stating, “A vegetarian option is available on request.” We added this sentence to test whether simply giving information about availability could increase the sales of vegetarian dishes. Customers seated in the back part of the restaurant received this menu. The menu distributed to customers seated in the front differed by listing the daily vegetarian and fish dishes but not the meat dish. Comparably to the menu distributed in the back, we added a sentence stating, “An option containing meat is available on request.” Thus the menu distributed in the front made it slightly less convenient to order the meat dish.[4] Customers had to summon a waiter and ask what the meat dish was to be able to consider it along with the options spelled out on the menu. On the other hand, the convenience of ordering the vegetarian dish increased for those customers seated in the front, compared with the setup before the experimental period and in the back part of the restaurant during the experiment. The

[2] As the restaurant has only 52 seats at 20 tables, which can be grouped together for more than two people, and it is quite busy during lunch, there is not much flexibility in seating the guests.
[3] It should be stressed that no modifications were made to the menu during those four weeks; it remained the same as during the restaurant’s previous operations. The restaurant’s menu had listed only two dishes for a long time, although a vegetarian option was available by special request.
[4] The rearranging of the menu most likely influenced behavior through several behavioral channels other than pure convenience. The experimental design and the resulting data do not allow us to disentangle the different channels. However, section 3 provides a discussion on the potential mechanisms.

convenience of ordering the fish dish remained the same across periods and areas. On both menus, the fish was the second dish presented on the menu. Consequently, the vegetarian and the meat dishes were presented in the same spot. For simplicity, the vegetarian dish was usually the same as the meat dish except that the meat was replaced by a vegetable, grain, or plant protein. An advantage of this setup is that other ingredients would not affect choice and would have a similar climate impact. In addition to the lunch options, the menus also listed two desserts, which were the same across treatments for the whole week.

The intervention lasted for three weeks, during which we collected daily sales data of the three lunch options by area in the restaurant, front and back. One advantage of the experimental design is that we have two control periods available. The pre-experimental period mainly serves as a control to check whether the behavior of the customers seated in the back part of the restaurant changed during the experimental period. If so, it indicates that even just adding information on the availability of a vegetarian dish can affect behavior. For evaluating the effect of making the meat less convenient to order, data from the back part of the restaurant served as the control during the intervention period. The control and treatment groups of customers were subject to the same dishes available and to the same external factors, such as weather conditions, holidays, and other daily variations, which could otherwise act as confounding factors. A major advantage of this design is that we can control for an important event that happened during our study: because of unexpectedly nice weather in May, the restaurant opened its outdoor serving area on May 9 instead of June 1 as originally planned. The restaurant staff made sure to define different areas of approximately the same size in the outdoor serving area within which to distribute the two different menus. However, the outdoor serving area did not feature any physical border between the two areas.

After the intervention, the control area menu (the one containing the meat and fish options only) was used in the whole restaurant to analyze whether the intervention had any effect after its termination.[5]

3. Possible channels for the experiment's influence on behavior

Our intervention made use of what Thaler and Sunstein (2008) call the choice architecture of decisions: we did not change the options served or the prices, but the context in which individuals made their choice—in our case, by changing the design of the menu. By varying

[5] We recognize the fact that one week is very short for an ex-post period. A longer observation period was impossible, as the lunch menu changed completely on June 1 to the restaurant's summer menu. Consequently, during the eight weeks in which data collection was possible, we collected four weeks of preintervention data, three weeks of intervention data, and one week of postintervention data.

both the order in which the alternatives were presented and the visibility of the alternatives, the treatment changed two aspects of the menu layout. These changes were intended to nudge people into choosing the vegetarian option by exploiting features of human decision-making that are summarized in what is called the dual process theory of thinking and deciding. Dual process theory describes thinking and deciding as subject to two modes: one fast and intuitive, and the other slow and reflective (see, for example, Kahneman, 2003). Under the assumption that decisions dominated by the intuitive mode are more responsive to changes in the choice architecture, nudging interventions often target areas where the degree of automaticity is assumed to be high, such as food choice (Marteau et al., 2012; Schulte-Mecklenbeck et al., 2013; Wansink and Sobal, 2007). We expect customers' behavior to respond to our nudge based on three factors relevant for intuitive decision-making: environmental cues, triggering or disrupting habits, and consumption norms.

Environmental cues. Cohen and Babey (2012) discuss how food choice is to a large extent governed by automatic and simplified processes, where environmental cues play an important role. For example, how salient a food item features in a decision problem affects its likelihood of being chosen. Giving less room to the meat dish and describing it in less detail than the other two dishes decreases its salience (Cohen and Farley, 2007; Wansink and Sobal, 2007) and can nudge individuals toward the vegetarian or the fish dish. In the most extreme case, if individuals are inattentive when ordering, they might miss the possibility of a third option altogether. Another aspect that varied between the meat-convenient and the vegetarian-convenient menus is the order in which the dishes were presented, with either the meat or the vegetarian option coming first. Previous experiments have shown that items listed first have a higher probability of being chosen (Dayan and Bar-Hillel, 2011; Policastro et al., 2015), which in our case should increase sales shares of the items listed first each day.

Triggering or disrupting habits. If a customer orders a certain dish out of habit, changing the menu layout and organization can affect ordering behavior by disrupting that habit. The social psychology literature defines a habit as "a process by which a stimulus automatically generates an impulse towards action, based on learned stimulus-response associations" (Gardner, 2015). Changes in circumstances that remove the stimulus can disrupt the habit by preventing the impulse toward the automatic behavior (Verplanken and Wood, 2006). If customers habitually order the meat dish as a response to seeing it displayed first on the menu, removing the stimulus and replacing it with something else can shift ordering behavior away from the meat.

Consumption norms. Presenting two options more prominently can also reveal information about what is standard or acceptable to choose at a restaurant—that is, the norm. Although the menus do not make use of descriptive or injunctive social norms explicitly, making the meat option less convenient to order might reveal the norm that most people at this restaurant choose the vegetarian or the fish dish. If that is the case, our intervention would work in a similar way to the use of social norms in order to reduce water and electricity consumption, such as in Allcott (2011) or Ferraro and Price (2013), where different types of information on average consumption levels are given. Such nudges are usually categorized as social information interventions (see, for example, Lehner et al., 2015; Schubert, 2017) that employ an "imitate-the-majority heuristic" (Artinger et al., 2016): people use social information to determine majority behavior and follow it.

Another channel through which consumption norms could operate is through status utility (Bernheim, 1994). As meat consumption recently has received considerably negative attention in Swedish media,[6] both for being a major driver of climate change and for being unhealthy if consumed in large quantities, deviating from a perceived norm of choosing vegetarian or fish dishes can reduce status utility. If violating a norm sufficiently lowers an individual's utility from consumption, such as via status loss or social sanctions, consumption norms do not act as a mere nudge but affect the economic incentives of the three options.

In a similar spirit, there are two additional potential channels by which our intervention might have changed behavior and that do not work via the fast, intuitive decision-making system but affect the (perceived) cost-benefit ratio of the dishes on the menu: increase in nonmonetary costs and quality signal.[7]

Increase in nonmonetary costs. Making the meat option less convenient to order increases its nonmonetary costs: customers have to stop a waiter and ask for a description of the dish. This increase in costs can vary with the individual characteristics of a customer, depending, for example, on how much discomfort it causes to ask or how time-constrained he or she is. As the waiter will return shortly after having seated the guests to deliver bread, water, and salad, time costs can be assumed to be small. Costs associated with discomfort will potentially be higher, and if the added costs of asking outweigh potential additional utility from consum-

[6] The Swedish Food Agency reduced its recommended maximum meat consumption to 500 grams (g) per week in 2015 and explicitly referred to negative impacts on health and the environment from high meat consumption (Swedish Food Agency, 2017).

[7] One more potential channel to consider is the provision of information on the availability of the vegetarian dish. Compared with the preintervention period, both the vegetarian and the meat-convenient menu add information on the availability of the vegetarian dish, which can lead to an expansion of the choice set for customers who, before the intervention, believed the only options were meat and fish. Comparing preintervention sales with sales in the meat-convenient area can be used to identify and control for such pure information effects.

ing the less convenient option, the change in the menu will affect a customer's ordering behavior.

Quality signal. Customers might interpret the fact that dishes are featured more or less prominently on the menu as a quality signal. If that is the case, because all three dishes cost the same, expected quality will play a role in ordering behavior. If the vegetarian and fish dishes have higher expected quality than the meat dish when the meat is made less convenient to order, this will decrease the share of meat dishes sold.

4. Data and results

The intervention took place from May 2 until May 20, 2016. During that time, the restaurant did not serve the lunch menu on Ascension Day and the Friday following it, resulting in 13 days of sales data with separate menus. We also collected total weekly sales of the three options for the four weeks before the intervention (April 4–30) and for five days after the intervention (May 22–27). Average sales were around 64 dishes per day during the first five weeks of the experiment (the preintervention period and the first week of the intervention), when only the indoor area was open. During the last four weeks of the experiment (two weeks of intervention and one week postintervention), the restaurant opened its outdoor seating and sold about 114 dishes per day during the two-hour lunch period.[8] The complete sales data collected can be found in appendix Table A.1.

4.1. The effect of menu design on food choice

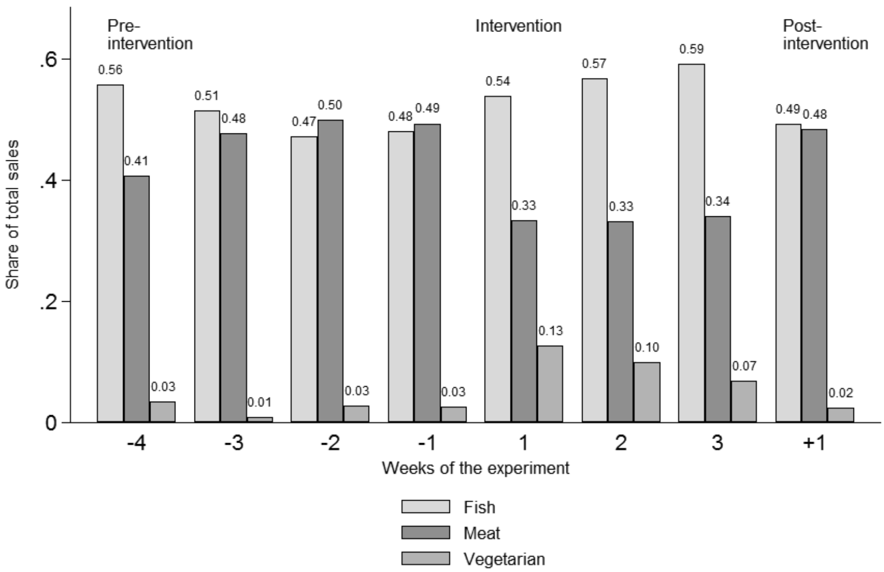
First, we show the aggregate results for the whole restaurant. We conduct chi-squared tests for changes in ordering behavior across the two periods. Figure 1 shows the sales shares of the meat, fish, and vegetarian options for the four weeks prior to the intervention, the three experimental weeks, and the one-week postexperimental period. On average, only 2.5 percent of all dishes sold were vegetarian without the vegetarian option on the menu.[9] The remaining lunches sold were distributed approximately equally across the meat and the fish dishes. In the

[8] As a result of opening the outdoor serving area, the number of total sales increased considerably starting with the second week of the intervention. However, the shares of the dishes sold in the control area did not significantly change with the opening of the outdoor area. Within the treatment area, the composition of dishes sold changed significantly over the course of the three-week intervention (see Figure 2 and the discussion in section 4.b).

[9] Based on the development of sales shares especially of the meat and fish dishes during the preintervention period (weeks –4 to –1), we test for a trend in preintervention sales by comparing the distribution of choices on the three options. A chi-squared test shows that there were no significant changes in the distribution of choices with time ($p = 0.123$). Looking at each dish separately confirms this result (meat: $p = 0.09$; fish: $p = 0.13$; vegetarian: $p = 0.28$).

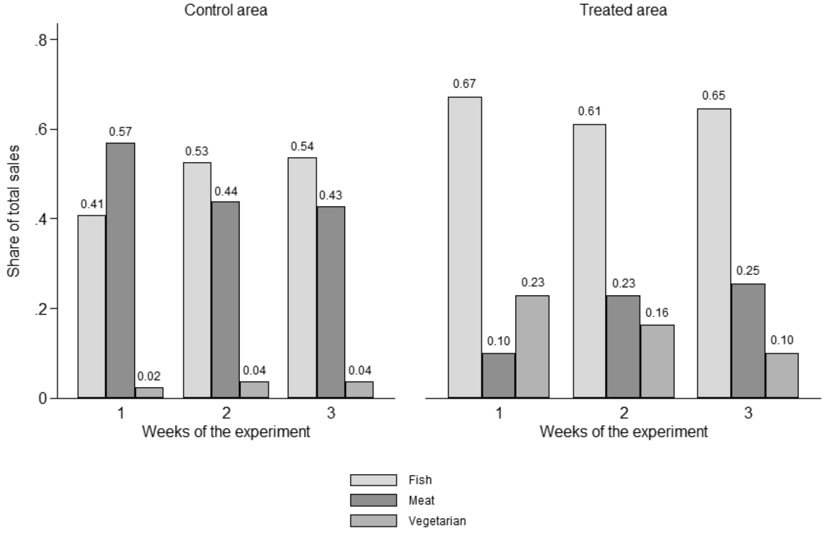
weeks of the intervention (1–3), the share of meat dishes sold overall dropped from 47 to 34 percent on average, a reduction of 38 percent ($p < 0.01$). Especially when considering that only about half of the restaurant was treated, this was a large reduction, and it stayed consistent over the three weeks of the experiment. The vegetarian dishes jumped from 3 to 9 percent on average (a 200 percent increase, $p < 0.01$) but with a downward trend over time. The weekly sales of fish dishes steadily increased during the intervention. On average, the increase was around 8 percentage points, from 50 to 57 percent ($p < 0.01$). A chi-squared test on changes in the overall distribution of meals across the treatment confirms that meal choices differed significantly between the two periods ($p < 0.01$).

