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Contents

Chapter 1

Introduction

This is a cumulative dissertation consisting of four chapters, each of which includes one self-contained paper. Despite the papers not being dependent on one another, there is an underlying connection between the four. All the papers included in this dissertation are concerned with topics that are relevant for managerial effectiveness, the performance of companies, and the accompanying social outcomes.

In the first paper, we focus on organizational change and how to increase the likelihood of it succeeding by applying two different nudges[1] (default rule and recommendation nudge). Understanding how the success rate of change projects can be increased is essential for companies due to constantly changing market environments (Moran & Brightman, 2000; Woodward & Hendry, 2004) and many reports of projects failing because of employees' resistance (Burnes, 2015; Cândido & Santos, 2015; Jørgensen et al., 2014; Jørgensen et al., 2008; Pardo del Val & Fuentes, 2003). Hence, our paper supports current efforts in finding practical solutions to an ever-growing problem in the managerial literature and day-to-day work of managers.

In the second paper, I take a look at risky and uncertain managerial decisions and whether a default rule can guide managerial decision-making in these two similar but distinct domains. Shareholders want to steer their managers to act in the best interests of the shareholders by maximizing current and future profits. Therefore, there is a need for neutral managerial behavior, such as taking reasonable risks when they are expectably profitable for the company (Lovallo et al., 2020). However, reports indicate that managers are often too risk-averse (González et al., 2013; Krivkovich & Levy, 2015; MacCrimmon & Wehrung, 1990; Milidonis & Stathopoulos, 2014; Milkman et al., 2009; Schwenk, 1984; Simon et al., 2000). The paper helps to shed light on whether default rules can be applied to the two predominant domains of managerial decisions (risk and uncertainty).

The third paper follows up on this by investigating whether a recommendation nudge can guide managerial behavior in risky managerial situations. Again, the motivation is that managers are too risk-averse (Lovallo et al., 2020). We dive deeper into how a recommendation nudge can be fruitful and whether this type of nudging suffers as a result of materializing after rather than prior to the

[1] Nudges are "any aspect of the choice architecture that alters people's behavior ... without forbidding any options or significantly changing their economic incentives" (Thaler & Sunstein, 2008, p. 6). The concept is rooted in libertarian paternalism (Thaler & Sunstein, 2003). Based on this, it is both feasible and legitimate to affect others' behavior while respecting their freedom of choice and acting in their best interests.

start of the decision-making process. Hence, the paper provides further insights into how nudging can support neutral managerial behavior and under which conditions a recommendation nudge is impactful.

Finally, in the fourth paper, we examine how unethicality is spreading and whether gender matters in this process. Understanding how people adapt to surrounding unethical acts and whether gender is a relevant factor are of the utmost importance for companies. Failing to either prevent or intervene can lead to widespread unethicality (Cohn et al., 2014; O'Brien, 2003), with potentially devastating outcomes such large-scale social damage due to bankruptcy (e.g., Wirecard: Davies, 2020; and Enron: Emshwiller & Smith, 2001) and large-scale ecological damage (e.g., Volkswagen: Boston, 2017). Our paper offers insights into how merely thinking and factually learning about others' dishonesty influences males' and females' lying in a subsequent situation.

Overall, this dissertation combines four papers that aim to provide insights into how the economic outcomes of companies and the accompanying social wellbeing can be protected or enhanced. More specifically, this dissertation offers investigations into how companies can be more successful when starting a change project by using nudging, can avoid managerial risk or uncertainty aversion by implementing a default rule, and can avoid managerial risk aversion by introducing a recommendation nudge at different stages of the decision-making process. All of these are primarily aimed at supporting business efforts but also aid social wellbeing by guiding employee behavior towards supporting change and managerial decision-making towards more neutrality. The lack of neutral managerial decisions and supportive behavior for change by employees can put economic growth in jeopardy. In addition, the last paper offers an experimental study that aims to understand the gender-specific spread of unethicality. This work sheds light on a very important issue: enabling further research to try to find practical solutions for resisting unethicality and, therefore, avoiding the economic, social and ecological damages caused by its spread.

Regarding the methods applied, all the papers feature experimental work settled in the discipline of behavioral economics (Friedman & Cassar, 2004; Friedman & Sunder, 1994). Therefore, induced value theory (Smith, 1976) is applied in the experiments presented in this dissertation. Following Friedman and Sunder (1994, pp. 12–13), the "key idea of induced value theory is that proper use of a reward medium allows an experimenter to induce pre-specified characteristics, and the subjects' innate characteristics become largely irrelevant." Therefore, the experiments conducted for this dissertation are incentivized based on a structure that is determined by the experimenter and known by the participants. In it, the participants' decisions affect their payoff. This structure builds on monotonicity, salience, and dominance (Friedman & Sunder, 1994). Monotonicity implies that the subject prefers more rather than less of the reward medium (e.g., money); salience means that the subject's decisions actually affect the amount of reward medium she receives; dominance is achieved if a subject's utility from participating in the experiment depends primarily on the amount of reward medium she receives.[2]

[2]My co-authors of the respective papers and I recognize gender as a non-binary variable. In this dissertation, the feminine pronoun is used generically for reasons of simplicity only.

Behavioral experiments can feature a number of different games that measure behavior and can be conducted either in a laboratory (Falk & Heckman, 2009; Friedman & Cassar, 2004; Friedman & Sunder, 1994) or in the field (Banerjee & Duflo, 2017; Harrison & List, 2004). Additionally, experimenters can use online versions of experiments that are either more laboratory-like or more similar to field experiments (Arechar et al., 2018; Bakshy et al., 2014; Birnbaum, 2004; Lumsden & Damer, 2018). Moreover, there is the option to set up a laboratory in the field (Gneezy & Imas, 2017).

The first paper uses a framed, discrete, threshold public good game (Palfrey & Rosenthal, 1984) with two different roles for the participants. We argue that this modified game mirrors the key characteristics of an effort-dependent organizational change. It was conducted as an experiment in the laboratory. In contrast, the other three papers present online experiments. The second paper includes a framed multiple price list (Holt & Laury, 2002) modified to create the domain of uncertainty on top of risk (Ellsberg, 1961; Moore & Eckel, 2006; Ross et al., 2012); I use it to measure the effect of nudging on the level of aversive behavior under risk and uncertainty and investigate the moderating effect of the domain. The third paper uses the same framed multiple price list (Holt & Laury, 2002) to elicit an individual's level of risk aversion. Here, we test the effect of a nudge placed either before or after the beginning of the decision-making process for the risky situation at hand. In the fourth paper, two distinct honesty games are used. More precisely, the first game is the dot task (Gino et al., 2010), and the second game is a modified die-rolling task (Fischbacher & Föllmi-Heusi, 2013) that uses visualization similar to that of Kocher et al. (2018). We implement two distinct situations to investigate the effect of spreading. Various control measures and questionnaires are used to elicit control variables based on the topics and research questions of the respective papers. All papers use a variety of statistical tools for analyzing the experimental data, including non-parametric and parametric approaches for testing for treatment effects (Athey & Imbens, 2017; Moffatt, 2015).

My contributions to the above-mentioned papers are as follows. For the first paper, my two co-authors, Dr. Marvin M. Müller and Dr. Erich Renz, and I developed the research question and designed the experiment together. As neither of them had yet conducted an experiment, I led the project and computerized the experiment. We organized the laboratory sessions together, and I organized the payment. We contributed approximately equally to writing the first draft of the paper, while I was responsible for organizing and analyzing the data and bringing the results to the table. This dissertation includes an improved version compared with the one featured in the dissertations of my two co-authors due to improvements made based on referees' feedback. The improvements were done mostly by me in collaboration with my two co-authors. The second paper is my single-author paper. I am responsible for the whole project (research question; experimental approach; treatment manipulations; experimental procedure; data analysis; and writing). The second and the third papers share parts of the experimental design. It was developed in collaboration with Dr. Marvin M. Müller and Dr. Erich Renz. For the third paper, I contributed by realizing the online experiment, which included development, recruitment, and payment. Together, we developed the

treatment manipulation together and contributed approximately equally to writing the first draft of the paper. I contributed by organizing and analyzing the data. Again, the version featured in this dissertation is an improved variant when compared with the one presented in the dissertations of my two co-authors. The improvements were done by me based on feedback we received. The fourth paper is joint work with Prof. Dr. Sebastian J. Goerg and Prof. Dr. Lilia Zhurahovska. My co-authors contributed to developing the research question and guided my contribution in designing and developing the experiment. I contributed by recruiting subjects and organizing the payments for the online experiment. In addition, I created the first version of the data analysis, which was improved after extensive feedback provided by my two co-authors, and I contributed a first draft, which was partially enhanced by my two co-authors and finally brought into its current form by me.

The remainder of the dissertation is organized as follows: Chapter 2 includes the first paper, in which we investigate whether nudges (pro-change default and recommendation) are potentially a promising tool for promoting employee support for organizational change. Chapter 3 includes the second paper, in which I dive into risky and uncertain managerial decisions and whether a pro-neutrality default can be effective in promoting more neutral behavior. The third paper, presented in Chapter 4, follows up by testing a pro-neutrality recommendation placed either before or after the beginning of the decision-making process in a risky managerial situation. Finally, the fourth paper is included in Chapter 5. In it, we investigate how unethicality is spreading. More precisely, in our experiment, we test for the effects of merely thinking and factually learning about others' unethical behavior on one's own decision to lie.

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Chapter 2

Pay or Nudge Employees Into Change? – An Experimental Investigation of the Effect of Nudging for Organizational Change*

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Abstract

Organizational change often fails due to employee resistance resulting in unforeseen expenses, delays, or other disruptions in organizations. In our experiment, we compare behavioral interventions – a pro-change default rule and a pro-change recommendation – with a costly pay raise to foster supportive behavior. Our findings support the effectiveness of a default nudge in enhancing change-related success and indicate that a recommendation nudge has no significant effect in a change-related scenario. In addition, a pay raise has a positive effect likely triggered by positive reciprocity. If this pay raise fails to materialize, we report supporting evidence for negative reciprocity. We compare the effect sizes of these nudges and the pay raise to provide insights into the relative cost-efficiency of nudge interventions compared with pay raises for organizational change. We conclude with managerial implications.

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2.1 Introduction

Due to dynamic customer preferences and market environments, companies have to adapt their products or services and change their underlying processes and structures consistently (Moran & Brightman, 2000). This is often accompanied by increased cost pressure as a result of intensified competition and leads companies to restructure their departments, introduce new technologies, or restaff as part of their change strategies (Woodward & Hendry, 2004). Findings on the success and failure rates of change projects are controversially discussed in the research literature (Cândido & Santos, 2015; Gilley et al., 2009; Hughes, 2011). However, evidence from management practice indicates a pessimistic view of the relative success of change projects related to predefined time, budget, or quality goals (Jørgensen et al., 2014; Jørgensen et al., 2008). A crucial reason for failed organizational change is the employee resistance to change (Burnes, 2015; Heidenreich & Spieth, 2013; Pardo del Val & Fuentes, 2003). This dispositional resistance causes opposing thoughts, feelings, or behaviors toward organizational change and can thus lead to the delay in or failure of change projects (Erwin & Garman, 2010; Oreg, 2006; Piderit, 2000), even though both employers and employees usually benefit from the success of a change project (Oreg et al., 2011; Young, 2009). For organizational change to succeed, change projects must be supported by a critical mass of employees (Moran & Brightman, 2000). Therefore, while some supporters may still bring a small benefit, organizational change can only be successful if the critical threshold of support is achieved or exceeded (Nadler, 1981; Torchia et al., 2011).

With regard to change management and the need to achieve a critical threshold of support, recent research has focused on the effect of a pay raise in overcoming resistance to change (Krügel & Traub, 2018) and is built on research investigating an increase in salary as a deliberate choice made by employers to elicit positive reciprocity (Akerlof, 1982; Charness, 2004; Charness & Haruvy, 2002; Gneezy & List, 2006; Maximiano et al., 2007; Rigdon, 2002). However, in view of the increased cost pressure on companies, it seems implausible to assume that employers will motivate their staff with pay raises to encourage them to invest time and effort in supporting an organizational change. Besides financial benefits, other conceivable approaches to stimulate behavioral choices in favor of supporting change include training, negotiation, or even coercion (Dent & Goldberg, 1999; Heidenreich & Talke, 2020). All of these methods have in common that they are either not based on voluntary effort or can become costly due to financial expenditure or a high level of resource intensity. Although it seems necessary to investigate more cost-efficient options, previous studies have not focused on the effectiveness of cost-efficient behavioral approaches for inducing organizational change.

We argue that a cost-efficient option for implementing organizational change is to use nudges. Nudges are "any aspect of the choice architecture that alters people's behavior ... without forbidding any options or significantly changing their economic incentives" (Thaler & Sunstein, 2008, p. 6). While nudge research has its origins in the political and public health domain (Halpern & Sanders, 2016; Hummel & Maedche, 2019; Szaszi et al., 2018), more recent research into and

application of nudging have expanded to organizational settings (Ruggeri, 2019; Soman & Yeung, 2020). For example, in a field experiment at a Chinese workplace, Wu and Paluck (2021) show how decal nudges linked to cultural beliefs can be used to reduce floor waste in a factory. In another field experiment, Kalil et al. (2021) demonstrates that sending personalized text messages, including goal setting and feedback on behavior, to parents leads to increased attendance and reduced chronic absenteeism in preschool programs in the US. Feng et al. (2020) find that activating a heuristic to consider diverse groups leads to selecting more diverse job candidates. For nudges in general, the context in which a nudge is used greatly impacts its effectiveness (Hauser et al., 2018; van Kleef & van Trijp, 2018). Although the nudge literature is extensive and reached the research on organizations, previous research lacks, to the best of our knowledge, insights into the use of nudges in the specific context of organizational change.

Our study aims to answer the question of whether the positive effect of a pay raise can be reproduced by applying cost-efficient nudges to foster pro-change, supportive behaviors in employees. To achieve this, we simulate a situation of organizational change depending on a critical threshold of support by using an experimental setting that builds on a discrete, threshold public good game (Palfrey & Rosenthal, 1984). This type of game contains the most important characteristics of organizational change including, but not limited to, a threshold for success based on employee support, uncertainty about future payoffs, and the consequent low decision confidence of employees (Kotter, 1995; Kotter & Schlesinger, 1979; 2008). In this experimental setting, we investigate the effect of two distinct nudges: a default setting in favor of the organizational change from which subjects could opt-out if they prefer, and a recommendation for supportive behavior based on the average monetary outcomes for completely supportive behavior versus completely unsupportive behavior. Furthermore, we test the effectiveness of a pay raise based on the employer's decision. This design allows us to contribute to the existing literature in two ways: First, we investigate the effectiveness of two distinct nudges that are easy to implement in a real change management setting for fostering supportive behavior. We are confident that our research and the results provided in this paper will help practitioners when starting their endeavors in designing a pro-change choice architecture. Second, we can compare the nudges' effectiveness with a deliberate salary increase. With that, we strengthen the stream of research trying to investigate the cost-efficiency of nudging by enriching it towards nudging for organizational change.

Building on our experimental investigation, our findings show that a default rule produces an effect in favor of supporting organizational change. In contrast, our results indicate the lack of such an effect if a pro-change recommendation nudge is implemented. Additionally, a higher payment has a positive effect with regard to employees' willingness to support, while a relatively lower payment decreases this willingness accordingly. Our results indicate both positive and negative reciprocity by employees based on the employer's decision and her underlying intention. Our work contributes to the existing literature both with regard to pay raises and to using nudges to influence the behavior of employees in a change environment. To the best of our knowledge, we are the first to test nudges in an organizational change setting. Moreover, with our analysis, we are able to compare

the willingness of employees to support change due to a modification of the economic incentives and in situations in which no such modification takes place but nudging is applied. Finally, our work shows how much a nudge is allowed to cost relative to a pay raise.

2.2 Hypotheses

In this section, we develop three hypotheses that we test in our experiment. We first focus on the effect of a pro-change default rule (Hypothesis 2.1). Second, we follow with a hypothesis for the effect of a pro-change recommendation nudge (Hypothesis 2.2). Finally, we present two hypotheses covering the potential positive and negative effects of a deliberate choice by the employer (Hypothesis 2.3a and Hypothesis 2.3b).

2.2.1 Default Nudge

With regard to nudging, we first focus on default nudges. Previous research essentially points to three potential channels or drivers for the effect of defaults (Dinner et al., 2011; Jachimowicz et al., 2019; Johnson et al., 2012; McKenzie et al., 2006). First, the existence of a default minimizes the mental or physical effort of the individual, which is much to the liking of most individuals. Second, there is a kind of implied endorsement of the pre-selection by the choice architect allowing the decision-maker to guess what the choice architect's preferences are. Third, the default creates a reference point, the status quo, from which any deviation can create a sense of loss to which decision-makers are generally averse. When considering the effect of defaults reported in research on organizations, Venema et al. (2018) show that defaults help to create and sustain new behaviors in an organization's transition to new work practices. More specifically, the share of sedentary work is reduced when height-adjustable desks are given a default setting of standing height. Furthermore, Thaler and Benartzi (2004) find that automatic enrollment in a retirement savings program raises employee savings. We argue that a default in an organizational context is naturally more overt than, for example, an opt-out option on an enrollment in a saving plan. This is because default rules favoring support for organizational change are apparent. For example, being invited to a change-related training program by default is rather overt. It would be unrealistic to assume that change initiatives can be implemented unobtrusively with employees. Therefore, change must have some level of transparency because individuals are still making autonomous decisions to support or oppose organizational change. Although a subtle nudge can often be more effective than a more obvious nudge (Beshears & Kosowsky, 2020), various studies have shown that transparency in a default nudge does not diminish its effectiveness (Bruns et al., 2018; Loewenstein et al., 2015; Steffel et al., 2016). As our view of organizational change implies a version of the public good game, it is interesting to consider what Fosgaard and Piovesan (2015) find in their experimental work. They show that a mental state manipulated toward cooperation (the default state) can be used to nudge people's decision-making so that they become more cooperative in a standard public good game. Building on the findings of research on defaults, we involve individuals by default in a

pro-change decision to achieve the critical threshold, as we assume that the default serves as a point of reference during an organizational change with an uncertain outcome. When this is implemented in an upcoming organizational change, individuals can (still) decide not to support change at any time and at their own discretion without additional costs or disadvantages. The assumed effect is covered in Hypothesis 2.1:

Hypothesis 2.1. *Supportive behavior for change is higher when a pro-change default is in place than when it is not implemented.*

2.2.2 Recommendation Nudge

Recommendations help to reduce cognitive effort and uncertainty in decision-making, thereby decreasing the difficulty of making a choice (Fitzsimons & Lehmann, 2004; Smith et al., 2005). A recommendation can be classified as a descriptive and/or evaluative label that has proven to be a nudge with effective guidance (Cadario & Chandon, 2020). Descriptive labels provide additional information that would otherwise have to be self-obtained. Evaluative labels help to interpret information by means of additional symbols or notes. Comparable literature has, for instance, shown positive effects for highlighting positive product features (Newell & Siikamäki, 2014), emphasizing losses against gains in enrollment programs (Keller et al., 2011), and ensuring information disclosure when lending money (Bertrand & Morse, 2011). Considering the characteristics of organizational change and how it can be illustrated by a public good situation, we refer to Barron and Nurminen (2020), who demonstrate in an experimental setting how evaluative labeling can be used to promote cooperation in a public good game. In doing so, they stimulate participants to make higher contributions by designating contributions above a certain threshold as "good". Additionally, the literature suggests that a recommendation that can be trusted by the decision-maker increases the effectiveness of the intervention (Jachimowicz et al., 2019). Therefore, we hypothesize that in organizational change, where the potential consequences of the outcome are communicated, the positive impact of a pro-change recommendation will work in favor of the change.

Hypothesis 2.2. *Supportive behavior for change is higher when a pro-change recommendation is in place than when it is not implemented.*

2.2.3 Deliberate Wage Choice

Our study aims to answer whether the potential effect of a pay raise can be reproduced by using nudges to increase the chance of observing supportive behaviors in employees. To do so, in addition to the investigation of the potential effectiveness of a pro-change default rule and a pro-change recommendation, we look at the effect on employee behavior when we give the employer the choice of a potential pay raise. We assume that when the employer makes a deliberate choice to pay a higher or a lower compensation for the work at hand, a higher wage fosters more supportive behavior as a result of eliciting positive reciprocity (Akerlof, 1982; Charness, 2004; Gneezy & List, 2006). However, there might also be a drawback to such a choice. Ockenfels et al. (2015)

point out that falling behind a reference point that is perceived as fair payment is likely to reduce performance. This finding is in line with experimental results in regard to reference points and potential negative reciprocity based on these points (Brandts & Solà, 2001). Engelmann and Ortmann (2009) and Pereira et al. (2006) find corresponding results in modified gift-exchange games, showing that negative reciprocity is not only easy to trigger but also a relevant force in such interactions. We consider this effect to be based on the underlying intention rather than the payoff consequences following Gächter and Thöni (2010) and their experimental work regarding the fair-wage hypothesis. Therefore, given this deliberate choice by the employer, a lower wage may also elicit negative reciprocity when compared with a situation without a choice, and, hence, a low wage is the only option. The employees' response may depend on the wage provided by the employer, not in terms of its actual value but in comparison to the alternative option. Hypothesis 2.3a and Hypothesis 2.3b capture these points and aim to provide new insights into how a deliberate wage choice affects the willingness of employees to support an organizational change.

Hypothesis 2.3a. *Supportive behavior for change is higher when an employer decides to offer a higher wage than when no deliberate choice is possible.*

Hypothesis 2.3b. *Supportive behavior for change is lower when an employer decides to offer a lower wage than when no deliberate choice is possible.*

2.3 Experimental Design and Procedure

To test our hypotheses of a pro-change default, a pro-change recommendation, and a deliberate wage choice and their effect on organizational change/supportive behavior in organizations, we utilize a change-framed, modified, discrete, threshold public good game.[3] To model an organizational context, we follow Maximiano et al. (2007) by introducing groups of six participants modeling organizational units as multi-worker companies with an employer and five employees each. With our experimental setup, we implement the situation of an organizational change with the following relevant characteristics. First, an organizational change needs a critical threshold of supporting employees to be successful. Second, supporting the organizational change is costly for the employees since it causes additional physical or mental effort. Third, the employees benefit from a successful organizational change either by directly receiving a bonus payment or through long-term job security. Fourth, while organizational changes are welfare-enhancing for employees on average, this is not the case for all employees because it increases the risk of some being laid off. Therefore, organizational change induces a feeling of insecurity in many employees (Oreg et al., 2011; Schweiger & Denisi, 1991).[4]

[3] For the basic model, see Palfrey and Rosenthal (1984), for a continuous threshold public good game, see Andreoni (1998), Chamberlin (1974), and McGuire (1974). In the light of the literature on social dilemmas, our game could also be described as an n-player chicken game or as a fixed stag hunt game (Pacheco et al., 2009; Taylor & Ward, 1982; Ward, 1990).

[4] Note that the first three characteristics are features of the original discrete, threshold public good game while the fourth results from our modification.

The technical procedure of our experiment is as follows. In each group, one of the six participants is randomly chosen to be the employer and five are employees. The roles (employer/employee) remain the same over the course of 20 repetitions. Between periods, group compositions are randomly rearranged using a random re-matching protocol. Participants are informed about their payoff and the decisions of other participants in their group after each period. Before the experiment starts, all participants are informed about the group size, their individual role, the payoffs of both roles, the matching protocol, and the feedback procedure. The aforementioned organizational change is based on the supportive behavior of the employees. In addition to the labor market framing, we use a frame in which we describe change in general, organizational change, and support for change to enhance participants' immersion in the context of organizational change. We test four distinct institutions, which are explained in the following subsections: Baseline, Default, Recommendation, and Choice.

$$\pi_{\text{employee}}(\text{support}) = \begin{cases} 60 - 20 + 50 = 90, & \text{if successful organizational change and not laid off} \\ 30 - 20 \quad\;\; = 10, & \text{if successful organizational change and laid off} \\ 60 - 20 \quad\;\; = 40, & \text{if unsuccessful organizational change} \end{cases} \tag{2.1}$$

$$\pi_{\text{employee}}(\text{no support}) = \begin{cases} 60 \quad\;\; + 50 = 110, & \text{if successful organizational change and not laid off} \\ 30 \quad\;\; = 30, & \text{if successful organizational change and laid off} \\ 60 \quad\;\; = 60, & \text{if unsuccessful organizational change} \end{cases} \tag{2.2}$$

2.3.1 General Design

We start by describing the Baseline institution. Each of the 20 periods starts with the employer accepting the fixed wage of 60 points first. Second, the employees are informed about the wage. Third, each employee anonymously and simultaneously selects whether to support the organizational change or not. Supportive behavior results in costs of 20 points due to additional work effort that is needed to adjust to or implement whatever is needed for the organizational change to be successful. Fourth, the organizational change takes place and is decided on by a simple majority vote, that is, at least 3 out of 5 employees have to choose to support the organizational change. Fifth, if the organizational change was successful, one of the five employees is laid off randomly. The laid-off employee receives half of the wage reduced by the decision-depending effort.[5] The other employees receive their full wages minus the decision-depending effort plus an additional bonus payment of 50 points. This bonus payment is utilized to include an efficiency enhancement of the organizational

[5] We provide more insights on this in Appendix 2.A.

change and allow employees to benefit from it. This financial benefit might also be interpreted as long-term job security.[6] If the organizational change is not successful, no employees are laid off, and all employees receive their full wage minus the decision-depending effort. Equation 2.1 and Equation 2.2 give an overview of the payoffs for an employee based on the support decision and the outcome of the organizational change.[7]

The employer's payoff depends on the success of the organizational change and the number of supporting employees. First, the employer directly profits from supportive behavior by receiving 10 points for each supportive employee. Second, the employer receives an additional revenue of 107.5 points for a successful organizational change and 35 points for an unsuccessful one.[8] The following formula describes the employer's payoff based on the employees' support decisions in the Baseline institution:[9]

$$\pi_{employer}(\#\text{ supporters}) = \begin{cases} 157.5, & \text{if 5 employees support} \\ 147.5, & \text{if 4 employees support} \\ 137.5, & \text{if 3 employees support} \\ 55, & \text{if 2 employees support} \\ 45, & \text{if 1 employee supports} \\ 35, & \text{if 0 employees support} \end{cases} \tag{2.3}$$

In Equation 2.3, the first three cases (with 3 to 5 employees showing supportive behavior) represent a successful organizational change, while the last three cases (with 0 to 2 employees showing supportive behavior) represent an unsuccessful organizational change.

All relevant parameters are common knowledge for the participants. After each period, participants are informed about the realization of the organizational change, the potential layoff, and their individual points. Five of 20 periods are randomly chosen to be payoff-relevant for each participant; this is also known to all participants.[10] We use a conversion rate of 30 points to €1.00 (approximately US$1.10 at the time of the data collection).

[6] Note that by setting the parameters to these values, the expected value of a successful organizational change is higher than that of an unsuccessful organizational change.

[7] In the experimental instructions, participants receive a table featuring the payoffs based on the employees' decisions and whether the organizational change was successful or unsuccessful. Appendix 2.B includes an English translation of the instructions and Appendix 2.C includes the original screens of the main experiment, including said table.

[8] For more details on the employer's revenues and their link to the employees' payoffs, see Appendix 2.A.

[9] In the experimental instructions, participants receive a decision tree with one branch for the fixed wage and an additional two branches for a successful and an unsuccessful organizational change; two tables present the payoffs based on the number of employees supporting. Appendix 2.B includes an English translation of the instructions, and Appendix 2.C includes the original screens of the main experiment, including said decision tree.

[10] This random incentive system uses within-subject randomization (for a discussion, see Baltussen et al., 2012). The smaller the number of paid periods, the more a single period tends to be unimportant. The larger the number of paid periods, the more a participant can estimate the payoff of past periods. Both can lead to biases in participants' behaviors. There is no optimal response to the question of how many periods should be paid in the literature. We decided to pay five.

2.3.2 Design of the Default and Recommendation Nudge

In the DEFAULT institution, we investigate the effect of a pro-change default rule. It is implemented by using a pre-selection of supportive behavior on the employee's decision screen (see Appendix 2.C for screens of the main experiment). In addition, participants are informed about this pre-selection in the instructions.

In the RECOMMENDATION institution, we investigate the effect of a pro-change recommendation nudge. We descriptively inform subjects about the payoff when all employees support the change compared to the payoff when all employees decide not to support the change. In addition to this descriptive label, we also add the evaluative label "recommended" (translated quote from the participants' decision screen provided in Appendix 2.C) for supportive behavior on the decision screen. In both institutions involving a nudge, all other features are exactly the same as in the BASELINE institution.

$$\pi_{employer}(\textit{high wage}, \#\text{ supporters}) = \begin{cases} 135, & \text{if 5 employees support} \\ 125, & \text{if 4 employees support} \\ 115, & \text{if 3 employees support} \\ 30, & \text{if 2 employees support} \\ 20, & \text{if 1 employee supports} \\ 10, & \text{if 0 employees support} \end{cases} \tag{2.4}$$

2.3.3 Design of the Deliberate Choice

In the CHOICE institution, the employer makes a deliberate choice between two wage levels. The employer starts the round by choosing a lower or higher wage of 60 points or 80 points, respectively. The low wage in the CHOICE institution is equal to the fixed wage level in the BASELINE institution. Therefore, a high wage increases the payoff for all employees by 20 points if the organizational change is not successful or it increases the payoff of the laid-off employee by 10 points and for the remaining employees by 20 points if the organizational change is successful. In contrast, if the employer chooses the low wage, there is no difference in regard with the employee's payoffs compared to the BASELINE institution. For the employer, the high wage comes with increased cost. This is modeled by a reduced revenue of 85 points for a successful organizational change and 10 points for an unsuccessful one.[11] Therefore, if the employer chooses the high wage, the payoff for the same number of supportive employees is decreased when compared with what is formalized in Equation 2.3. Equation 2.4 describes the employer's payoff in the case of the high wage being chosen, based on the employees' support decisions in the CHOICE institution. If the employer

[11] For an explanation of the laid-off employee's payoff and more details on the employer's revenues and their link to the employees' payoffs, see Appendix 2.A.

chooses the low wage, her payoff based on the employees' support decisions is the same as in the BASELINE institution (see Equation 2.3).[12]

To sum up, in our experiment, four distinct institutions are included. In BASELINE, there is a change-framed, modified, discrete, threshold public good game. For DEFAULT and RECOMMENDATION, a pro-change default rule and recommendation nudge are added, respectively. In CHOICE, a deliberate wage choice by the employer is added. Table 2.1 presents the institutions and the key feature of each one.

Institution	**Key feature**
BASELINE	——
DEFAULT	Pre-selection of supportive behavior
RECOMMENDATION	Recommendation for showing supportive behavior
CHOICE	Deliberate choice by employer between two wage levels (low and high)

Table 2.1: Institutions and Key Features

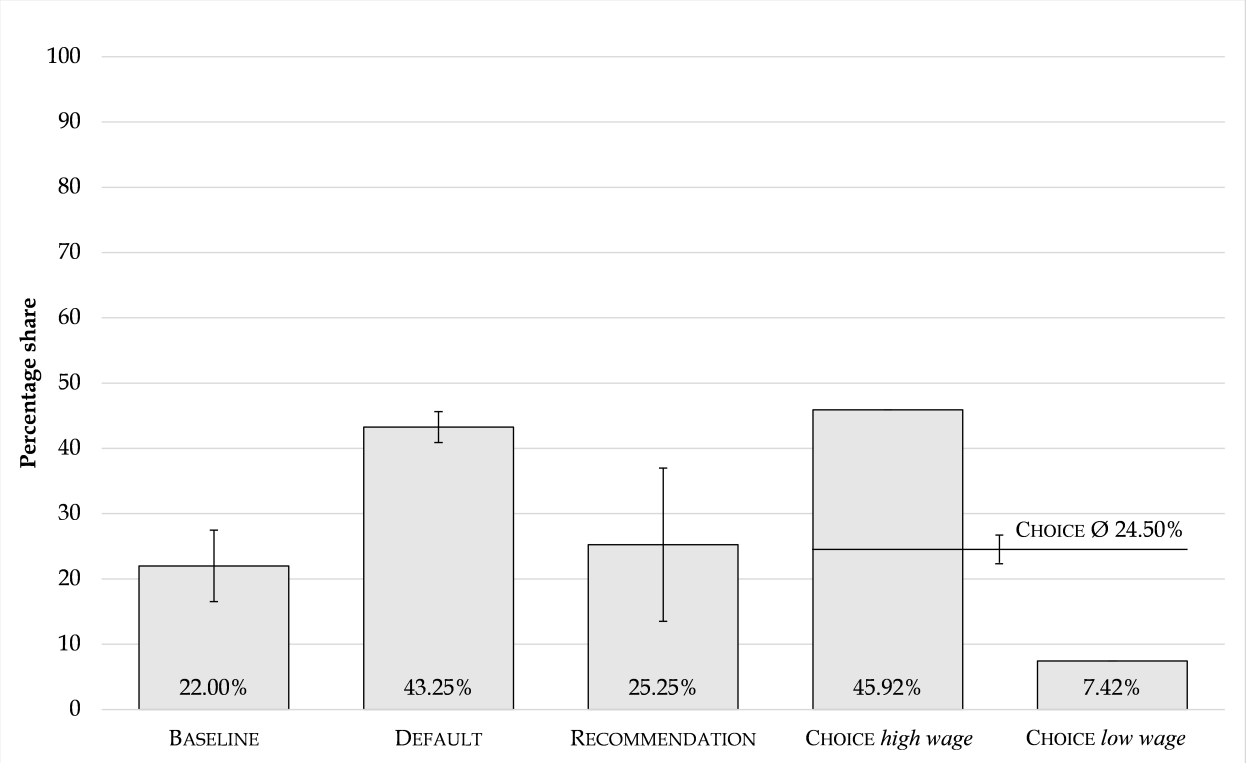
2.3.4 Additional Controls

In addition to the main experiment described above, we also use pre-experimental and post-experimental control measures. We use the equality equivalence test (Kerschbamer, 2015) in order to elicit the subjects' inequality preferences prior to the main experiment. After the main experiment, we elicit the subjects' risk attitudes by using the multiple price list compiled by Holt and Laury (2002) in its simplified version by Balafoutas et al. (2012). Both of these measures are payoff-relevant. Participants are informed about the outcomes at the end of the session. We also use the following non-incentivized questionnaires as additional controls: the dispositional resistance to change scale (Oreg, 2003; Oreg et al., 2008), the positive and negative reciprocity scale (Dohmen et al., 2008; 2009), the general risk aversion scale (Dohmen et al., 2011; Richter et al., 2017), and a set of sociodemographic questions. Since we do not derive hypotheses on correlations between these controls and the main behavioral measure, we add them to our parametric analysis as additional explanatory variables to test the robustness of the results. While we can show that the inclusion of these controls does not change the results significantly, we do not discuss the coefficients and p-values associated with these control variables.