Figure 1. Shares of total sales, with intervention during weeks 1–3.



Second, we look at the sales for the two menus separately. Because absolute sales vary over days and weeks, we show only the percentages of sales in the figures for comparison, but we conduct chi-squared tests using absolute values to test for differences in ordering behavior. All absolute values can be found in appendix Table A.1. Figure 2 contains the sales shares for the three-week intervention period. The left panel shows the sales for the meat menu, and the right panel for the vegetarian menu. Overall, meal choices differed significantly between the treated and control areas ($p < 0.01$). Of all dishes sold, 15 percent were vegetarian in the vegetarian area, but only 3.5 percent were vegetarian in the meat area ($p < 0.01$). The share of meat dishes sold was 46 percent on average in the meat menu area but less than half of that, 21 percent, in the vegetarian menu area ($p < 0.01$). This drop was larger than the increase in vegetarian sales shares, and consequently, the share of fish dishes sold also increased, from 51 to 64 percent ($p < 0.01$). In absolute terms, a little more than 1 out of 10 people who would have chosen meat in the meat area switched to the vegetarian dish in the vegetarian area, and similarly, 1 out of 10 switched to choosing fish. In the meat menu area, adding a statement about the availability of the vegetarian dish did not affect sales significantly. The share of vegetarian dishes sold remained low (between 2 and 4 percent) during the whole intervention. Thus we can rule out that merely providing information on the availability of a vegetarian dish was responsible for the treatment effect. In the vegetarian menu area, the share of vegetarian dishes sold decreased over time. The last three columns in Figure 1 show that switching to the old menu layout, though still keeping the note that a vegetarian dish was available, immediately restored the pretreatment sales shares. Hence, we conclude that the intervention had no lasting effects.

Figure 2. Shares of sales for the two menus separately during the intervention period



A valid concern regarding the experimental setup could be spillover effects between the two areas of the restaurant, especially during the weeks when the outdoor serving area was opened. That could be the case, for example, if customers seated in the meat menu area observed the waiters serving vegetarian dishes to customers in the vegetarian menu area or vice versa, which could influence their choice. Spillover effects could also occur from regular customers who were exposed to one of the menus on one visit to the restaurant and a different menu on another visit. Both types of spillovers would downward-bias our treatment effect. Our results can therefore be considered lower bounds of the true effect. Within an area and at the same table, there could also have been reinforcing effects that were captured by the treatment effect. If the first person was nudged to choose either the meat or the vegetarian dish, then others at the table might follow suit or else deliberately deviate from that choice to create variety. In a study with children, Angelucci et al. (2015) find reinforcing choices, but in a study with adults in a restaurant, Ariely and Levav (2000) present evidence for a love of variety in group choices. Since we have no information on the sequence in which orders were placed, we cannot identify such peer effects.

One point often raised when discussing nudging toward vegetarian food is that customers might not feel satiated or might use the healthy main course as an excuse to order an unhealthy dessert. We examined the number of desserts ordered for both groups, but as the total number of desserts ordered was very low ($\leq$6 per day), it was not possible to test this hypothesis. Compared with the pre-experimental period, the total sales of desserts did not increase. The menu price included water, which is what most Scandinavians drink for lunch. There was no change for any additional beverages ordered during the experimental period. We thus find no evidence for any compensational behavior in our data. We cannot, however, rule out that individuals may have compensated in the afternoon or evening by eating more meat or making other unhealthy food choices.

4.2. Development of the treatment effect over time

Figure 2 shows that there was a decrease in the treatment effect over time. In the treated area, the share of vegetarian dishes goes down from 23 percent during the first week to 10 percent during week three, while the share of meat dishes sold increases. While treatment effects are statistically significant when comparing the treated and the control areas separately for each week (chi-squared tests, $p < 0.01$ for each week), a chi-squared test shows that the distribution of choices changed significantly over the three weeks of the intervention within the treated area ($p < 0.01$). Testing specifically for differences in the sales of vegetarian dishes per week shows that all weeks differ significantly from each other at least at a 10 percent level of significance (week 1 versus week 2: $p < 0.01$; week 1 versus week 3: $p < 0.01$; week 2 versus week 3: $p = 0.10$). In the control area, no significant changes occurred during the intervention period.[10]

What could have caused this trend in the treatment effect? There are several potential explanations for the decline we observe. In the light of the potential channels discussed in section 3, all walk-in customers should be equally affected by the nudge, whether they visited the restaurant in week one, two, or three of the experiment. Thus, although the decline of the treatment effect over the course of the experiment is quite pronounced, we do not expect it to fade away completely if we had kept the nudge in place for a longer time period. However, in connection with the opening of the outdoor seating area, the composition of customers might have changed. Customers who visited during the last two weeks could have differed from customers who visited during the first week and may have reacted less to the nudge.[11]

Another explanation for the decline in the treatment effect could be that the staff got less careful in implementing the experimental design, especially in connection with the opening of the outdoor seating, such as by handing out control menus erroneously in the treated area and vice versa. To the best of our knowledge, however, this was not the case. Any changes in the implementation of the experiment should also have shown up in the control area, but as discussed above, sales patterns with respect to the vegetarian dish did not change over time in the control area.

A third explanation is the presence of regular customers who had experience with the pre-intervention menu. The restaurant reports having a high number of regular customers, around 20 percent. For the first three days of the intervention, we have data on the choices of customers who were identified by the waiters as regulars; 52 out of a total of 254 guests during the first week belong to that group.[12] Although the data are limited, they paint a clear picture. Of the 23 regulars exposed to the vegetarian menu, 17 ordered fish, 6 ordered the vegetarian dish, and none ordered meat. Of the 29 regulars exposed to the meat menu, 17 ordered fish, 12 ordered the meat, and none ordered the vegetarian option. The shares match the total sales

[10] This holds when looking both at all three choices simultaneously and at only the share of vegetarian dishes sold.

[11] There is a theoretical possibility that the customer composition changed as a result of the nudge, such as if people recommended or did not recommend that others visit the restaurant after having eaten there while the nudge was in place, and this in turn could have influenced how effective our treatment was. However, such indirect effects are hard to quantify within a given time frame, but such changes in customer composition should also have shown up in the control area.

[12] We only know the total number of regular guests that week, not the number of distinct individuals. Hence, we cannot rule out that some of the regulars visited the restaurant more than once during the three days it was open.

shares of that week. These customers all had experience with the previous menu featuring a choice between meat and fish and have likely tried both types of dishes at some point. One can also assume that because they are regulars, the meat and fish dishes correspond well to their preferences; that is, those customers are regulars because they like the dishes usually featured on the menu. With respect to the potential channels discussed above, it is unlikely that they interpret the menu layout as a quality signal or a norm of what is standard to order at that restaurant. The increase in nonmonetary costs from summoning a waiter to ask about a dish might also be lower for that subgroup than for walk-in customers, as they know the waiters and the procedures of the restaurant.

For those customers, a change in saliency of the dishes and a disruption of habits seems to be the most likely explanation for the initial treatment effect. However, as Wood and Neal (2009) explain, people can revert to their habitual behavior relatively easy after deviation to an alternative behavior. Giving in to the nudge in the first place but reverting to familiar (and preferred) choices afterward could generate the declining treatment effect we can observe in Figure 2. As we do not have any follow-up data on this group and do not know anything about the behavior of regulars who visited the restaurant more than once during our experiment, we cannot draw firm conclusions on this point. More detailed information and long-term data on regulars are needed to investigate this interesting subgroup further.

Our finding that experienced users change their behavior, at least initially, is in contrast to Löfgren et al. (2012), who show that experienced users are harder to nudge and override defaults more often than inexperienced users in an experiment using default settings. Our results show that even experienced users change their behavior, at least initially. However, the decision in our experiment, choosing lunch at a regularly visited restaurant, is different from the one studied by Löfgren and colleagues, where the intervention targeted carbon offsetting from flights. Choosing a lunch involves lower stakes and is a frequently repeated action for the regulars. Thus it will most likely be dominated by the intuitive, fast system and will be more responsive to the nudge. Another explanation of the difference in findings could be that regret from trying something new as a result of a nudge will likely be lower in the case of choosing lunch than in the higher-stakes, low-frequency case.

5. Discussion and Conclusion

We have shown that a simple and inexpensive rearrangement of the menu that changes the convenience of ordering meat can contribute toward a reduction in meat consumption without any measurable negative effects.[13] Making it less convenient to order meat significantly increased the shares of both vegetarian dishes and fish dishes sold. From a climate change perspective, this is still a positive change, as eating fish entails less climate-relevant emissions per kilogram (kg) than most kinds of meat (Röös, 2014).[14]

How much of a climate impact did the intervention have? A brief example can put it into perspective. On one occasion, the meat dish included a piece of beef, while the vegetarian option was grilled cabbage. A conservative estimate of the CO_2 emissions of a 150 g piece of Swedish beef is 4 kg (Röös, 2014). For the cabbage, it is 0.03 kg. That day, 42 percent of customers exposed to the meat menu ordered the beef, but only 16 percent of those presented with the vegetarian menu did so. With roughly 50 people in each group, that amounted to 84 kg of CO_2 from meat in the meat menu group but only 32 kg from meat in the vegetarian menu group. To put this into perspective, average emissions from driving a car in Sweden are around 0.16 kg of CO_2 per kilometer. Clearly, both the reduction in CO_2 and the cost differential for the restaurant varies depending on the type of meat and vegetarian substitute served. Another day, the meat dish was grilled chicken, while the vegetarian menu featured tofu. Chicken and soy substitutes such as tofu entail approximately the same amount of climate-relevant emissions per kg (Röös, 2014). Any overall evaluation of climate benefits also crucially depends on the assumption that customers do not compensate for having chosen a vegetarian lunch by indulging in meat later that day or the day after. Complete information about food choices is quite challenging to obtain, and to the best of our knowledge, no experiment has yet been conducted that examines substitution effects over time. More research in that area is needed to identify total climate effects of nudges aiming at reducing meat consumption.

We have identified several potential channels through which intervention can affect what customers order. As we did not change prices or what was offered, we expect that most of the effect was related to the use of decision heuristics. However, our nudge could also have changed the (perceived) cost-benefit ratio of the dishes. Most likely, different channels were

[13] Anecdotally, no customers complained about the food during the experimental period. If someone noticed a change in the menu, the staff explained that they were trying out some new dishes, and all customers accepted this explanation. Since the sales data is dependent on weekday and weather, we cannot reliably test whether the intervention had an effect on sales, as sales only increased over time. We cannot rule out that customers who tried the vegetarian option and did not like it decided not to come back to the restaurant. We can, however, say that as a result of the experiment, restaurant management decided to push the vegetarian menu more (i.e., they do not expect negative returns from selling more vegetarian dishes). As mentioned above, we do know that no one left the restaurant after looking at the menu.

[14] Consuming fish entails less climate-relevant emissions than beef, lamb, pork, and mixed meats (such as minced meat) and approximately as much as chicken.

at work for different customer groups, such as regulars versus occasional visitors. However, because of the lack of disaggregated data, we cannot test any hypotheses with respect to the effect on different subgroups. Such data would also be desirable to analyze and explain the downward trend in the treatment effect that we observe.

We conclude that even in restaurants with an initially low share of vegetarian customers, there is room to decrease the share of meat dishes sold in favor of vegetarian and fish dishes without banning options or changing prices, and this can be done in a fast, easy, and profitable way.[15] Around two out of ten customers who would have chosen meat switched to either vegetarian or fish dishes. Clearly, it would be interesting to validate the effect size in other settings. Restaurants that either cater to vegetarians or are meat-focused venues such as steakhouses will most likely see smaller effects from the same kind of intervention due to self-selection of the patrons into the restaurant. The most promising settings are restaurants that attract customers based on their quality of food and not on their focus on serving meat or vegetarian food. In our sample, 1 out of 10 people would switch from meat to vegetarian food if it is made convenient and salient. So for any restaurant managers hoping to reduce their climate impact, a clear policy recommendation is to have a vegetarian choice available and make it a prominent choice on the menu. Restaurants should not present vegetarian food as a special diet that customers need to inquire about, as this creates hassle costs that will tip people on the margin toward choosing the “normal” meat dish instead.

The shift to vegetarian food was strongest in the first week of the intervention. A conservative interpretation of this result leads to the conclusion that the nudge might work best in a setting with a lower share of regular customers, so that more people experience the nudge as new. The observed decrease in the treatment effect over the course of the intervention shows the need for more research on the impact of nudges over time in order to formulate recommendations on long-term strategies.

To conclude, the sizable results in our experiment are a promising first step for further research on how to effectively reduce meat consumption. Although we cannot rule out any negative spillover effects on profits, our evidence points toward the contrary, with stable sales and higher profit, especially when comparing our intervention with a reduction of choice by banning the meat option, which would most certainly keep guests from eating at this

[15] According to the restaurant’s management, purchasing costs are around 30 percent lower for vegetarian than for meat dishes. Preparation of vegetarian dishes is slightly more time-consuming than producing the other dishes, so personnel costs are higher. However, taking all costs into account, it is not more expensive to produce vegetarian dishes than meat or fish dishes. Overall, the restaurant’s management deemed the intervention to have had positive effects on profits but could not quantify the magnitude of this effect.

restaurant. Nevertheless, more research is needed to verify these hypotheses. Public or private sector agents that want to limit the climate impact of food consumption should work proactively with restaurants to develop, implement, and test customized nudging strategies to realize the potential gains from this approach.

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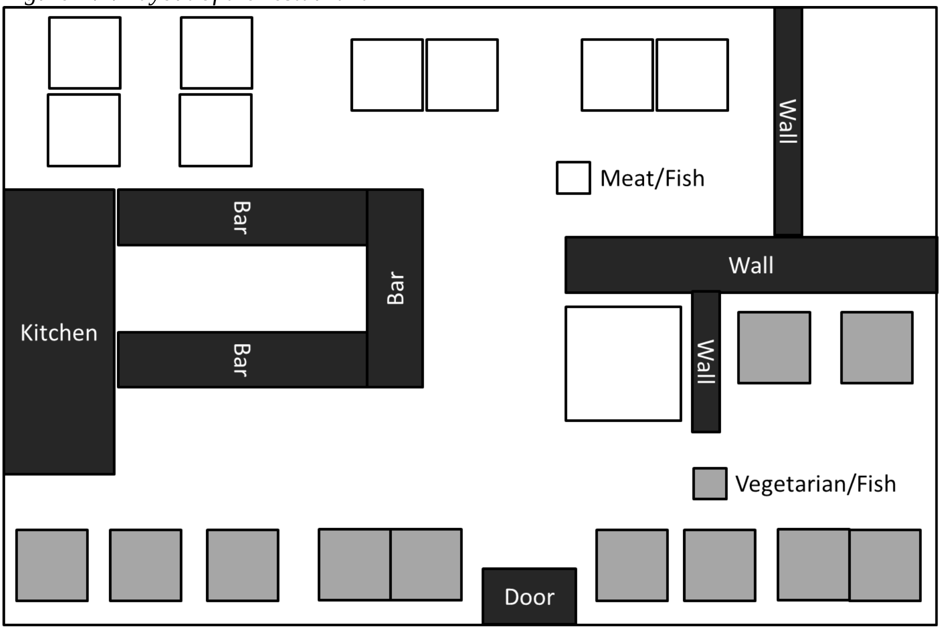
Appendix

Table A.1. Total sales and sales shares in percentages of the three lunch options available across periods and treatments

		Meat		**Fish**		**Vegetarian**		**Total sales**	
Convenient option to order[a]		*Meat*	*Veg*	*Meat*	*Veg*	*Meat*	*Veg*	*Meat*	*Veg*
Baseline period	Week 1	119		163		10		292	
		40.75%		*55.82%*		*3.42%*		*100%*	
	Week 2	113		122		2		237	
		47.68%		*51.48%*		*0.84%*		*100%*	
	Week 3	160		151		9		320	
		50.00%		*47.19%*		*2.81%*		*100%*	
	Week 4	187		182		10		379	
		49.34%		*48.02%*		*2.64%*		*100%*	
	Total	**579**		**618**		**31**		**1,228**	
		47.15%		***50.33%***		***2.52%***		***100%***	
	Average sales/day	29		30.9		1.6		61.4	
Intervention	Week 1 (3 days)	70	13	50	88	3	30	123	131
		56.91%	*9.92%*	*40.65%*	*67.18%*	*2.44%*	*22.90%*	*100%*	*100%*
	Week 2	142	66	171	177	12	47	325	290
		43.69%	*22.76%*	*52.62%*	*61.03%*	*3.69%*	*16.21%*	*100%*	*100%*
	Week 3	106	69	133	175	9	27	248	271
		42.74%	*25.46%*	*53.63%*	*64.58%*	*3.63%*	*9.96%*	*100%*	*100%*
	Total	**318**	**148**	**354**	**440**	**24**	**104**	**696**	**692**
		45.69%	***21.39%***	***50.86%***	***63.58%***	***3.45%***	***15.03%***	***100%***	***100%***
	Average sales/day	24.5	11.4	27.2	33.8	1.8	8.0	53.5	53.2
Postintervention	Total (5 days)	285		280		14		579	
		49.22%		***48.36%***		***2.42%***		***100%***	
	Average sales/day	57		56		2.8		115.8	

[a] The fish option was equally convenient to order across periods and treatments and is therefore omitted here.

Figure A.1. Layout of the restaurant



Note: Dark grey squares are tables with the vegetarian/fish menu, and white squares are tables with the meat/fish menu.

Figure A.2. Examples of the Meat/Fish and Vegetarian/Fish Menus

Meat/Fish

Torsdag

Bakat lammlägg med pumpasallad, getost och aubergineris

Chillibakad kolja "Loose aioli" med bakad svamp, lotusrot och pak soi

Fredag

Sotad biff med refried beans, fetaost, rödkål och stekt sallad

Panerad spätta med relish "delish" aioli, libabröd och pickles

Chokladtryffel eller en kula sorbet 35sek

Vi har även vegetariskt alternativ.

Lunchen kostar 110sek och då ingår en plocktallrik, ekologisk surdegsbaguette samt stilla vatten.

Vegetarian/Fish

Torsdag

Kryddbakad pumpa med cruditee, aubergineris och getost

Chillibakad kolja "Loose aioli" med bakad svamp, lotusrot och pak soi

Fredag

Grillad kål "Rosé" med refried beans, fetaost och stekt sallad

Panerad spätta med relish "delish" aioli, libabröd och pickles

Chokladtryffel eller en kula sorbet 35sek

Vi har även kött alternativ.

Lunchen kostar 110sek och då ingår en plocktallrik, ekologisk surdegsbaguette samt stilla vatten.

Note: The boxes around the dishes and around the additional sentence were not on the menu but have been added by the authors to aid the reader.

Chapter III

Exp Econ
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ORIGINAL PAPER

Fairness versus efficiency: how procedural fairness concerns affect coordination

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Abstract We investigate in a laboratory experiment whether procedural fairness concerns affect how well individuals are able to solve a coordination problem in a two-player Volunteer's Dilemma. Subjects receive external action recommendations, either to volunteer or to abstain from it, in order to facilitate coordination and improve efficiency. We manipulate the fairness of the recommendation procedure by varying the probabilities of receiving the disadvantageous recommendation to volunteer between players. We find evidence that while recommendations improve overall efficiency regardless of their implications for expected payoffs, there are behavioural asymmetries depending on the recommendation: advantageous recommendations are followed less frequently than disadvantageous ones and beliefs about others' actions are more pessimistic in the treatment with recommendations inducing unequal expected payoffs.

Keywords Coordination · Correlated equilibrium · Recommendations · Procedural fairness · Volunteer's Dilemma · Experiment

JEL Classification C72 · C91 · D63 · D83

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1 Introduction

Coordination problems are frequent in everyday interactions. Consider a situation at work in which exactly one volunteer is needed for serving on a workplace committee or writing the report from a meeting. If one person volunteers, everyone will benefit from the report being written or from a well-functioning committee. However, volunteering is time-consuming and hence costly to the individual, so everyone prefers someone else doing it. In order to avoid a situation where no one volunteers (or too many sign up), the employees have to solve a coordination problem. Where no formal rules are established, such problems can be solved with the help of some mechanism—for example, by a social norm determining who should do the task (the youngest team member or the oldest, etc.), via a coin toss, or by a third party (e.g. the boss) picking the one who should do the job. However, such a mechanism might lead to some individuals contributing more often, while others frequently escaping from investing time. External mechanisms that imply different likelihoods of being picked as a volunteer across individuals might be perceived as unfair by both the picked volunteer and the beneficiaries.

In this paper, we examine experimentally whether procedural fairness plays a role for how well individuals are able to solve a coordination problem in a two-player Volunteer's Dilemma (Diekmann 1985). In this game, it is sufficient that one member of a group volunteers in order to provide a public good and make everyone better off. However, volunteering induces costs that are specific to the provider. As it does not matter who volunteers, two pure-strategy efficient Nash equilibria but no dominant strategies exist.[1] Without any additional mechanism, coordination on one of the equilibria can be difficult to achieve. Both under-provision (no one volunteers) and over-provision (too many people volunteer) constitute inefficient outcomes resulting from coordination failure.[2] To overcome the coordination problem, we give participants action recommendations—either to play the costly action or to abstain from it. Both players know their own recommendation and also which recommendation the other player receives. By allowing individuals to condition their action on the recommendation they receive, coordination on an efficient outcome can be achieved even without direct communication. Correlated equilibria become attainable, which can raise expected payoffs above Nash equilibrium payoffs (Aumann 1974, 1987). While fairness certainly plays a role in many experimental games, a coordination game like the Volunteer's Dilemma is especially suitable to study how fairness of an external mechanism affects behaviour, as there are no dominant strategies and large potential efficiency gains.

We manipulate the fairness of the recommendation procedure by varying the probabilities with which subjects receive a recommendation to volunteer.[3] By doing

[1] For more theoretical and experimental results on the Volunteer's Dilemma, see Darley and Latane (1968), Latane and Rodin (1969), Diekmann (1993), Weesie (1993, 1994) and Myatt and Wallace (2008).

[2] For an overview on coordination failures in laboratory experiments, see Camerer (2003) and Devetag and Ortmann (2007).

[3] Another way to manipulate procedural fairness in experiments is to vary the probability of the random draw that assigns the roles of the subjects in the experiment. Grimalda et al. (2016) find that individuals respond to the probability of being either the advantaged proposer or the disadvantaged receiver in a following Ultimatum game.

so, we alter the expected payoffs of following recommendations between subjects. Our definition of procedural fairness focuses on players' sensitivity towards those differences in expected payoffs. We evaluate the behaviour of advantaged and disadvantaged individuals with respect to following recommendations, the resulting coordination rates, and earnings both in comparison to situations without any recommendations and compared to a fair mechanism.

Previous experimental studies show that fair action recommendations often enhance efficiency. Van Huyck et al. (1992) find that subjects follow public, non-binding announcements if they do not conflict with payoff-dominance. Furthermore, subjects are more likely to follow announcements if they induce equal average payoffs compared to unequal average payoffs across a session. Croson and Marks (2001) study a threshold public good game and find that individual recommendations about each subject's contribution increase efficiency in contrast to a situation without recommendations. Duffy and Fisher (2005) show that potentially irrelevant public announcements about market conditions can help subjects coordinate on "sunspot equilibria" in laboratory financial markets. Cason and Sharma (2007) show that private action recommendations are followed if players believe that their counterparts will follow as well. Duffy and Feltovich (2010) find that subjects follow private recommendations if they are payoff-enhancing compared to the Nash equilibrium, but do not follow recommendations resulting in payoffs lower than in Nash equilibrium.

Most previous experiments use mechanisms that treat players symmetrically, such that all players can expect the same payoffs before the recommendation is realized. We will examine if a coordination mechanism that systematically puts one party at a disadvantage implies efficiency losses compared to such fair mechanisms. Our work is closely related to Anbarci et al. (2017), who investigate the impact of payoff-asymmetry on following recommendations in Battle of the Sexes games. By varying payoff asymmetry and the availability of recommendations between treatments, they study whether recommendations that point at both Nash equilibria with equal probability improve coordination. They find, as predicted, that subjects are less likely to follow recommendations in games with higher payoff asymmetry. While Anbarci et al. vary the payoff matrix of the underlying game and keep the probabilities of the recommendations of the two equilibria equal, we keep the payoffs constant and use the probabilities with which we recommend each of the two Nash equilibria to manipulate procedural fairness across treatments. By doing so, expected payoffs of following a recommendation before it is realized vary between players.

Previous experimental work suggests that people do not only care about ex-post inequality of outcomes, but also about procedural fairness, of which ex-ante inequality in expected payoffs is an important aspect (Bolton et al. 2005; Krawczyk and Le Lec 2010; Brock et al. 2013; Linde and Sonnemans 2015). Closely related to our experiment is Bolton et al. (2005), who study ultimatum games where first moves are decided by lotteries. Via the calibration of the lottery, the expected value of the proposal is manipulated. They find that low proposals are more acceptable if the lottery is judged fair compared to a lottery that is biased towards the disadvantageous outcome. Theoretical models such as by Trautmann (2009), Krawczyk (2011) or Saito (2013) account for the empirically observed importance of procedural fairness by incorporating expected payoffs into the utility function.

We investigate whether inequality in expected payoffs affects the efficiency of action recommendations as a coordination mechanism with the help of three experimental treatments: subjects play a Volunteer's Dilemma and receive either (1) no recommendations, (2) efficient recommendations that induce *equal* expected payoffs as long as both subjects follow the recommendations, and (3) efficient recommendations that induce *unequal* expected payoffs as long as both subjects follow.[4] This allows us to answer the following questions: Does inequality in expected payoffs matter for the decisions to follow action recommendations? Do differences in expected payoffs reduce efficiency gains of external recommendations in a coordination game? And does the behaviour of advantaged and disadvantaged individuals differ with regard to following the recommendations?

Our results show that most of the subjects are more concerned about efficiency and potential gains from coordination rather than about differences in expected payoffs. Recommendations increased efficiency in comparison to a treatment without any coordination mechanism in both the case with equal and unequal expected payoffs. We find that subjects are more likely to follow recommendations that give secure payoffs, even if they are disadvantageous, i.e. induce the equilibrium with a comparatively lower payoff for the player. While there were no significant differences in following recommendations between treatments, we find differences in individuals' beliefs about others' actions between treatments.

2 Analytical framework

2.1 Action recommendations in the Volunteer's Dilemma

Table 1 presents the basic set-up of the two-player Volunteer's Dilemma. A public good is provided if at least one player volunteers. Both players decide simultaneously between X (volunteer) and Y (not volunteer).[5] Each player receives a if at least one of them volunteers and 0 if no one volunteers. A volunteer bears the cost c, $c > 0$. Both players are better off when volunteering compared to a situation in which no one volunteers: $a > a - c > 0$.