[12] In the experimental instructions, participants receive a decision tree with two branches for the two wage levels and an additional two branches for a successful and unsuccessful organizational change, two tables present the payoffs based on the number of employees supporting. Appendix 2.B includes an English translation of the instructions, and Appendix 2.C includes the original screens of the main experiment, including said decision tree.

2.3.5 Participants and Data Collection

The experiment was conducted at a laboratory in Germany. Eight sessions with 192 participants took place in December 2019 and January 2020. All participants were undergraduate or graduate students with various majors (approximately 40% economics, 11% humanities, 10% sciences, 39% other). The average participant was 26.40 years old ($SD = 5.04$) and 55.21% reported identifying as female (Binominal probability test: $p = 0.170$). ORSEE (Greiner, 2015) was used for recruitment. Participants earned an average payoff of €13.44 (approximately US$14.78 at the time of the data collection). The minimum payoff was €7.70, and the maximum was €25.00. The sessions lasted about 75 to 90 minutes. The experiment was computerized using z-Tree (Fischbacher, 2007).



Note: Error bars represent the standard error based on the four matching groups of each institution (BASELINE, DEFAULT, RECOMMENDATION, and CHOICE).

Figure 2.1: Employees' Support Decisions over Institutions and Employer's Decisions

2.4 Results

We test four distinct institutions with regard to the employees' willingness to support an organizational change: BASELINE, DEFAULT, RECOMMENDATION, and CHOICE. BASELINE has no additional features other than the support-dependent organizational change. We test two different types of nudges: a pro-change DEFAULT rule and a pro-change RECOMMENDATION. In CHOICE, we are testing for the potential effect of a pay raise and, in addition, for the potential negative effect of the absence of this pay raise. We find a varying rate of support between these institutions.

Figure 2.1 shows the rate of costly supportive behavior among employees. In the BASELINE institution, 22.00% of employees' decisions are in favor of supporting the organizational change, implying a costly investment in support.[13,14]

2.4.1 Impact of the Default Rule

In this subsection, we investigate the effectiveness of a pro-change DEFAULT rule. In the institution in which this nudge is in place, the support rate rises to 43.25% and is higher than in any other institution. This is statistically significantly different from BASELINE (exact Mann-Whitney test: $p = 0.029$). In addition, we find a significant difference between DEFAULT and BASELINE in our parametric analysis (Table 2.2 Model 4: $p = 0.001$). Taking the non-parametric and parametric results for the pro-change DEFAULT together, we find evidence that a such a rule is indeed effective in fostering supportive behavior. Therefore, Hypothesis 2.1 is supported. This result extends previous findings on effective default rules in a standard public good game (Fosgaard & Piovesan, 2015) and in other domains (for vaccination, see Chapman et al., 2010; for financial products, see Brune et al., 2017; for consumer choice, see Steffel et al., 2016). Moreover, our result renders previous findings more credible in terms of their validity for a change management situation.

Result 2.1. *A pro-change default fosters support for organizational change.*

2.4.2 Impact of the Recommendation

We now take a look at the pro-change RECOMMENDATION. In the institution in which this nudge is in place, the support rate is 25.25%. We find no evidence of the support rate in RECOMMENDATION differing from BASELINE in our non-parametric analysis (exact Mann-Whitney test: $p = 0.886$) or in any model of our parametric analysis (Model 4: $p = 0.879$). Although we find no significant difference between RECOMMENDATION and DEFAULT in our non-parametric analysis ($p = 0.343$), we do find a statistically significant difference using the parametric analysis (Model 4 Wald test: $p = 0.002$). Overall, we find no support for the effectiveness of this nudge for change-management-related applications. Therefore, Hypothesis 2.2 is not supported. This result is in contrast with a

[13]For our non-parametric analysis, we use the average support rate of a matching group (12 participants with random re-matching in groups of six and fixed roles) over all 20 periods. This leads to a total number of observations for this analysis of 16. Therefore, we use the exact Mann-Whitney test (Harris & Hardin, 2013). All of our results are also supported when using the standard Mann-Whitney test.

[14]For our parametric analysis, we use the employee's individual decision to support or not support the organizational change in each period. Therefore, the total number of observations for this analysis is $3,200$. Table 2.2 shows the results using random effects logistic regressions. Four models are used. In Model 1 we test for the effects of the nudges (DEFAULT and RECOMMENDATION) and the employers' decision (CHOICE *high wage* and *low wage*). In Model 2, we control for the period of the experiment. In Model 3, we additionally control for two lagged variables. Finally, in Model 4, a number of additional controls are added. We find that supportive behavior is reduced in later periods. In addition, we can report evidence that being laid off has no effect while being part of a group with a successful organizational change increases supportive behavior significantly in the subsequent round. For the results in Table 2.2, we use the interaction between the institution CHOICE and its wage level to test for CHOICE *low wage* and CHOICE *high wage* separately. Appendix 2.D presents the results without this interaction. To check for further robustness, we also apply mixed effects logistic regression to all parametric estimations presented. All findings presented here are supported. We present a summarizing table with all hypotheses and matching results in Appendix 2.E.

recent finding by Barron and Nurminen (2020), who show the effectiveness of a recommendation in the shape of an evaluative label for promoting cooperation in a public good game. As for the discussion by Hauser et al. (2018) and van Kleef and van Trijp (2018) on the failure and success of nudging interventions, this can be explained by moderating effects of the underlying context in which a nudge is placed in. Therefore, it seems plausible that a recommendation nudge is not unfolding in a change management scenario. Our result is valid for a specific setting that includes, but is not limited to, features of a public good game.

Result 2.2. *A pro-change recommendation fosters no support for organizational change.*

	Model 1	Model 2	Model 3	Model 4
DEFAULT	1.597^{***}	1.668^{***}	1.652^{***}	1.600^{**}
	(0.450)	(0.470)	(0.476)	(0.501)
RECOMMENDATION	0.234	0.246	0.142	0.085
	(0.482)	(0.501)	(0.513)	(0.560)
CHOICE *high wage*	1.677^{***}	1.847^{***}	1.882^{***}	1.722^{***}
	(0.441)	(0.461)	(0.469)	(0.503)
CHOICE *low wage*	-1.310^{**}	-1.427^{**}	-1.460^{**}	-1.580^{**}
	(0.437)	(0.454)	(0.485)	(0.567)
Period		-0.086^{***}	-0.079^{***}	-0.084^{***}
		(0.012)	(0.013)	(0.013)
Success in preceding period			0.340^{**}	0.421^{**}
			(0.173)	(0.187)
Laid off in preceding period			0.177	0.248
			(0.278)	(0.310)
Constant	-1.891^{**}	-1.070^{**}	-1.252^{**}	-3.454
	(0.322)	(0.356)	(0.361)	(2.564)
Additional controls	NO	NO	NO	YES
Wald-χ^2	59.3	93.59	101.43	114.88
$p(\chi^2)$	< 0.001	< 0.001	< 0.001	< 0.001
Number of observations	3,200	3,200	3,040	2,641
Number of groups	160	160	160	139

Note: In all models, the dependent variable is employees' supportive behavior (*organizational change supported = 1/not supported = 0*) and BASELINE is the reference group. Results of random effects logistic regression with standard error clustering at the individual level are reported. Standard errors are in parentheses. Model 3 includes a reduced number of observations due to lagged variables ("Success in preceding period" and "Laid off in preceding period"). Model 4 features additional controls in the form of an equality equivalence measure (Kerschbamer, 2015), risk aversion (measured with a multiple price list by Balafoutas et al., 2012), dispositional resistance to change (Oreg, 2003), positive and negative reciprocity (Dohmen et al., 2009), general risk aversion (Dohmen et al., 2011), and a set of sociodemographic attributes. This is why, Model 4 includes a reduced number of observations due to the exclusion of 21 participants for either showing inconsistent behavior in the inequality measure, showing inconsistent behavior in the risk measure, or reporting unreasonable values (age below 18, number of semesters above 36) in our sociodemographic controls. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 2.2: Results of Random Effects Logistic Regressions

2.4.3 Impact of the Deliberate Wage Choice

In the CHOICE institution, the employer has the choice of providing a higher or lower wage. While the higher wage is greater than the wage provided in the other institutions without an employer's decision, the lower wage is the same wage level that was provided as standard in the other institutions, (BASELINE, DEFAULT, and RECOMMENDATION). The support rate in the CHOICE institution (24.50%) does not differ significantly from that in BASELINE (exact Mann-Whitney test: $p = 0.886$). We find that with a *high wage*, supportive behavior increases significantly (Model 4: $p = 0.001$) compared to BASELINE (45.92% from 22.00%). We attribute this effect to positive reciprocity: Employees are willing to engage in costly effort as a response to the kind decision of the employer.[15] In a similar fashion, if the employer decides against implementing the higher wage and instead in favor of the lower one, supportive behavior is significantly (Model 4: $p = 0.005$) reduced compared to BASELINE (7.42% from 22.00%). We attribute this effect to negative reciprocity: Employees refrain from supporting in response to the perceived unkind action by the employer. This result is in line with the fair-wage hypothesis and the adaptation of reference points (Brandts & Solà, 2001; Gächter & Thöni, 2010; Ockenfels et al., 2015). Using additional Wald tests, we find that CHOICE *low wage* is significantly worse at stimulating support than any other institution (Model 4 Wald tests compared to DEFAULT: $p < 0.001$; RECOMMENDATION: $p = 0.002$; and CHOICE *high wage*: $p < 0.001$). Therefore, Hypothesis 2.3a and Hypothesis 2.3b are supported for positive and negative reciprocity based on a higher and lower wage, respectively. These findings support previous results by Engelmann and Ortmann (2009) and Pereira et al. (2006) regarding positive and negative reciprocity in gift-exchange games.

Result 2.3a. *The realization of a pay raise stimulates support for organizational change.*

Result 2.3b. *If a potential pay raise fails to materialize, support for organizational change is reduced.*

2.4.4 Comparison of the Two Nudges

We also look at the comparison between the two institutions featuring a nudge: DEFAULT and RECOMMENDATION. As stated above, while the two nudge institutions do not differ significantly, using non-parametric analysis (exact Mann-Whitney test: $p = 0.343$), we find support for a difference in our parametric analysis (Model 4 Wald test: $p = 0.002$). In addition, we find no significant

[15] Technically speaking, there are two differences between CHOICE *high wage* and BASELINE. First, there is a deliberate wage decision by the employer in CHOICE, resulting in a wage that is based on this decision. Second, in CHOICE *high wage*, there is a higher wage than in BASELINE. We conclude that the difference between CHOICE *high wage* and BASELINE is based on positive reciprocity triggered by the employer's decision that is perceived by the employees as indicating kind intentions. In contrast, there is also the potential explanation that a higher wage might have an effect due to the higher amount itself instead of being a result of the employer's decision. However, Blount (1995) finds that participants adapt their response to the active decision of another participant rather than the numerical value associated with a specific outcome. This is in line with previous research reports on the effect of a pay raise in a reform task (Krügel & Traub, 2018). The higher wage might have caused concerns in risk-averse participants, as it is associated with a higher risk because a successful organizational change includes the possibility of being laid off. Therefore, if anything, our results for CHOICE *high wage* might be underestimated.

difference between DEFAULT and CHOICE *high wage* ($p = 0.770$), but we find a difference between RECOMMENDATION and the *high wage* case in CHOICE ($p < 0.001$). We find some evidence that a pro-change DEFAULT rule is more effective than a pro-change RECOMMENDATION in fostering supportive behavior. We interpret this finding in two ways. First, previous work indicates that a default nudge is often highly effective (Jachimowicz et al., 2019; Sunstein, 2014); therefore, a default rule implementing a pre-selection favoring the change promises to be more effective in fostering supportive behavior than a pro-change recommendation labeling support as recommended. Second, a recent model by Löfgren and Nordblom (2020) features the distinction between pure and preference nudges. In terms of this theory, a default is considered to be a pure nudge, while a recommendation is a preference nudge. Their model predicts that decision confidence is expected to moderate the effectiveness of these two types of nudges. With low decision confidence, pure nudges, such as default rules, are expected to influence the decision-maker more strongly than preference nudges, such as recommendations. Therefore, the greater effect of the pro-change DEFAULT rule is also in line with the theory by Löfgren and Nordblom (2020) given that a change management setting entails low decision confidence for employees (Gist & Mitchell, 1992; Maurer, 2001; Schweiger & Denisi, 1991). Therefore, as an additional finding, we infer that a pro-change default rule may be better suited than a pro-change recommendation for change management applications.

2.4.5 Cost Analysis of Nudging

Benartzi et al. (2017) report high cost-efficiency of nudging. To focus on the implications for the potential practical implementation of the two nudges in comparison to a pay raise, we investigate their efficiency in our experimental setup. The pay raise in the CHOICE institution is the only situation in which the employer faces additional cost when compared with the BASELINE institution. For a pay raise, the employer has to pay a quarter of the employees' wages.[16] To make the necessary adjustments, we can recalculate the average payoff of the employer in CHOICE *high wage*, assuming no additional cost, as it is implemented for the other three institutions (BASELINE, DEFAULT, and RECOMMENDATION). The resulting average payoffs per round can be found in Table 2.3.

Depending on the cost situation in the BASELINE institution, an employer invests on average 23.91 points in additional wage cost for an additional income of 36.36 points in CHOICE *high wage*. This yields a net profit of 12.45 points. Based on our results, the employer is better off using a DEFAULT nudge, given it has zero cost as it is the case in our experimental setup. The employer's average net profit from this intervention is 28.75 points. To answer the question of how much a default nudge is allowed to cost to still be an efficient option compared to a pay raise, we use the following approach. First, to make both institutions (CHOICE *high wage* and DEFAULT) comparable – there is still a difference between paid wage and received wage – we assume that the allowed cost of DEFAULT are paid by the employer similarly to the cost of the pay raise. Second, within this process, we correct for the share of successful organizational changes. We conclude that a DEFAULT nudge can cost up to 67.55% of the cost of a pay raise and still be as efficient as the pay raise.

[16] For more details and the underlying concept, see Appendix 2.A.

The same procedure can be used to calculate the potential cost allowance for a Recommendation nudge. However, as we find no evidence of its effectiveness, and the average payoff for the employer is lower in the Recommendation institution than in the Choice *high wage* case, we do not report this value here.[17] Overall, this analysis supports the high cost-efficiency of default rules, which is in line with the results by Benartzi et al. (2017) for nudging in general.

Institution	**Average payoff of employer**	**Average payoff w/o wage cost**	**Share of success**	**Relative cost allowance (to pay raise)**
Baseline	53.25 points (29.80 points)	n/a	10.00%	
Choice *low wage*	39.52 points (12.29 points)	n/a	1.12%	
Choice *high wage*	65.70 points (46.91 points)	89.61 points (52.73 points)	43.66%	100%
Default	82.00 points (43.24 points)	n/a	35.00%	67.55%
Recommendation	62.58 points (41.24 points)	n/a	20.63%	n/a

Note: Standard deviations are in parentheses.

Table 2.3: Average Payoff for the Employer and Relative Cost Allowance of the Nudges

2.5 Conclusions

Our results indicate that nudging can be a tool with an economically relevant and significant effect in change management scenarios. Thus, we contribute to the nudging and change management literature, as previous research on organizational change mostly lacks a focus on nudging and the experimental testing of its effects, while previous research on nudges lacks a specific focus on organizational change. As discussed above, organizational change has a fairly specific set of characteristics. It is important to investigate nudges in a contextual manner due to the moderating effects introduced by the environment in which they are used (Hauser et al., 2018; van Kleef & van Trijp, 2018). With regard to change management settings, we find strong supporting evidence for the effectiveness of implementing a pro-change default rule. In practical situations, such a

[17] To extend this discussion, the calculated cost allowance of the recommendation nudge is −12.77%. This supports our results so far that the default is more efficient than the recommendation. However, as stated in the main text, this analysis is for academic purposes only. A practical implication is not to be derived from this negative value or the difference in the cost allowance between these two nudges.

default rule can be implemented by automatically registering employees for a kick-off meeting for an organizational change, setting up accounts for employees in new software systems, or changing the default software system used by employees. Ebert and Freibichler (2017), for instance, discuss nudging as an integral part of management practice to increase knowledge worker productivity. They illustrate how default nudges can lead to more focused work time and condensed meeting time. Furthermore, our results support the findings of Benartzi et al. (2017) regarding the cost-efficiency of nudging compared to traditional interventions. In our experiment, a default would have been allowed to cost approximately 70% of what a pay raise costs. The pro-change recommendation nudge turns out to be ineffective in fostering supportive behavior, implying that this nudge is not a valid option for organizational change.

Our experimental results also provide support for ensuring that a pay raise works as intended by triggering positive reciprocity and fostering supportive behavior in situations involving change. This is in line with what Krügel and Traub (2018) report for their findings regarding a deliberate pay raise in a reform task; that task, however, lacks a specific change management context. In addition, and in contrast with Krügel and Traub (2018), our analysis of employees' reactions to a deliberate choice by their employer also shows that negative reciprocity is triggered when a pay raise fails to materialize. This is in line with Gächter and Thöni (2010), Ockenfels et al. (2015), and Pereira et al. (2006). Therefore, if a pay raise fails to materialize, the negative effect might be much more severe for organizational change than previously thought. For example, if a public crisis occurs and a potential bonus payment for specific jobs is discussed, the subsequent lack of materialization might trigger an unavoidable failure of any organizational change based on the updated reference point. It remains an open question whether additional actions could counteract such a lack of materialization.

Change is a group activity and today's work environment is driven by group work, interpersonal relationships, and creative processes. Our experiment partially features characteristics of a group activity but we did not focus on personal relationships between a manager and her staff or between staff members. Such dynamics might be relevant for the success rate of organizational change (Amiot et al., 2006; Coch & French Jr., 1948; Lewin, 1948; Pearce & Sims, 2002; Schoenberg et al., 2016). Further research can build on our experimental work to enhance the understanding of intragroup dynamics as a potential moderator of nudging in organizational change settings.

Overall, our paper is the first to examine the effectiveness of two distinct nudges, a pro-change default rule and a pro-change recommendation, in an organizational change situation and compare their effectiveness to the effects of a deliberate pay raise decision by the employer. The pro-change default rule turns out to be almost as effective as a pay raise, while the pro-change recommendation has no significant effect. This difference between the default nudge and the recommendation nudge is in line with recent work by Löfgren and Nordblom (2020) that considers low decision confidence, which seems to be a straightforward assumption for employees participating in and experiencing organizational change (Oreg & Goldenberg, 2015). Our results provide a starting point for additional work testing the theory by Löfgren and Nordblom (2020) and their postulation of the moderating

effect of decision confidence with regard to pure nudges, such as defaults, and preference nudges, such as recommendations.

In practical terms, managers tasked with delivering organizational change would best serve themselves by applying a pro-change default rule if pay raises are not feasible due to increased cost pressures or if their materialization is uncertain.

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Appendix 2.A Chronology of an Organizational Change

This appendix serves to explain the underlying concept of our design choices. These details were not communicated to the participants and are not essential for making an informed decision.

We divide an organizational change into two essential phases. In the first phase, the organization engages in some sort of effort to implement the change at hand. In the second phase, the new situation is achieved (or not) and this new (or old) situation and its consequences are realized. We refer to the first phase as the *implementation phase* and to the second as the *realization phase*. Since their relative lengths vary depending on the company and the specific organizational change, we set them equal to each other. This has the following consequences for the payoffs.

All employees receive half of their wage (30 or 40 points) for the implementation phase. If the organizational change is successful, the four employees who are not laid off receive the second half of their wage (30 or 40 points) in the realization phase while the laid-off employee does not receive any further wage (0 points). If the organizational change is unsuccessful, all employees receive two times half of their wage, that is, their full wage (60 or 80 points). Therefore, all employees always receive the full wage except for the laid-off employee, who receives only half of it. Costly supportive behavior is only relevant for making the organizational change successful and is, therefore, only "paid" in the implementation phase (20 points). Together with the bonus payment (50 points), this results in the payoff functions, as stated in Equation 2.1 and Equation 2.2.

Furthermore, the payoff of the employer is linked to the employees' payoffs. The employer receives a basic revenue (110 points) and then receives additional income (10 points) for every supportive employee. To pay the wages of the employees, the company takes a quarter of the wages from the employer's revenue. This transformation of employees' wage to employers' costs is necessary to ensure approximately equal payoff opportunities for employers and employees in our experiment. If the organizational change is successful, the company receives additional revenue (265 points). It gives the bonus payment to the four retained employees ($4 \cdot 50$ points) and allocates the remaining part of the additional revenue (65 points) to the employer. Summing up results in Equation 2.3, Figure 2.A.1 shows the chronology of a successful organizational change with supportive employees.

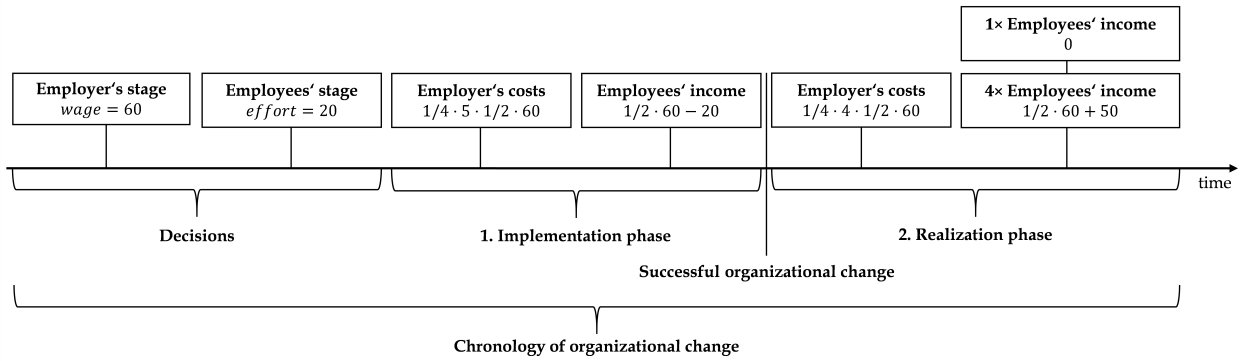


Figure 2.A.1: Chronology of a Successful Organizational Change With Supportive Employees

Apart from other important characteristics, for example, the specific frame and treatment manipulations, our concept of payoffs and employees' effort deviates from what Krügel and Traub (2018) use in their reform task regarding the payment of the laid-off employees. In their concept, some employees receive no wage despite their supportive behavior being used to make the organizational change successful in the first place. From our point of view, this shows a lack of coherence. We introduce a more realistic chronology of organizational change. Although all of these features are unknown to the participants, we believe that this procedure makes the modeled situation of an organizational change feel more realistic to the participants.

Appendix 2.B Instructions

Here, we present the translated instructions of our experiment. The "General Information" is provided at the beginning of the experiment and is identical in all treatments. The "Instructions for Part 2" were provided after the equality equivalence test (Kerschbamer, 2015). We point out differences between the four institutions, Baseline, Default, Recommendation, and Choice, with footnotes. All footnotes included below are not part of the original instructions. Note that we retained the original line spacing.

General Information

Welcome to the experiment. Read the instructions carefully and follow the rules to make money. Payment is made in cash immediately after participation in the experiment. In total, the experiment will last about 60 minutes.

During the experiment, the term "points" is used instead of euros. Points will be converted to euros according to the following scheme: **30 points = 1 euro**.

The experiment consists of **three separate parts**. Each part is explained separately. The instructions for the first and third parts are displayed on the screen. The instructions for the second part are given out when all participants have completed the first part. You can earn money in each part. The payout from each part depends only on your decisions in that part. **At the end of the experiment, you will receive information about your earnings from the three separate parts**. The sum of your earnings is rounded to two decimal places and paid to you in cash. You will first be asked to answer a few questions.

Do not talk to the other participants during the experiment. If you have a question, raise your hand and wait until a member of staff approaches you. We will then answer your question. It is of utmost importance to follow the rules; otherwise, the results of the experiment may be distorted or unusable.

Instructions for Part 2

General Description

Your decisions in the first and second parts have no effect on the payouts from the third part of the experiment, just as your decision from the first part of the experiment has no effect on the payout in the second part.

The second part consists of **20 periods**. Therefore, the same decisions are repeated 20 times. At the beginning of the second part, you are randomly assigned to a **role**, (**"employee" or "employer"**), and informed about your role on the screen. Your role remains the same during the 20 periods.

In each period, you will be randomly assigned to a **group of 6 people** consisting of **1 employer** and **5 employees**. The composition of the group is **determined randomly in each period** and therefore changes. Participants in your group in Period 1 may be different from participants in your group in Period 2, and so on.

In this experiment, companies are faced with the **situation of an organizational change** (from here on referred to as "change"). If a majority (3, 4 or 5) of the employees in the group support the change, the change is successful and leads to a wage bonus for some of the employees as well as to higher profits for the employer; at the same time, one of the five employees will be laid off.

The employer starts the period with a fixed wage (60 points) that her employees receive. After the employer has started the round, the employees are informed about the wage.[18] The employees then choose their effort ("support" or "no support"). If the majority (3, 4 or 5) of the employees choose "support", the change is successful.[19,20]

Two examples illustrate this:

- **Successful change: 3, 4, or 5 employees choose "support"**
  and 2, 1, or 0 employees choose "no support".
- **Unsuccessful change: 0, 1, or 2 employees choose "support"**
  and 5, 4, or 3 employees choose "no support".

The following two pages explain how the earnings in the second part of the experiment are obtained for employees. Then the earnings for employers are explained.

[18] Different in CHOICE: The employer starts the period by choosing the wage (60 or 80 points) that her employees receive. After the employer has decided on a wage, the employees are informed about the wage.

[19] Additionally in DEFAULT: "Support" is pre-selected on the employees' screen.

[20] Additionally in RECOMMENDATION as an additional paragraph: **"Support" is marked as recommended on the employee's screen. This recommendation is based on the average earnings of the group: If all five employees choose "support", each participant earns an average of around 88 points. If, on the other hand, all five employees choose "no support", each participant earns on average about 56 points and, therefore, about 32 points less.**

Earnings of Employees I/II

If you are in the "employee" role and choose "support", you will be charged 20 points, which will be deducted from your earnings. If you choose "no support", there will be no costs for you:

- Costs for **"support": 20 points**
- Costs for **"no support": 0 points**

If the **change is unsuccessful** (see above), **all 5 employees receive their full wage** (60 points)[21] **minus the cost of the chosen effort** (20 points for "support" or 0 points for "no support").

If the **change is successful**, ...

1 employee is "laid off" (1 in 5 employees, 20 percent chance). This employee receives only the first half of her wage (30 points)[22] **minus the costs for the chosen effort** (20 points for "support" or 0 points for "no support"). Half results from the fact that the employee only receives the first half of her wage before the layoff.

The other **4 employees** receive their **full wage** (60 points)[23] and a wage bonus (50 points) **minus the cost of the chosen effort** (20 points for "support" or 0 points for "no support").

The following rules relate to the earnings of employees:

- **Unsuccessful change:**
  - → All **5 employees receive: wage − costs**
- **Successful change:**
  - → **1 randomly selected employee receives:** $\frac{wage}{2}$ **− costs**
  - → **4 employees receive: wage + wage bonus − costs**

Table 1 on the next page shows the corresponding earnings based on your effort, the outcome of the change, and the layoff. This table is displayed on the screen when you make your decision as an employee.[24]

[21] Different in CHOICE: 60 or 80 points
[22] Different in CHOICE: 30 or 40 points
[23] Different in CHOICE: 60 or 80 points
[24] Different in CHOICE: Tables 1 and 2 show the corresponding earnings based on the chosen employer wage, your effort, the outcome of the change, and the layoff. These tables are displayed on the screen when you make your decision as an employee.

Earnings of Employees II/II

At the wage of **60 points** ...	... the payment for "no support" is ...	... the payment for "support" is ...
for a **successful change** and **not** laid off (4 of 5 employees)	110 points	90 points
for a **successful change** and laid off (1 in 5 employees)	30 points	10 points
for an **unsuccessful change** (5 of 5 employees)	60 points	40 points

Table 1: Earnings of Employees[25,26]

At the wage of **80 points** ...	... the payment for "no support" is ...	... the payment for "support" is ...
for a **successful change** and **not** laid off (4 of 5 employees)	130 points	110 points
for a **successful change** and laid off (1 in 5 employees)	40 points	20 points
for an **unsuccessful change** (5 of 5 employees)	80 points	60 points

Table 2: Earnings of Employees ($wage = 80$)[27]

[25] This table is shown in all treatments.
[26] Additionally in CHOICE: ($wage = 60$)
[27] This table is shown in CHOICE only.

Earnings of Employers I/II

If you are in the "employer" role, your earnings are calculated based on[28] the number of employees who have chosen "support" or "no support", and the outcome of the change.

If the **change is unsuccessful** (see above), the employer receives 85 points plus 15 points for each employee who chooses "support", 5 points for each employee who chooses "no support", minus the wage divided by four (15 points)[29] for each of the 5 employees in the group.

If the **change is successful** (see above), the employer receives 150 points plus 15 points for each employee who chooses "support" and 5 points for each employee who chooses "no support", minus the wage divided by four (15 points)[30] for 4.5 employees in the group. 4.5 results from the fact that an employee is "laid off" and receives only the first half of her wage. The employee's wage bonus is not deducted from the employer's earnings.

The following rules relate to the earnings of employers:

- **Unsuccessful change:**

$$\begin{aligned} &\textbf{85 points} \\ &+ \textbf{(15 points} \cdot \textbf{number of "support")} \\ &+ \textbf{(5 points} \cdot \textbf{number of "no support")} - (5 \cdot \tfrac{wage}{4}) \end{aligned}$$

- **Successful change:**

$$\begin{aligned} &\textbf{150 points} \\ &+ \textbf{(15 points} \cdot \textbf{number of "support")} \\ &+ \textbf{(5 points} \cdot \textbf{number of "no support")} - (4.5 \cdot \tfrac{wage}{4}) \end{aligned}$$

Figure 1 on the next page shows the corresponding earnings based on the outcome of the change and the number of employees who chose "support" or "no support". The employer earns more if more employees choose "support" and if the change is successful.[31]

[28] Additionally in CHOICE: the chosen wage,

[29] Different in CHOICE: 15 or 20 points

[30] Different in CHOICE: 15 or 20 points

[31] Different in CHOICE: Figure 1 on the next page shows the corresponding earnings based on the chosen wage, the outcome of the change, and the number of employees who chose "support" or "no support". The employer chooses a wage (60 or 80 points) and earns more if more employees choose "support" and if the change is successful.

Earnings of Employers II/II

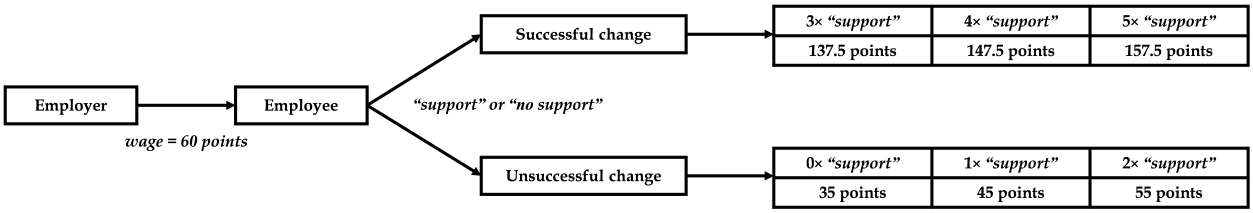


Figure 1: Earnings of Employers[32]

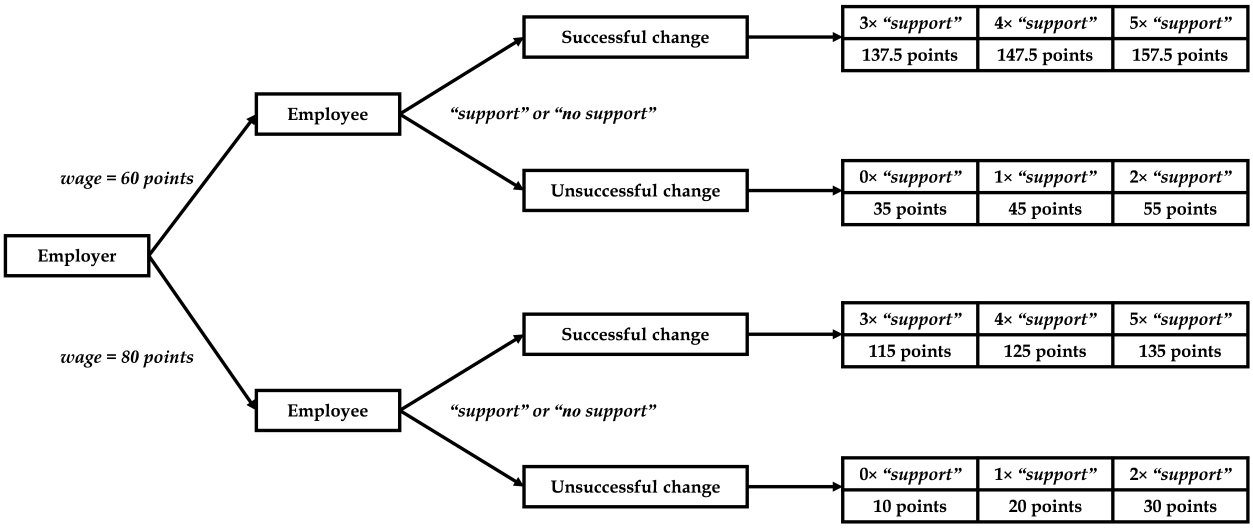


Figure 1: Earnings of Employers[33]

[32] This decision tree is shown in all treatments but Choice.
[33] This decision tree is shown in Choice only.

Information per Period for Both Roles (Employees and Employers)

After each period, you will know...

- the **number of employees** in your group **who have chosen "support" or "no support"**
- the **outcome of the change**
- the **outcome of the layoff** (only for employees and only if the change is successful)
- **your earnings in this period**.