The game has no dominant strategy. There are two pure strategy Pareto-efficient Nash equilibria (NE), (X, Y) and (Y, X), in each of which one of the players volunteers and the other does not, granting the payoff $a - c$ to the volunteer who plays X and a to the player playing Y. However, this equilibrium requires Nash conjectures, i.e., players having correct beliefs about other players' actions.

[4] Our study hence uses a definition of fairness different from that of, for example, Kahneman et al. (1986), who define fairness in terms of reference points and framing, or, Konow (2001), who finds context-dependence of fairness. We limit our study to investigating procedural fairness as defined in the articles and models cited above.

[5] In the experiment, names of actions were framed in a neutral way (X, Y) in order to avoid framing effects.

Table 1 Payoff matrix of the Volunteer's Dilemma

	Player 2	
	X (volunteer)	Y (notvolunteer)
Player 1		
X (volunteer)	$a - c, a - c$	$a - c, a$
Y (not volunteer)	$a, a - c$	0, 0

Furthermore, the game has a mixed strategy Nash equilibrium (MNE), in which each of the players volunteers with probability $1 - \frac{c}{a}$ and takes no action (Y) with probability $\frac{c}{a}$. The expected payoff for each player in the MNE is

$$\pi^e_{Nash} = a - c. \tag{1}$$

The introduction of direct, private action recommendations can improve coordination by helping to avoid over- and under-provision. Given that both players know which recommendation the other one receives, they can correlate their strategies via the recommendations given: either player 1 receives a recommendation to play X and player 2 to play Y, or the other way round. If both players follow these recommendations, inefficient outcomes (X, X) and (Y, Y) are avoided and one of the efficient outcomes (X, Y) or (Y, X) is achieved. Correlated equilibria (CE) that raise expected payoffs above Nash payoffs become attainable. If the distribution of recommendations to both players is common knowledge, each player can calculate expected payoffs of a recommendation mechanism for herself and the other player (Aumann 1974, 1987).

2.2 Procedural fairness

We use the distribution of recommendations to vary the expected payoffs between players. Let the probability of player 1 receiving recommendation Y and player 2 receiving recommendation X be denoted with p, $p > 0$. Given our set of possible recommendations, the probability that player 1 will get recommendation X and player 2 will get recommendation Y equals $1 - p$. Under the assumption that both players believe that the other one will follow the recommendation, no one has an incentive to deviate after the recommendation is realized, since a unilateral deviation would decrease her payoff. Hence, any convex combination of equilibria suggestions (X, Y) and (Y, X) constitutes a CE, independent of the value of p.

Assuming the other player will follow her recommendation, expected payoffs from following for player 1 are:

$$\pi^e_1 = pa + (1 - p)(a - c) = a - (1 - p)c, \tag{2}$$

and for player 2:

$$\pi^e_2 = p(a - c) + (1 - p)(a) = a - pc. \tag{3}$$

Equations 2 and 3 show that correlating their strategies via following the action recommendations is individually rational for both players, as expected payoffs from a strategy to follow the recommendation are higher than the expected payoff from playing the MNE. If both players follow recommendations, the sum of expected payoffs is raised above the sum of NE payoffs.

Expected payoffs from a CE vary with the probability the two action recommendations are given. As can be seen from Eqs. 2 and 3, expected payoffs depend on the value of p. If $p = 0.5$, both players can expect

$$\pi^{e}_{1,2} = a - 0.5c \tag{4}$$

as equilibrium payoffs.

For any value of p different from 0.5, expected payoffs from following recommendations will differ between player 1 and player 2. Differences in expected payoffs have been identified as an important aspect of procedural fairness. In contrast to outcome fairness models, such as models developed by Fehr and Schmidt (1999), Charness and Rabin (2002) or Bolton and Ockenfels (2000), where the difference in payoffs to be received matters for decision-making, individuals who care about procedural fairness take additional factors into account, such as expected payoffs or the feasibility of an equal split. For example, Trautmann (2009) develops a procedural fairness model based on the Fehr–Schmidt model, but replaces differences in realized payoffs by differences in *expected* payoffs in the utility function. Besides absolute payoffs received, individuals care both about advantageous and disadvantageous inequalities in expected payoffs, but disutility from disadvantageous inequality is higher.

We adopt this definition of procedural fairness as differences in expected payoffs. As we keep the game's underlying payoff structure constant across treatments, individuals who are purely motivated by distributional fairness should not base their decision to follow a recommendation on the value of p. In contrast, if players care about procedural fairness, p as a determinant of expected payoffs becomes relevant for decision-making. For $p > 0.5$, player 1's expected earnings will be greater than player 2's, as the likelihood of receiving a recommendation "*Y*" (not to volunteer) is higher than receiving recommendation "*X*" (to volunteer). If a player cares about procedural fairness, and disutility from inequality in expected payoffs outweighs utility gains from the increase in expected payoffs, he will not follow recommendations. As aversion towards disadvantageous procedures is usually assumed to be higher than aversion towards advantageous procedures, it can be expected that disadvantaged players follow the recommendations less frequently.

Table 2 The experimental calibration of the Volunteer's Dilemma

	Player 2	
	X	*Y*
Player 1		
X	5, 5	5, 10
Y	10, 5	0, 0

3 Experimental design

Table 2 shows the normal form of the Volunteer's Dilemma game that subjects play. The payoff structure with $a = 10$ and $c = 5$ captures situations with high gains to both parties if one volunteers, high costs for the volunteer and zero payoffs to both parties when no one volunteers, and is in line with previous experimental work on the Volunteer's Dilemma (Rapoport 1988; Diekmann 1993).

In each session, 24 subjects participate. One half of the subjects is randomly assigned to the role of player 1; the rest of the subjects take the role of player 2. The role does not change during the experiment. The game is repeated for 30 rounds without any feedback between the rounds. In each round, subjects in the role of player 1 are randomly matched into pairs with subjects in the role of player 2. This matching procedure keeps the number of independent observations high and prevents subjects from developing strategies depending on past behaviour (e.g. subjects in Duffy et al. 2017 alternate when being repeatedly matched with the same partner).

The experiment has a between-subject design and consists of three treatments. In our first treatment, to which we will refer as *Baseline*, subjects play a standard Volunteer's Dilemma game without any action recommendations. It serves as a benchmark to evaluate the effectiveness of the coordination mechanisms in other treatments.

Action recommendations are introduced in the remaining two treatments. In treatment *CD50*, equal probabilities are assigned to the two pure-strategy NE, which leads to the same number of recommendations to volunteer for both players. This treatment's primary purpose is to measure the changes in coordination in comparison to the *Baseline* treatment. In the third treatment (*CD90*), different probabilities are assigned to action recommendations leading to the two pure-strategy NE. The desired NE for player 1, (*Y*, *X*), is recommended with probability 0.9 and the NE that puts player 2 at an advantage (*X*, *Y*) is recommended with probability 0.1. Thus, player 2 receives three advantageous recommendations (*Y*), while player 1 receives 27 such recommendations. This treatment allows us to study the effects of inequality in expected payoffs on coordination rates and efficiency. Table 3 summarizes our treatments and the expected payoffs to both players in each treatment. Expected payoffs are 5 points if the MNE is played. When action recommendations are followed, they increase to 7.5 points for both players in *CD50*,

Table 3 Summary of the experimental design

Treatment	Recommendation	Expected payoff player 1	Expected payoff player 2
Baseline	None	5	5
CD50	$P(X, Y) = 0.5$, $P(Y, X) = 0.5$	7.5	7.5
CD90	$P(X, Y) = 0.1$, $P(Y, X) = 0.9$	9.5	5.5

and to 9.5 and 5.5 points for player 1 and 2 respectively in *CD90* (7.5 points on average).

Each round has the same structure. In all treatments, we present players with the normal form of the game on-screen. In the treatments with a coordination mechanism, *CD50* and *CD90*, subjects are also shown the probabilities of receiving each recommendation, their own recommendation for the round, and the recommendation their counterpart receives. The series of recommendations subjects receive were randomly generated before the experiments and are the same across sessions of a treatment. The series of recommendations for player 1 in both treatments can be found in the electronic supplementary material. Subjects do not receive any feedback about outcomes or past behaviour of other players until the very end of the experiment.

The experiment has a neutral framing. On-screen and in the printed instructions, subjects in the other role are called "the participant you are matched with". Player 1 is called "Red participant", player 2 "Blue participant". The possible actions of the players are called *X* and *Y*. The coordination mechanism is called "recommendation" and we explain its working and consequences extensively in the instructions.[6] It is displayed on the screen with the sentence "the recommendation is: ...", directly above the field where subjects enter their decision.

After the experiment, we elicited risk preferences with an investment task proposed by Gneezy and Potters (1997). Subjects were endowed with 10 points (each point worth 10 euro cents) and had to decide about an investment in a risky asset. The asset had a probability of 0.5 of being successful: in this case it paid 2.5-fold the invested amount. With a probability of 0.5, the asset was not successful and the invested amount was lost.[7] Subjects could invest any integer between 0 and 10 into the asset.

Furthermore, socio-demographic information was collected in a questionnaire after the experiment (age, gender, field of study, number of semesters in university). We also conducted two tests to account for possible effects of personality on behaviour, the Big Five personality traits (the BFI-S by Gerlitz and Schupp 2005) and Locus of Control (the IEC itinerary by Rotter 1966 in a German translation by Rost-Schaude et al. 1978). In the *CD50* and *CD90* treatments, two questions about the recommendations were included. Firstly, we elicited the beliefs about following behaviour of the participants in the other role ("Do you think that the participants in the other role followed the recommendation?"). The answer could be given on a scale with four items: all participants followed the recommendation, most participants followed it, most did not follow the recommendation, nobody followed the recommendation.[8] Answers were summarized into a binary variable taking the value 1 if subjects answered that they believed other player always or most of the

[6] Before running the experiments, we conducted two pilot sessions of *CD50* and *CD90*. Subjects in these pilots had problems understanding the part of the instructions dedicated to the recommendations. As this part is central, we clarified it and supplied more information; for example, we explicitly stated that the probability that both matched participants at the same time get recommendations *X* or *Y* is zero.

[7] The calibration used in the risk task has been introduced by Charness and Gneezy (2010).

[8] The belief elicitation was conducted after the experiment and was not incentivized. The scale of four items with verbal descriptions of the others' following behaviour was chosen for the belief elicitation to avoid potential problems with correct expression of probabilities among subjects; see Erev et al. (1993).

time followed the recommendation, and 0 otherwise. Subjects were also asked whether or not they felt disadvantaged by the recommendations.

Only after filling in the questionnaire, subjects were presented with the actions chosen by themselves and by the participant they were matched with in each round, the two randomly chosen rounds for the payment, and the payoffs from the risk elicitation task. The rounds chosen for payoff were the same for all subjects within a session. The exchange rate was 0.75 euros per point. Average total payoffs were 13.87 euros (including a show-up fee of 4 euros), with a minimum of 4.50 euros and a maximum of 21.50 euros. Payoffs were rounded up to the next full ten cents.

We conducted three sessions of each treatment, and in total 216 subjects participated. The experiments were conducted in MELESSA, the Munich Experimental Laboratory for Economic and Social Sciences, in January 2015. Each session lasted between 60 and 75 min. Instructions were read out loud and were available on paper throughout the experiment. To make sure that subjects understood the instructions, a computer-based quiz was conducted and the experiment only started after all subjects answered all control questions correctly. Subjects had the opportunity to individually ask questions (which rarely happened). All subjects answered the quiz correctly. We did neither exclude subjects from the experiment nor observations from the analyses. Full instructions for the *CD50* treatment with a screen-shot and control questions of all treatments can be found in the electronic supplementary material. The experiment was programmed in z-Tree (Fischbacher 2007) and participants were recruited via the ORSEE recruitment software (Greiner 2004).

4 Hypotheses

We hypothesize that the existence of a coordination mechanism increases coordination and hence the earnings of players, as found in previous studies (for example, Cason and Sharma 2007; Duffy and Feltovich 2010). However, it is unclear how procedural fairness concerns affect the efficiency of action recommendations as a coordination mechanism. If preferences for payoff maximization are stronger than procedural fairness concerns, we will observe higher coordination rates than without recommendations, even when the coordination mechanism is unfair. On the other hand, if procedural fairness concerns are stronger than efficiency concerns, individuals will disregard the coordination mechanism. This lets us formulate the following hypotheses:

Hypothesis 1 Coordination rates and earnings in treatments with recommendations that induce *equal* expected payoffs (*CD50*) are higher than in treatments without recommendations (*Baseline*).

Hypothesis 2a Coordination rates and earnings in treatments with recommendations that induce *unequal* expected payoffs (*CD90*) are higher than in treatments without recommendations (*Baseline*), if payoff maximization concerns are stronger than procedural fairness concerns.

Alternatively:

Hypothesis 2b Coordination rates and earnings in treatments with recommendations that induce *unequal* expected payoffs (*CD90*) are not higher than in treatments without recommendations (*Baseline*), if procedural fairness concerns are stronger than payoff maximization concerns.

Findings from experimental studies on procedural fairness show that individuals are less likely to accept biased procedures, even if this is connected with forgoing monetary payments (see for example Bolton et al. 2005). Hence, we predict that people are less likely to follow recommendations if they induce inequality in expected payoffs, in contrast to the case when they induce equal expected payoffs.

Hypothesis 3 Coordination rates and earnings in treatments with recommendations that induce *equal* expected payoffs (*CD50*) are higher than in treatments with recommendations that induce *unequal* expected payoffs (*CD90*).

The frequency of coordination on one of the pure strategy NE in the treatments with coordination mechanism stems from individuals' propensity to follow recommendations. We predict that individuals are less likely to accept recommendation procedures (i.e. follow recommendations) that systematically favour one of the players.

Hypothesis 4a Subjects follow the recommendations in treatments with a coordination mechanism that induces *unequal* expected payoffs (*CD90*) less frequently than in treatments with a coordination mechanism that induces *equal* expected payoffs (*CD50*).

More specifically, we expect that disadvantaged players are more sensitive to procedural unfairness than advantaged players. Following Bolton et al. (2005) and Trautmann (2009), we assume that individuals dislike being put at a disadvantage more than being in an advantaged position.

Hypothesis 4b Subjects in the role of the *disadvantaged* player follow the recommendations less frequently than the subjects in the role of the *advantaged* player in treatments with a coordination mechanism that induces unequal expected payoffs (*CD90*).

5 Results

5.1 Aggregate analysis

Table 4 presents mean values on contribution rates (playing *X*), coordination rates on one of the two pure-strategy NE (*X*, *Y*) or (*Y*, *X*), rates of following recommendations and point earnings across all subjects and rounds for each treatment. We use Wilcoxon signed-rank tests for testing single or matched samples and Wilcoxon rank-sum tests for testing unmatched samples using a 5% significance level, unless otherwise stated.[9] Contribution rates amount to about 60% in all treatments, with no significant differences between treatments. However, due to the fact that subjects receive recommendations and follow them in more than 75% of cases, coordination rates are higher in treatments *CD50* and *CD90* compared to *Baseline*. The differences in coordination rates have an impact on efficiency in terms of earnings, which are lowest in *Baseline*, followed by *CD90*, and are highest in *CD50*.

As a robustness check, coordination rates and point earnings were also calculated using average values of the variables for all possible pairings, i.e. in each round we calculated for each subject in how many cases, out of possible 12 pairings, they would coordinate on one of two NE and what would be the corresponding payoff. The rates presented in the table are averages over these values over all 30 rounds. The results are similar to the values calculated based on the realized pairings, with one exception concerning the difference in earnings. Based on all possible pairings, earnings in *CD90* are not significantly higher than earnings in *Baseline* but lower than in *CD50*, although this difference is only weakly significant. This change in significance motivates a more detailed discussion of individual and total earnings in Sect. 5.2, where regression results confirm the results from realized pairings.

As subjects were randomly matched in every round and no feedback was supplied, we do not expect any learning over time. Figures illustrating the averages of coordination rates and the decisions to follow the recommendations over the course of 30 rounds in different treatments can be found in the electronic supplementary material. Using Mann-Kendall tests, we do not find evidence for monotonic time trends, which allows us to aggregate the round-level data at the subject level for analysis.

Figure 1 gives a more detailed overview of play across treatments, comparing the observed frequencies of the four possible outcomes with the predictions of MNE and CE in the treatments *CD50* and *CD90*. Players in *Baseline* coordinate on one of the efficient outcomes significantly less often than 0.5, the rate predicted by MNE ($p = 0.016$).[10] Both in *CD50* and *CD90*, coordination rates are significantly higher than in *Baseline* and higher than 0.5: 0.657 and 0.619 respectively ($p < 0.001$ for each comparison). Surprisingly, coordination rates between the treatments with recommendations are not significantly different from each other (see Table 4).

Figure 1 shows that players did not follow the recommendations all the time ($p < 0.001$ for both treatments), as the predicted outcome frequencies of the two NE were not reached. An exception is outcome (X, Y) in *CD90*, which was expected to

[9] In all aggregate tests, we have pooled the data at the subject level across rounds to achieve independent observations. Furthermore, since coordination rates are the same for both types of players (a successful coordination by definition requires two players of different type matched with each other), we have run the tests only on the data from one type of player.

[10] In *Baseline*, we cannot reject the hypothesis that outcome (X, Y) was achieved 25% of time, but we can reject this hypothesis for outcome (Y, X). This result is surprising, since outcomes (X, Y) and (Y, X) are perfectly symmetric. Even though it seems that type 1 players chose strategy X more frequently than type 2 players, we cannot reject the hypothesis that both types of players played X with equal proportions. One possible explanation is a relatively low number of observations; another possible reason might be the emergence of conventions that can differ between populations and is facilitated by labels (Van Huyck et al. 1997).

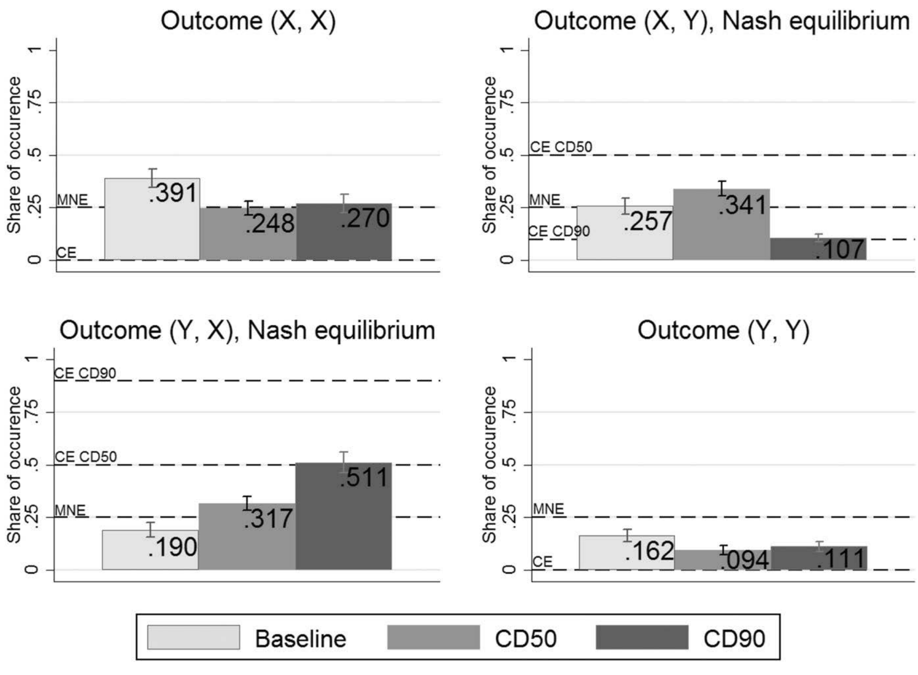

Fig. 1 Outcomes played across treatments with MNE and CE predictions and 95% confidence intervals

be reached 10% of the time. Giving recommendations substantially reduces both under-provision (*Y*, *Y*) and over-provision (*X*, *X*) compared to *Baseline*, although levels of over-provision are still relatively high. This can be explained by the fact that players chose strategy *X*, which guaranteed a low payoff, more frequently than the payoff-uncertain strategy *Y* that resulted in higher payoff if both players followed their recommendations, but in a payoff of zero if the recommendation *X* was not followed.[11]

Figure 2 illustrates to which extent the recommendations are followed by treatment and player type. We find no significant differences between treatments or player types. On average, 79% of all recommendations were followed in *CD50*, while 75% were followed in *CD90*.

Results of pairwise comparisons of average earnings between treatments are provided in the last rows of Table 4. As earnings depend on the subjects' ability to coordinate, the results reflect the findings on coordination. We find significant differences in average earnings between *Baseline* and *CD50* and in average earnings between *Baseline* and *CD90*, but no significant differences in earnings between the treatments with recommendations. Average earnings in the treatments with recommendations are significantly lower than predicted by CE; while average predicted expected earnings in both *CD50* and *CD90* are 7.5, subjects earned only 6.17 ($p < 0.001$) in *CD50* and 5.99 in *CD90*. However, this was still significantly more than predicted by MNE in both treatments (5 points, $p < 0.001$).

[11] The hypothesis of equal frequencies of choosing *X* and *Y* is supported only for type 2 players in treatment *CD50*.

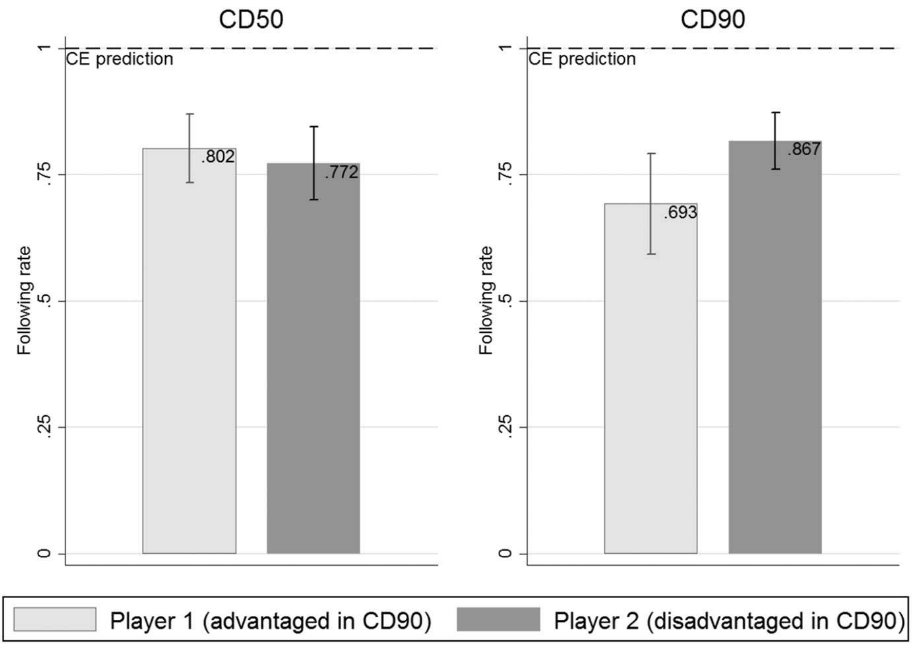

Fig. 2 Following rates by player type in treatments *CD50* and *CD90* with 95% confidence intervals

Figure 3 presents mean earnings by player type and the comparison with predicted earnings of MNE and CE. In the baseline treatment, average earnings of type 1 players were 5.14 points, in line with MNE predictions, while they were significantly higher than the MNE prediction for type 2 players (5.48 points). However, the comparison of earnings between players shows no significant difference in payoffs between type 1 and type 2 players. In *CD50*, earnings of both players are not significantly different from each other, and lie between the earnings predicted by MNE and CE. In the treatment with unfair recommendations, type 2 players earned on average 4.98 points, which is close to what MNE predicts, but significantly lower than predicted by CE. Advantaged type 1 players earn 7.00 points, which is significantly different from MNE and CE predictions. From comparing these earnings with the theoretical benchmarks, we conclude that introducing an unfair procedure constitutes a Pareto improvement compared to a situation without any coordination procedure. Advantaged subjects were significantly better off, while disadvantaged subjects did not lose compared to MNE predictions.[12]

These findings lead us to the first three results: we do not reject Hypotheses 1 and 2a, but we reject Hypotheses 2b and 3:

[12] These results are robust to potential effects of the round matching by recalculating all possible earnings of players in *CD90*, as described in the introduction of this section.

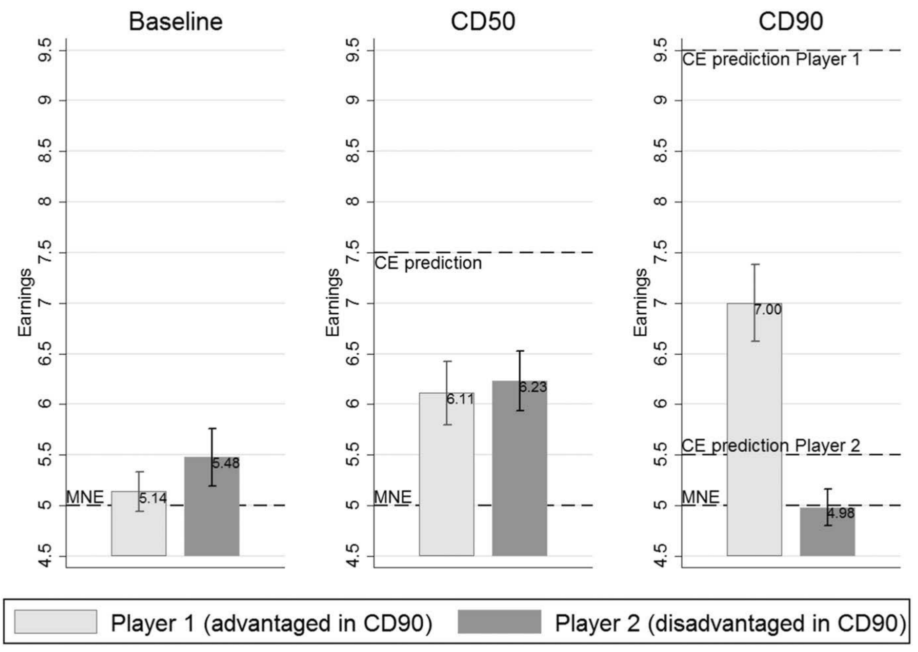

Fig. 3 Earnings by player type in all treatments with 95% confidence intervals

Result 1 Action recommendations that induce equal expected payoffs for both players improve coordination rates and earnings compared to the situation without recommendations.

Result 2 Action recommendations that favour one of the players while putting the other one at a disadvantage improve coordination rates and earnings compared to the situation without recommendations.

Result 3 There are no significant differences in coordination rates and average earnings between treatments with fair and unfair recommendations. Hence, in aggregate terms, procedural fairness concerns seem to play a less important role than efficiency concerns.

5.2 Analyses of individual following behaviour and individual earnings

Next, we examine individual determinants of the decision whether to follow a recommendation or not. Descriptive statistics on the subjects' characteristics across treatments can be found in the electronic supplementary material. Randomization of subjects into treatments was successful, except for differences in the share of females and economics students. Thus, we will control for these variables in our regressions.