Payment for Both Roles: (Employee and Employer)

At the end of Part 2 of this experiment, the computer randomly selects

5 out of 20 periods.

The sum of your earnings during these 5 periods will be paid to you. Remember that points will be converted into euros according to the following exchange rate:

30 points = 1 euro.

Just as for the first part of this experiment, you will only receive your payout from the second part after the third part and the answering of some questions at the very end of the experiment. The sum of the payout is rounded to two decimal places.

You will now find some questions on the screen to help you understand the second part. As soon as all participants have answered all questions correctly, the first period of the second part begins.

Appendix 2.C Screens of the Laboratory Experiment

Subjects in all treatments participate in the same additional control measures. Additional screens for these parts are available upon request. Here, we present the screens of the main experiment only.

In the main experiment, participants are either in the role of an employer or of an employee. If the participant is an employer, she receives the information about her role once, as can be seen in Figure 2.C.1. Likewise, if she participates as an employee, she receives the relevant information (see Figure 2.C.2).

The following screens are repeated as described above for each of the 20 periods. In Baseline, participants in the role of employers start the round (see Figure 2.C.3). Then, participants in the role of employees learn about the wage level, which was fixed in Baseline, and decide on their change-related behavior, as can be seen in Figure 2.C.4. Finally, in each round, participants in both roles receive feedback about the outcome of that period's interaction. An example of this feedback for employers can be seen in Figure 2.C.5. The outcome for the employee depends on whether she was laid off, and Figure 2.C.7 and Figure 2.C.6 show examples of a laid-off and retained employee, respectively.

While most screens are the same in Default and Recommendation, the employees' decision screens varies. The realization of the default rule is shown in Figure 2.C.8. Likewise, Figure 2.C.9 shows how the pro-change recommendation is implemented.

Again, in Choice, most screens are as presented above for Baseline. However, the decision screens of the two roles are different. Specifically, Figure 2.C.10 shows the wage choice screen for a participant in the role of an employer. See Figure 2.C.11 for the employees' screen.

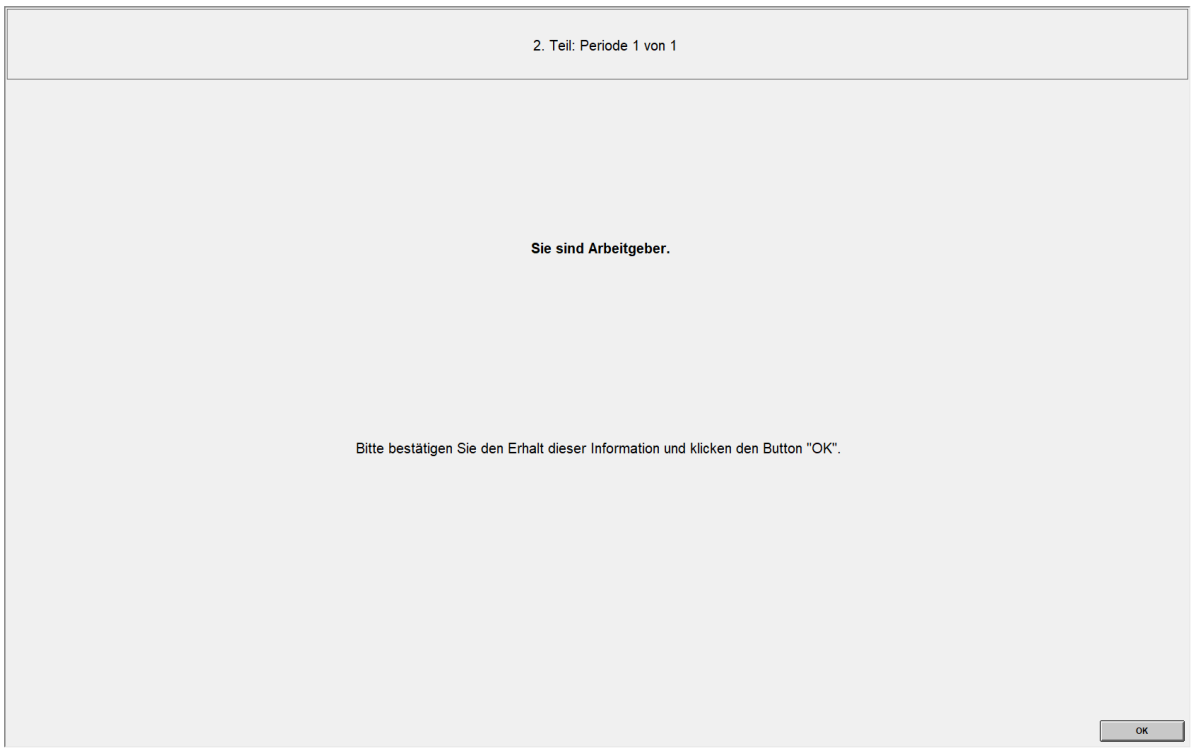


Figure 2.C.1: Role Information for Employer

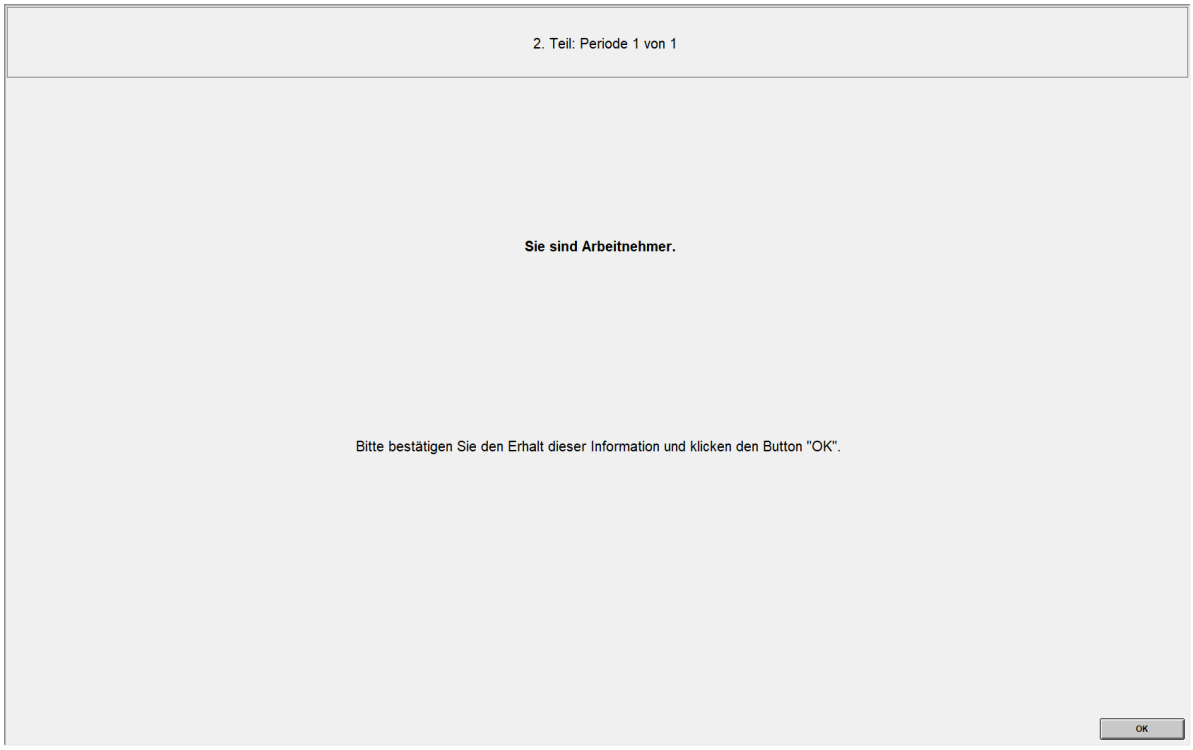


Figure 2.C.2: Role Information for Employee

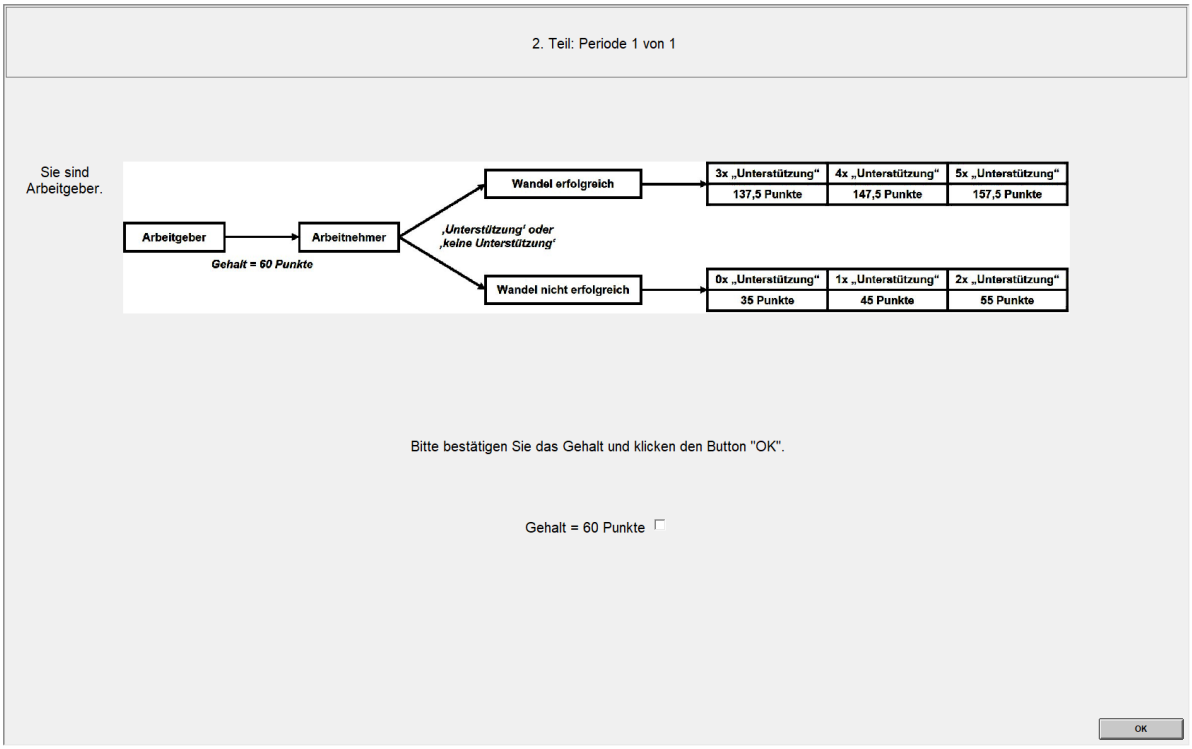


Figure 2.C.3: Employer's Decision in Baseline

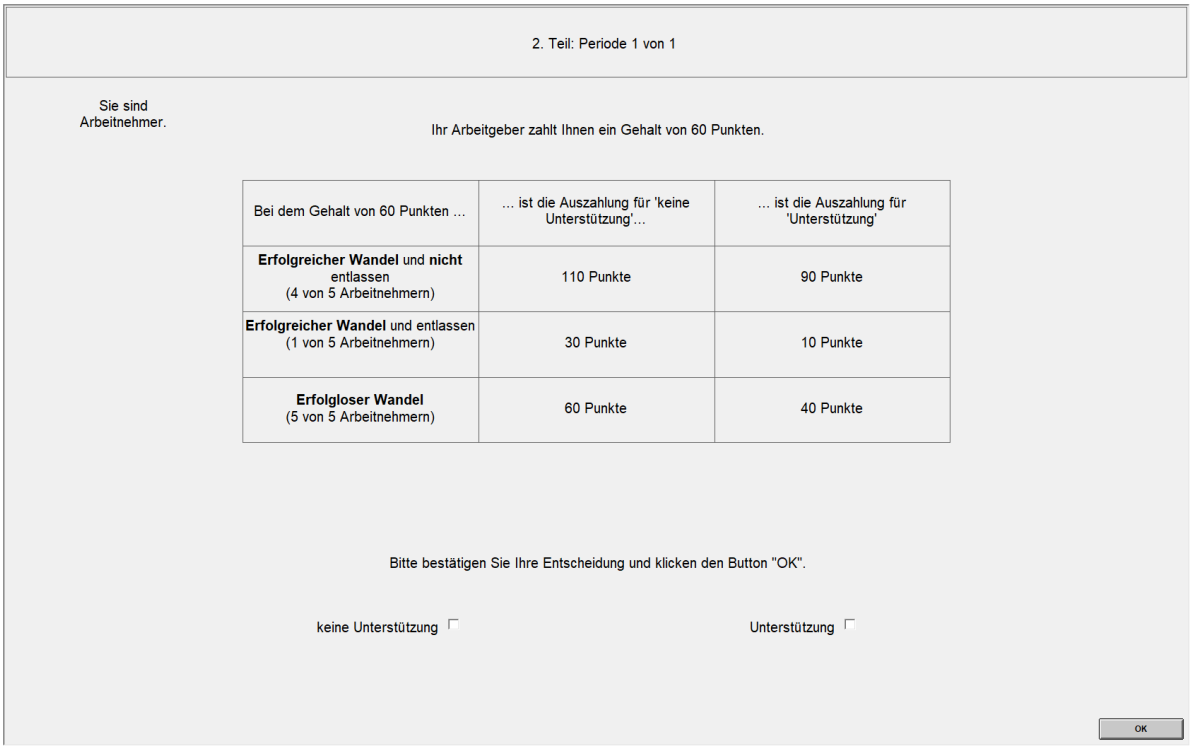


Figure 2.C.4: Employee's Decision in Baseline

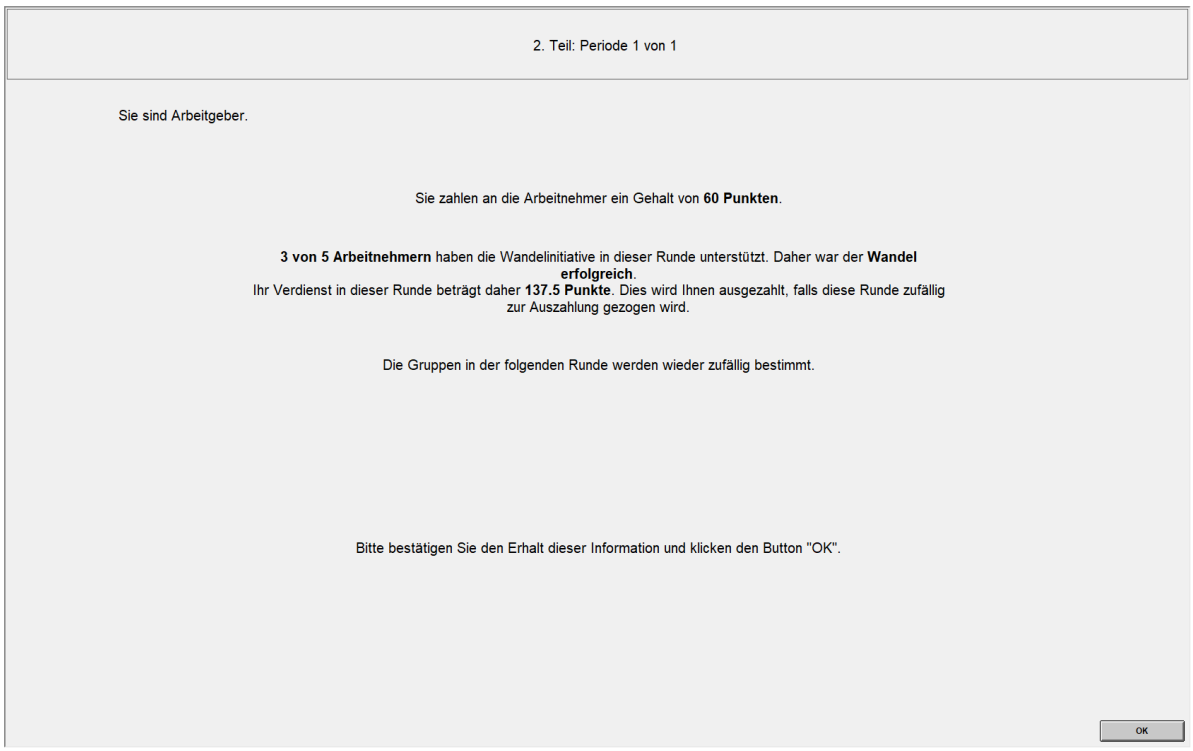


Figure 2.C.5: Feedback for Employer in Baseline

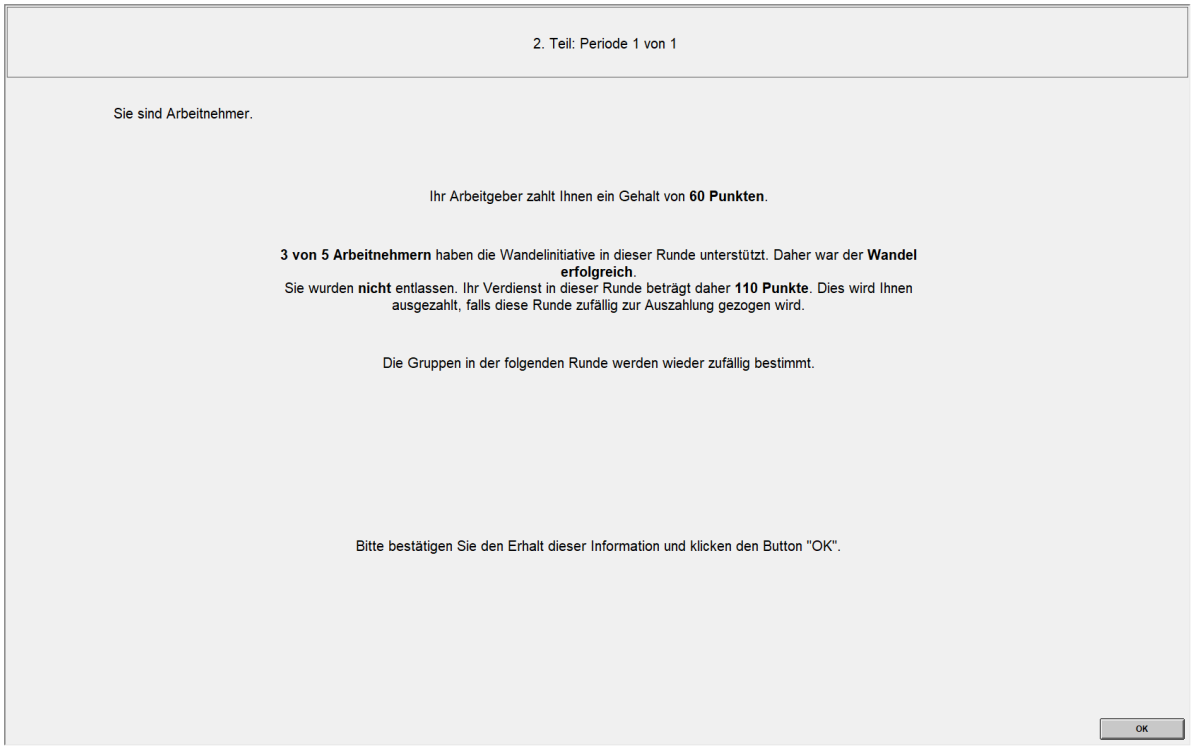


Figure 2.C.6: Feedback for Employee when not laid off in Baseline

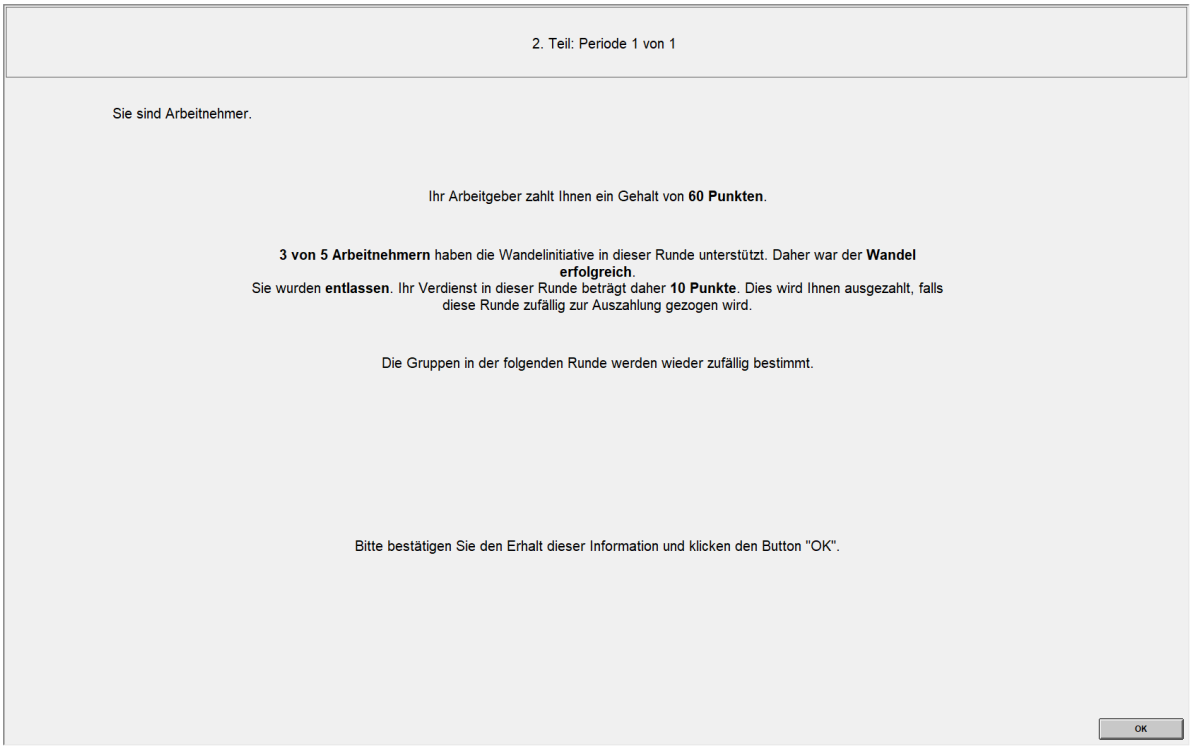


Figure 2.C.7: Feedback for Employee when laid off in Baseline

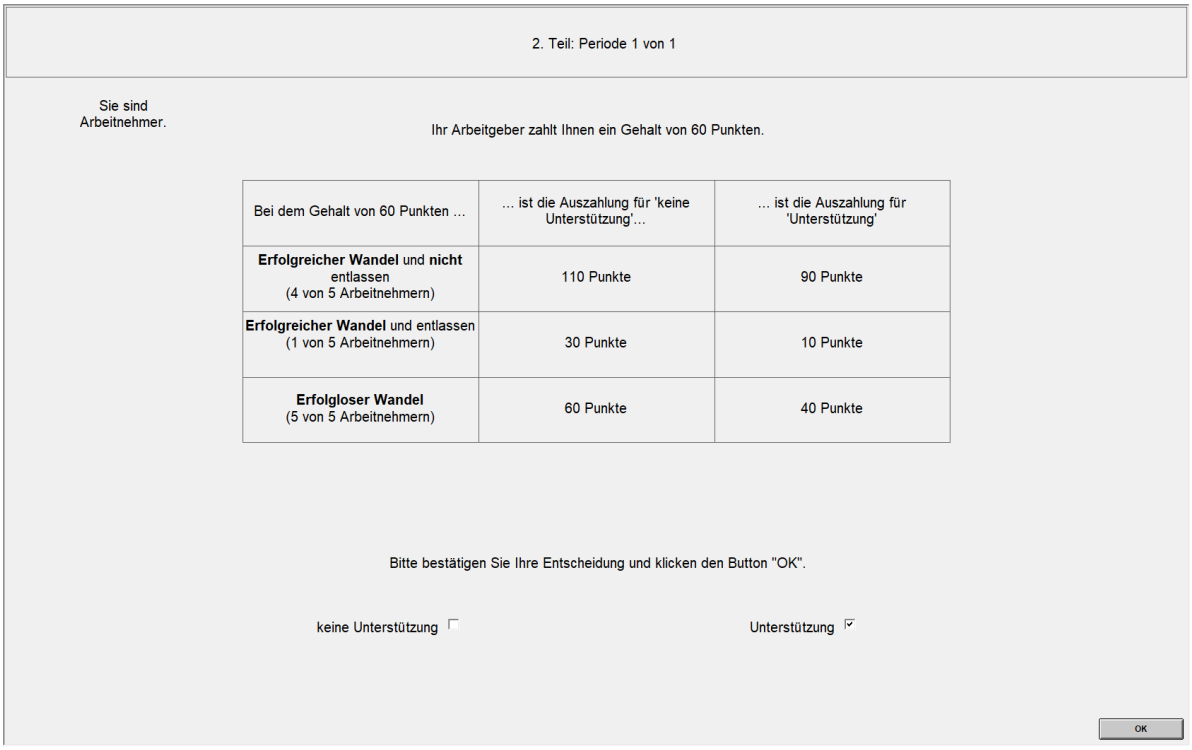


Figure 2.C.8: Employee's Decision in Default

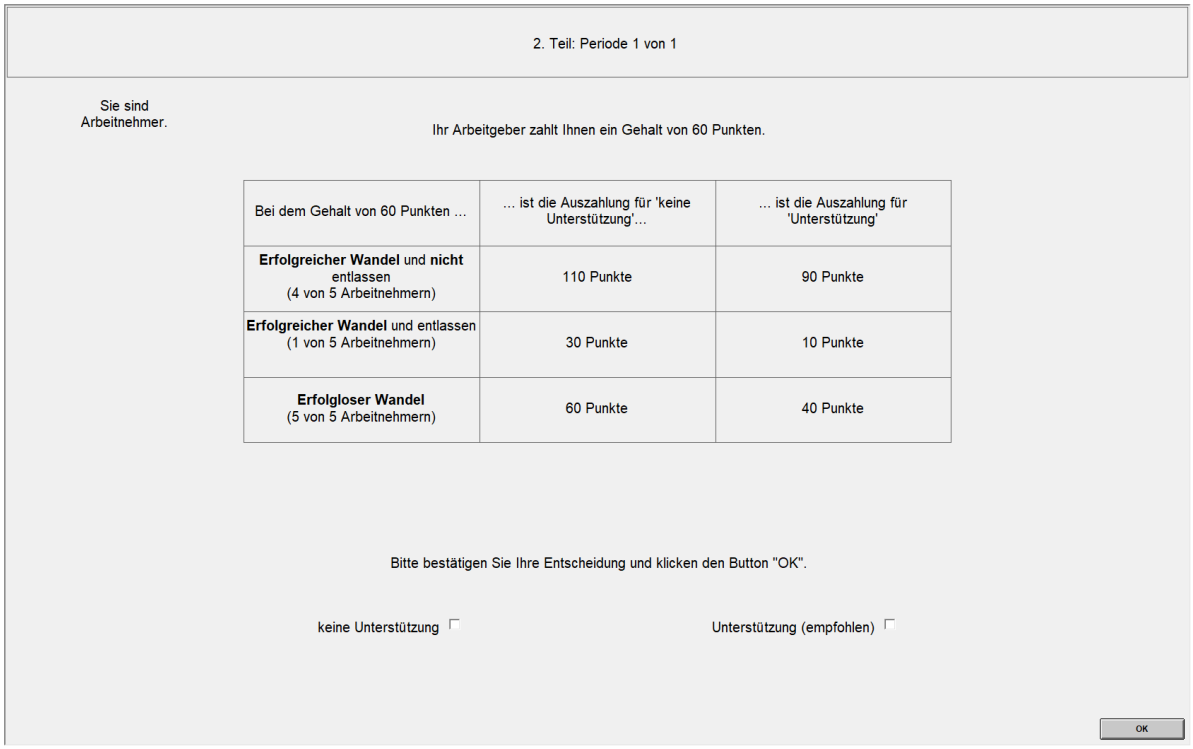


Figure 2.C.9: Employee's Decision in Recommendation

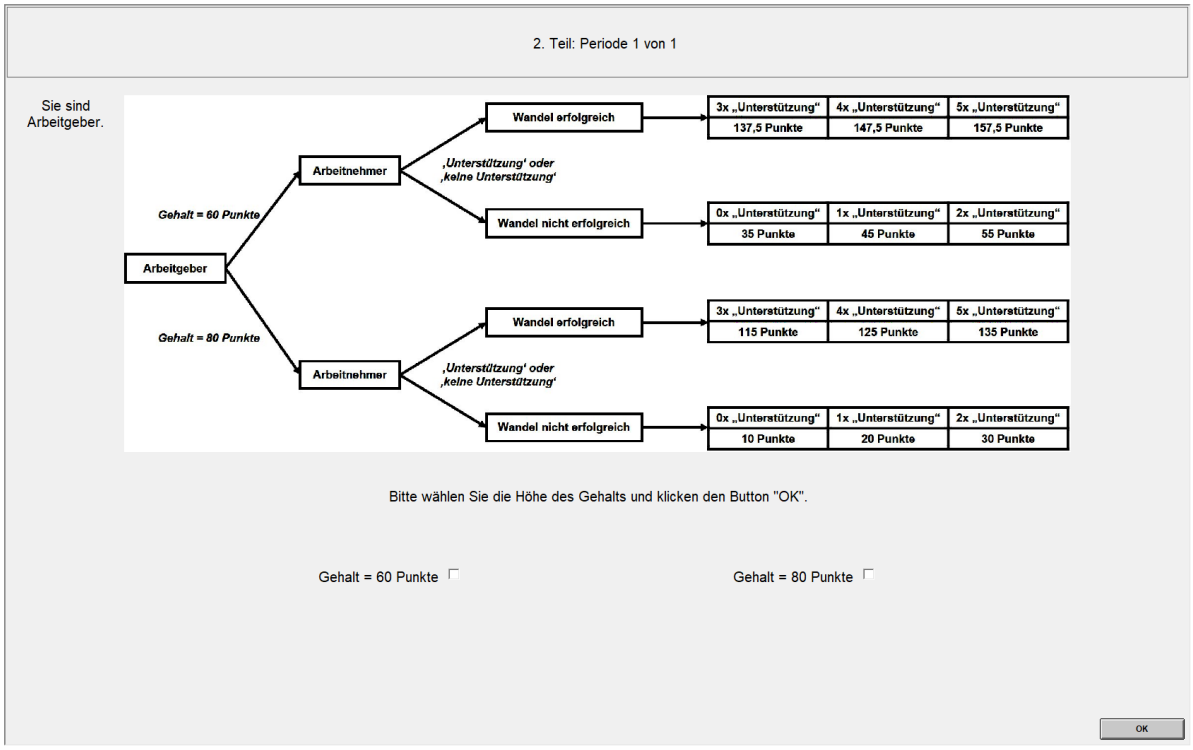
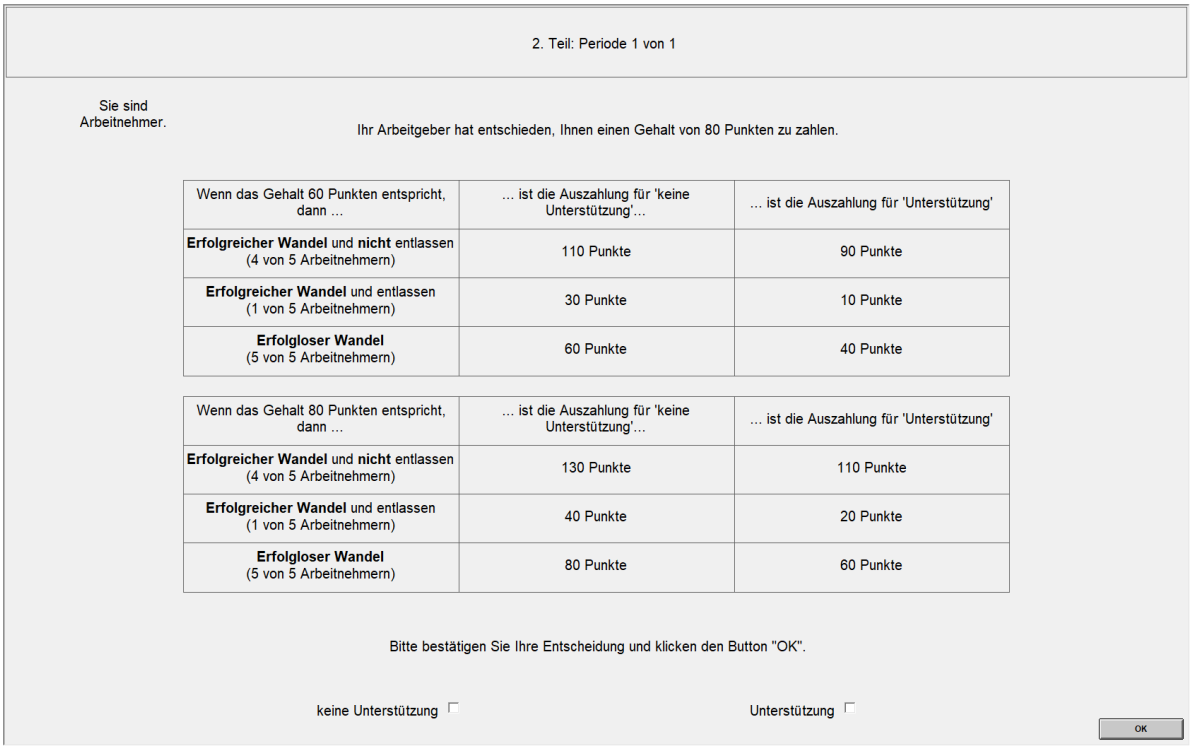


Figure 2.C.10: Employer's Decision in Choice



2. Teil: Periode 1 von 1

Sie sind Arbeitnehmer.

Ihr Arbeitgeber hat entschieden, Ihnen einen Gehalt von 80 Punkten zu zahlen.