Table 4 Key variables in all treatments and pairwise comparisons

	Means			*p*-values of pairwise comparisons		
	Baseline	*CD50*	*CD90*	*Baseline-CD50*	*Baseline-CD90*	*CD50–CD90*
Contribution rate	0.614	0.577	0.580	0.172	0.754	0.302
Coordination rate	0.447	0.657	0.619	<0.001	<0.001	0.553
Coordination rate*	0.471	0.657	0.617	<0.001	0.002	0.783
Following rate	–	0.787	0.754	–	–	0.533
Earnings	5.308	6.171	5.991	<0.001	0.005	0.134
Earnings*	5.428	6.167	5.981	<0.001	0.355	0.069

Pairwise comparisons use Wilcoxon rank-sum tests. For tests on following rates and and earnings, data was collapsed at the subject level ($n = 72$ in each treatment). Since coordination rates are the same for both types of players, $n = 36$ in each treatment

* Values based on all possible pairings

Table 5 shows the results of linear probability model (LPM) regressions with the dependent variable taking value 1 if a player followed the recommendation and 0 otherwise.[13] In Model 1, individuals' behaviour is explained by treatment and type of player, as well as the interaction between the two. Type 1 players, who were advantaged by the coordination mechanism in *CD90*, are less likely to follow the recommendation than type 1 players in *CD50*, although this effect is only marginally significant. There are no significant differences in following recommendations between type 2 players in *CD50* and *CD90* ($p = 0.321$). Testing the linear combination of parameters reveals that disadvantaged players in *CD90* follow the recommendations more often than advantaged players in *CD90* ($p = 0.028$).

Model 2 includes a dummy variable capturing if a subject received a favourable recommendation not to volunteer (i.e., to play *Y*) and its interaction with the treatment variable. This is the recommendation that potentially results in a payoff of 10, given both players follow their recommendation. However, if the other player does not follow the recommendation to volunteer, both players will earn zero points. Following a *Y*-recommendation thus always comes with the uncertainty of receiving zero. Players are significantly less likely to follow recommendation *Y* compared to recommendation *X*. While individuals are averse towards the possibility of getting zero payoff, procedural (un)fairness does not significantly affect one's decision to follow a *Y*-recommendation, as the interaction effect between *CD90* and receiving an advantageous recommendation is very close to zero. Once the variable capturing the type of recommendation is included, the coefficient of the interaction term between treatment and player type becomes insignificant, indicating that the difference in the behaviour of type 1 players between treatments stems mainly from the fact that type 1 players in *CD90* receive more advantageous recommendations than type 1 players in *CD50*. There are no significant differences across players

[13] Since we are mainly interested in interaction effects between treatment and type of player, we use LPM regressions to analyse the data, as resulting interaction terms cannot be interpreted in the same way in non-linear models as in linear models (for contributions to this discussion see e.g. Ai and Norton 2003; Greene 2010; Puhani 2012; Karaca-Mandic et al. 2012).

Table 5 Linear probability model on following the recommendations

	Model 1 coef./SE	Model 2 coef./SE	Model 3 coef./SE	Model 4 coef./SE
Treatment				
CD90	−0.109*	−0.036	0.028	−0.001
	(0.059)	(0.058)	(0.056)	(0.073)
Type of player				
Player 2	−0.030	−0.030	−0.006	−0.037
	(0.048)	(0.048)	(0.040)	(0.040)
Treatment*Type of player				
CD90 × player 2	0.154**	0.020	−0.020	0.007
	(0.074)	(0.068)	(0.056)	(0.057)
Advantageous recomm.				
Yes		−0.154***	−0.154***	−0.154***
		(0.046)	(0.046)	(0.046)
Treatment*Advantageous recomm.				
CD90 × yes		−0.013	−0.013	−0.013
		(0.071)	(0.071)	(0.071)
Others follow				
Yes			0.288***	0.275***
			(0.034)	(0.035)
Constant	0.802***	0.879***	0.639***	0.764***
	(0.033)	(0.035)	(0.044)	(0.165)
Control variables	No	No	No	Yes
Adj. R^2	0.012	0.036	0.128	0.142
Number of cases	4320	4320	4320	4320

Control variables include round, session dummies, female dummy, economics/business student dummy, below-average risk aversion dummy, Locus of Control, Big Five

Significance levels * 10%, ** 5%, *** 1%. Standard errors clustered at the subject level

within or between the treatments with recommendations. One possible interpretation might be that it is not the unfair procedure per se that decreases the likelihood of following, but the uncertainty of the outcome. Individuals are willing to reject the favourable procedure to secure a lower payment, instead of dealing with the uncertainty if the other player will follow a recommendation that puts her at a disadvantage. Our results are in line with Van Huyck et al. (1990), who study how individuals behave when facing strategic uncertainty in coordination games with multiple equilibria and found support for individuals choosing actions that maximize minimum payoffs. In our study, strategy X is a maximin strategy, as volunteering ensures that the public good is provided and hence grants payoff of 5 to the provider.

To explore whether beliefs about others' behaviour regarding recommendations affect the decision to follow recommendations across treatments and player types, we include a variable that captures subjects' beliefs about how others react to recommendations (Model 3). This variable was elicited via the non-incentivized post-experimental questionnaire. In line with previous research (Cason and Sharma 2007), beliefs matter for individual behaviour. Those who believe that individuals in the role of the other player follow recommendations are more likely to follow them as well. It is also a sign that subjects understood that it is best for them to follow the recommendations if others do so.[14]

In the specification of Model 4, we control for the following variables: gender, period effects, session effects and subject of studies, as well as risk aversion and personality traits (measured by Locus of Control and Big Five tests), which seem not to be correlated with following the recommendations and have a very small effect on the coefficients of the other variables as well as on the goodness of fit.[15] It might seem surprising that elicited risk preferences are not significant in explaining the decision to follow recommendations, which entails strategic uncertainty, but similar results have been found in previous studies. For example, Kocher et al. (2015) show that there is no relation between risk preferences and cooperation in a public good game. The authors argue that preferences towards risk stemming from nature might differ from the preferences towards uncertainty resulting from actions of another person (see also Bohnet et al. 2008).[16]

The analysis of individual-level behaviour in response to recommendations leads to results 4a and 4b, in which we reject hypotheses 4a and 4b:

Result 4a In the treatment with the coordination mechanism that induces unequal expected payoffs (*CD90*), subjects do not follow the recommendations less than in the treatment with a coordination mechanism that induces equal expected payoffs (*CD50*).

Result 4b Disadvantaged players do not follow recommendations significantly less often than advantaged players or players in the fair treatment. However, there are differences in how players react to advantageous recommendations: these are followed less often than disadvantageous recommendations.

We also conducted OLS regressions on individual point earnings. The results in Table 6 corroborate previous findings: following recommendations is a payoff-enhancing strategy for all players. Advantaged players in *CD90* earn significantly more than disadvantaged players in *CD90* and type 1 players in *CD50*, who in turn earn more than type 1 players in *Baseline*; type 2 players in *CD50* earn more than type 2 players in *Baseline* or disadvantaged players in *CD90*. There are no

[14] A more detailed analysis of the relevance of beliefs is conducted in the next subsection.

[15] Only individuals with a more pronounced trait Neuroticism follow recommendations significantly less often ($p = 0.012$).

[16] As robustness checks, we ran panel regressions, probit regressions and logistic regressions with odds ratios. Results are consistent with our LPM results and are available upon request. We also analysed whether there are significant gender differences between treatments. While women follow recommendations significantly more frequently than men in treatment *CD50*, the relation is insignificant in *CD90* and has the opposite sign. Furthermore there are no differences between women in *CD50* and *CD90*, nor between men in these conditions. To conclude, while women do follow recommendations more frequently than men if the procedure is fair, these gender differences disappear in an unfair environment. The results are available from the authors upon request.

Table 6 OLS regressions on earnings

	Model 1 coef./SE	Model 2 coef./SE	Model 3 (CD50&CD90) coef./SE	Model 4 (CD50&CD90) coef./SE	Model 5 (CD50&CD90) coef./SE
Treatment					
CD50	0.972***	0.995***			
	(0.18)	(0.29)			
CD90	1.861***	1.730***	0.889***	1.149***	1.146***
	(0.21)	(0.29)	(0.24)	(0.14)	(0.19)
Type of player					
Player 2	0.338**	0.310**	0.120	0.191	0.121
	(0.17)	(0.15)	(0.21)	(0.13)	(0.14)
Treatment*Type of player					
CD50 × player 2	−0.218	−0.331			
	(0.27)	(0.26)			
CD90 × player 2	−2.356***	−2.336***	−2.139***	−2.504***	−2.414***
	(0.26)	(0.25)	(0.29)	(0.18)	(0.19)
Follow recommendations					
Yes				2.378***	2.344***
				(0.13)	(0.14)
Constant	5.139***	5.186***	6.111***	4.204***	4.187***
	(0.10)	(0.57)	(0.15)	(0.14)	(0.46)
Control variables	No	Yes	No	No	Yes
Adj. R^2	0.050	0.059	0.055	0.160	0.161
Number of cases	6480	6480	4320	4320	4320

Control variables include round, session dummies, gender, economics/business student dummy, below-average risk aversion dummy, Locus of Control, Big Five

Significance levels * 10%, ** 5%, *** 1%. Standard errors clustered at the subject level

significant differences in payoffs between disadvantaged players in *CD90* and type 2 players in the *Baseline* treatment.

We further test the effects of the treatments on total earnings as a robustness check to our findings in Table 4. For Model 1 and Model 2 we create a hypothetical player whose earnings is the average of type 1 and type 2 players and calculate the marginal effects of the different treatments. For Model 1 the marginal effects are 0.181 for *CD50* compared to *CD90* ($p = 0.219$) and −0.683 for *Baseline* compared to *CD90* ($p < 0.001$). For Model 2 the marginal effects are 0.267 for *CD50* compared to *CD90* ($p = 0.345$) and −0.562 for *Baseline* compared to *CD90* ($p = 0.018$). Using this approach, we further confirm our findings reported in Table 4 concerning earnings: there is no difference in average earnings between treatments with recommendations, however earnings in both treatments: *CD50* and *CD90* are significantly higher than earnings in *Baseline*.[17]

5.3 The role of beliefs

Our analysis shows that beliefs play an important role for individual behaviour. We are now going to analyse the relationship between beliefs and the fairness of the recommendation procedure. Our dependent variable describing beliefs takes value 1 if a subject believes that everyone or most of the players in the other role follow recommendations. There is a significant relationship between treatment and beliefs (chi-square test $p = 0.042$), 64% of players in *CD90* believe that players in the other role will follow the recommendations, while it is 79% of all players in *CD50*. A further decomposition of data by type of player shows that these differences in beliefs are driven by type 1 players. 75% believe that others follow in *CD50*, while it is only 61% in *CD90* ($p = 0.035$ for the sub-sample of type 1 players). Hence, the advantaged players, knowing that others are put at a disadvantage, expect them to follow the recommendations less frequently. This may indicate that those players believe that disadvantaged players are concerned about the fairness of the procedure.

Beliefs of players correspond well with observed behaviour, with an exception for advantaged players in *CD90*: disadvantaged players in *CD90* follow recommendations significantly more often than advantaged players believe them to do (one-sided test of proportions $p = 0.027$).

To investigate whether these differences in beliefs are related to individuals' behaviour, we compare whether following rates differ with beliefs. Following rates are positively correlated with beliefs ($p < 0.001$ for each treatment). Players who believe that others follow, do follow themselves to a larger extent in both treatments (see Table 7). Conditional on individual beliefs, there are no differences in average following rates of both player types between treatments. The left panel of Table 7 displays following rates in treatment *CD50*, contingent on type of player and beliefs. In this treatment, both players were treated fairly by the coordination mechanism and their expected payoffs were the same; hence, we do not expect any differences in following the recommendation between players. Although there seems to be a small difference conditional on believing that others do not follow the recommen-

[17] We also estimated the marginal effects of treatments in OLS regressions based on all possible pairings of type 1 and type 2 players within a session. This approach gives us qualitatively identical results.

Table 7 Following rates contingent on subjects' beliefs and player type in *CD50* and *CD90*

Others follow	CD50		Wilcoxon rank-sum *p*	CD90		Wilcoxon rank-sum *p*
	Type of player			Type of player		
	Player 1	Player 2		Player 1 (advantaged)	Player 2 (disadvantaged)	
Yes	0.841	0.851	0.636	0.873	0.854	0.495
	n = 30	n = 27		n = 22	n = 24	
No	0.606	0.537	0.120*	0.410	0.742	0.001
	n = 6	n = 9		n = 14	n = 12	
Wilcoxon rank-sum *p*	0.008	<0.001		<0.001	0.050	

* *p*-value based on the exact statistic, since the number of observations in two groups is below 25

dation, this is not statistically significant. The right panel of Table 7 shows following rates in treatment *CD90* contingent on beliefs and type of player. There is a significant difference in following recommendations between advantaged and disadvantaged players who think that other subjects mainly do not follow recommendations. Regardless of their beliefs, disadvantaged players follow their recommendations most of the time, while advantaged players only follow recommendations around 40% of the time if they believe others mostly do not follow.

Next, we look at following rates as a response to either a disadvantageous or advantageous recommendation. Figure 4 provides following rates for different types of recommendations contingent on beliefs. Recommendations to volunteer (*X*) are followed around 80% of the time, regardless of beliefs (see the left panel). If a player receiving recommendation *X* believes that her counterpart does not follow the received recommendation, not following her own recommendation involves the risk of getting zero, and apparently this risk outweighs the chance of getting the higher payoff. Beliefs are correlated with the decision to follow only when individuals receive the advantageous recommendation *Y* that involves the risk of getting zero payoff, as can be seen from the right panel. Players follow that recommendation significantly more often if they believe players in the other role do follow their recommendation as well.

These findings lead us to the following result:

Result 5 Advantaged individuals in the treatment with an unfair coordination mechanism believe less frequently that everyone or most of their counterparts follow recommendations than individuals in the treatment with a fair coordination mechanism. Furthermore, beliefs are correlated with following rates only when following the recommendation does not guarantee a safe payoff.

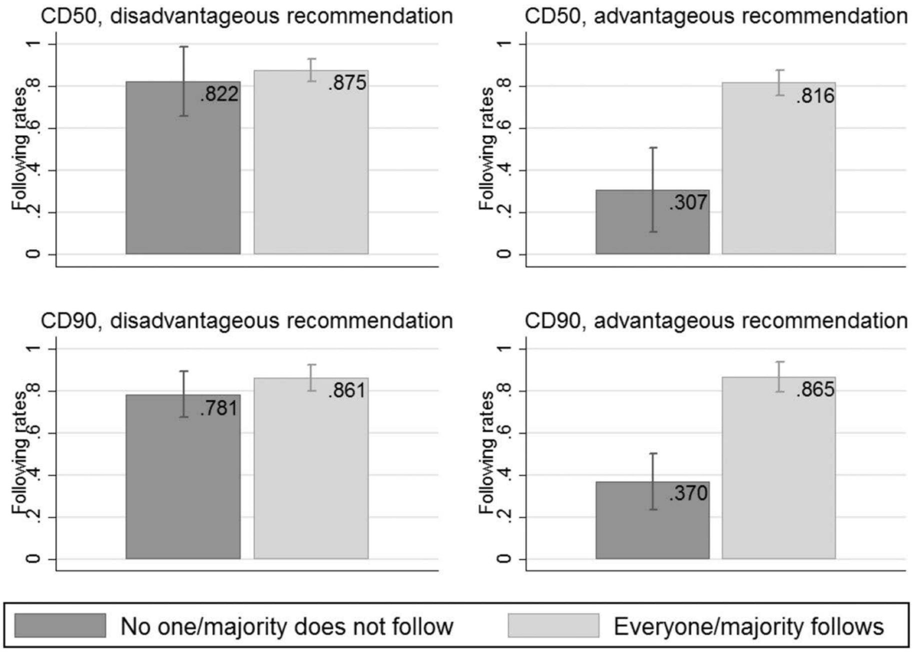

Fig. 4 Following rates contingent on subjects' beliefs and type of recommendation in *CD50* and *CD90* with 95% confidence intervals

6 Discussion and conclusion

Our study highlights the benefits of external action recommendations in improving coordination. We demonstrate that the existence of such a coordination mechanism increases efficiency, even if one party is strongly favoured by the mechanism. When individuals are confronted with a situation in which they face uncertainty about the behaviour of the other party, recommendations play an important role for coordination, even if it induces inequality in expected payoffs.

The findings from the study can be applied in coordination mechanisms where fairness might play a role, for example, informal rules governing the exploitation of common pool resources. While there might be many outcome allocations that guarantee sustainability, inequality in the expected harvest can lead to destabilization of the governing institutions (Klain et al. 2014; Cox et al. 2010). On a larger scale, preventing the catastrophic consequences of climate change can be modelled as a coordination game with multiple equilibria (Tavoni et al. 2011; DeCanio and Fremstad 2013; Madani 2013). In this context, action recommendations can be understood as the suggestion of an equilibrium profile by a 'global planner' (Forgó et al. 2005). This suggestion does not necessarily have to imply equal expected payoffs (Beg et al. 2002; Thomas and Twyman 2005). A negotiation process that is perceived as fair by all parties has been identified as an important prerequisite to reach an agreement (Winkler and Beaumont 2010; Lange et al. 2010; Rübbelke 2011).

We find that subjects follow disadvantageous recommendations more frequently than advantageous ones, which is in line with the results of Eckel and Wilson (2007), who show that signals of actions that are less risky but lead to a Pareto-inferior NE are more likely to be followed in a coordination game, compared to signals aiming at implementing a Pareto-superior NE involving more payoff-uncertainty. The authors find that signals to play the less risky but inefficient action are readily followed. Similarly, Brandts and Macleod (1995) find that the choice of strategy is affected by the minimum payoff that one can gain by playing it in a coordination game with recommended play. In other words, less risky strategies involving less payoff-uncertainty are more likely to be followed even if they constitute Pareto-inferior equilibria.

Our results corroborate findings of Hong et al. (2015). In their experiment, subjects had to trade off a fair distribution of payoffs against an increasing sum of payoffs. The authors estimate social welfare preferences and find that the majority of the individuals weakly prefers efficiency over equality.

However, our findings differ from Anbarci et al. (2017) where subjects received external recommendations that implied ex-ante payoff-equality but ex-post inequality in Battle of the Sexes games. The authors report generally higher following rates than we do, but find that subjects disregard the recommendations more often when payoff-asymmetry increases. Potential reasons for these discrepancies can be found in differences in the experimental design. Firstly, Anbarci et al. use a game with two outcomes that imply zero payoff to both players. This might explain why they find higher following rates. A second difference is that subjects in their experiments receive feedback between interactions, which makes it possible for subjects to condition their following behaviour on past outcomes, which can make payoff differences more salient. Thirdly, they change the payoff matrix across treatments and keep the probabilities of their recommendations constant, while we keep the matrix constant and measure the impact of the fairness of the recommendation procedure. Our interpretation of the differing results of both studies is that individuals are more sensitive towards payoff (distributional) inequality than towards process inequality. Yet, for conclusive evidence further research has to be conducted explicitly comparing preferences for distributional fairness with preferences for procedural fairness.

Furthermore, the study by Bolton et al. (2005) can help explain the high acceptance of our unfair recommendation procedure. The authors show that a biased procedure is more likely to be accepted if an unbiased procedure is not feasible. In our study, subjects can either follow recommendations that put one of them in a disadvantaged position or reject it; however, rejection implies a substantial loss of efficiency. There is no fair coordination procedure available in treatment *CD90*. Potentially, if an unfair procedure was publicly chosen over the fair one, rejection rates of the recommendations could be higher.

Moreover, it is possible that subjects would reject unfair action recommendations to a larger extent if they were picked by other subjects instead of the experimenters, in a similar fashion as they reject unfair ultimatum proposals more often if they are chosen by a subject using a 'monocratic' rule compared to a 'democratic' rule, as for example in Grimalda et al. (2008). In our experiment, the procedure was chosen by the experimenter and subjects were randomized into the roles of player 1 and player 2. Randomization into roles could be seen as a fair procedure, reducing potential concerns about the lottery determining expected earnings. However, Bolton et al. (2005) observe rejections of unfair procedures even if they are implemented by an experimental lottery similar to our study. Furthermore, in a post-experimental questionnaire, we asked subjects if they feel disadvantaged and learned that significantly more type 2 players in *CD90* feel disadvantaged than type 1 players in the same treatment (Wilcoxon rank-sum test, $p < 0.001$). More research is needed to identify the characteristics of situations in which unfair procedures are rejected versus situations in which such procedures are accepted. This is also crucial for policy-makers to understand when policy suggestions, for example on public good provision, will face resistance and when they will be accepted by the general public.

Our study is limited to cases that can be represented as one-shot situations, as subjects had only a low probability of encountering their current "partner" repeatedly in our experiments. It would be of interest to investigate in future research how outcomes change when subjects learn the outcomes after every encounter. It might well be the case that procedural fairness considerations become more salient when individuals are allowed to learn over time.

Our choice of game was guided by the non-existence of strictly dominated strategies, high potential gains from following the recommendations, and the applicability to threshold public good provision. However, we think that the influence of procedural fairness concerns can be important in other games as well. Examining the sensitivity of our results with respect to different types of games and payoff structure (e.g. by varying the difference between payoffs in case of coordination and miscoordination) is left to further research.

In general, the role of beliefs that are formed when individuals face different procedures deserves further investigation. In our study, beliefs were elicited only after the whole experiment in a non-incentivized task and were not contingent on the type of recommendation. Our results indicate that subjects might hold wrong beliefs about how others react to recommendations when facing a procedure treating individuals unequal. As incorrect beliefs can lead to further inefficiencies if subjects act in accordance with them, additional research is necessary to explore their role in driving people's behaviour in situations in which concerns about procedural fairness and efficiency, as well as strategic uncertainty, are involved.

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Electronic Supplementary Material to "Fairness versus efficiency: how procedural fairness concerns affect coordination" by Verena Kurz, Andreas Orland and Kinga Posadzy

Tables

Table 1: Balancing test on observables

	Means			Differences		
	Baseline	CD50	CD90	CD50-Baseline	CD90-Baseline	CD50-CD90
female	0.514	0.653	0.569	-0.139*	-0.055	0.083
age	24.46	23.96	23.54	0.500	0.917	0.417
semesters	4.028	4.403	4.028	-0.375	0	0.375
econ	0.472	0.278	0.431	0.194**	0.042	-0.153*
Risk aversion task	6.403	6.625	6.944	-0.222	-0.542	-0.319
Personality traits						
Locus of Control[a]	11.72	12.17	12.64	-0.444	-0.917	-0.472
Neuroticism[b]	12.17	13.03	12.31	-0.861	-0.139	0.722
Extraversion[b]	14.79	15.04	15.21	-0.250	-0.417	-0.167
Openness[b]	15.04	15.85	15.49	-0.806	-0.444	0.361
Agreeableness[b]	15.29	16.03	15.29	-0.736	0	0.736
Conscientiousness[b]	15.43	15.15	15.60	0.278	-0.167	-0.444
N	72	72	72			

Significance levels : $*$: 10% $**$: 5%. Two-sided t-tests.

[a] Locus of Control can range from 0 to 23. We added up the external answers to the questions, hence a higher LoC means that a subject is more external and believes that her life and decisions are controlled by environmental factors rather than by herself.

[b] Each of the Big Five traits can range between 3 and 21. We added up the answers given on seven-item Likert scales to the three questions for each trait. A higher score means that the trait is more pronounced.

Figures

Figure 1: The series of recommendations in *CD50* and *CD90* for Player 1

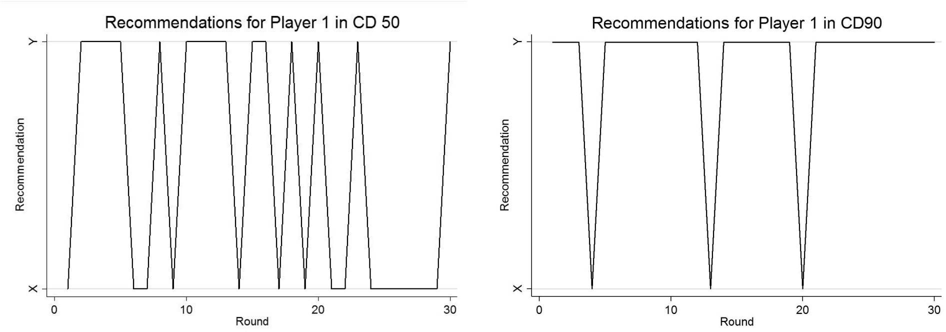


Figure 2: Average coordination rates over time in all three treatments

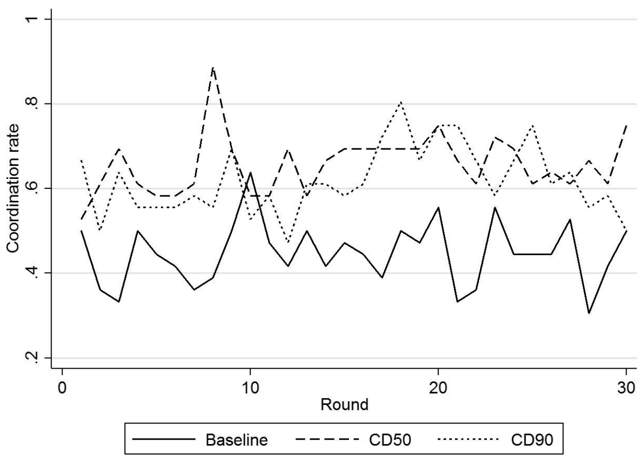


Figure 3: Average following rates over time in *CD50* and *CD90*

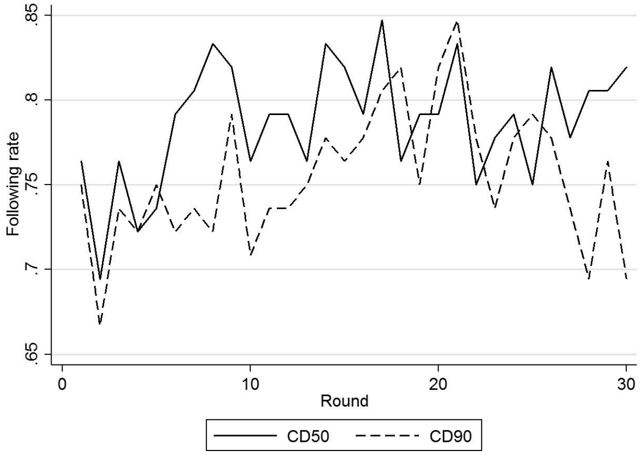


Figure 4: Average earnings over time in all three treatments

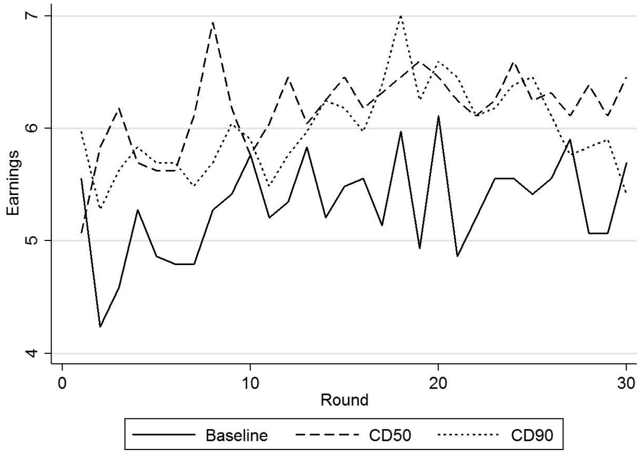


Figure 5: Average following rates over time in *CD50*, separately for Player 1 and Player 2