Wenn das Gehalt 60 Punkten entspricht, dann ...	... ist die Auszahlung für 'keine Unterstützung'...	... ist die Auszahlung für 'Unterstützung'
Erfolgreicher Wandel und **nicht** entlassen (4 von 5 Arbeitnehmern)	110 Punkte	90 Punkte
Erfolgreicher Wandel und entlassen (1 von 5 Arbeitnehmern)	30 Punkte	10 Punkte
Erfolgloser Wandel (5 von 5 Arbeitnehmern)	60 Punkte	40 Punkte

Wenn das Gehalt 80 Punkten entspricht, dann ...	... ist die Auszahlung für 'keine Unterstützung'...	... ist die Auszahlung für 'Unterstützung'
Erfolgreicher Wandel und **nicht** entlassen (4 von 5 Arbeitnehmern)	130 Punkte	110 Punkte
Erfolgreicher Wandel und entlassen (1 von 5 Arbeitnehmern)	40 Punkte	20 Punkte
Erfolgloser Wandel (5 von 5 Arbeitnehmern)	80 Punkte	60 Punkte

Bitte bestätigen Sie Ihre Entscheidung und klicken den Button "OK".

keine Unterstützung ☐ Unterstützung ☐

OK

Figure 2.C.11: Employee's Decision in Choice

Appendix 2.D Regressions Without Interaction Term

	Model 1	Model 2	Model 3	Model 4
Default	1.573***	1.618***	1.573***	1.583**
	(0.444)	(0.455)	(0.461)	(0.484)
Recommendation	0.228	0.236	0.123	0.130
	(0.475)	(0.488)	(0.497)	(0.542)
Choice	0.435	0.446	0.528	0.436
	(0.363)	(0.372)	(0.377)	(0.433)
Period		−0.069***	−0.064***	−0.069***
		(0.011)	(0.012)	(0.012)
Success in preceding period			0.450**	0.517**
			(0.164)	(0.179)
Laid off in preceding period			0.177	0.272
			(0.278)	(0.306)
Constant	−1.868***	−1.203***	−1.370**	−3.022
	(0.317)	(0.348)	(0.352)	(2.400)
Additional controls	NO	NO	NO	YES
Wald-χ^2	15.27	50.43	60.61	81.47
$p(\chi^2)$	0.002	< 0.001	< 0.001	< 0.001
Number of observations	3,200	3,200	3,040	2,641
Number of groups	160	160	160	139

Note: In all models, the dependent variable is employee's supportive behavior (*organizational change supported = 1/not supported = 0*), and Baseline is the reference group. Results of random effects logistic regression with standard error clustering at the individual level are reported. Standard errors are in parentheses. Model 3 includes a reduced number of observations due to lagged variables ("Success in preceding period" and "Laid off in preceding period"). Model 4 features additional controls in the form of an equality equivalence measure (Kerschbamer, 2015), risk aversion (measured with a multiple price list by Balafoutas et al., 2012), dispositional resistance to change (Oreg, 2003), positive and negative reciprocity (Dohmen et al., 2009), general risk aversion (Dohmen et al., 2011), and a set of sociodemographic attributes. This is why Model 4 includes a reduced number of observations due to the exclusion of 21 participants for either showing inconsistent behavior in the inequality measure, showing inconsistent behavior in the risk measure, or reporting unreasonable values (age below 18, number of semesters above 36) in our sociodemographic controls. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 2.D.1: Results of Random Effects Logistic Regressions Without Interaction

Appendix 2.E Summary of Hypotheses and Results

Number	Hypothesis	Result	Support
2.1	Supportive behavior for change is higher when a pro-change default is in place than when it is not implemented.	A pro-change default fosters support for organizational change.	YES
2.2	Supportive behavior for change is higher when a pro-change recommendation is in place than when it is not implemented.	A pro-change recommendation fosters no support for organizational change.	NO
2.3a	Supportive behavior for change is higher when an employer decides to offer a higher wage than when no deliberate choice is possible.	The realization of a pay raise stimulates support for organizational change.	YES
2.3b	Supportive behavior for change is lower when an employer decides to offer a lower wage than when no deliberate choice is possible.	If a potential pay raise fails to materialize, support for organizational change is reduced.	YES

Table 2.E.1: Summary of Hypotheses and Results

Chapter 3

Default Rule to Guide Managerial Decision-Making – An Experimental Study Under Risk and Uncertainty*

Kim Leonardo Böhm†

Abstract

Managers often make decisions under risk or uncertainty, for example, when deciding on a business venture. One tool for promoting risk-neutral and uncertainty-neutral behavior – that is, behavior that is most beneficial for the company and involves taking chances responsibly – is nudging. Default nudges in particular have been shown to have an economically strong effect and to be cost-efficient at the same time. In this paper, an online experiment with a business venture frame is used to test a pro-neutrality default rule. The default reduces risk-aversive behavior significantly but turns out to be ineffective in situations under uncertainty. A potential moderating effect is discussed in light of the underlying channels of the default effect. Default rules are especially beneficial in managerial situations, since they do not require the manager to process additional information, unlike a recommendation or social norm nudge. Additional practical implications are discussed. In summary, managerial decision-making (and internal and external guidance for it) might benefit from utilizing nudging in the form of default rules.

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3.1 Introduction

Shareholders employ managers to take on responsibility for their company's activities. This means that shareholders are the principals while their managers serve as agents. In this principal-agent relationship, shareholders face a serious problem due to conflicting aims:

On the one hand, shareholders want to steer their managers to act in the shareholders' best interests, to make decisions in line with maximizing current and future profits and to lead and develop the company and its products or services into new, profitable markets. To do so, managers have to display risk-neutral and uncertainty-neutral behavior, such as taking reasonable risks when they are expectably profitable for the company (Lovallo et al., 2020). According to Stirling (2003, p. 4), "a rational decision is one that conforms ... to a set of rules that govern behavior." More strictly speaking, some argue that "decision-makers are rational if and only if their assessments of probabilities obey the laws of probability" (for the quote, see p. 2 in Herrmann, 2017; for an overview of rational behavior under probabilities, see Lyon, 2010).[34] Following early work on the principal-agent theory (Eisenhardt, 1989; Rees, 1985a; 1985b), there is a large body of literature on how the contract between the principal and the agent in firms can be organized more efficiently, for example, through incentivizing or monitoring the agent to align interests between the principal and the agent (Miller, 2005; Sappington, 1991). In a systematic literature review, Hoskisson et al. (2017) highlight research that addresses managerial risk-taking subject to compensation incentives or monitoring.

On the other hand, shareholders do not want to restrict the manager's maneuvering room too much. If they limit the decision space of their managers by implementing incentives for a specific behavior or a certain level of risk-taking paired with corresponding monitoring, managers may not be able or willing to react in the interest of the company in case of unforeseen market developments and sudden crises. This lack of adaptation might, in turn, not be in the best interest of the shareholders. For example, Deakin (2014) argues that more legal shareholder power might come at the price of corporate value (for empirical work, see Belloc, 2013). Likewise, Bhagat and Black (2002) find that more power for a monitoring board is *not* associated with higher company performance and point out that this is a finding contrary to what many institutional investors believe.

Managerial decisions are often characterized by situations in which risk or uncertainty prevails (Collis, 1992; March & Shapira, 1987), such as possible investments, innovation developments, strategic changes within the firm, or new market entries (Hoskisson et al., 2017). One important managerial sphere is business venturing, which can take place under risk or uncertainty: where risky decisions can still be weighed up to some extent (e.g., the manager gathers data on an already existing smartphone market that she can use to make appropriate forecasts for a new smartphone

[34] For reasons of readability, I substitute both "risk neutrality" and "uncertainty neutrality" with "neutrality" and both "risk aversion" and "uncertainty aversion" with "aversion." Likewise, I use "aversive behavior" for behavior showing risk aversion or uncertainty aversion and "neutral behavior" for behavior showing no risk aversion or uncertainty aversion. For this paper, neutral behavior is optimal in the sense of the definitions provided by Herrmann (2017) and Stirling (2003), while aversive behaviors are acts that deviate from this optimum.

model), the outcome of decisions under uncertainty is wholly unknown (e.g., the manager does not have data on a smartphone market if her firm is the first player in the market to offer a smartphone). Therefore, uncertainty implies a status of ambiguity for the decision (Klibanoff et al., 2005; for early work differentiating these two domains based on the information the decision-maker has regarding the expected result, see Ellsberg, 1961; Keynes, 1937; Knight, 1921). Milkman et al. (2009) argue that errors in managerial decision-making are costly. In business venturing situations, managerial risk neutrality or managerial uncertainty neutrality (in the sense of making decisions that maximize expected profits or gains for the shareholder) is essential for ensuring the future prosperity of the organization (Sauner-Leroy, 2004) and defending its position as an incumbent against disruptive market developments (Christensen, 1997; Markides, 2006). Although some managers in a company fail with their risky or uncertain decisions, the losses can be compensated by higher profits from other managers; this is not the case if the managers make risk-averse or uncertainty-averse decisions. Despite the given advantages of neutrality among managers, management still has a human side, that is, managers can suffer from aversion and other influences like any other human being (Teal, 1996). Indeed, we observe aversive behavior in management decisions within all corporate hierarchies due to manifold reasons (Glaser et al., 2016; González et al., 2013; Krivkovich & Levy, 2015; MacCrimmon & Wehrung, 1990; Milidonis & Stathopoulos, 2014; Milkman et al., 2009; Schwenk, 1984; Simon et al., 2000). Several potential sources for such behavioral deviations have been discussed, including fear of loss (Kahneman & Tversky, 1979), regret aversion (Zeelenberg & Beattie, 1997), blame aversion (Eijkelenboom et al., 2019; Hood, 2007), and inability to practice decision-making under risk and uncertainty (Thaler & Sunstein, 2008).

3.2 Development of Hypotheses

Tversky and Kahneman (1983) introduce the concept of two systems of thinking (for an overview of different approaches to dual-process theory, see Stanovich & West, 2000). System 1 thinking is characterized as fast, automatic, and unconscious, allowing for rapid decisions (Kahneman, 2011). However, speed comes with the drawback that such decisions might be vulnerable to flaws and errors (Milkman et al., 2009). System 2 thinking is slow, effortful, conscious, and follows a logical approach (Kahneman, 2011). Previous research on guiding or enhancing managerial decision-making has focused mainly on leveraging System 2 thinking to counteract or reduce System 1 errors (Milkman et al., 2009). Examples include managerial training (Morewedge et al., 2015; Sellier et al., 2019), mindfulness (Hafenbrack et al., 2014; Karelaia & Reb, 2015), warnings (Döbrich et al., 2014), recommendation nudges (Böhm et al., 2021b), and other similar approaches (Graf et al., 2012). What all of these have in common is that they shift managerial decision-making from fast and intuitive System 1 thinking to slower, more conscious, more explicit System 2 thinking. Another possibility is that these approaches aid the current decision-making process if the manager is already applying System 2 thinking. Milkman et al. (2009) point out that another potential approach to improve decision-making is to leverage System 1 thinking directly.

One tool for steering people into a specific direction is nudging (Böhm et al., 2021a; Hummel

& Maedche, 2019; Thaler & Sunstein, 2008). Some nudges, for example, recommendations, are considered to affect System 2 thinking. Other nudges, such as default rules, influence the decision-maker directly in her System 1 thinking (Sunstein, 2016). Sunstein (2014, pp. 584–585) postulates that "default rules may well be the most effective nudges ... [and] ... have sometimes been found to have even larger impacts than significant economic incentives." However, to the best of my knowledge, previous research has not tested whether such a System 1 intervention is effective in promoting neutral behavior. Therefore, in this paper, I test whether a pro-neutrality default rule is effective in promoting more neutral behavior in risky and uncertain business venturing decisions.

In a recent meta-analysis of default effects, Jachimowicz et al. (2019) find that defaults are one of the most widely used types of nudge and often have a relevant and significant effect. Although the degree of effectiveness varies, there are many examples of effective behavioral intervention with a default. For example, Johnson and Goldstein (2003) find that implementing an opt-out instead of an opt-in rule for organ donation is very effective in fostering more organ donors – a finding that is replicated in many countries. Other success stories have been found for higher retirement savings (Madrian & Shea, 2001; Thaler & Benartzi, 2004), healthier employee behaviors (Venema et al., 2018), and fewer rejecting behaviors towards product innovations on the part of customers (Kuester et al., 2015). In contrast to the aforementioned examples, one might argue that decisions made by managers are mostly non-binary. If the decision at hand is non-binary, a default rule can serve as an anchor (Tversky & Kahneman, 1974). For example, Dhingra et al. (2012) report that default rules pull dictator choices in a dictator game towards the specific default value. Likewise, Goswami and Urminsky (2016) find that the values of default rules affect the donation level in charity-giving, favoring the explanation that such a default value does, indeed, serve as an anchor (for a field experiment with a similar result, see Zarghamee et al., 2017).

Concerning the underlying mechanism of defaults, Dinner et al. (2011) experimentally investigate three psychological channels of default rules: *endorsement*, *ease*, and *endowment* (for additional experimental evidence, see Bang et al., 2020; Tannenbaum et al., 2017). First, *endorsement* builds on the finding that a default can be perceived as a means of conveying a recommendation from the choice architect to the decision-maker (McKenzie et al., 2006). Therefore, the effectiveness of the default may partly be dependent on whether the decision-maker trusts the choice architect and believes in an alignment of preferences. Second, it is likely more convenient to stick to the pre-selected option instead of making the cognitive effort to re-evaluate it or any other form of effort needed to change it (for work about defaults discussed regarding the path of least resistance, see Choi et al., 2002). Consequently, *ease* is higher when it is more effortful to switch from the pre-selected option to another option (Zúñiga-Fajuri, 2015). Similarly, Johnson et al. (2012) argue that if a default rule is implemented, the decision-maker may evaluate whether the pre-selection satisfies her interests instead of taking on the complete decision-making process at hand. Therefore, the pre-selection becomes the easy choice. Third, Dinner et al. (2011) also hypothesize that other options are evaluated in reference to the pre-selected option, making the pre-selection the endowed option (Kahneman et al., 1991; Kahneman & Tversky, 1979). The strength of *endowment* depends

on the extent to which the decision-maker is convinced that the pre-selection is the status quo (Kahneman et al., 1991; Samuelson & Zeckhauser, 1988). Moshinsky and Bar-Hillel (2010) find supporting evidence for this idea. They randomly label one option as the "status quo" and find that the random label increases the attractiveness of the corresponding option.

Regarding a pro-neutrality default rule implemented for risky situations, I hypothesize a neutral-behavior-promoting effect for such a default rule based on the three channels introduced above. First, a pro-neutrality default and its value of risk-taking can serve as an *endorsement* and can, therefore, be seen partly as a recommendation of (more) neutral behavior in the specific situation. Due to the information available in risky business venturing decisions, the decision-maker is capable of verifying the value of the default as the optimal decision in the sense of it being profit-maximizing. Second, it is easier to stick to the pre-selected value of risk-taking and potentially compare other behavioral options to the pre-selected behavioral choice (*ease*). Again, due to the information provided, the decision-maker can compare the expected outcome of the default rule to the outcome of other options. Third, the pre-selection serves as the status quo when starting the decision-making process. Importantly, in a risky situation, the default value can also serve as an anchor, as it may present a starting value in the decision-making process. Under risk, the decision-maker is able to calculate the expected outcome of each option fairly easily. Therefore, if the decision-maker starts the more analytical decision-making process, the expected outcome of the default rule might be her starting point and become the anchoring value. Therefore both a status quo and an anchor effect might be present because of this *endowment* with a behavioral choice. Overall, a pro-neutrality default should promote risk-neutral behavior in managerial situations under risk.

Hypothesis 3.1. *A pro-neutrality default promotes risk-neutral behavior in managerial situations under risk.*

Concerning the same pro-neutrality default rule for an uncertain business venturing decision, I also hypothesize an effect that promotes neutral behavior. However, regarding the three potential channels, the picture is less clear due to the different domain and its effect. First, a pro-neutrality default can still serve as an *endorsement* and can, therefore, be partly perceived as a recommendation of (more) neutral behavior in the specific situation. With less information available, the decision-maker cannot necessarily verify that the value of the default is in her best interests. Still, given that the choice architect does not vary between the two domains, the level of trust might not be different, and the default might still have an endorsing effect in uncertain business venturing decisions. Second, the pro-neutrality default should still feature *ease* in uncertain situations. While a less informative situation might make it more difficult for the decision-maker to just compare the pre-selection to the alternative behavioral choices, she still has to show no effort at all if she sticks with the pre-selection. Third, regarding the endowing effect of a pro-neutrality default rule, it seems most likely that the status-quo effect is roughly as pronounced in uncertain situations as it is in risky decisions. However, previous research indicates differences in the decision-making processes under risk and under uncertainty (Moore & Eckel, 2006; Ross et al., 2012). Therefore, while the status-quo effect might be similarly present, the value of the default might not serve as an anchor.

This is because it is harder for the decision-maker to pinpoint the exact outcome of the options, including the one provided by the pro-neutrality default nudge. This, in turn, might dampen the analytical process for which the default value is the anchor. Overall, a pro-neutrality default should promote uncertainty-neutral behavior in managerial situations under uncertainty.

Hypothesis 3.2. *A pro-neutrality default promotes uncertainty-neutral behavior in managerial situations under uncertainty.*

My hypothesis development, building on previous research about default rules and the three underlying psychological channels, yields the same direction for the effect of a pro-neutrality default on behavior in risky and uncertain managerial situations. However, I also hypothesize a moderating effect of the underlying domain (risk or uncertainty) on the default's effectiveness. Hauser et al. (2018) and van Kleef and van Trijp (2018) point out the need to investigate nudge effects in the specific context, as the context itself might be a moderator of the effectiveness. More specifically for defaults, Jachimowicz et al. (2019) find systematic differences in the effect of default rules based on the decision environment they are implemented in. An uncertain managerial situation differs from a risky situation in the amount of information available. In uncertain situations, the decision-maker is not able to pinpoint the optimal decision herself, evaluate each option by calculating the expected outcome, and verify the default rule by calculating the expected outcome of the pre-selected option.

Concerning the three channels of the effect of default rules introduced above, this implies the following. First, for a pro-neutrality default to serve as an *endorsement*, the decision-maker must trust that the pre-selection is in her best interests. In uncertain situations, the decision-maker cannot verify that the pre-selected value is in her interests due to the unknown probabilities. On the one hand, it might be that the decision-maker's trust in the choice architect suffers from this vagueness; indeed, trust has been found to be a relevant moderating channel (Tannenbaum et al., 2017). On the other hand, the trust could also be unchanged, as the choice architect does not change when the context does. Therefore, the *endorsement* effect of a pro-neutrality rule is likely to be either weaker under uncertainty than under risk or stays the same. Second, concerning the *ease* of sticking with the pre-selection, one might argue that it is the same in both domains; however, given less information in an uncertain situation, it might be more difficult to apply a quick comparison of the pre-selected option against alternative options, as the decision-maker cannot evaluate each option by calculating the corresponding expected outcome (Johnson et al., 2012). As stated above, the default option is still the one featuring the least amount of effort and letting the decision-maker walk on the path of least resistance (Choi et al., 2002). Therefore, the *ease* effect of a pro-neutrality rule is likely to be either weaker or the same under uncertainty than under risk. Third, while the status-quo effect likely remains unchanged between domains, the anchoring effect might vary. Moore and Eckel (2006) and Ross et al. (2012) argue that the decision-making process differs under risk and under uncertainty. Therefore, with the decision-maker being unable to pinpoint the optimal decision and calculate the expected outcome of each option, including the pre-selected one, the anchoring effect is likely reduced. Therefore, the *endowment* effect of a pro-neutrality rule is likely to be weaker under uncertainty than under risk. In summary, I hypothesize that the features of

uncertainty in a business venturing situation have a negative moderating effect on the effectiveness of the pro-neutrality default rule.

Hypothesis 3.3. *Uncertainty in a managerial situation has a negative moderating effect on the effectiveness of a pro-neutrality default.*

3.3 Experimental Design

3.3.1 General Overview

The online experiment consists of two tasks (Task 1 and Task 2) from which one is randomly selected to be payoff-relevant for the subject. Task 2 features an additional control and is described in Subsection 3.3.5. Task 1 is the main behavioral measure and is described in detail in Subsection 3.3.2. It resembles a managerial decision and allows for a deviation from the optimum and, therefore, enables investigation of the neutrality-enhancing effect of a pro-neutrality default rule. For this purpose, I utilize a multiple price list featuring either risk or uncertainty (for a similar experimental measure, see Barham et al., 2014).[35] In addition, a business venture frame is added to the experiment to mirror a managerial decision more closely. Therefore, with this experimental approach, I am able to explore whether a pro-neutrality default rule is feasible for promoting neutral behavior of managers in business venture situations featuring either risk or uncertainty.

↓ **Treatment / Domain** →	*Risk*	*Uncertainty*
Baseline	Baseline *Risk* $N = 96$	Baseline *Uncertainty* $N = 46$
Default	Default *Risk* $N = 51$	Default *Uncertainty* $N = 54$

Note: The number of subjects in each group is presented. Additional details are described in Subsection 3.3.6.

Table 3.1: Outline of the Experimental Groups

In this paper, I test one treatment against the corresponding control group in two types of business venture situations. In addition to the behavioral intervention (none in Baseline vs. a pro-neutrality default rule in Default), I investigate the potential moderating effect of the underlying

[35]For the original multiple price list on which the experimental measure builds, see Holt and Laury (2002). I differentiate risk and uncertainty similarly to Moore and Eckel (2006) and Ross et al. (2012). Drichoutis and Lusk (2016) provide a discussion for why varying the payoffs (like Barham et al., 2014) is more suitable for the experiment presented here than varying the probabilities (like Holm et al., 2013; Holt & Laury, 2002; Koudstaal et al., 2016).

domain (*Risk* or *Uncertainty*). Default rule effects are potentially moderated by the context of the decision environment (Hauser et al., 2018; Jachimowicz et al., 2019; van Kleef & van Trijp, 2018). This yields four groups in total in this 2×2 between-subject design. Each subject participates in one of these groups and therefore faces a decision either under *Risk* or *Uncertainty*, making a decision with no default (Baseline) or with a pro-neutrality Default in place. Table 3.1 shows an outline of the four experimental groups and the corresponding number of subjects included.

3.3.2 Technical Realization of the Managerial Decision

In Baseline *Risk*, every participant plays the role of a manager and has to decide in which design she wants to offer her products on the market. Here, "design" encompasses all core features that the product has (for a similar definition, see Utterback & Abernathy, 1975). The participant has to decide between a traditional and modern design for 11 distinct products simultaneously. The traditional design is well-established on the market, such that the manager knows the exact price that the customers are willing to pay. Therefore, deciding in favor of the traditional design yields a sure payoff of \$2.00. Deciding on the modern design involves a risk. This design has not been tested on the market yet, and the manager does not know how the customers will respond. For reasons of simplicity, it is assumed that customers could only react in two ways: They either prefer the modern design, resulting in a willingness to pay a high price for the product, or they prefer the traditional design, resulting in a lower price. Based on market analysis, the outcomes occur with equal probabilities of 50%. To visualize this in the framed experiment, subjects are provided with the information that out of 100 customers, 50 are willing to pay a high price and 50 are willing to pay a low price. Of these 100 customers, one is randomly chosen to be the one that determines the payoff for the scenario. If a customer who is willing to pay a high price is chosen, the subject earns \$4.00. If, however, a low-paying customer is selected, the participant earns an amount between \$2.00 and \$0.00. This value differs between the 11 products. For example, the payoff for a low-paying customer is \$2.00 for Product 1, but it decreases to \$0.00 for Product 11. A full representation of the multiple price list can be found in Appendix 3.A.[36] If Task 1 is the payoff-relevant task, one of the 11 products and the corresponding subject's decision is randomly selected to be payoff-relevant.

The experimental measure described above provides a unique switching point for each subject or no switching point at all if a subject always opts for the modern design.[37] For the analysis, it is assumed that each subject has a constant relative risk aversion (CRRA) and the isoelastic utility function $u(\pi) = \frac{\pi^{1-\gamma}-1}{1-\gamma}$ for $\gamma \neq 1$ and $u(\pi) = ln(\pi)$ for $\gamma = 1$, with π denoting the payoff and γ the CRRA coefficient (Arrow, 1971; Pratt, 1964). Depending on the unique switching point, a CRRA coefficient is assigned to each subject and used as the main dependent variable in the statistical

[36] To clarify, each subject plays the game for herself. Therefore, neither do her decisions influence the payoff of any other subject nor does any decision by other subjects influence her payoff. In addition, all subjects are informed about all the rules at the beginning of the experiment, and all relevant features are common knowledge.

[37] For reasons of consistent behavior, subjects are allowed to switch from the modern design to the traditional design (i.e., from right to left in the multiple price list) only once.

analysis. In particular, I follow Barham et al. (2014) and set the CRRA coefficient to the lower bound of the resulting interval. The method to deduce the CRRA coefficients from the respective switching point is presented in detail in Appendix 3.B. In addition to the CRRA coefficient, the unique switching point can also be deployed as the dependent variable in the analysis. For this, the coding of the switching point is reversed, enabling me to compare the results easily.[38] Appendix 3.C provides an overview of the dependence between the switching point, the CRRA coefficient, and the reversed switching point.

3.3.3 Implementation of the Default Nudge

The default is a pre-selection of the modern design for Products 1–10 and the traditional design for Product 11. Nudges must be favorable for the decision-maker (Thaler & Sunstein, 2008). This is fulfilled here because the expected payoff of the modern design is higher than the payoff of the traditional design for the first 10 products. For Product 11, the expected payoffs are equal; therefore, the safer option is pre-selected. Consequently, the default rule is to pre-select the most neutral decision, making it a pro-neutrality default in this setting. While I justify the exact value of the default rule in this paper, this explanation is not provided to participants in the experiment. To ensure that the subjects understand and could recall the pre-selection correctly, the default is implemented by pre-selecting the corresponding items and changing the color of the border of the pre-selected items permanently so that the participants do not lose track of what was pre-selected (see Appendix 3.D for screens of the online experiment).

3.3.4 Realization of Uncertainty

Between groups under *Risk* (Baseline *Risk* and Default *Risk*) and *Uncertainty* (Baseline *Uncertainty* and Default *Uncertainty*), the underlying domain of the situation varies and is the only difference compared to the respective equivalent in the other domain. While the risky situation resembles a business venture in which the market is relatively well explored, the uncertain situation resembles a business venture into a market with less information. Here, ambiguity is induced by using an unknown probability distribution over the customer's willingness to pay if the subject decides in favor of the modern design instead of the known, equal probabilities. To visualize this in the framed experiment, subjects are *not* provided with information about how many out of 100 customers are willing to pay a high price and how many are willing to pay a low price. Calculating the expected profits by integrating over all possible distributions of probabilities yields the same value as in the *Risk* domain.[39] Following Barham et al. (2014), Gilboa and Schmeidler (1989), and

[38] Note that the CRRA coefficient is decreasing when switching at a higher product number, while the switching point is increasing. It is helpful to reverse the switching point to gain estimates that have equal signs. Technically speaking, this is necessary because the level of shown risk aversion in the decision decreases with switching in a lower row.

[39] Note that no information about the distribution of probabilities is provided to the participants in the experiment. Because of this, one can make the assumption that an uncertainty-neutral participant assumes a uniform distribution over the probabilities. This leads to a neutral participant assigning a probability of 50% on average to each of the two outcomes. For a similar approach, see Barham et al. (2014).

Klibanoff et al. (2005), a coefficient for uncertainty aversion is calculated similarly to the CRRA coefficient γ in *Risk*. This coefficient can be interpreted as the sum of risk aversion and ambiguity aversion (for formal proof, see Barham et al., 2014). This coefficient is used as the main dependent variable for the *Uncertainty* domain in the analysis. For the sake of simplicity and to avoid confusion in the non-parametric and parametric testing, the same term ("CRRA coefficient") is used and denoted by γ when referring to the coefficient for uncertainty aversion.

3.3.5 Additional Controls

In addition to the main experiment in Task 1, a set of additional controls are elicited. Task 2 measures the subject's mathematical abilities with an alteration of the Berlin Numeracy Test (Cokely et al., 2012). To avoid cheating due to the online nature of the experiment and the public knowledge about the items of the Berlin Numeracy Test, the items are adapted in presentation and wording without changing the actual mathematical skills needed. Previous findings by Park and Cho (2019) and Riepe et al. (2022) support a relationship between numeracy and a person's risk aversion. If Task 2 is payoff-relevant, the subject's performance in the numeracy test is paid. For each correct item, subjects receive \$1.00. Therefore, the potential payoff of Task 2 ranges from \$0.00 to \$4.00. In addition to the two incentivized tasks, additional controls are added at two points in the experiment. First, after Task 1, subjects are asked to rate how confident they are in decision-making situations like the one they have been asked to decide on in Task 1 and to report on their preference for rational or intuitive reasoning (Butler et al., 2014). Second, at the end of the experiment, five additional questionnaires are implemented. The general risk aversion scale (Dohmen et al., 2011) is used to elicit the subjects' propensity to take risks. Additional self-measures include tolerance for ambiguity (Budner, 1962; Herman et al., 2010), personal attitude towards entrepreneurship and perceived behavioral control regarding entrepreneurial capacity (Liñán & Chen, 2009), and dispositional resistance (Oreg, 2003). The experiment concludes with a set of sociodemographic questions about the participants' gender, age, employment status, educational level, household income, contribution to household income, and the number of adults and children in the household. As I do not derive hypotheses on correlations between these controls and the main behavioral measure, I add them to my parametric analysis as additional explanatory variables to test the robustness of the results. While I can show that the inclusion of these controls does not change the results significantly, I do not discuss the coefficients and p-values associated with these control variables.

3.3.6 Subjects, Setting, and Procedure

A total of 93 subjects participated in Baseline (*Risk*: $N = 47$, *Uncertainty*: $N = 46$). Böhm et al. (2021b) use the same experimental measure, feature two other treatments, and share the same Baseline *Risk* group. In addition, one of their two treatments, which are described in the following chapter of this dissertation, features the Baseline-like decisions under *Risk* of another

49 participants.[40] These observations are used additionally to form the BASELINE *Risk* group ($N = 96$) for this paper. Another 105 subjects participated in DEFAULT (*Risk*: $N = 51$, *Uncertainty*: $N = 54$), making the total number of participants in this paper 247. Variations in the number of participants per group other than the aforementioned pooling under *Risk* result from randomization. Of the 247 participants included here, 3 identified as neither female nor male. Of the remaining 244, 53.69% identified as female (Binomial probability test: $p = 0.276$). The average age in years was 33.47 ($SD = 11.93$). All data collection took place in November 2020 using Qualtrics for realizing the experiment and Prolific (Palan & Schitter, 2018) for recruiting and paying subjects. The sample was limited to US citizens with a high school degree or a higher educational level who were born in the US . The median subject took 12.70 minutes to finish the experiment. The average payoff was US$2.57 ($SD = 1.24$), including a US$1.00 show-up fee. The minimum payoff was US$1.00, and the maximum was US$5.00.

3.4 Results

In this paper and the experiment presented in it, I test whether a pro-neutrality DEFAULT is effective in promoting more neutral behavior in managerial decisions involving *Risk* or *Uncertainty*. For this, a pro-neutrality DEFAULT is implemented in a risky and uncertain business venturing decision. I compare these groups (DEFAULT *Risk* and DEFAULT *Uncertainty*) with corresponding BASELINE groups on a between-subject level. Using non-parametric and parametric tests, the default's effectiveness under *Risk* and *Uncertainty* is investigated in Subsection 3.4.1. In addition, in Subsection 3.4.2, I test for the moderating effect of the domain of the business venture on the effectiveness of the DEFAULT using non-parametric and parametric methods.[41]

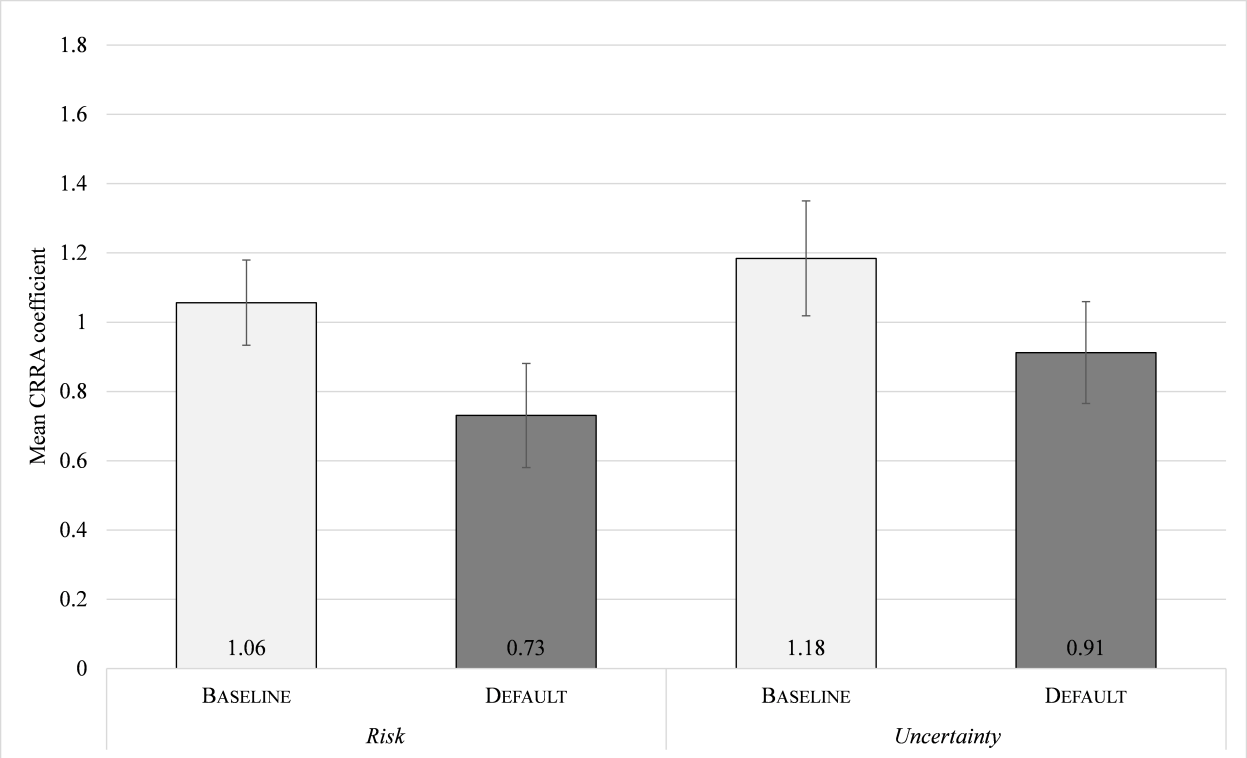
For the following analyses, I use the CRRA coefficient, calculated as described above and in more detail in Appendix 3.B. The CRRA coefficient allows measurement of the subject's deviation from the optimal decision in the specific situation. In other words, the CRRA coefficient provides a measure of how far off the subject is when compared with neutrality, as described by Herrmann (2017) and Stirling (2003). In the experimental design, the CRRA coefficient ranges from -0.09 (risk-loving) to 0 (risk-neutral) to 3.76 (very risk-averse).[42,43]

[40] More precisely, in the following chapter of this dissertation, the effect of a pro-neutrality recommendation nudge before and after making an initial decision in a risky business venturing situation is investigated. Further elements of the experimental design and procedure are the same despite the treatment manipulation and the additional domain featured in this paper. Subjects in POST-RECOMMENDATION *Risk* participate in an experimental variant that is the very same in the first part as BASELINE *Risk*. Therefore, these observations are also used as part of BASELINE *Risk*. I find no evidence that the decisions of the original BASELINE *Risk* group differ significantly from the initial BASELINE-like decisions in POST-RECOMMENDATION (Mann-Whitney test using the CRRA coefficient: $|z| = 1.598$, $p = 0.110$; and the reversed switching point: $|z| = 0.589$, $p = 0.556$). Therefore, throughout this paper, these observations are pooled. The presented results do not differ relevantly when excluding the BASELINE-like decisions of the POST-RECOMMENDATION treatment.