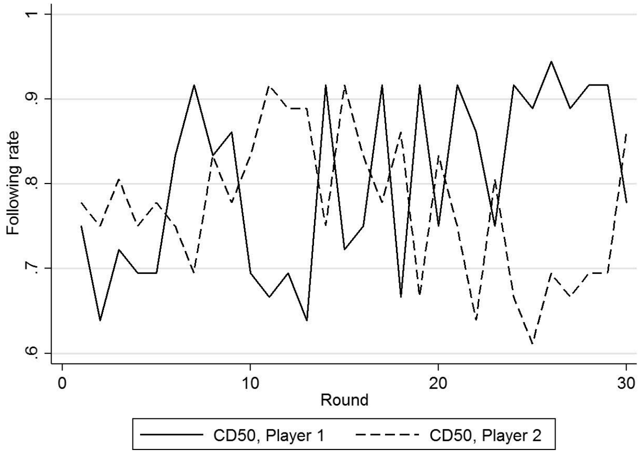
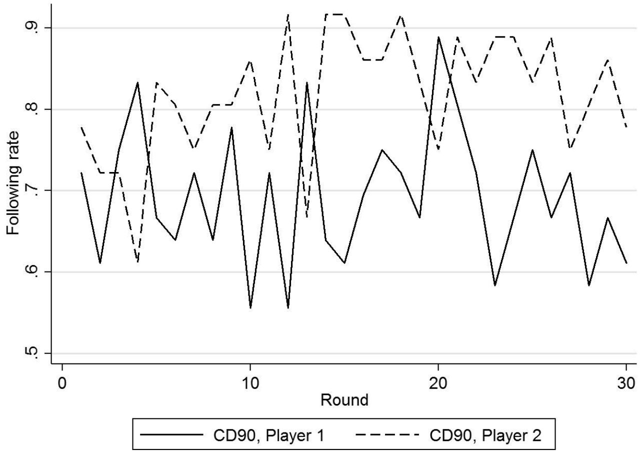


Figure 6: Average following rates over time in *CD90*, separately for Player 1 and Player 2



Translation of the Experimental Instructions for the *CD50* Treatment

General Instructions

Welcome to this experiment! Please read the instructions carefully. They are identical for all participants. During the experiment, you and the other participants are asked to make decisions. All money you earn will be paid to you privately in cash at the end of the experiment. In addition, you will receive a show-up fee of 4 euros.

During the experiment, it is forbidden to talk with the other participants, to use mobile phones, or to start other programs at the computer. Please also turn off all electronic devices. If you do not follow these rules, you will be excluded from the experiment and all payments.

If you have a question, please raise your hand. An experimenter will then come and answer your question quietly. If the question is relevant for all participants, we will repeat it publicly and answer it.

Part I of the experiment

Roles and the number of rounds

In this part of the experiment, you will be asked to make a decision in each of 30 rounds, which will be described below. 24 people participate in today's experiment. Before the first round begins, all participants will be randomly divided for today's experiment into two equal-sized groups. One group is called the Red Participants, and the other is called the Blue Participants. The group you are in will stay the same throughout the experiment.

In each round, you will be randomly matched to a person in the other group. You have an equal chance of 1-to-12 of being matched to any particular person in the other group. You will never interact with participants belonging to the same group as you. You will not be told the identity of the person you are matched with, nor will that person be told your identity, even after the end of the session. All the decisions you make, and the other information you provide us, will remain confidential.

The structure of the experiment in each round

All rounds are identically structured. Both you and the person you are matched with have two choices available: X and Y. The choices that you and your matched participant make jointly determine your point earnings for the round. The following table shows how the amount of points depending on your and your matched participant's decisions is determined:

Payment table

		Blue Participant	
		X	Y
Red Participant	X	Red earns: 5 Blue earns: 5	Red earnst: 5 Blue earnst: 10
	Y	Red earns: 10 Blue earns: 5	Red earns: 0 Blue earns: 0

In each round, one of the four cells in the above table will be relevant to your point earnings. If you are a Red Participant, your choice of X or Y will determine which row of the table

the relevant cell belongs to. Your matched participant's choice of X or Y will determines the column.
If you are a Blue Participant, the situation is reversed: your choice of X or Y will determine which column of the table the relevant cell belongs to, and your matched Red Participant's choice of X or Y will determine which row the relevant cell belongs to.
In both cases, your choices, as well as the choices of the participant you are matched with, determine the relevant cell. The first number in the relevant cell represents the Red Participant's point earning for the round and the second number represents the Blue Participant's point earning for the round.

- If the Red Participant chooses X and the blue participant chooses Y, Red earns 5 points and Blue earns 10 points.
- If both participants choose X, both each receive 5 points.
- If both participants choose Y, both each receive 0 points.
- If the Red Participant chooses Y and the blue participant chooses X, Red earns 10 points and Blue earns 5 points.

Recommendations

Before you choose your action for each round, both you and the participant you are matched with will be given recommendations on the screen. In any round, there are two possible recommendations. Those are generated according to the following rules:

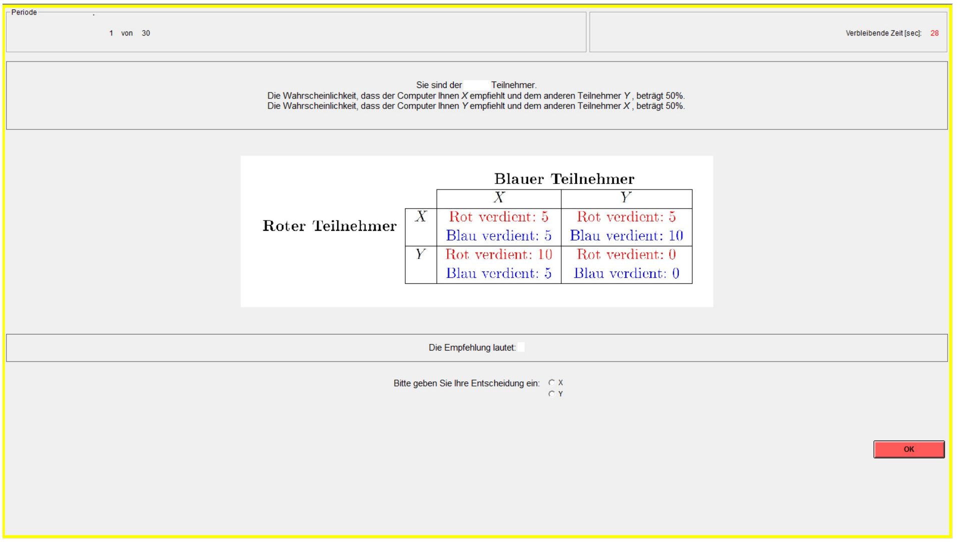
- There is a 50% chance (on average 5 out of 10 times) in each round that it will be recommended that the Red Participant choose $\boldsymbol{X}$ and the Blue Participant choose $\boldsymbol{Y}$.
- There is a 50% chance (on average 5 out of 10 times) in each round that it will be recommended that the Red Participant choose $\boldsymbol{Y}$ and the Blue Participant choose $\boldsymbol{X}$.

It will never happen that you are recommended to both choose X or both choose Y . These recommendations are optional; it is up to you whether or not to follow them. Notice that your recommendation also gives you information about the recommendation that was given to the person matched to you. The recommendations themselves have no direct effect on the points you can earn. The following table summarizes these recommendations and their likelihoods.

		Blue Participant	
		X	Y
Red Participant	X	never recommended Red earns=5, Blue earns=5	recommended with 50% probability (5 out of 10 times) Red earns=5, Blue earns=10
	Y	recommended with 50% probability (5 out of 10 times) Red earns=10, Blue earns=5	never recommended Red earns=0, Blue earns=0

Your decision

Each participant makes his or her decision without knowing the decision of the other participant. The following figure shows the example of a screen-shot where you enter your decision:



You should make your decision within the proposed 30 seconds. The computer program gives you as much time as you need, even though this takes more than the 30 seconds. After that time, you will be shown the request “Please make your decision now”.

After all participants have made their decisions and clicked the red OK button, the next round will start immediately. You will not receive any information on the decision of the other participant or the point earnings. This information will be provided to you after the end of the experiment.

As a reminder: You will be re-matched with a participant in the other role before each round.

Earnings from part I of the experiment

After round 30, the computer program will randomly select two rounds. The total number of points you earn in these two rounds will be converted into cash at an exchange rate of 75 euro cents per point. The two rounds chosen for the payments hold for all the participants. You will be informed at the end of the experiment which two rounds were chosen for payment.

Part II of the experiment

The second part of the experiment is independent from the first part. Both the instructions and the exchange rate from points to euros for part II will be different from part I. All necessary information and the exchange rate will be shown on the computer screen after the end of the first part. If you have questions concerning part II, raise your hand. An experimenter will then come to your place to answer your questions quietly.

After part II of the experiment

After the second part of the experiment, the computer will show a questionnaire. After you have filled in the questionnaire completely, you will see a summary of all your decisions and the decisions of the participants you were matched with. It will also show you your earnings. The earnings are calculated from the points you received in both parts of the experiment and the respective exchange rates. Your cash payment is this amount plus the 4 euros show-up fee.

If you have any questions, please raise your hand now. If there are no further questions, the experiment will start with a short quiz at the computer. This quiz is solely conducted to test your understanding of these instructions and has no influence on your payment.

Translation of the Questions with Correct Answers of the On-screen Quiz in the *Baseline* Treatment

- Right or wrong: I stay in all 30 rounds a Red or Blue participant. (Right)
- Right or wrong: I will meet in all 30 rounds the same participant in the other role. (Wrong)
- Right or wrong: I can observe the other participant's choice of X or Y before I make my own choice of X or Y. (Wrong)
- Assume you are the Red participant. If you choose X and the other participant Y, what are your point earnings? (5)
- Assume you are the Blue participant. If you choose Y and the other participant X, what are your point earnings? (10)
- Assume that you and the other participant choose X. How many points earns each of you? (5)
- Assume that you and the other participant choose Y. How many points earns each of you? (0)
- Right or wrong: At the end of the experiment I receive the earnings of two randomly chosen rounds in part I of the experiment at an exchange rate of 0.75 euros per point. (Right)

Translation of the Questions with Correct Answers of the On-screen Quiz in the *CD50* Treatment

- Right or wrong: I stay in all 30 rounds a Red or Blue participant. (Right)
- Right or wrong: I will meet in all 30 rounds the same participant in the other role. (Wrong)
- Right or wrong: If the recommendation of my computer is X, then the other participant's recommendation is also X. (Wrong)
- What is the probability of receiving a Y recommendation for Red participants? (50
- What is the probability of receiving an X recommendation for Red participants? (50
- Out of 10 recommendations, how many X recommendation sees a Red participant on average? (5)
- Out of 10 recommendations, how many Y recommendation sees a Red participant on average? (5)
- What is the probability of receiving a Y recommendation for Blue participants? (50
- What is the probability of receiving an X recommendation for Blue participants? (50
- Out of 10 recommendations, how many X recommendation sees a Blue participant on average? (5)
- Out of 10 recommendations, how many Y recommendation sees a Blue participant on average? (5)
- Right or wrong: I can observe the other participant's choice of X or Y before I make my own choice of X or Y. (Wrong)
- Assume you are the Red participant. If you choose X and the other participant Y, what are your point earnings? (5)
- Assume you are the Blue participant. If you choose Y and the other participant X, what are your point earnings? (10)
- Assume that you and the other participant choose X. How many points earns each of you? (5)
- Assume that you and the other participant choose Y. How many points earns each of you? (0)
- Right or wrong: At the end of the experiment I receive the earnings of two randomly chosen rounds in part I of the experiment at an exchange rate of 0.75 euros per point. (Right)

Translation of the Questions with Correct Answers of the On-screen Quiz in the *CD90* Treatment

- Right or wrong: I stay in all 30 rounds a Red or Blue participant. (Right)
- Right or wrong: I will meet in all 30 rounds the same participant in the other role. (Wrong)
- Right or wrong: If the recommendation of my computer is X, then the other participant's recommendation is also X. (Wrong)
- What is the probability of receiving a Y recommendation for Red participants? (90
- What is the probability of receiving an X recommendation for Red participants? (10
- Out of 10 recommendations, how many X recommendation sees a Red participant on average? (1)
- Out of 10 recommendations, how many Y recommendation sees a Red participant on average? (9)
- What is the probability of receiving a Y recommendation for Blue participants? (10
- What is the probability of receiving an X recommendation for Blue participants? (90
- Out of 10 recommendations, how many X recommendation sees a Blue participant on average? (9)
- Out of 10 recommendations, how many Y recommendation sees a Blue participant on average? (1)
- Right or wrong: I can observe the other participant's choice of X or Y before I make my own choice of X or Y. (Wrong)
- Assume you are the Red participant. If you choose X and the other participant Y, what are your point earnings? (5)
- Assume you are the Blue participant. If you choose Y and the other participant X, what are your point earnings? (10)
- Assume that you and the other participant choose X. How many points earns each of you? (5)
- Assume that you and the other participant choose Y. How many points earns each of you? (0)
- Right or wrong: At the end of the experiment I receive the earnings of two randomly chosen rounds in part I of the experiment at an exchange rate of 0.75 euros per point. (Right)

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