[41] Appendix 3.E provides an overview of the hypotheses and results discussed in the following subsections.

[42] Of the 247 subjects, 217 provided a decision for which the CRRA coefficient could be calculated. For a detailed explanation, see Appendix 3.B.

[43] The results presented here do not differ relevantly when using the reversed switching point of the subject in the multiple price list instead of the CRRA coefficient based on this switching point. Some points of interest are stated.



Note: Error bars represent the standard errors.

Figure 3.1: Level of Aversive Behavior in Business Venturing Decisions

3.4.1 Effect of a Default

Figure 3.1 shows the corresponding mean CRRA coefficient for all four groups. I first discuss the effect of the pro-neutrality DEFAULT under *Risk*. The mean CRRA coefficient in BASELINE *Risk* is 1.06 ($SD = 1.11$). Implementing a pro-neutrality DEFAULT rule under *Risk* reduces the mean CRRA coefficient to 0.73 ($SD = 1.03$). The difference is highly statistically significant (Mann-Whitney test: $|z| = 2.320$, $p = 0.020$). Using ordinary least squares regressions, I find supporting evidence for this difference. Table 3.2 shows the regression results of three models for each of the two domains. Model 1 features the effect of the pro-neutrality default in a risky environment with no additional controls. Model 2 adds additional questionnaires about personality traits and abilities as controls. Model 3 additionally adds sociodemographic information as controls.[44] While in Model 1 the 10% significance level for the effect of the DEFAULT is just barely missed ($p = 0.103$), the effect is supported in Models 2 and 3 ($p < 0.029$). For further robustness, I used the reversed switching point as the dependent variable, ranging from rather risk-loving to highly risk-averse between 0 and 11. Results of the ordered probit regressions using the reversed switching point can be found in Appendix 3.F. The effect of a DEFAULT under *Risk* is supported there in all three corresponding

[44]The results for all six models in Table 3.2 do not differ relevantly when using tobit regressions with the CRRA coefficient as the dependent variable and a lower limit (-0.09) as well as an upper limit (3.76).

models ($p < 0.030$). Putting all the results together, I find support for Hypothesis 3.1. A pro-neutrality DEFAULT under *Risk* reduces the aversive behavior in favor of more neutral behavior.

Result 3.1. *A pro-neutrality default promotes risk-neutral behavior in managerial situations under risk.*

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
		Risk			*Uncertainty*	
DEFAULT	−0.326	−0.442**	−0.515**	−0.272	−0.139	−0.104
	(0.198)	(0.198)	(0.204)	(0.221)	(0.207)	(0.223)
Constant	1.056***	2.062*	2.681**	1.184***	0.919	1.041
	(0.120)	(1.138)	(1.294)	(0.163)	(1.108)	(1.279)
Additional Questionnaires	NO	YES	YES	NO	YES	YES
Sociodemographics	NO	NO	YES	NO	NO	YES
R^2	0.021	0.165	0.248	0.017	0.296	0.347
Number of observations	129	129	128	88	88	86

Note: The dependent variable for all six models is the level of aversive behavior measured by the individual-specific CRRA coefficient. Standard errors are presented in parentheses. For Models 1 to 3, the reference group is BASELINE *Risk*. For Models 4 to 6, the reference group is BASELINE *Uncertainty*. In Models 2 and 5, additional controls are added. These controls include numeracy (Cokely et al., 2012), decision confidence, preference for reasoning (Butler et al., 2014), general risk aversion (Dohmen et al., 2011), tolerance for ambiguity (Herman et al., 2010), personal attitude towards entrepreneurship and entrepreneurial capacity (Liñán & Chen, 2009), and resistance to change (Oreg, 2003). In Models 3 and 6, sociodemographic characteristics are added as additional control variables. These characteristics are gender, age, employment status, educational level, household income, own contribution to household income, and the number of adults and children in the household. Model 3 includes a slightly smaller number of observations than Model 2 due to the exclusion of one subject who identified as neither female nor male. Likewise, Model 6 includes a slightly smaller number of observations than Model 5 due to the exclusion of two subjects who identified as neither female nor male. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.2: Results of Ordinary Least Squares Regressions

Now, I focus on the effect of the pro-neutrality DEFAULT in an environment featuring *Uncertainty*. The mean CRRA coefficient in BASELINE *Uncertainty* is 1.18 ($SD = 1.05$). Implementing a pro-neutrality DEFAULT rule under *Uncertainty* reduces the mean CRRA coefficient to 0.91 ($SD = 1.02$). This difference turns out not to be significant (Mann-Whitney test: $|z| = 1.351$, $p = 0.178$). Using ordinary least squares regressions (see Models 4 to 6 in Table 3.2), this lack of significance is also found in the parametric analysis. Again, Model 4 features the pure treatment effect, Model 5 adds additional personality controls, and Model 6 additionally features sociodemographic controls. In all three models, the effect of the pro-neutrality DEFAULT for an uncertain business venturing decision is not supported ($p > 0.221$). This holds when using the reversed

switching point as the dependent variable (see Appendix 3.F, $p > 0.219$). In summary, I find no support for Hypothesis 3.2. A pro-neutrality DEFAULT under *Uncertainty* seems not to significantly reduce aversive behavior, although the means of the two groups show a tendency in line with the corresponding hypothesized effect.

Result 3.2. *A pro-neutrality default does not promote uncertainty-neutral behavior in managerial situations under uncertainty.*

In summary, using non-parametric pairwise and parametric testing, I find support for the effectiveness of a pro-neutrality DEFAULT for risky business venturing decisions but not for uncertain business venturing decisions. The parametric analysis with the CRRA coefficients yields an average effect of the DEFAULT under *Risk* of 0.515 units of the CRRA coefficient holding other control factors constant (see Model 3 in Table 3.2). Regarding the total span of the CRRA coefficient in the experiment of 3.88 units, this indicates an average reduction in aversive behavior by 13.27%.

3.4.2 Moderating Effect of the Domain

In Hypothesis 3.3, I argued that when the business venturing decision features uncertain characteristics and, therefore, the decision takes place in the domain of *Uncertainty*, the effectiveness of a pro-neutrality DEFAULT is negatively affected. Therefore, I argued for a negative moderating effect for that domain. To investigate this, let us first consider the difference in the mean CRRA coefficient between domains in BASELINE. There is only a slight difference between domains in BASELINE (mean CRRA coefficient of BASELINE *Risk*: 1.06; BASELINE *Uncertainty*: 1.18), which turns out not to be significant (Mann-Whitney test: $|z| = 0.896$, $p = 0.371$). This finding replicates the previous findings of no difference or a minimal difference by Barham et al. (2014), Holm et al. (2013), and Koudstaal et al. (2016). Therefore, if the effect of the DEFAULT is stronger under *Risk* than under *Uncertainty*, I should find a significant difference between the mean CRRA of DEFAULT *Risk* (0.73) and DEFAULT *Uncertainty* (0.91). Indeed, there is weakly significant evidence that these two groups differ (Mann-Whitney test: $|z| = 1.650$, $p = 0.099$). This is slightly more pronounced when using the reversed switching point for this non-parametric test (Mann-Whitney test: $|z| = 1.708$, $p = 0.088$). In addition, as stated above, I find support by utilizing parametric and non-parametric testing that there is an effect of the pro-neutrality DEFAULT under *Risk* but not under *Uncertainty*.

Table 3.3 presents results of ordinary least square regressions investigating a potential moderating effect of the domain on the effectiveness of the DEFAULT. Model 1 features the effect of the DEFAULT, the influence of the domain (*Uncertainty*), and the moderating effect of the domain (DEFAULT$\times$*Uncertainty*). Again, Model 2 features additional personality controls, and Model 3 additionally controls for sociodemographic characteristics. In all three models, an effect of the pro-neutrality DEFAULT is supported ($p < 0.097$). As shown in Subsection 3.4.1, this result builds on the effectiveness of the DEFAULT under *Risk*. In line with the non-parametric test between BASELINE groups, no difference between *Uncertainty* and *Risk* in BASELINE is found in all three

models ($p > 0.533$). Regarding the moderating effect of the domain, the parametric analysis yields no support in any of the three models ($p > 0.458$). These results hold when using the reversed switching point in ordered probit regressions (see Appendix 3.G). For further robustness, I follow Good (2013) and Heckman et al. (2010) and use a permutation testing for the difference in effectiveness between the DEFAULT under *Risk* and *Uncertainty* compared to BASELINE observations. The procedure and graphical representations of the results are presented in Appendix 3.H. In line with the parametric and non-parametric analysis, the permutation test yields no support for a moderating effect of the domain (using the CRRA coefficient: $p = 0.873$; and the reversed switching point: $p = 0.633$).

	Model 1	Model 2	Model 3
DEFAULT	-0.326^{*}	-0.355^{*}	-0.398^{**}
	(0.195)	(0.190)	(0.197)
Uncertainty	0.128	0.086	0.078
	(0.205)	(0.199)	(0.204)
DEFAULT× *Uncertainty*	0.053	0.209	0.221
	(0.299)	(0.291)	(0.298)
Constant	1.056^{***}	1.536^{*}	1.884^{**}
	(0.117)	(0.818)	(0.915)
Additional Questionnaires	NO	YES	YES
Sociodemographics	NO	NO	YES
R^2	0.022	0.147	0.180
Number of observations	217	217	214

Note: The dependent variable for all three models is the level of aversive behavior measured by the individual-specific CRRA coefficient. Standard errors are presented in parentheses. For all three models, the reference group is BASELINE *Risk*. In Model 2, additional controls are added. These controls include numeracy (Cokely et al., 2012), decision confidence, preference for reasoning (Butler et al., 2014), general risk aversion (Dohmen et al., 2011), tolerance for ambiguity (Herman et al., 2010), personal attitude towards entrepreneurship and entrepreneurial capacity (Liñán & Chen, 2009), and resistance to change (Oreg, 2003). In Model 3, sociodemographic characteristics are added as additional control variables. These characteristics are gender, age, employment status, educational level, household income, own contribution to household income, and the number of adults and children in the household. Model 3 includes a slightly lower number of observations than Model 2 due to the exclusion of three subjects who identified as neither female nor male. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.3: Results of Ordinary Least Squares Regressions with Interaction

Overall, I lack clear evidence that the effect of a pro-neutrality DEFAULT is moderated by the underlying domain of the business venturing situation. Some evidence is provided by finding an effect under *Risk* but not under *Uncertainty*. However, additional research is needed to further

investigate the moderating relationship between pro-neutrality interventions and the underlying domain. Therefore, based on these results, I conclude that there is no moderating effect.[45]

Result 3.3. *Uncertainty in a managerial situation has no moderating effect on the effectiveness of a pro-neutrality default.*

3.5 Discussion, Implications, and Further Research

This paper investigates whether a pro-neutrality default is effective in promoting neutral behavior in risky and uncertain business venturing decisions. A positive effect from such a default rule is hypothesized for both domains. In addition, a negative moderating effect of uncertain features of a business venture and the respective underlying domain is expected. This paper offers three main results. First, a pro-neutrality default rule promotes more risk-neutrality. In other words, it reduces aversive behavior in favor of more neutral behavior in managerial situations under risk. Second, such a default rule seems to be ineffective in business venturing decisions under uncertainty: It lacks an effect in reducing aversive behavior in this domain. Third, I cannot report a significant moderating effect of uncertain characteristics of the underlying domain on the effectiveness of a pro-neutrality default. Some evidence is provided by finding an effect under risk but not under uncertainty. However, the corresponding analysis cannot support the moderating effect.

The effectiveness of the pro-neutrality default in managerial situations under risk is in line with previous success stories of defaults. For example, Böhm et al. (2021a), Thaler and Benartzi (2004), and Venema et al. (2018) find positive effects of defaults on employees' behavior in different situations. Defaults are very effective and helpful in promoting specific behaviors (Sunstein, 2014; 2019) and, at the same time, are often highly cost-efficient (Benartzi et al., 2017). This paper adds to this stream of research by providing a successful test of a pro-neutrality default improving managerial decision-making under risk. Although previous research holds many examples of when defaults are effective, their use for managerial guidance is, to the best of my knowledge, still rare (Ebert & Freibichler, 2017a; Milkman et al., 2009; Zhang & Cueto, 2017). Besides Ebert and Freibichler (2017b), very few have argued that nudging can be a viable tool for the monitoring board to guide managerial activity. While I find support for the effectiveness of a pro-neutrality default in a risky situation, the presented experiment is a stylized online experiment and does not cover any aspect of a principal-agent relationship between the shareholders, their monitoring

[45] Recent research by White et al. (2021) suggests that more decision time weakens the effectiveness of defaults. While not the primary focus of this experimental work, I also investigate the moderating effect of endogenous decision time. To be more precise, splitting the DEFAULT groups by participants with more and less decision time than the average participant in the corresponding BASELINE group yields a significant effect for the DEFAULT under *Risk* and no effect for the DEFAULT under *Uncertainty* for both subgroups, respectively. Comparing the two subgroups (above and below average decision time) of DEFAULT groups yields no significant difference in either domain. In addition, using the parametric Johnson-Neyman technique (for more details, see Spiller et al., 2013) indicates no moderating effect of the level of decision time on the effectiveness of the default. Therefore, while White et al. (2021) find such a moderating effect, previous work by Dinner et al. (2011) and Steffel et al. (2016) as well as the presented experimental results here indicate no support for this moderating effect.

board, and the managers actively leading the company. Further research may build on mine and offer additional robustness by applying field experimental methods.

Jachimowicz et al. (2019) find in their meta-analysis, that defaults are often effective, but some studies report a null effect. For example, Reiter et al. (2012) report that some defaults are ineffective in a healthcare environment. In line with this, I find that the pro-neutrality default has no effect on managerial behavior in situations under uncertainty. This lack of significance can be attributed either to the mere fact that there is no such effect or to the behavioral measure used in this experiment. Böhm et al. (2021b) argue that the measure might not be nuanced enough to detect small differences, which would also help to explain the lack of a significant difference between domains in the control condition. Regardless, there is no support for the effect of a pro-neutrality default under uncertainty. Practitioners need to be alerted that nudging, while it may be beneficial in many cases, cannot be used without experimentally testing its effect in the field. Simply taking a nudge, such as a pro-neutrality default, and implementing it might not serve the purpose but may result in no effect or, in extreme cases, even a backfiring effect (Bolton et al., 2020; Krijnen et al., 2017; Sunstein, 2017).

One reason for mixed findings in terms of the effectiveness of defaults lies in the moderating effects of factors in the decision environment. Here, the domain of the business venture is hypothesized as a moderating factor. Such a business venture can either be risky and, therefore, relatively assessable, or uncertain with extremely limited information. Hauser et al. (2018) argue that beliefs, barriers, and context play vital roles in the effectiveness of nudging. More specifically for defaults, Dinner et al. (2011) finds three channels of defaults (*endorsement*, *ease*, and *endowment*). With these three channels of defaults in mind, research and practice can try to anticipate the relative effectiveness of defaults in specific situations. I argue that the effect of the pro-neutrality default should be negatively moderated by uncertainty within business venturing situations, but this paper does not support this hypothesis. Additional research is needed to pinpoint moderating forces and the exact channel(s) that are moderated (positively and/or negatively). Although many researchers report economically relevant effects of nudges (Hummel & Maedche, 2019; Johnson et al., 2012; Sunstein, 2014; 2019; Thaler & Sunstein, 2008), including default nudges (Congiu & Moscati, 2022; Dinner et al., 2011; Jachimowicz et al., 2019), choice architects are still inaccurate in their estimations of the default effect (Zlatev et al., 2017). Further work needs to be directed towards choice architects, such as institutional investors and members of monitoring boards (Ebert & Freibichler, 2017b), making sure to improve the abilities of practitioners to successfully implement nudges, especially default rules (for practical approaches, see Böhm & Renz, 2022; Renz & Böhm, 2020).

In summary, a pro-neutrality default is effective in promoting more risk-neutral behavior by shifting the decision-maker towards more neutrality. The neutrality-promoting effect of such a default is not supported in uncertain situations. A moderating effect from the domain is also not supported. Further research should investigate the specific channels and potential moderating effects or test the current findings in real-life situations.

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Appendix 3.A Multiple Price List

Product	Traditional design	Modern design	
		High price	Low price
1	$2.00	$4.00	$2.00
2	$2.00	$4.00	$1.60
3	$2.00	$4.00	$1.30
4	$2.00	$4.00	$1.00
5	$2.00	$4.00	$0.80
6	$2.00	$4.00	$0.70
7	$2.00	$4.00	$0.60
8	$2.00	$4.00	$0.50
9	$2.00	$4.00	$0.40
10	$2.00	$4.00	$0.20
11	$2.00	$4.00	$0.00

Table 3.A.1: Multiple Price List

Appendix 3.B CRRA Coefficient

Calculation of the CRRA Coefficient. For this example of how to calculate the CRRA coefficient based on the switching point, it is assumed that a participant switches from the modern design to the traditional design at Product 5. Therefore, the utility from deciding on the traditional design for Product 5 must be at least as high as from deciding on the modern design, while the reverse applies for Product 4:

$$
\begin{array}{lrccl}
 & 0.5 \cdot u(4) + 0.5 \cdot u(0.8) & \leq & u(2) & \leq \; 0.5 \cdot u(4) + 0.5 \cdot u(1) \\
\Leftrightarrow & 0.5 \cdot \frac{4^{1-\gamma}-1}{1-\gamma} + 0.5 \cdot \frac{0.8^{1-\gamma}-1}{1-\gamma} & \leq & \frac{2^{1-\gamma}-1}{1-\gamma} & \leq \; 0.5 \cdot \frac{4^{1-\gamma}-1}{1-\gamma} + 0.5 \cdot \frac{1^{1-\gamma}-1}{1-\gamma} \\
\Leftrightarrow & 0.5 \cdot (4^{1-\gamma}-1) + 0.5 \cdot (0.8^{1-\gamma}-1) & \leq & 2^{1-\gamma}-1 & \leq \; 0.5 \cdot (4^{1-\gamma}-1) + 0.5 \cdot (1^{1-\gamma}-1) \\
\Leftrightarrow & 4^{1-\gamma}-1+0.8^{1-\gamma}-1 & \leq & 2 \cdot 2^{1-\gamma}-2 & \leq \; 4^{1-\gamma}-1+1^{1-\gamma}-1 \\
\Leftrightarrow & 4^{1-\gamma}+0.8^{1-\gamma} & \leq & 2 \cdot 2^{1-\gamma} & \leq \; 4^{1-\gamma}+1^{1-\gamma} \\
\Rightarrow & 0.65 & \lessapprox & \gamma & \lessapprox \; 1
\end{array}
$$

The CRRA coefficient is set to the lower bound of this interval. Participants who always choose the modern design are either risk-neutral or risk-loving. For practical reasons, their CRRA coefficient is set to -0.09 (Barham et al., 2014). Participants who always decide on the traditional design – in particular, those who decide for the very first product for a sure \$2.00 instead of at least \$2.00 with a chance of 4.00 – cannot be rationalized under the aspect of standard risk or uncertainty aversion. Their CRRA coefficient is ∞. The resulting CRRA coefficients, depending on the respective switching point in the multiple price list and the switching points themselves in the reversed order, are represented in Table 3.C.1.

Appendix 3.C Switching Point, CRRA Coefficient, and Reversed Switching Point

Switched at product	**CRRA coefficient**	**Reversed switching point**
1	∞	11
2	3.76	10
3	1.86	9
4	1	8
5	0.65	7
6	0.52	6
7	0.4	5
8	0.31	4
9	0.22	3
10	0.09	2
11	0	1
no switch	-0.09	0

Table 3.C.1: Constant Relative Risk Aversion Coefficients and Reversed Switching Points

Appendix 3.D Screens of the Online Experiment

In all treatments, the first screen is the same general introduction (see Figure 3.D.1).

In the BASELINE *Risk* treatment, participants are instructed for Task 1, as shown in Figure 3.D.2, Figure 3.D.3, and Figure 3.D.4. On the next screen, the participants in this treatment receive a reminder and the multiple price list (see Figure 3.D.5 and Figure 3.D.6). An error message is implemented for inconsistent behavior, as shown in Figure 3.D.7. This is also true for all other treatments. The screens provided here for BASELINE *Risk* are identical to those received by participants in POST-RECOMMENDATION *Risk* for the BASELINE-like decision.

Some of the instructions to participants for Task 1 are different in the BASELINE *Uncertainty* treatment. The differences are shown in Figure 3.D.8 and Figure 3.D.9. Under *Uncertainty*, the reminder is also changed (see Figure 3.D.10). Missing figures imply no changes compared to BASELINE *Risk*.

Some of the instructions to participants for Task 1 are different in the DEFAULT *Risk* treatment. The differences are shown in Figure 3.D.11. Missing figures imply no changes compared to BASELINE *Risk*. The DEFAULT also causes an adaptation of the reminder and the multiple price list (see Figure 3.D.12 and Figure 3.D.13).

Differences between BASELINE *Uncertainty* and BASELINE *Risk* due to the additional ambiguity are introduced to DEFAULT *Uncertainty* and DEFAULT *Risk*, respectively, in the same fashion. Additional screens for this treatment are available upon request.

In all treatments, the online experiment continues with Figure 3.D.14. Based on the domain, the decision confidence question is presented either as in Figure 3.D.15 or Figure 3.D.16. On the next screen, all participants receive the identical question about the decision mode (see Figure 3.D.17).

The next part of the online experiment is identical in all treatments. First, participants transition into Task 2 (see Figure 3.D.18). Second, instructions for Task 2 (see Figure 3.D.19) and Task 2 itself (see Figure 3.D.20) are provided . Then, the second questionnaire part is introduced (see Figure 3.D.21). On the following screens, the questionnaires are presented (for general risk aversion, see Figure 3.D.22; for tolerance for ambiguity, see Figure 3.D.23, Figure 3.D.24, Figure 3.D.25, and Figure 3.D.26; for entrepreneurial attitudes, see Figure 3.D.27 and Figure 3.D.28; for entrepreneurial capacities, see Figure 3.D.29 and Figure 3.D.30; and for resistance to change, see Figure 3.D.31 and Figure 3.D.32). On the following two screens, sociodemographic questions are asked (see Figure 3.D.33, Figure 3.D.34, Figure 3.D.35, Figure 3.D.36, and Figure 3.D.37).

For all participants, the online experiment ends with the last screen, see Figure 3.D.38.

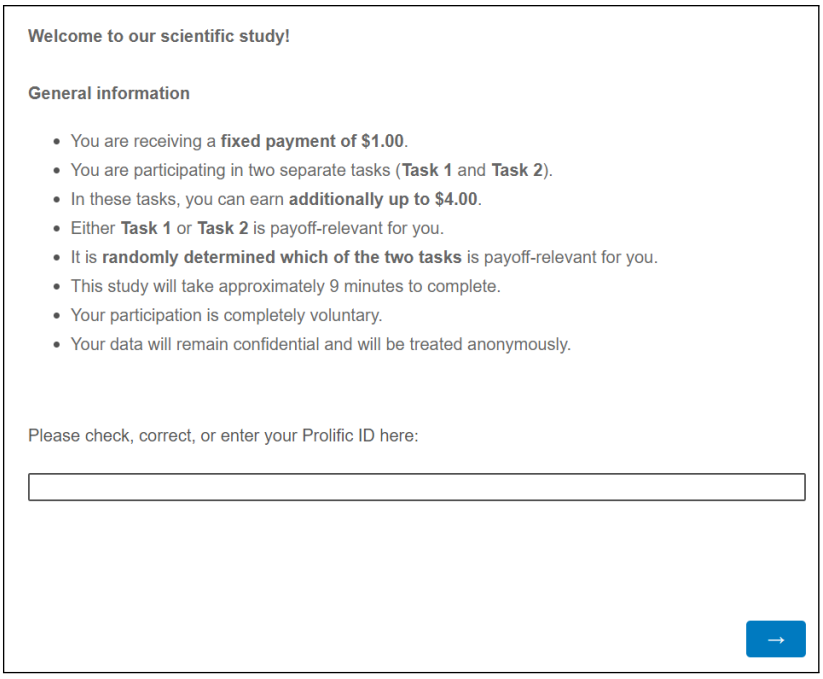
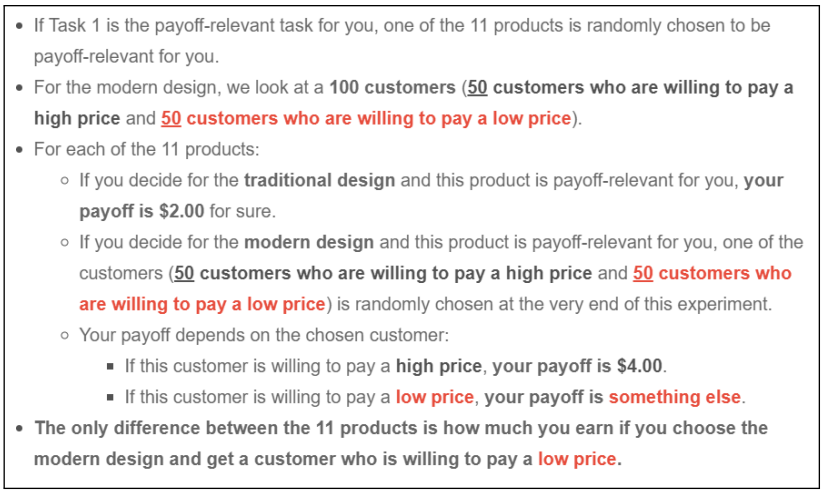


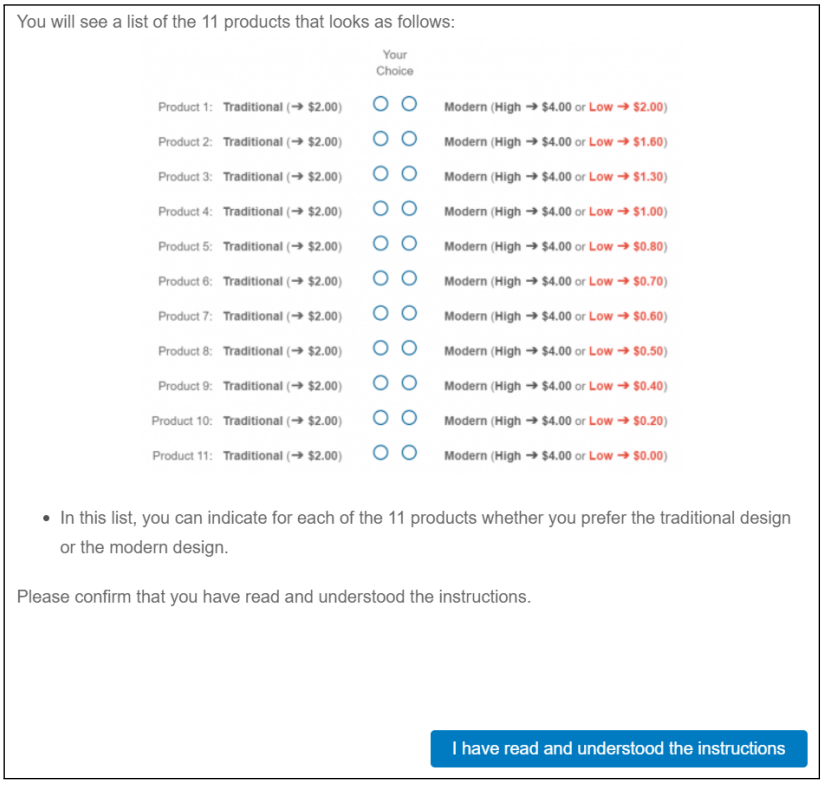
Figure 3.D.1: General Introduction for All Treatments

Figure 3.D.2: Task 1 Instructions for Baseline *Risk* 1/3



- If Task 1 is the payoff-relevant task for you, one of the 11 products is randomly chosen to be payoff-relevant for you.
- For the modern design, we look at a **100 customers** (**50 customers who are willing to pay a high price** and **50 customers who are willing to pay a low price**).
- For each of the 11 products:
  - If you decide for the **traditional design** and this product is payoff-relevant for you, **your payoff is $2.00** for sure.
  - If you decide for the **modern design** and this product is payoff-relevant for you, one of the customers (**50 customers who are willing to pay a high price** and **50 customers who are willing to pay a low price**) is randomly chosen at the very end of this experiment.
  - Your payoff depends on the chosen customer:
    - If this customer is willing to pay a **high price**, **your payoff is $4.00**.
    - If this customer is willing to pay a **low price**, **your payoff is something else**.
- **The only difference between the 11 products is how much you earn if you choose the modern design and get a customer who is willing to pay a low price.**

Figure 3.D.3: Task 1 Instructions for Baseline *Risk* 2/3



You will see a list of the 11 products that looks as follows:

		Your Choice	
Product 1:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $2.00)
Product 2:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $1.60)
Product 3:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $1.30)
Product 4:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $1.00)
Product 5:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.80)
Product 6:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.70)
Product 7:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.60)
Product 8:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.50)
Product 9:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.40)
Product 10:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.20)
Product 11:	Traditional (→ $2.00)	○	○ Modern (High → $4.00 or Low → $0.00)

- In this list, you can indicate for each of the 11 products whether you prefer the traditional design or the modern design.

Please confirm that you have read and understood the instructions.

I have read and understood the instructions

Figure 3.D.4: Task 1 Instructions for Baseline *Risk* 3/3

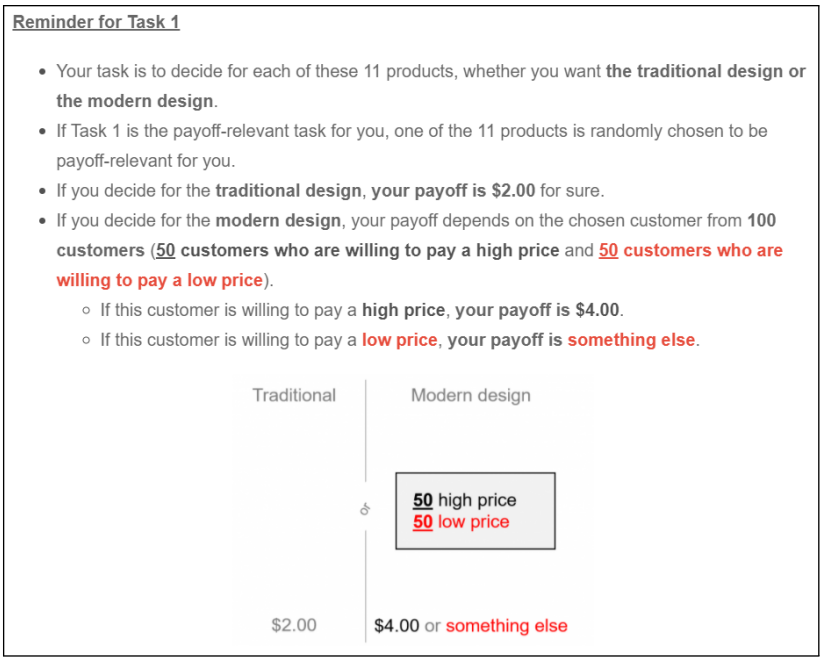


Figure 3.D.5: Task 1 for Baseline *Risk* 1/2

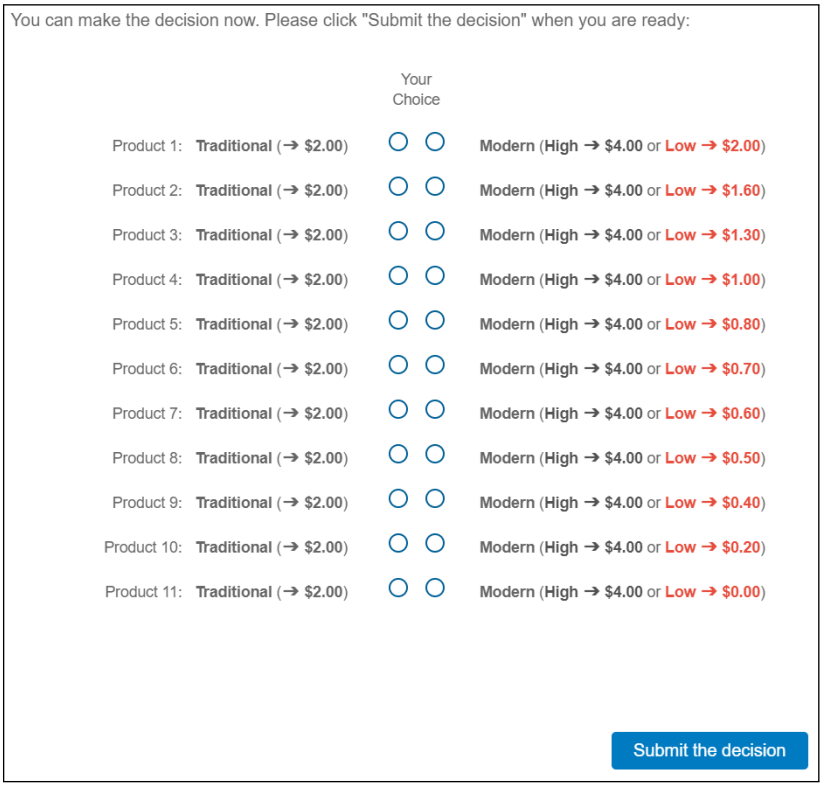


Figure 3.D.6: Task 1 for Baseline *Risk* 2/2

Figure 3.D.7: Error Message in Task 1 for Baseline *Risk*

Figure 3.D.8: Task 1 Instructions for Baseline *Uncertainty* 1/3

- If Task 1 is the payoff-relevant task for you, one of the 11 products is randomly chosen to be payoff-relevant for you.
- For the modern design, we look at a **100 customers** (an **unknown number of customers who are willing to pay a high price** and an **unknown number of customers who are willing to pay a low price**).
- For each of the 11 products:
  - If you decide for the **traditional design** and this product is payoff-relevant for you, **your payoff is $2.00** for sure.
  - If you decide for the **modern design** and this product is payoff-relevant for you, one of the customers (**?? customers who are willing to pay a high price** and **?? customers who are willing to pay a low price**) is randomly chosen at the very end of this experiment.
  - Your payoff depends on the chosen customer:
    - If this customer is willing to pay a **high price**, **your payoff is $4.00**.
    - If this customer is willing to pay a **low price**, **your payoff is something else**.
- **The only difference between the 11 products is how much you earn if you choose the modern design and get a customer who is willing to pay a low price.**

Figure 3.D.9: Task 1 Instructions for Baseline *Uncertainty* 2/3

Reminder for Task 1

- Your task is to decide for each of these 11 products, whether you want **the traditional design or the modern design**.
- If Task 1 is the payoff-relevant task for you, one of the 11 products is randomly chosen to be payoff-relevant for you.
- If you decide for the **traditional design**, **your payoff is $2.00** for sure.
- If you decide for the **modern design**, your payoff depends on the chosen customer from **100 customers** (an **unknown number of customers who are willing to pay a high price** and an **unknown number of customers who are willing to pay a low price**).
  - If this customer is willing to pay a **high price**, **your payoff is $4.00**.
  - If this customer is willing to pay a **low price**, **your payoff is something else**.

Figure 3.D.10: Task 1 for Baseline *Uncertainty* 1/2

Figure 3.D.11: Task 1 Instructions for DEFAULT *Risk* 3/3

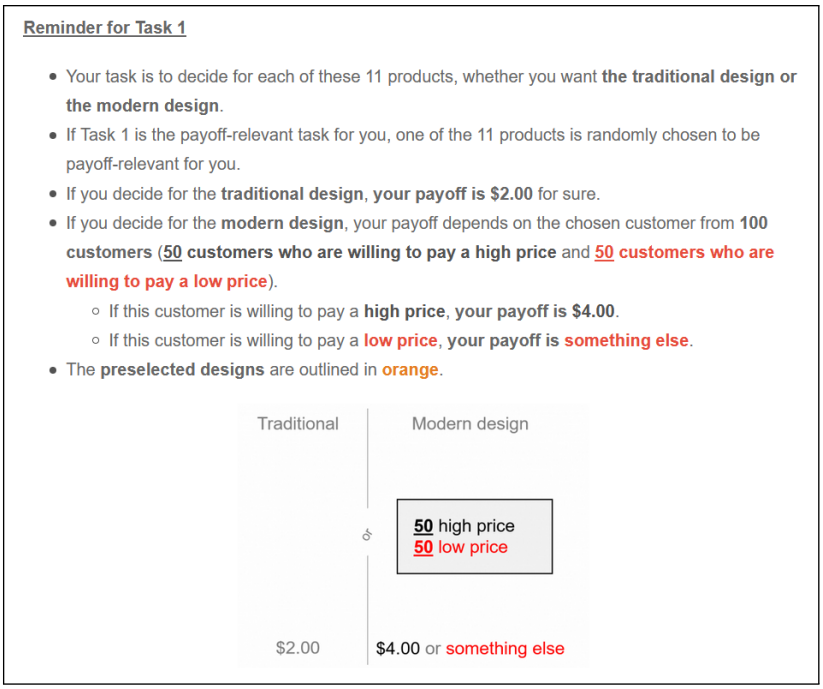


Figure 3.D.12: Task 1 for DEFAULT *Risk* 1/2

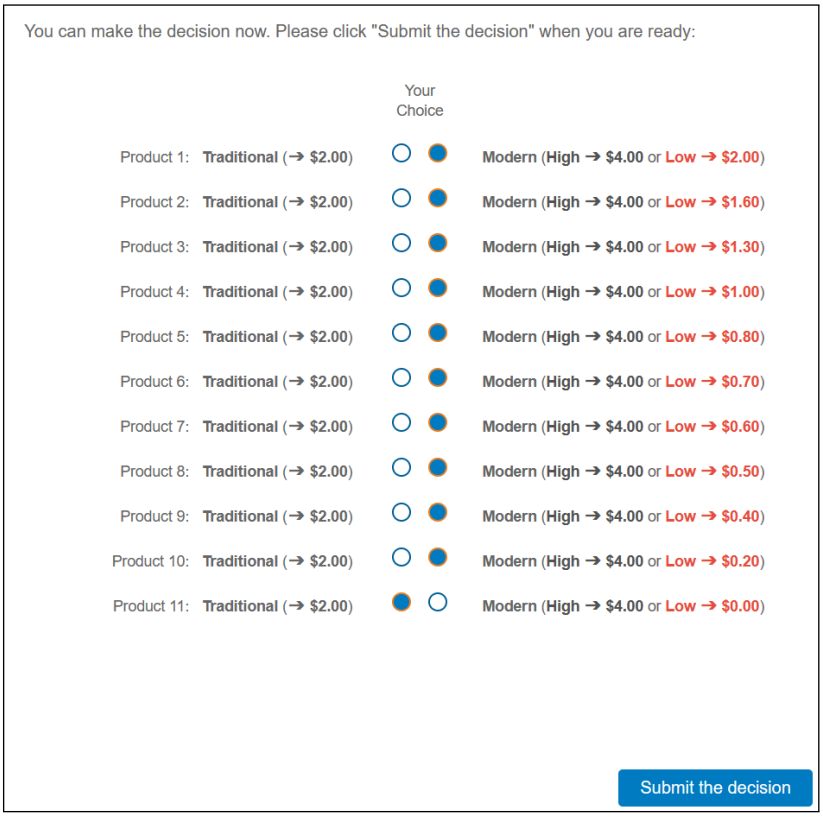


Figure 3.D.13: Task 1 for DEFAULT *Risk* 2/2

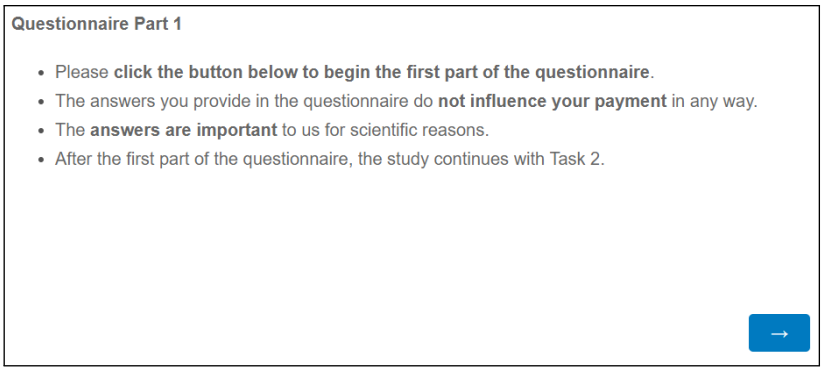


Figure 3.D.14: Introduction of First Questionnaire Part for All Treatments

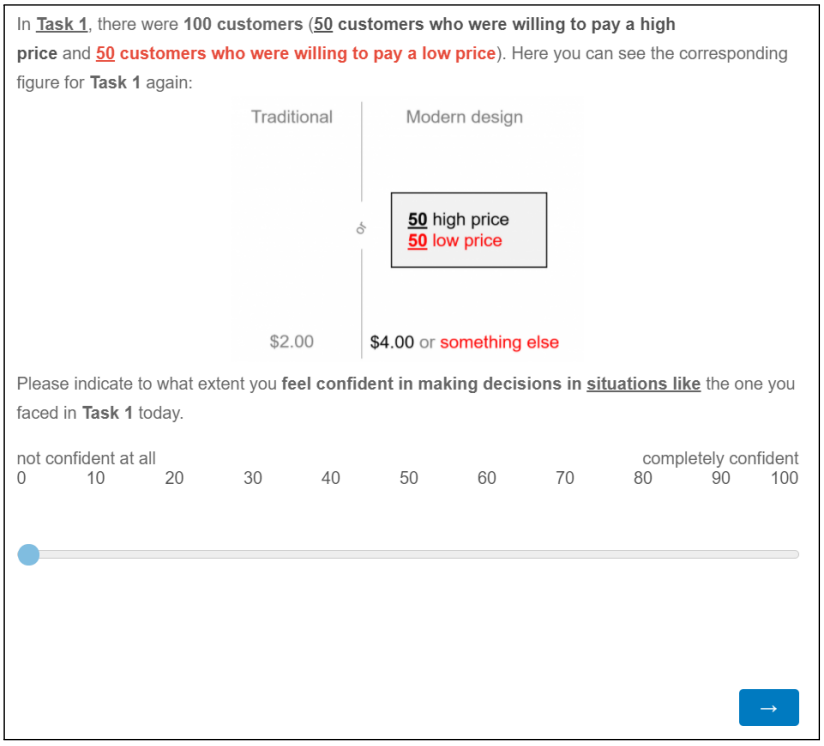


Figure 3.D.15: Question about Decision Confidence for All *Risk* Treatments

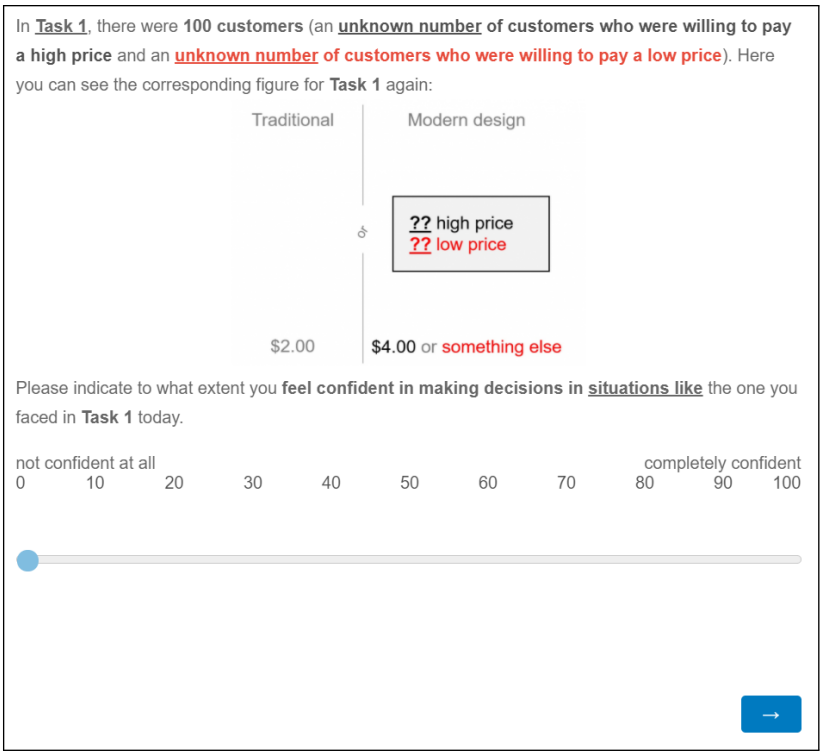


Figure 3.D.16: Question about Decision Confidence for All *Uncertainty* Treatments

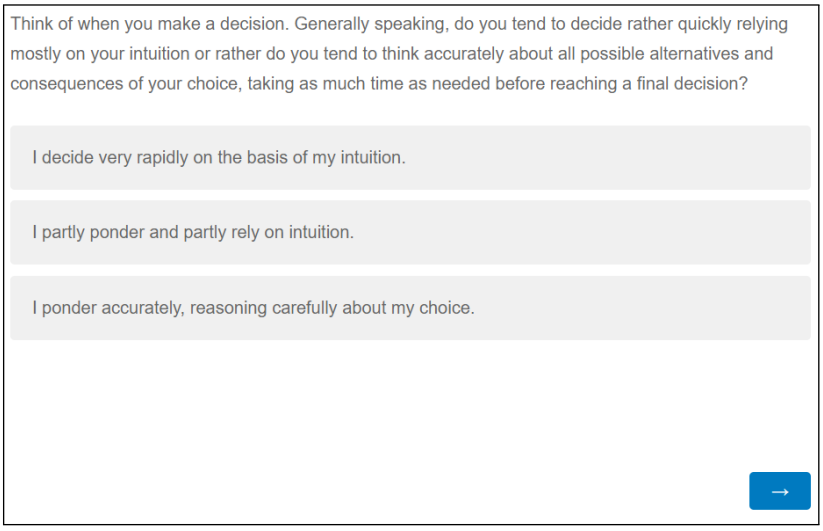


Figure 3.D.17: Question about Decision Mode for All Treatments

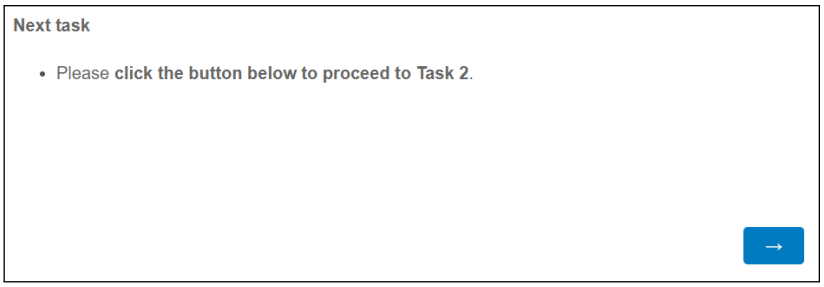


Figure 3.D.18: Transition from Task 1 to Task 2 for All Treatments

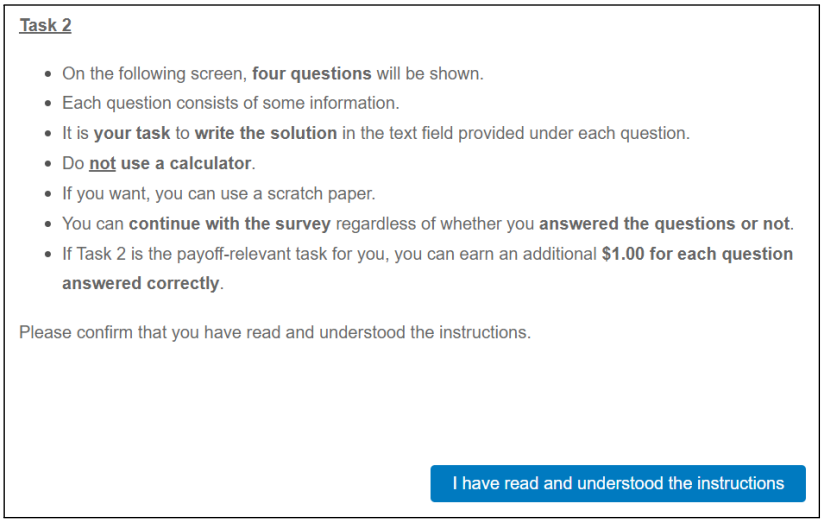


Figure 3.D.19: Task 2 Instructions for All Treatments

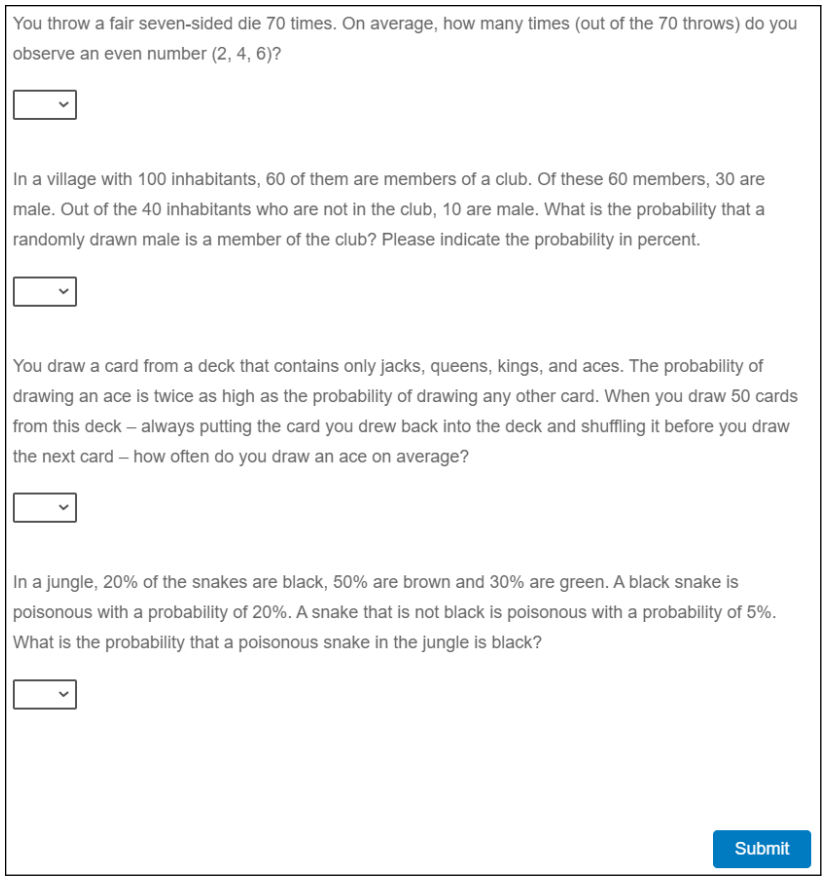


Figure 3.D.20: Task 2 for All Treatments

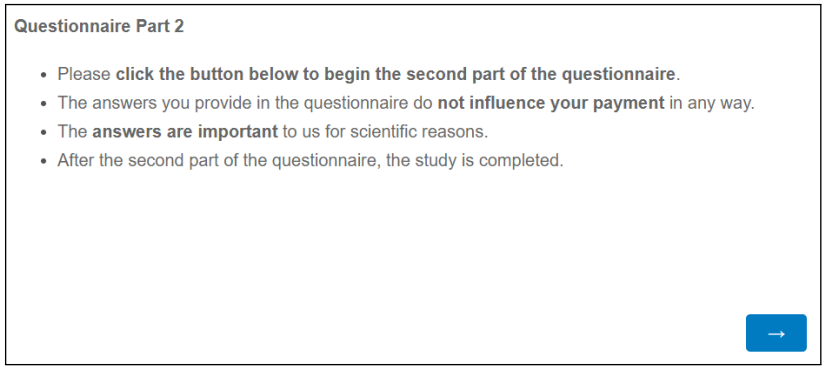


Figure 3.D.21: Introduction of Second Questionnaire Part for All Treatments

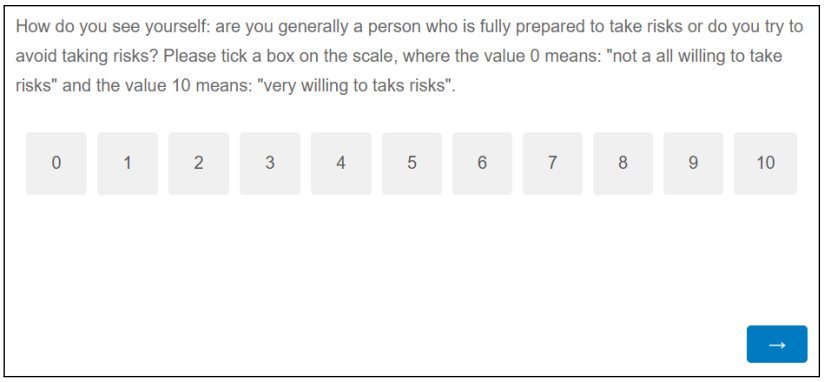


Figure 3.D.22: General Risk Aversion Questionnaire for All Treatments

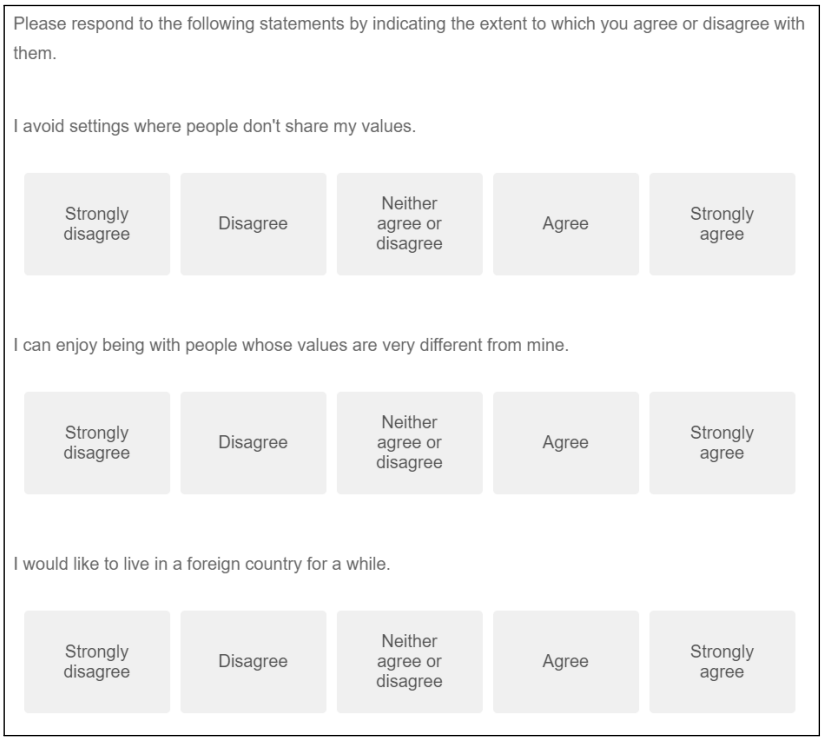


Figure 3.D.23: Tolerance for Ambiguity Questionnaire for All Treatments 1/4

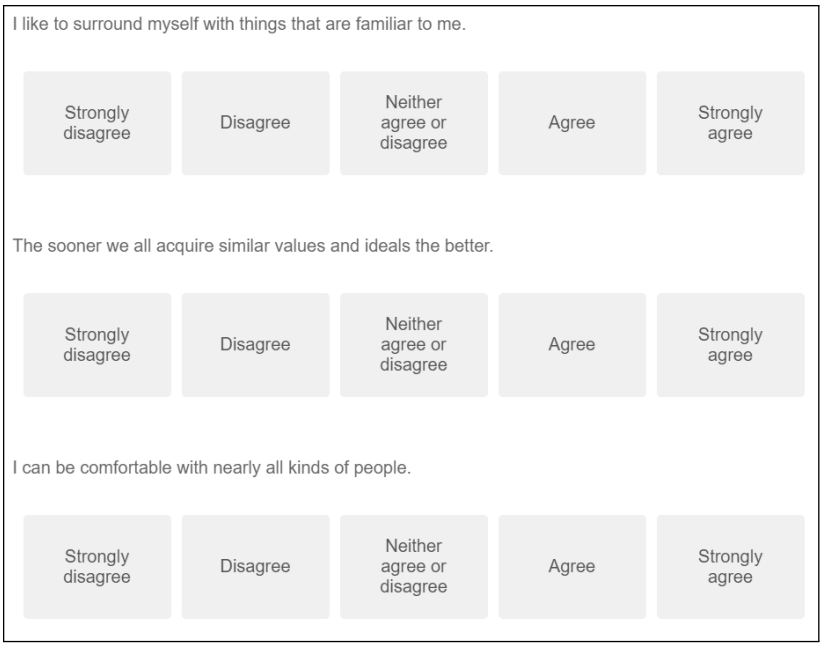


Figure 3.D.24: Tolerance for Ambiguity Questionnaire for All Treatments 2/4

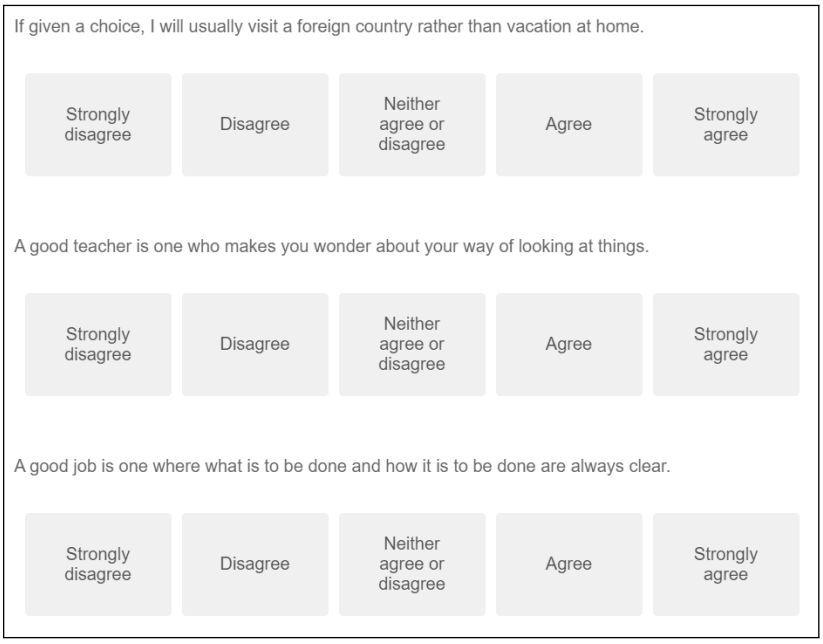


Figure 3.D.25: Tolerance for Ambiguity Questionnaire for All Treatments 3/4

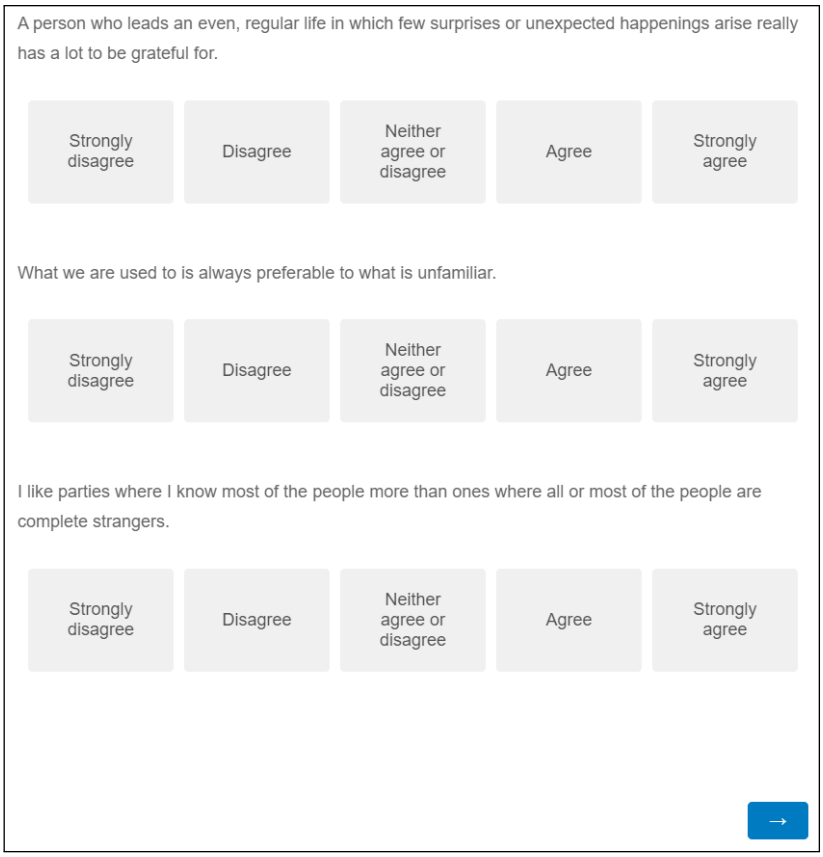


Figure 3.D.26: Tolerance for Ambiguity Questionnaire for All Treatments 4/4

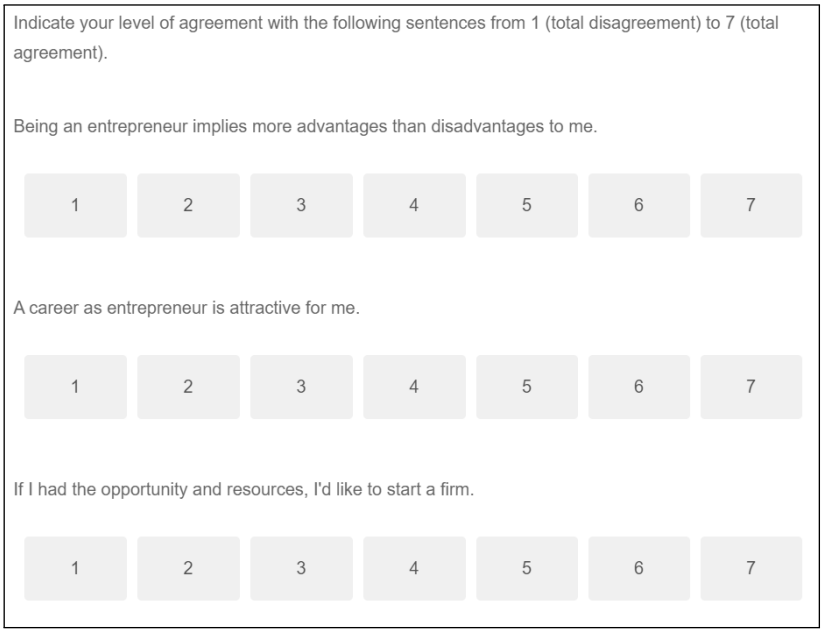


Figure 3.D.27: Entrepreneurial Attitude Questionnaire for All Treatments 1/2

Figure 3.D.28: Entrepreneurial Attitude Questionnaire for All Treatments 2/2

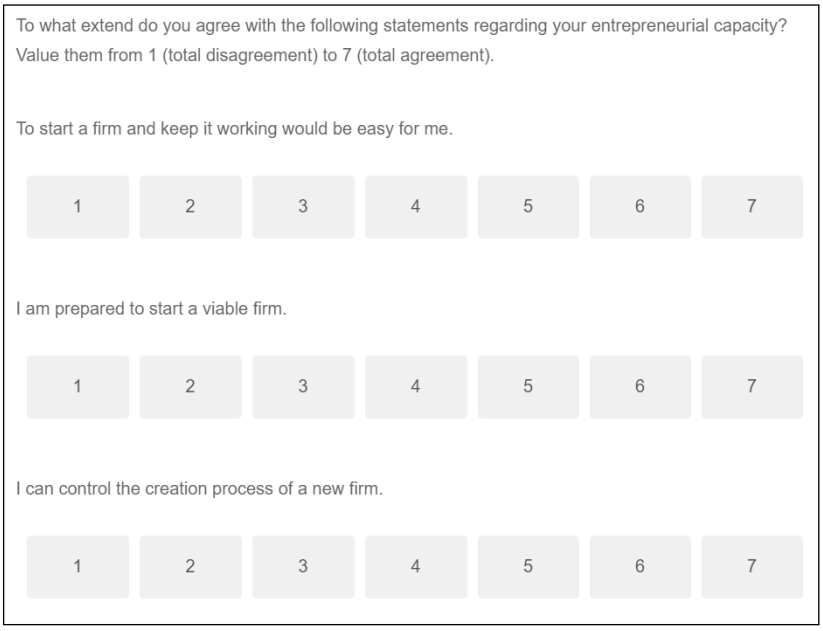


Figure 3.D.29: Entrepreneurial Capacity Questionnaire for All Treatments 1/2

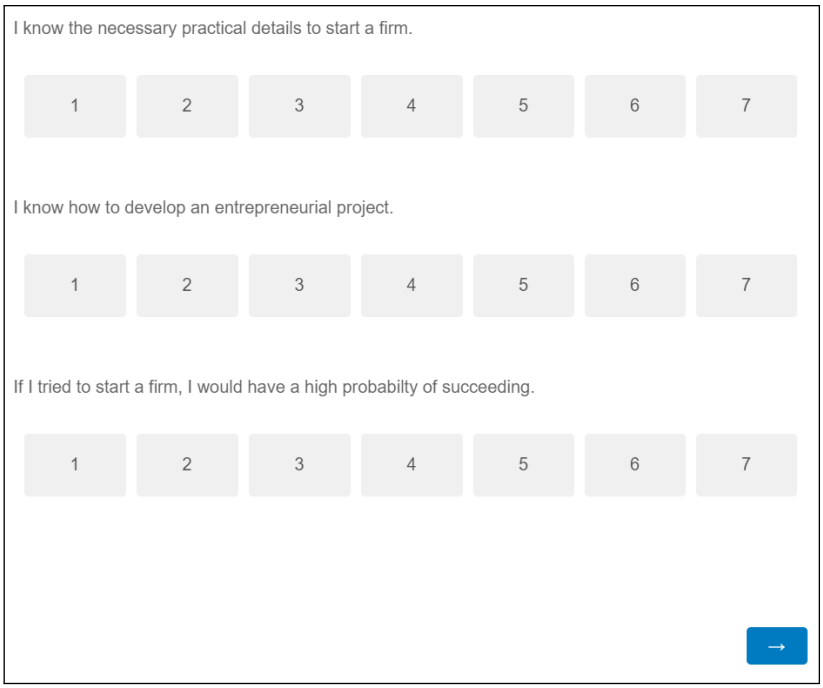


Figure 3.D.30: Entrepreneurial Capacity Questionnaire for All Treatments 2/2

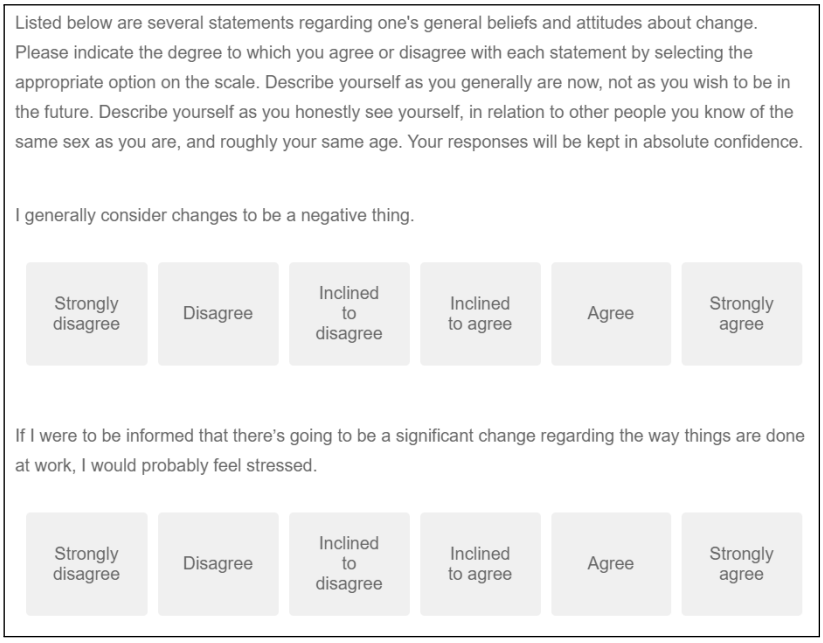


Figure 3.D.31: Resistance to Change Questionnaire for All Treatments 1/2

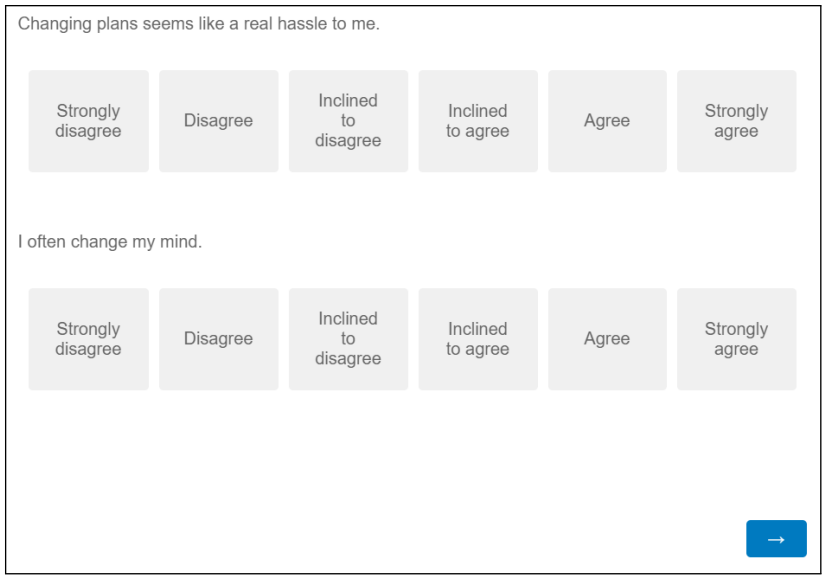


Figure 3.D.32: Resistance to Change Questionnaire for All Treatments 2/2

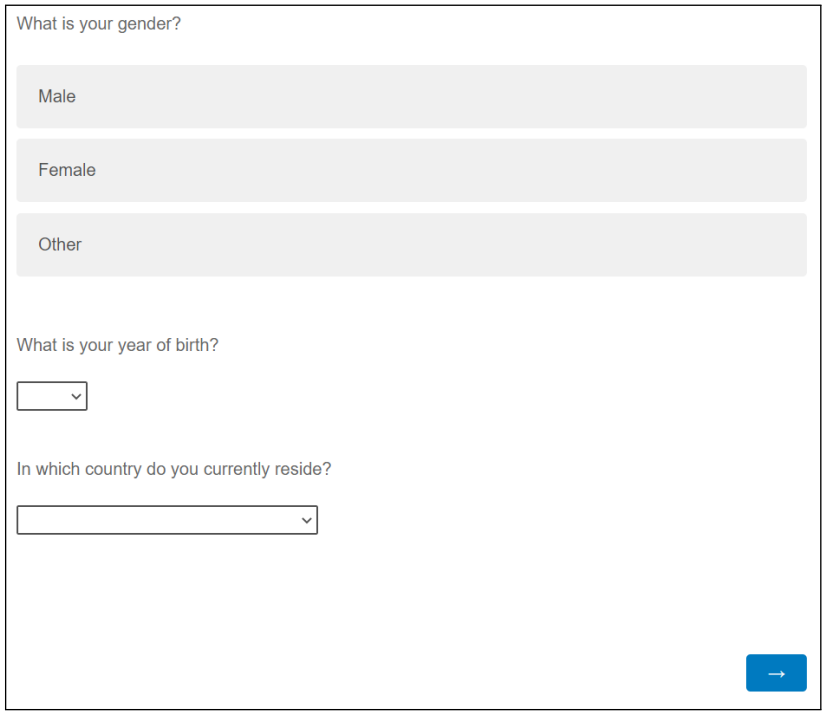


Figure 3.D.33: Sociodemographic Questions for All Treatments 1/5

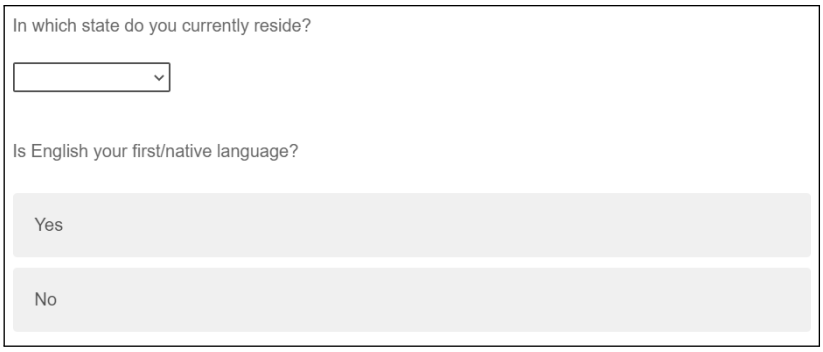


Figure 3.D.34: Sociodemographic Questions for All Treatments 2/5

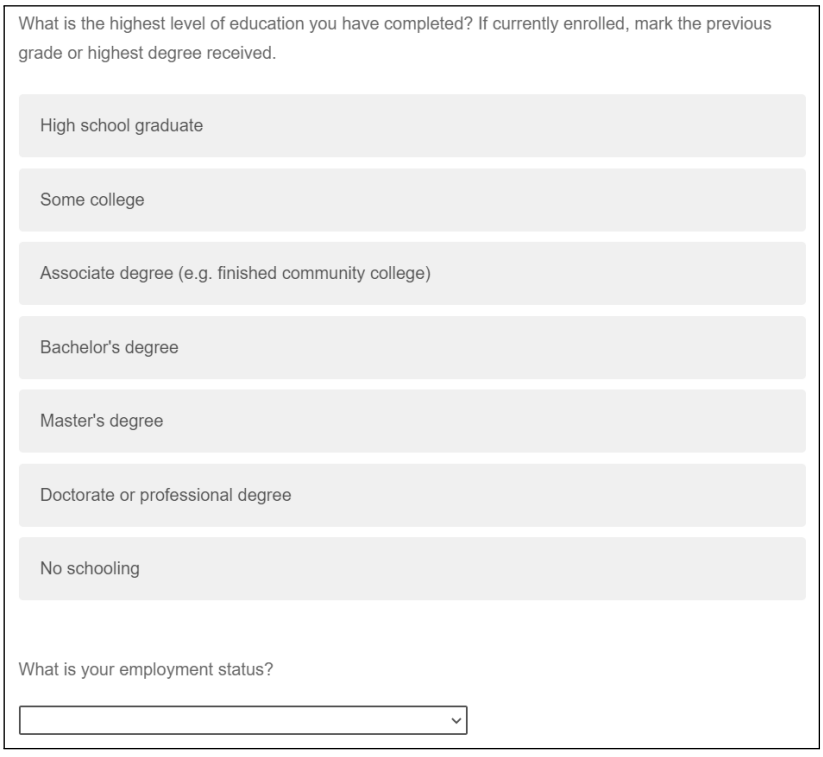


Figure 3.D.35: Sociodemographic Questions for All Treatments 3/5

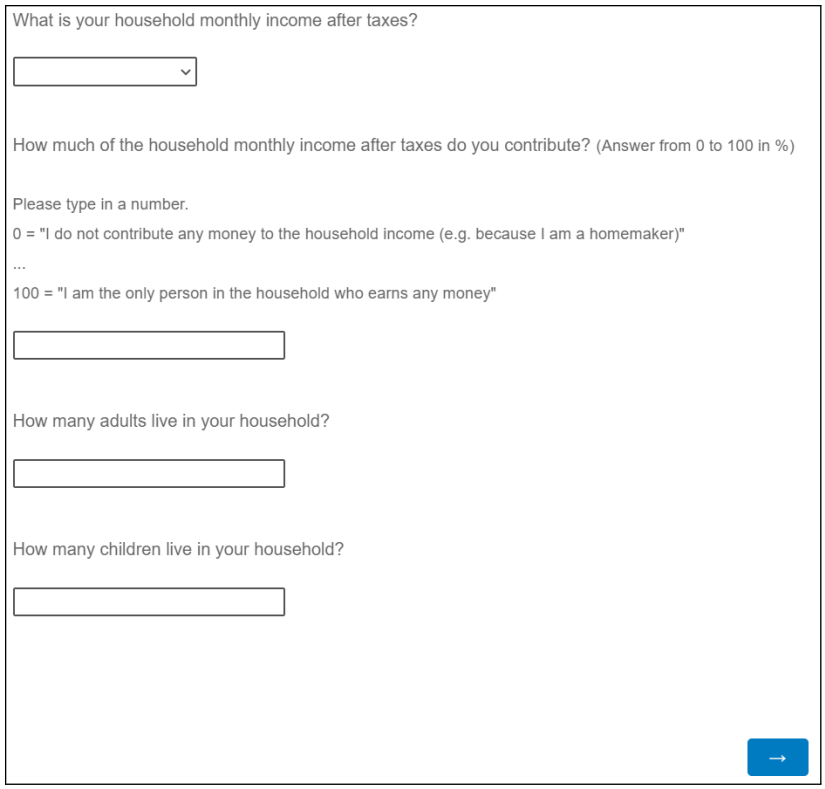


Figure 3.D.36: Sociodemographic Questions for All Treatments 4/5

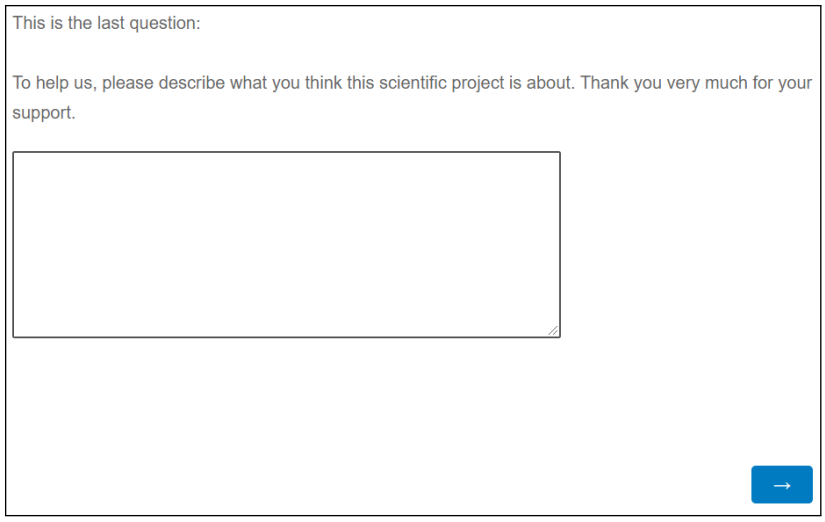


Figure 3.D.37: Sociodemographic Questions for All Treatments 5/5

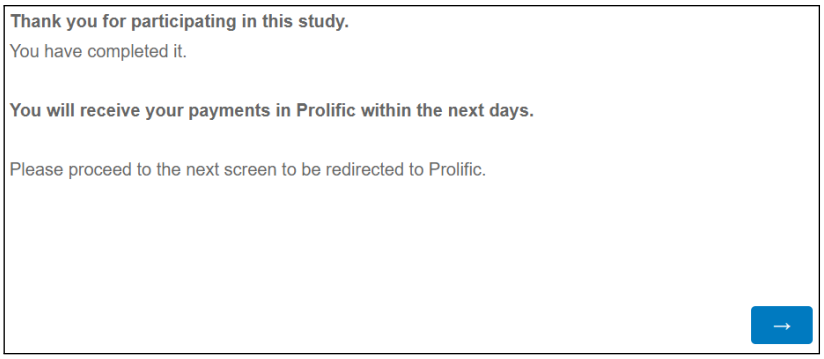


Figure 3.D.38: Last Screen for All Treatments

Appendix 3.E Summary of Hypotheses and Results

Number	Hypothesis	Result	Support
3.1	A pro-neutrality default promotes risk-neutral behavior in managerial situations under risk.	A pro-neutrality default promotes risk-neutral behavior in managerial situations under risk.	YES
3.2	A pro-neutrality default promotes uncertainty-neutral behavior in managerial situations under uncertainty.	A pro-neutrality default does not promote uncertainty-neutral behavior in managerial situations under uncertainty.	NO
3.3	Uncertainty in a managerial situation has a negative moderating effect on the effectiveness of a pro-neutrality default.	Uncertainty in a managerial situation has no moderating effect on the effectiveness of a pro-neutrality default.	NO

Table 3.E.1: Summary of Hypotheses and Results

Appendix 3.F Ordered Probit Regressions

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
		Risk			*Uncertainty*	
DEFAULT	-0.394^{**}	-0.526^{***}	-0.588^{***}	-0.254	-0.208	-0.188
	(0.180)	(0.186)	(0.195)	(0.208)	(0.216)	(0.225)
Constant	1.435^{***}	2.317^{**}	4.090^{***}	1.631^{***}	2.127^{*}	2.853^{**}
	(0.160)	(1.026)	(1.210)	(0.229)	(1.170)	(1.295)
Additional Questionnaires	NO	YES	YES	NO	YES	YES
Sociodemographics	NO	NO	YES	NO	NO	YES
Wald-χ^2	4.78	21.62	41.58	1.50	18.02	21.33
$p(\chi^2)$	0.029	0.010	< 0.001	0.220	0.035	0.212
Number of observations	147	147	146	100	100	98

Note: The dependent variable for all six models is the level of aversive behavior measured by the individual-specific reversed switching point. Standard errors are presented in parentheses. For Models 1 to 3, the reference group is BASELINE *Risk*. For Models 4 to 6, the reference group is BASELINE *Uncertainty*. In Models 2 and 4, additional controls are added. These controls include numeracy (Cokely et al., 2012), decision confidence, preference for reasoning (Butler et al., 2014), general risk aversion (Dohmen et al., 2011), tolerance for ambiguity (Herman et al., 2010), personal attitude towards entrepreneurship and entrepreneurial capacity (Liñán & Chen, 2009), and resistance to change (Oreg, 2003). In Models 3 and 6, sociodemographic characteristics are added as additional control variables. These characteristics are gender, age, employment status, educational level, household income, own contribution to household income as well as the number of adults and children in the household. Model 3 includes a slightly smaller number of observations than Model 2 due to the exclusion of one subject who identified as neither female nor male. Likewise, Model 6 includes a slightly smaller number of observations than Model 5 due to the exclusion of two subjects who identified as neither female nor male, $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.F.1: Results of Ordered Probit Regressions

Appendix 3.G Ordered Probit Regressions with Interaction

	Model 1	Model 2	Model 3
DEFAULT	-0.410^{**}	-0.520^{***}	-0.547^{***}
	(0.179)	(0.183)	(0.188)
Uncertainty	0.133	0.141	0.168
	(0.185)	(0.188)	(0.192)
DEFAULTx*Uncertainty*	0.167	0.331	0.354
	(0.272)	(0.278)	(0.283)
Constant	1.461^{***}	2.154^{***}	3.021^{***}
	(0.142)	(0.765)	(0.853)
Additional Questionnaires	NO	YES	YES
Sociodemographics	NO	NO	YES
Wald-χ^2	7.78	27.72	38.60
$p(\chi^2)$	0.051	0.004	0.005
Number of observations	247	247	244

Note: The dependent variable for all three models is the level of aversive behavior measured by the individual-specific reversed switching point. Standard errors are presented in parentheses. For all three models, the reference group is BASELINE *Risk*. In Model 2, additional controls are added. These controls include numeracy (Cokely et al., 2012), decision confidence, preference for reasoning (Butler et al., 2014), general risk aversion (Dohmen et al., 2011), tolerance for ambiguity (Herman et al., 2010), personal attitude towards entrepreneurship and entrepreneurial capacity (Liñán & Chen, 2009), and resistance to change (Oreg, 2003). In Model 3, sociodemographic characteristics are added as additional control variables. These characteristics are gender, age, employment status, educational level, household income, own contribution to household income, and the number of adults and children in the household. Model 3 includes a slightly smaller number of observations than Model 2 due to the exclusion of three subjects who identified as neither female nor male. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.G.1: Results of Ordered Probit Regressions with Interaction

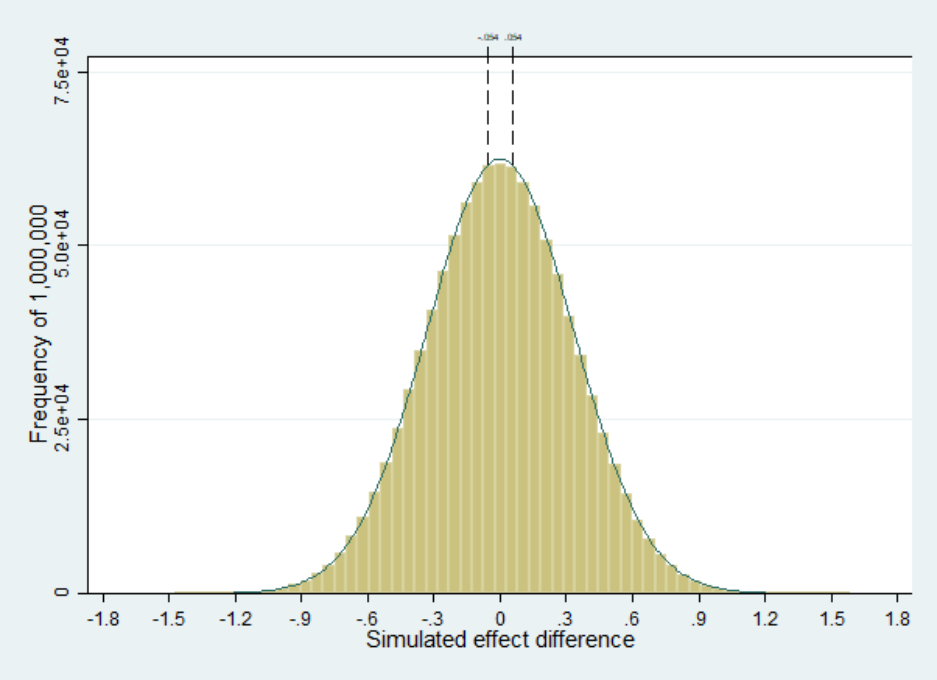
Appendix 3.H Outcomes of Permutation Tests

This appendix presents results of the simulation of differences under the null hypothesis that there is no difference in the effectiveness of the DEFAULT under *Risk* and *Uncertainty*. Put simply, I test the likelihood, assuming that the null hypothesis is true, of an outcome being as extreme as or more extreme than I observe in the experiment. For a detailed discussion of the method, see Good (2013) and Heckman et al. (2010).

I decided to create the distribution shown in Figure 3.H.1 by sampling 40 subjects from the BASELINE *Risk*, BASELINE *Uncertainty*, DEFAULT *Risk*, and DEFAULT *Uncertainty* groups, using their individual CRRA coefficients. Forty was chosen because it is the minimum number of observations for one specific group in a specific domain. The 40 subjects from BASELINE and DEFAULT are paired randomly and separately in each domain. As a next step, the difference for each pair is calculated and randomly re-assigned to originate from *Risk* or *Uncertainty*. This random re-assignment is what makes the distribution to be the one for when the null hypothesis is true. Afterward, the two means of all pair differences of these randomly assigned domains are subtracted and the final value is saved. I repeated these steps $1,000,000$ times. Therefore, the simulated values are the mean differences in effectiveness of a pro-neutrality DEFAULT placed either under *Risk* or *Uncertainty* and given that the domain does not matter.

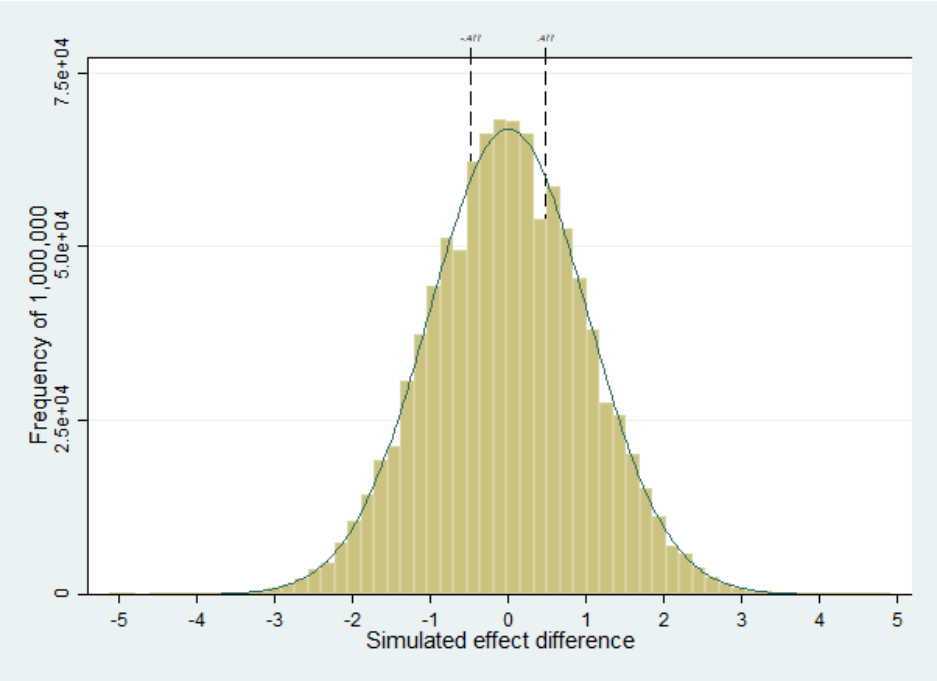
I test a two-sided alternative hypothesis stating that the effect of a DEFAULT is different under *Risk* than under *Uncertainty*. The observed value is $|0.054|$ units of the CRRA coefficient. This is the difference between the average differences between BASELINE *Risk* and DEFAULT *Risk* (approximately $1.06 - 0.73 = |0.33|$) as well as BASELINE *Uncertainty* and DEFAULT *Uncertainty* (approximately $1.18 - 0.91 = |0.27|$). The result indicates that the null hypothesis cannot be rejected ($p = 0.873$).

For further robustness, I use the same procedure described above but with the subjects' reversed switching points instead of their CRRA coefficients (see Figure 3.H.2). The observed value is $|0.477|$ steps of the reversed switching point. This is the difference between the average differences between BASELINE *Risk* and DEFAULT *Risk* (approximately $7.02 - 5.49 = |1.53|$) as well as BASELINE *Uncertainty* and DEFAULT *Uncertainty* (approximately $7.61 - 6.51 = |1.10|$). The result indicates that the null hypothesis cannot be rejected ($p = 0.633$).



Note: Permutation test simulation for when the null hypothesis is true. Individual-specific CRRA coefficient is used for the simulation. Absolute observed value marked. Corresponding two-sided test: $p = 0.873$.

Figure 3.H.1: Simulated Differences of Effectiveness Between Domains (CRRA Coefficient)



Note: Permutation test simulation for when the null hypothesis is true. Individual-specific reversed switching point is used for the simulation. Absolute observed value marked. Corresponding two-sided test: $p = 0.633$.

Figure 3.H.2: Simulated Differences of Effectiveness Between Domains (Reversed Switching Point)

Chapter 4

Recommendations to Guide Managerial Decision-Making – An Experimental Study Under Risk*

Kim Leonardo Böhm†, Dr. Marvin M. Müller‡, Dr. Erich Renz§

Abstract

Business venturing and other decisions in companies are risky endeavors. Many companies face the problem of their managers taking too little risk and being risk-averse beyond neutrality. We test a pro-neutrality, cognitively oriented recommendation nudge and find a positive effect regardless of when the recommendation is made. More specifically, we place the nudge intervention either prior to or after starting the decision-making process. In contrast to many other forms of nudging, recommendations come with the added benefit of helping with information processing, affecting System 2 thinking, and, therefore, allowing for easy application in real-life scenarios. In this paper, the results of an online experiment with a business venture frame are presented. Additional practical implications and avenues for further research are discussed. In summary, pro-neutrality recommendations might be useful for aiding managerial decision-making towards less risk aversion and, therefore, less forgone potential profit.

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4.1 Introduction

Managers often face decisions under risk, for example, when deciding on investments, product developments, and business ventures (Hoskisson et al., 2017). In many cases, companies behave too risk-averse and lose profits due to a lack of risk-neutral managerial behavior (Christensen, 1997; Lovallo et al., 2020; Markides, 2006; Milkman et al., 2009; Sauner-Leroy, 2004). In this paper, we investigate how managerial risk neutrality can be promoted. Such neutral behavior can be described as "[conforming] to a set of rules that govern behavior" (Stirling, 2003, p. 4) or – more specifically for risky situations – as "[obeying] the laws of probability" (Herrmann, 2017, p. 2).[46]

Previous research highlights the potential of incentives for governing risk-taking (Gigerenzer, 2014). While this approach might promote neutral behavior, it tampers with the manager's decision space, potentially limiting her ability to react to unforeseen market developments and crises appropriately. In the previous chapter of this dissertation, a nudge in the form of a pro-neutrality default rule is experimentally tested in risky and uncertain business venture situations. Böhm (2021) finds a positive effect for a pro-neutrality default rule under risk but not under uncertainty. However, he could not clearly support a moderating effect for the underlying domain experimentally introduced by uncertain characteristics of the decision. Coming from a practitioner's perspective, another type of nudge discussed is a recommendation. For example, Kahneman et al. (2011) show how recommendations can be potent for important and substantial managerial decisions. Harvey and Fischer (1997) demonstrate that people like to take advice from others, partly to share responsibility and partly to improve their decision-making. However, to the best of our knowledge, there is no experimental work on whether a pro-neutrality recommendation is indeed effective in promoting more neutral behavior in risky situations. Research on decision aids to establish neutrality in risky decision-making is a current research gap. For instance, Zhang and Cueto (2017) deduce from a systematic literature review that general management research lacks evidence of how decision aids can help with risk aversion. Errors in managerial decision-making, for instance, when not obeying the laws of probability and therefore forgoing potential profit, are costly (Milkman et al., 2009). Therefore, there is a great need for such research and its transfer to managerial practice.

In this paper, we test the effectiveness of a pro-neutrality recommendation for managerial decisions under risk. The nudging goal is to promote more neutral behavior in managerial situations. Some might believe that a pro-neutrality recommendation might not always be favorable for the manager deciding on the matter at hand, potentially risking her bonus payment or reputation in the process. There is a reason why such recommendations are still feasible and ought to be considered. Even if it is "a nudgy recommendation" (Karlsen & Andersen, 2019, p. 1) because it challenges the nudgee to choose behavior that is different from what she would otherwise do, it also "change[s] the behavior ... for a greater common good" (Karlsen & Andersen, 2019, p. 1). Similarly, while it

[46] For reasons of readability, we substitute risk neutrality with "neutrality" and risk aversion with "aversion". Likewise, we use "aversive behavior" for behavior showing risk aversion and "neutral behavior" for behavior showing no risk aversion. For the purposes of this paper, neutral behavior is optimal in the sense of the definitions provided by Herrmann (2017) and Stirling (2003), while aversive behaviors are acts that deviate from this optimum.

might not be in the best interest of the manager personally, it certainly benefits her company and, therefore, herself in the long run.

In real-life situations, many recommendations may materialize after a manager learns about the decision at hand or even after she makes an initial decision. In such cases, the decision-making process starts prior to learning about and considering the content of the recommendation. Therefore, the decision-maker may perceive her preliminary decision as the status quo when the recommendation materializes. If the recommendation materializes after an initial decision is made, the potential effect of the recommendation may suffer from a status quo bias (Kahneman et al., 1991; Samuelson & Zeckhauser, 1988). We utilize a business-venture-framed online experiment with three groups, either with no behavioral intervention, with a pro-neutrality recommendation prior to starting the managerial decision-making process, or with such an intervention after initially deciding on the situation at hand. In our experiment, we find supporting evidence that a pro-neutrality recommendation is effective in promoting more neutral behavior, regardless of whether it is provided prior to starting the decision-making process or after an initial decision is made. In summary, we can support a positive effect of a pro-neutrality recommendation and find no evidence that this effect suffers from a status quo bias when the recommendation is provided later.

4.2 Hypothesis Development

There is comprehensive literature emphasizing that managers are still human, and, therefore, their behavior and decision-making are not flawless (Teal, 1996). We propose that situations featuring risk are difficult to anticipate and to practice in advance (Thaler & Sunstein, 2008), which is why managers often have to apply rules of thumb to navigate their business decisions (Mousavi & Gigerenzer, 2014). Although it is often unavoidable and efficient to use intuition and heuristics (Gigerenzer, 2007), relying too much on gut feelings may lead to judgment that deviates from neutrality based on rapid, but not necessarily well-considered, final evaluations (Kahneman, 2003). In addition, the literature shows that aversive behavior emerges when decisions involve high levels of complexity (Schwenk, 1984; Simon et al., 2000). Moreover, humans are, in general, averse towards risk (Dohmen et al., 2011; Holt & Laury, 2002). Such aversive behaviors may be due to a fear of loss (e.g., the manager prefers forgoing the chance to win a lot in order to not risk and potentially lose a little Kahneman & Tversky, 1979), an aversion to future regret (e.g., the manager wishes she had not made the investment back then Zeelenberg & Beattie, 1997), or a blame avoidance (e.g., the manager does not want to be held responsible if the investment does not pay off Hood, 2007). All of these sources are potentially reflected in managerial aversive behaviors (Hoskisson et al., 2017). In summary, a manager may consciously or unconsciously exhibit aversive behavior under risk.

We suggest that nudges improve managerial decisions by promoting less aversive behavior towards risk. Nudging is particularly suited to the case of managerial decision-making because it preserves the manager's freedom of choice and does not tamper with the manager's economic in-

centives but solely and softly steers her in a specific direction (Thaler & Sunstein, 2008). In this respect, recommendation nudges have proven to be effective nudges containing information a subject must otherwise acquire through additional efforts, such as learning or experience. Beyond that, recommendations can also help make information easier to process through supplementary labels or symbols. Cadario and Chandon (2020) present a fitting meta-analysis that supports the effectiveness of recommendations. In addition to other factors, they investigate three manifestations of recommendations: descriptive labels, evaluative labels, and visibility enhancements. The pro-neutrality recommendation tested in this paper features a descriptive label that borrows some characteristics of an evaluative label and includes a visibility enhancement (for more details, see Subsection 4.3.3). All of these are considered to be cognitively-oriented (Cadario & Chandon, 2020). A recommendation using these types of manifestations can be considered to affect System 2 thinking (for a more detailed discussion of the concept of two systems of thinking and dual-process theory, see Kahneman, 2011; Stanovich & West, 2000; Tversky & Kahneman, 1983; for more on how this influences guidance for managerial decision-making, see Böhm, 2021; Milkman et al., 2009).

Studies utilizing recommendations across disciplines indicate positive effects. Examples include highlighting positive product attributes to reduce hidden costs (Newell & Siikamäki, 2014), emphasizing losses of the non-preferred alternative versus gains of the preferred alternative in enrollment programs (Keller et al., 2011), disclosing information to promote a more beneficial approach when lending money (Bertrand & Morse, 2011), and promoting collaboration in a public good game (Barron & Nurminen, 2020). However, there are also situations in which recommendations do not promote behavior in line with the nudging goal (Böhm et al., 2021). For risky decisions, a pro-neutrality recommendation featuring a descriptive label with an evaluative function may help the decision-maker to understand the rationale in the specific situation. It guides her behavior further towards neutrality. In addition, such a label highlights the optimal choice, making the recommendation feature a visibility enhancement. This recommendation is neutral in the sense that the recommended and highlighted choice reflects risk-neutrality and is most beneficial for the decision-maker. In summary, such a recommendation with cognitively oriented features helps the manager in her System 2 thinking to decide about the risky choice at hand. This hypothesized positive effect of a pro-neutrality recommendation is captured in Hypothesis 4.1.

Hypothesis 4.1. *A pro-neutrality recommendation that is provided before making a decision promotes risk-neutral behavior in managerial situations.*

So far, we have argued that the recommendation materializes and is considered by the manager prior to learning about the risky situation. However, in practical terms, a pro-neutrality recommendation may often arrive after the manager has learned about the situation or even after she has started her decision-making process and has reached an initial decision. While most of the aforementioned experiments feature a recommendation prior to starting the decision-making process (Barron & Nurminen, 2020; Keller et al., 2011; Newell & Siikamäki, 2014), others place the recommendation after the participants have learned about the situation and decisions involved (Bertrand & Morse, 2011; and partly Böhm et al., 2021). Maybe most prominently, recommenda-

tions for healthy eating are an example of nudging after the decision-making process has started and, sometimes, even after an initial decision has been made (Cadario & Chandon, 2020). For example, Thorndike et al. (2012) show a significant effect of evaluative labels even for food-related decisions that are, in part, habitual. Therefore, it seems very likely that a pro-neutrality recommendation can still be effective when it arrives after the decision-maker has started the decision-making process and come to an initial decision (see Hypothesis 4.2).

Hypothesis 4.2. *A pro-neutrality recommendation that is provided after making an initial decision promotes risk-neutral behavior in managerial situations.*

While recommendations have been found to be effective in other situations (e.g., healthy eating) both before and after the decision-making process has started, there is reason to believe that their effectiveness varies. This is especially true because risky decision-making is an analytical process (Moore & Eckel, 2006; Ross et al., 2012), and the effectiveness may depend on the process's starting value (Tversky & Kahneman, 1974). Therefore, if the decision-making process in a risky situation is started prior to the materialization of the pro-neutrality recommendation, a current state of this process free of the potential influence of such a recommendation is reached. This current state serves as an anchor or status quo when the pro-neutrality recommendation arrives later (Kahneman et al., 1991; Samuelson & Zeckhauser, 1988; Silver & Mitchell, 1990). In line with this argumentation, Nebel (2015) describes how managers prefer to operate within known approaches and rule out all alternative routes. Furthermore, once managers have gained access to a successful strategy, there may be no need for them to make variations to, for example, their product portfolio or to change their business strategy: They ignore further opportunities and risks, trusting in their functioning modus operandi (Biyalogorsky et al., 2006). This being the case, it seems likely that the effectiveness of a pro-neutrality recommendation in promoting risk-neutral behavior suffers from the (premature) start of the decision-making in risky situations. The current, potentially risk-averse state of the decision-making process may serve as an opposing force to the pro-neutrality recommendation. We capture this hypothesized difference in effectiveness in Hypothesis 4.3.

Hypothesis 4.3. *A pro-neutrality recommendation that is provided before making a decision has a stronger effect in promoting risk-neutral behavior than a pro-neutrality recommendation that is provided after making an initial decision in managerial situations.*

4.3 Experimental Design

4.3.1 General Overview

We conducted an online experiment featuring two tasks (Task 1 and Task 2), from which one is randomly selected to be payoff-relevant for the subject. Task 2 consists of additional controls and is described in Subsection 4.3.4. To investigate the neutrality-promoting effect of pro-neutrality recommendations in managerial situations under risk, Task 1 resembles such a managerial decision,

allowing the subject to deviate from the optimum. We utilize a multiple price list and add a business venture frame to mirror a corresponding managerial decision (for a similar experimental measure and framing, see Böhm, 2021; for a model explaining that the framing and other factors are potentially influencing the effect of nudges, see Hauser et al., 2018).[47]

Here, we test two different treatments against the corresponding control group in the business venturing context. These treatments are a pro-neutrality recommendation nudge that is placed before an initial business venturing decision is made (Pre-Recommendation) and a nudge of the same kind that is placed after the subject in the role of a manager has made an initial decision (Post-Recommendation). For these treatment manipulations, several potential interpretations are apparent. First, the different timing of the recommendations might describe two distinct types of employees. A more outspoken and proactive employee might provide her recommendation – which is optimal due to her being an expert and not directly dependent on the outcome – before the the manager starts making up her mind. A less outspoken, maybe even passive, employee might provide her recommendation later when she realizes that an initial decision has been made, and at that time, her recommendation may be useful or it may not. Second, the different types of recommendations might also reflect how hierarchical a company is. On the one hand, a flat organization that works like a start-up company might trigger earlier recommendations from employees to decision-makers; on the other hand, a distribution of power favoring high-power figures like founders, executives, or managers and featuring a low-powered workforce might negatively influence employees' willingness to pro-actively provide recommendations, especially when the boss' decision (or opinion) is still unknown (Huang et al., 2005).

With this variation in when a recommendation nudge could materialize, three groups of subjects are formed based on the level of intervention in this 3×1 between-subject design. Each subject participates in one of these groups and, therefore, faces a decision with no recommendation (Baseline), a pro-neutrality recommendation prior to starting the decision-making (Pre-Recommendation), or a pro-neutrality recommendation after an initial decision has been made (Post-Recommendation).

4.3.2 Technical Realization of the Managerial Decision

In Baseline *Risk*, every participant plays the role of a manager and has to decide in which design she wants to offer her products on the market. Here, "design" encompasses all core features of the product (for a similar definition, see Utterback & Abernathy, 1975). The participant has to decide simultaneously between a traditional and a modern design for 11 distinct products. The traditional design is well established on the market, such that the manager knows the exact price that the customers are willing to pay. Therefore, deciding in favor of the traditional design yields a sure

[47] The main behavioral measure used in the experiment presented in this chapter of the dissertation is the same one that was explained in the previous chapter (Böhm, 2021). For the original multiple price list on which the experimental measure builds, see Barham et al. (2014) and Holt and Laury (2002). In contrast to Böhm (2021), the experiment described here only features the domain risk. In addition, another nudge is used and varied based on when it materializes (see Subsection 4.3.3).

payoff of \$2.00. Opting for the modern design involves a risk. This design has not been tested on the market yet, and the manager does not know how the customers would respond. For reasons of simplicity, it is assumed that customers could only react in two ways: They either prefer the modern design, resulting in the willingness to pay a high price for the product, or they prefer the traditional design, resulting in a lower price. Based on market analysis, these outcomes occur with an equal probability of 50%. To visualize this in the framed experiment, subjects are provided with the information that out of 100 customers, 50 are willing to pay a high price and 50 are willing to pay a low price. Of these 100 customers, one is randomly chosen to be the one who would determine the payoff for the scenario. If a customer who is willing to pay a high price is chosen, the subject earns \$4.00. If, however, a low-paying customer is selected, the participant earns an amount between \$2.00 and \$0.00. This value varies between the 11 products. For example, the payoff for a low-paying customer is \$2.00 for Product 1, while it decreases to \$0.00 for Product 11. A full representation of the multiple price list can be found in Appendix 4.A.[48] If Task 1 is the payoff-relevant task, one of the 11 products and the corresponding subject's decision is randomly selected to be payoff-relevant.

The experimental measure described above provides a unique switching point for each subject or no switching point at all if a subject always opts for the modern design.[49] For the analysis, it is assumed that each subject has a constant relative risk aversion (CRRA) and the isoelastic utility function $u(\pi) = \frac{\pi^{1-\gamma}-1}{1-\gamma}$ for $\gamma \neq 1$ and $u(\pi) = ln(\pi)$ for $\gamma = 1$, with π denoting the payoff and γ the CRRA coefficient (Arrow, 1971; Pratt, 1964). Depending on the unique switching point, a CRRA coefficient is assigned to each subject and used as the main dependent variable in the statistical analysis. The method to deduce the CRRA coefficients from the respective switching point is presented in detail in Appendix 4.B. For our analysis, the CRRA coefficient is set to the lower bound of the resulting interval (Barham et al., 2014). In addition to the CRRA coefficient, the unique switching point can also be deployed as the dependent variable in the analysis.[50]

4.3.3 Implementation of the Recommendation Nudge

The recommendation suggests choosing the modern design for Products 1–10 and the traditional design for Product 11. Such a recommendation is favorable for the decision-maker (Thaler & Sunstein, 2008) because the expected payoff of the modern design is higher than the payoff of the traditional design for the first ten products. For Product 11, the expected payoffs are equal, so the safer option is recommended. Therefore, the recommendation features the most neutral

[48] To clarify, each subject plays the game for herself. Therefore, her decisions do not influence the payoff for any other subject, nor do any other subjects' decisions influence hers. In addition, all subjects are informed about all the rules at the beginning of the experiment, and all relevant features are common knowledge.

[49] For reasons of consistent behavior, subjects are allowed to switch from the modern design to the traditional design (i.e., from right to left in the multiple price list) only once.

[50] For this purpose, the coding of the switching point is reversed. Note that the CRRA coefficient is decreasing when switching at a higher product number, while the switching point is increasing. It is helpful to reverse the switching point to gain estimates that have equal signs. Technically speaking, this is necessary because the level of shown risk aversion in the decision decreases with switching in a lower row. Appendix 4.C provides an overview of the dependence between the switching point, the CRRA coefficient, and the reversed switching point.

decision in this risky situation. Hence, it is a pro-neutrality recommendation. While it is explained here in detail, the reasoning behind the recommendation is limited to "likely ... most profitable" (quote from the instructions provided in Appendix 4.D) and is not provided in more detail to the participants in the experiment. To ensure that the subjects recall the recommendation correctly, the recommended choices are labeled as recommended (see Appendix 4.D for screens of the online experiment).

In Pre-Recommendation, the pro-neutrality recommendation is placed at the beginning of the experiment before an initial decision is made by the subject. In Post-Recommendation, the subjects play Baseline until submitting their initial choices. After this, the pro-neutrality recommendation is implemented, and they are asked to re-evaluate their initial decision. Everything else is unchanged compared to Baseline. Figure 4.1 shows a general overview of the whole experiment, including the placement of the recommendations within Task 1.

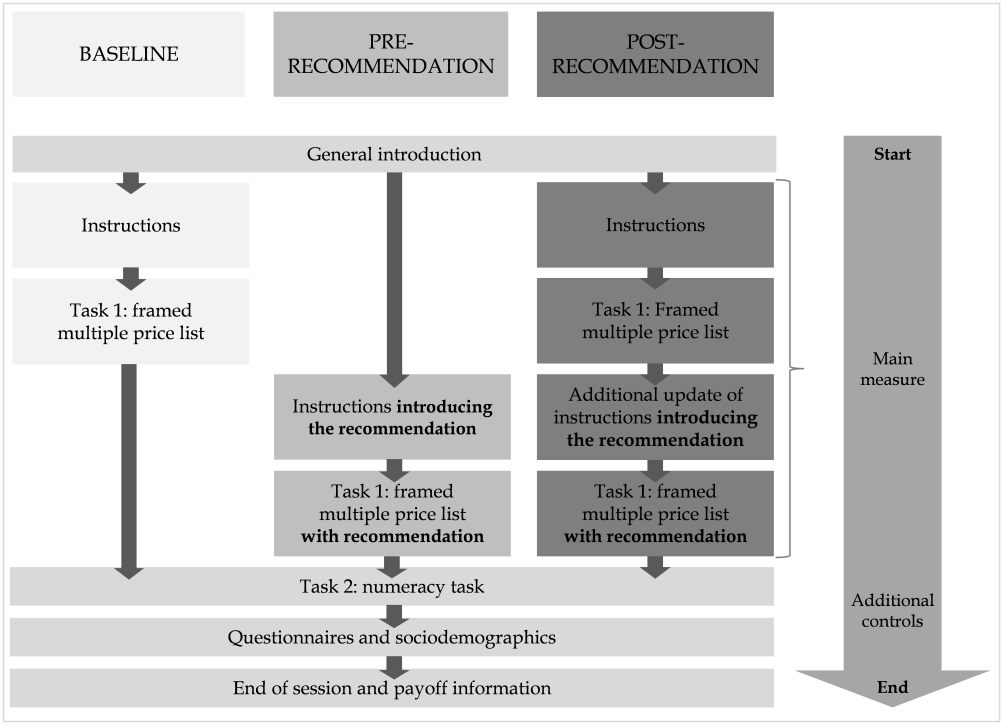


Figure 4.1: Overview of Experiment

4.3.4 Additional Controls

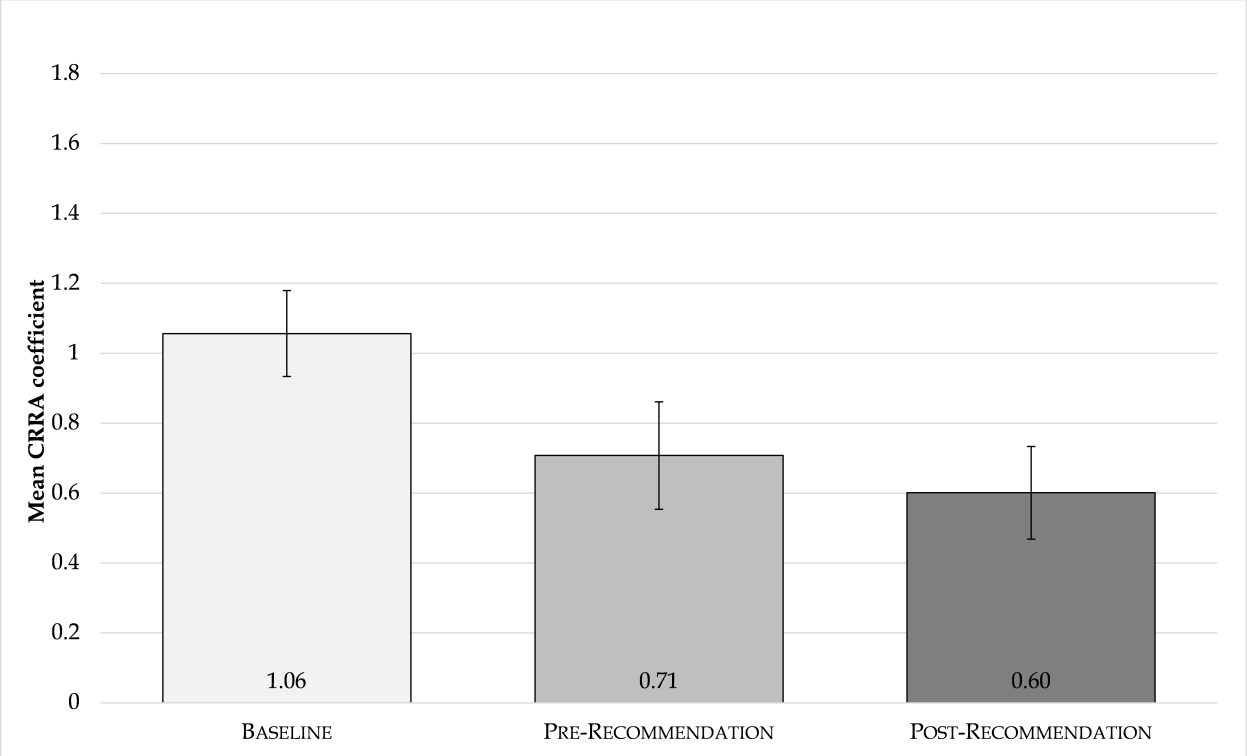
To support our investigation, a set of additional controls are elicited. Task 2 measures the subjects' mathematical abilities with an alteration of the Berlin Numeracy Test (Cokely et al., 2012). To avoid cheating due to the online nature of the experiment and public knowledge of the items of the Berlin Numeracy Test, the items are adapted in presentation and wording without changing the actual mathematical skills needed. Previous findings by Park and Cho (2019) and Riepe et al.

(2022) support a relationship between numeracy and a person's risk aversion. If Task 2 is payoff-relevant, the subject's performance in the numeracy test is paid. For each correct item, subjects receive \$1.00. Therefore, the potential payoff for Task 2 ranges from \$0.00 to \$4.00. In addition to the two incentivized tasks, additional controls are added at two points in the experiment. First, after Task 1, subjects are asked to rate how confident they are in decision-making situations like the one they were asked to decide on in Task 1 and to report on their preference for rational or intuitive reasoning (Butler et al., 2014). Second, at the end of the experiment, five additional questionnaires are implemented. The general risk aversion scale (Dohmen et al., 2011) is used to elicit the subjects' propensity to take risks. Additional self-measures include tolerance for ambiguity (Budner, 1962; Herman et al., 2010), personal attitude towards entrepreneurship, perceived behavioral control regarding entrepreneurial capacity (Liñán & Chen, 2009), and dispositional resistance (Oreg, 2003). The experiment concludes with a set of sociodemographic questions about each participant's gender, age, employment status, educational level, household income, contribution to household income, and the number of adults and children in their household. We derive no hypotheses on correlations between these controls and the main behavioral measure. Therefore, we add them to our parametric analysis as additional explanatory variables to test the robustness of the results. Adding these controls does not change the results significantly. Hence, we do not discuss coefficients and p-values associated with these control variables.

4.3.5 Subjects and Procedure

In total, we collected data from 149 subjects, with 47 subjects participating in Baseline, 53 in Pre-Recommendation, and 49 in Post-Recommendation. The Post-Recommendation treatment features Baseline-like decisions prior to receiving the recommendation (see Figure 4.1). As the experimental design between Baseline and Post-Recommendation does not vary, we decided to pool these observations.[51] Throughout this paper, we used the pooled Baseline group ($N = 96$) and make between-subject comparisons with the Pre-Recommendation group and partly between-subject and partly within-subject comparisons with the Post-Recommendation observations. Of the 149 participants included here, two identified as neither female nor male. Of the remaining 147 participants, 50.34% identified as female (Binomial probability test: $p > 0.999$). The average age in years was 32.71 ($SD = 10.48$). All data collection took place in November 2020 using Qualtrics for realizing the experiment and Prolific (Palan & Schitter, 2018) for recruiting and paying subjects. The sample was limited to US citizens with a high school degree or a higher educational level who were born in the US. The median subject took 11.92 minutes to finish the experiment. The average payoff was US\$2.45 ($SD = 1.21$), including a US\$1.00 show-up fee. The minimum payoff was US\$1.00, and the maximum was US\$5.00.

[51] We find no evidence that the initial decision in Post-Recommendation differ significantly from the decision in Baseline (Mann-Whitney test using the CRRA coefficient: $|z| = 1.598$, $p = 0.110$; reversed switching point: $|z| = 0.589$, $p = 0.556$). The presented results do not differ relevantly when excluding the Baseline-like decisions of the Post-Recommendation treatment.



Note: Error bars represent the standard errors.

Figure 4.2: Level of Aversive Behavior in Business Venturing Decisions

4.4 Results

In this paper, we test whether a pro-neutrality recommendation before making an initial decision (PRE-RECOMMENDATION) or after making an initial decision (POST-RECOMMENDATION) is effective in promoting more neutral behavior in risky managerial decisions when compared with no such intervention (BASELINE). Using non-parametric and parametric tests, the effects of the recommendations are investigated in Subsection 4.4.1. As described in Subsection 4.3.5, we pooled the BASELINE-like initial decisions in the POST-RECOMMENDATION treatment with the observations of the BASELINE group whenever feasible. Hence, the effect of the PRE-RECOMMENDATION is investigated between-subject to these pooled observations. In addition, the effect of the POST-RECOMMENDATION can be compared between-subject to the BASELINE group and within-subject to the BASELINE-like initial decisions in POST-RECOMMENDATION. In Subsection 4.4.2, we compare the effectiveness of these recommendations with regard to when they materialize.[52]

For the following analyses, we use the CRRA coefficient, calculated as described above and in more detail in Appendix 4.B. The CRRA coefficient allows the measurement of the subject's deviation from the optimal decision in a specific situation. In other words, it provides a measure of how far the subject is from neutrality, as described by Herrmann (2017) and Stirling (2003). In

[52] Appendix 4.E provides an overview of the hypotheses and results discussed in the following subsections.

the experimental design, the CRRA coefficient ranges from −0.09 (risk-loving) to 0 (risk-neutral) to 3.76 (very risk-averse).[53,54]

4.4.1 Effect of Recommendations

We observe a mean CRRA coefficient of 1.06 ($SD = 1.11$) in Baseline, which serves as a baseline level of aversive behavior in this risky business venturing decision. The means of the CRRA coefficients of all three groups are presented in Figure 4.2.

We first focus our attention on the effect of introducing a recommendation prior to making the decision. Such a Pre-Recommendation reduces the mean CRRA coefficient to 0.71 ($SD = 1.05$). The difference is statistically highly significant (Mann-Whitney test: $|z| = 2.734$, $p = 0.006$). We find accompanying support by means of parametric analysis. The results of the random effects generalized least squares regressions with clustered standard errors and the CRRA coefficient as the dependent variable are presented in Table 4.1. Model 1 features the pure treatment effects of both recommendations. Model 2 adds additional questionnaires about personality traits and abilities as controls. Model 3 additionally adds sociodemographic information as controls. The effect of the Pre-Recommendation is significant on the 10% level in Model 1 ($p = 0.078$) and the 5% level in Models 2 and 3 ($p < 0.035$). For further robustness, we used the reversed switching point as the dependent variable, ranging from rather risk-loving to highly risk-averse between 0 and 11. Results of the random effects ordered probit regressions using the reversed switching point can be found in Appendix 4.F. In all models, the effect of the Pre-Recommendation is significant on the 5% level ($p < 0.026$).[55,56] Therefore, support for Hypothesis 4.1 is found. A pro-neutrality recommendation provided before an initial decision takes place (Pre-Recommendation) reduces aversive behavior in favor of more neutral behavior.

Result 4.1. *A pro-neutrality recommendation that is provided before making a decision promotes risk-neutral behavior in managerial situations.*

In a second step, we investigate the effect of such a pro-neutrality recommendation when it materializes after an initial decision has taken place. Such a Post-Recommendation reduces

[53] In this paper, 198 data points (47 Baseline, 53 Pre-Recommendation, 49 Post-Recommendation, and 49 Baseline-like initial decisions from Post-Recommendation) from 149 subjects are included. Of these data points, 172 (42 Baseline, 47 Pre-Recommendation, 43 Post-Recommendation, and 40 Baseline-like initial decisions from Post-Recommendation) provided a decision for which the CRRA coefficient could be calculated. For a detailed explanation, see Appendix 4.B.

[54] The results presented here do not differ relevantly when using the reversed switching point of the subject in the multiple price list instead of the CRRA coefficient based on this switching point. Some points of interest are stated.

[55] The results regarding the effect of a Pre-Recommendation differ somewhat when using random effects tobit regressions with the CRRA coefficient as the dependent variable and a lower limit (−0.09) as well as an upper limit (3.76). The effect is not significant ($p = 0.135$) in a replica of Model 1, significant only on the 10% level ($p = 0.068$) in a replica of Model 2, and significant on the 5% level ($p = 0.019$) in a replica of Model 3. We are still confident to conclude a significant effect of a Pre-Recommendation in our experiment.

[56] When excluding the Baseline-like initial decisions from Post-Recommendation, the parametric results for Pre-Recommendation of the corresponding random effects generalized least squares, random effects ordered probit, and tobit regressions yield comparable results to what are described in the main text.

the mean CRRA coefficient to 0.60 ($SD = 0.87$). The difference is statistically highly significant for both the between-subject comparison with the observations of subjects in the BASELINE group (Mann-Whitney test: $|z| = 3.459$, $p < 0.001$) and the within-subject comparisons with the BASELINE-like initial decisions in the POST-RECOMMENDATION group (Wilcoxon signed-rank test: $|z| = 3.444$, $p < 0.001$). Regarding the parametric results in Table 4.1 and Table 4.F.1, we find supporting evidence, as all models yield a significance of the POST-RECOMMENDATION on the 1% level ($p < 0.001$).[57,58] Hence, we conclude that there is support for Hypothesis 4.2. A pro-neutrality recommendation provided after an initial decision has taken place (POST-RECOMMENDATION) reduces aversive behavior in favor of more neutral behavior.

Result 4.2. *A pro-neutrality recommendation that is provided after making an initial decision promotes risk-neutral behavior in managerial situations.*

4.4.2 Comparison of the Effectiveness

In Hypothesis 4.3, we argue that the effect of a POST-RECOMMENDATION might be smaller than the effect of a PRE-RECOMMENDATION because the initial decision might cause a status quo bias or interfere with the recommendation through another channel. We can investigate this question by means of non-parametric and parametric testing. First, by comparing the mean CRRA coefficients, we calculate that the level of aversive behavior is smaller in POST-RECOMMENDATION than in PRE-RECOMMENDATION, which is a finding that we have not expected. However, the difference between the two treatments turns out not to be statistically significant (Mann-Whitney test: $|z| = 0.253$, $p = 0.800$). Second, the estimated effect of a PRE-RECOMMENDATION is greater than that of a POST-RECOMMENDATION (Table 4.1 Model 3: $|b_{\text{PRE-RECOMMENDATION}}| = 0.484 > |b_{\text{POST-RECOMMENDATION}}| = 0.222$). While this is in line with our argumentation, the difference turns out not to be statistically significant (Wald test in Model 3: $\chi^2 = 1.85$; $p = 0.174$).[59,60] In other words, there is no support for stating that a POST-RECOMMENDATION suffers from the initial decision and, therefore, lacks the same level of neutrality-promoting effect as the PRE-RECOMMENDATION.

On the one hand, a potential reason for not being able to detect the difference correctly is the scale of our experimental measure. It seems plausible that a more nuanced scale, such as a continuous measure, is required to detect small differences between the two types of pro-neutrality recommendation. On the other hand, our results could be interpreted to show that there might be

[57] The results regarding the effect of a POST-RECOMMENDATION do not differ relevantly when using random effects tobit regressions with the CRRA coefficient as the dependent variable and a lower limit (-0.09) as well as an upper limit (3.76). Replications of all models still yield significance on the 1% level ($p < 0.002$).

[58] When excluding the BASELINE-like initial decisions from the POST-RECOMMENDATION, the parametric results for the POST-RECOMMENDATION of the corresponding random effects generalized least squares, random effects ordered probit, and tobit regressions yield comparable results to what are described in the main text.

[59] This result holds for all models and all the additional regression methods mentioned above.

[60] In addition to what is discussed in the main text, we can also apply permutation testing for the difference in effectiveness of the PRE-RECOMMENDATION and POST-RECOMMENDATION when compared with BASELINE. Again, we find no support for such a difference. Results and graphical representations of the corresponding simulations are available upon request.

no difference with regard to when such a recommendation materializes as long as it is provided at some point during the decision-making process.

Result 4.3. *A pro-neutrality recommendation that is provided before making a decision might have the same effect as a pro-neutrality recommendation that is provided after making an initial decision in regard to promoting risk-neutral behavior in managerial situations.*

	Model 1	Model 2	Model 3
Pre-Recommendation	-0.343^{*}	-0.379^{**}	-0.484^{**}
	(0.195)	(0.180)	(0.190)
Post-Recommendation	-0.219^{***}	-0.226^{***}	-0.222^{***}
	(0.059)	(0.060)	(0.064)
Constant	1.051^{***}	2.636^{**}	2.601^{*}
	(0.120)	(1.257)	(1.407)
Additional Questionaires	NO	YES	YES
Sociodemographics	NO	NO	YES
Wald-χ^2	15.53	54.40	69.26
$p(\chi^2)$	< 0.001	< 0.001	< 0.001
Number of observations	172	172	169
Number of groups	132	132	130

Note: The dependent variable for all three models is the level of aversive behavior measured by the individual-specific CRRA coefficient. Clustered standard errors are presented in parentheses. For all models, the reference group is Baseline. In Model 2, additional controls are added. These controls include numeracy (Cokely et al., 2012), decision confidence, preference for reasoning (Butler et al., 2014), general risk aversion (Dohmen et al., 2011), tolerance for ambiguity (Herman et al., 2010), personal attitude towards entrepreneurship and entrepreneurial capacity (Liñán & Chen, 2009), and resistance to change (Oreg, 2003). In Model 3, sociodemographic characteristics are added as additional control variables. These characteristics are gender, age, employment status, educational level, household income, own contribution to household income, and the number of adults and children in the household. Model 3 includes a slightly lower number of observations than Model 2 due to the exclusion of two subjects who identified as neither female nor male. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4.1: Results of Random Effects Generalized Least Squares Regressions

4.5 Discussion, Implications, and Further Research

Our findings support the idea that a pro-neutrality recommendation nudge helps to improve managerial performance in risky endeavors. More precisely, we find higher levels of neutral behavior when such a recommendation is provided than when it was not, regardless of when it materializes.

While Böhm et al. (2021) report no evidence for a recommendation being effective in supporting change, Böhm (2021) shows evidence for a pro-neutrality default promoting neutral behavior in

business venture decisions under risk. In contrast to Böhm (2021), this paper focuses on an alternative nudge – a recommendation – for such situations. We find clear support for a pro-neutrality recommendation being helpful for a business venture decision under risk. The relevant difference between a pro-neutrality default and a pro-neutrality recommendation is the system of thinking that the nudge affects. While a default rule is said to operate within System 1 thinking, a recommendation is considered to affect System 2 thinking (Böhm, 2021; Kahneman, 2011; Kahneman et al., 2011; Milkman et al., 2009). Indeed, some people prefer nudges that operate on a high level of thinking rather than on a more subconscious one (Jung & Mellers, 2016; Sunstein, 2016). Hence, a pro-neutrality recommendation that is cognitively oriented (Cadario & Chandon, 2020) might be the preferred tool. Our positive results are in line with other evidence for effective recommendation nudges tested in various situations and contexts (Barron & Nurminen, 2020; Bertrand & Morse, 2011; Keller et al., 2011; Newell & Siikamäki, 2014). This paper and the results provided in it may be helpful in stimulating more research on System 2 nudging, with a focus on supporting managerial decision-making in risky situations – a topic in great need of such research (Ebert & Freibichler, 2017; Milkman et al., 2009; Zhang & Cueto, 2017).

We find no support for the idea that the timing of such a recommendation within the managerial decision-making process matters. We argued that while the effect of a pro-neutrality recommendation should still be present when it materializes after an initial, potentially risk-averse decision has been made, the effectiveness should suffer from anchoring or the status quo effect (Kahneman et al., 1991; Samuelson & Zeckhauser, 1988). Our experimental results support a positive effect of a delayed, pro-neutrality recommendation and provide no evidence that the effectiveness varies between a pro-neutrality recommendation provided at the start of the decision-making process and one that materializes later. This might sound like good news given that delayed recommendations may be the result of less outspoken, maybe even passive, employees, a highly hierarchical organization, or a distribution of power favoring high-power figures (Huang et al., 2005); however, it is too soon to judge whether such organizational and operational features actually leave successful pro-neutrality nudge support untouched.

The experimental measure used in this paper is not very nuanced (Barham et al., 2014; Holt & Laury, 2002). Further research should consider using a finer tool to potentially be able to detect small behavioral differences. In our experiment, we apply a business venture frame to allow for the context dependency of nudging (Hauser et al., 2018; van Kleef & van Trijp, 2018). Still, our online experiment is a stylized version of an actual managerial decision featuring a risky business venture. Research building on our results might consider applying a field experimental approach.

Lovallo et al. (2020) points out that many companies struggle with being too risk-averse. In this paper, we present experimental results supporting the effectiveness of pro-neutrality recommendations in promoting more neutral behavior. Such nudges might be useful and cost-efficient (Benartzi et al., 2017) to apply in real-life scenarios when companies try to move managerial decision-making towards a less risk-averse approach (for a practical approach, see Böhm & Renz, 2022; Renz & Böhm, 2020).

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Appendix 4.A Multiple Price List

Product	Traditional design	Modern design	
		High price	Low price
1	$2.00	$4.00	$2.00
2	$2.00	$4.00	$1.60
3	$2.00	$4.00	$1.30
4	$2.00	$4.00	$1.00
5	$2.00	$4.00	$0.80
6	$2.00	$4.00	$0.70
7	$2.00	$4.00	$0.60
8	$2.00	$4.00	$0.50
9	$2.00	$4.00	$0.40
10	$2.00	$4.00	$0.20
11	$2.00	$4.00	$0.00

Table 4.A.1: Multiple Price List

Appendix 4.B CRRA Coefficient

Calculation of the CRRA Coefficient. For this example of how to calculate the CRRA coefficient based on the switching point, it is assumed that a participant switches from the modern design to the traditional design at Product 5. Therefore, the utility from deciding on the traditional design for Product 5 must be at least as high as from deciding on the modern design, while the reverse is the case for Product 4:

$$
\begin{array}{lrccl}
 & 0.5 \cdot u(4) + 0.5 \cdot u(0.8) & \leq & u(2) & \leq \; 0.5 \cdot u(4) + 0.5 \cdot u(1) \\
\Leftrightarrow & 0.5 \cdot \frac{4^{1-\gamma}-1}{1-\gamma} + 0.5 \cdot \frac{0.8^{1-\gamma}-1}{1-\gamma} & \leq & \frac{2^{1-\gamma}-1}{1-\gamma} & \leq \; 0.5 \cdot \frac{4^{1-\gamma}-1}{1-\gamma} + 0.5 \cdot \frac{1^{1-\gamma}-1}{1-\gamma} \\
\Leftrightarrow & 0.5 \cdot (4^{1-\gamma}-1) + 0.5 \cdot (0.8^{1-\gamma}-1) & \leq & 2^{1-\gamma}-1 & \leq \; 0.5 \cdot (4^{1-\gamma}-1) + 0.5 \cdot (1^{1-\gamma}-1) \\
\Leftrightarrow & 4^{1-\gamma}-1+0.8^{1-\gamma}-1 & \leq & 2 \cdot 2^{1-\gamma}-2 & \leq \; 4^{1-\gamma}-1+1^{1-\gamma}-1 \\
\Leftrightarrow & 4^{1-\gamma}+0.8^{1-\gamma} & \leq & 2 \cdot 2^{1-\gamma} & \leq \; 4^{1-\gamma}+1^{1-\gamma} \\
\Rightarrow & 0.65 & \lessapprox & \gamma & \lessapprox \; 1
\end{array}
$$

The CRRA coefficient is set to the lower bound of this interval. Participants who always decide for the modern design are either risk-neutral or risk-loving. For practical reasons, their CRRA coefficient is set to -0.09 (Barham et al., 2014). Participants who always decide on the traditional design – in particular, those who do so for the very first product for a sure \$2.00 instead of at least \$2.00 with the chance of 4.00 – cannot be rationalized under the aspect of standard risk aversion. Their CRRA coefficient is ∞. The resulting CRRA coefficients depending on the respective switching point in the multiple price list and the switching points themselves in the reversed order are represented in Table 4.C.1.

Appendix 4.C Switching Point, CRRA Coefficient, and Reversed Switching Point

Switched at product	**CRRA coefficient**	**Reversed switching point**
1	∞	11
2	3.76	10
3	1.86	9
4	1	8
5	0.65	7
6	0.52	6
7	0.4	5
8	0.31	4
9	0.22	3
10	0.09	2
11	0	1
no switch	-0.09	0

Table 4.C.1: Constant Relative Risk Aversion Coefficients and Reversed Switching Points

Appendix 4.D Screens of the Online Experiment

In all treatments, the first screen is the general introduction (see Figure 4.D.1).

In the Baseline treatment, participants are instructed for Task 1 as shown in Figure 4.D.2, Figure 4.D.3, and Figure 4.D.4. On the next screen, the participants in this treatment receive a reminder and the multiple price list (see Figure 4.D.5 and Figure 4.D.6). As described above, an error message was implemented for inconsistent behavior, as shown in Figure 4.D.7. This also applies to all other treatments. All the screens provided here for Baseline are provided for participants in Post-Recommendation for the Baseline-like initial decision.

In the Pre-Recommendation treatment, participants are partly differently instructed for Task 1. Differences are shown in Figure 4.D.8. Missing figures imply no changes compared to Baseline. The Pre-Recommendation also causes an adaptation of the reminder and the multiple price list (Figure 4.D.9 and Figure 4.D.10).

In the Post-Recommendation treatment, participants first receive the screens of the Baseline treatment for the instructions for Task 1 and Task 1 itself. Afterward, they receive an update of the instructions (see Figure 4.D.11), similar to the implementation of the recommendation provided with the first instruction in the Pre-Recommendation treatment. As an additional step, in Task 1, they are able to re-evaluate their previously selected choice in a multiple price list with the recommendation in place. This screen shows the identical multiple price list with the recommendation provided in the Pre-Recommendation treatment.

The subsequent parts of the online experiment are identical in all treatments. The online experiment continues by introducing the first questionnaire part, which features one question about decision confidence and one about decision mode. Afterward, participants transition into Task 2, which is the numeracy task. This is followed by the second questionnaire part, including general risk aversion, tolerance for ambiguity, entrepreneurial attitudes, entrepreneurial capacities, resistance to change, and a battery of sociodemographic questions. Screens for these parts are available upon request.

For all participants, the online experiment ends with the last screen, as can be seen in Figure 4.D.12.

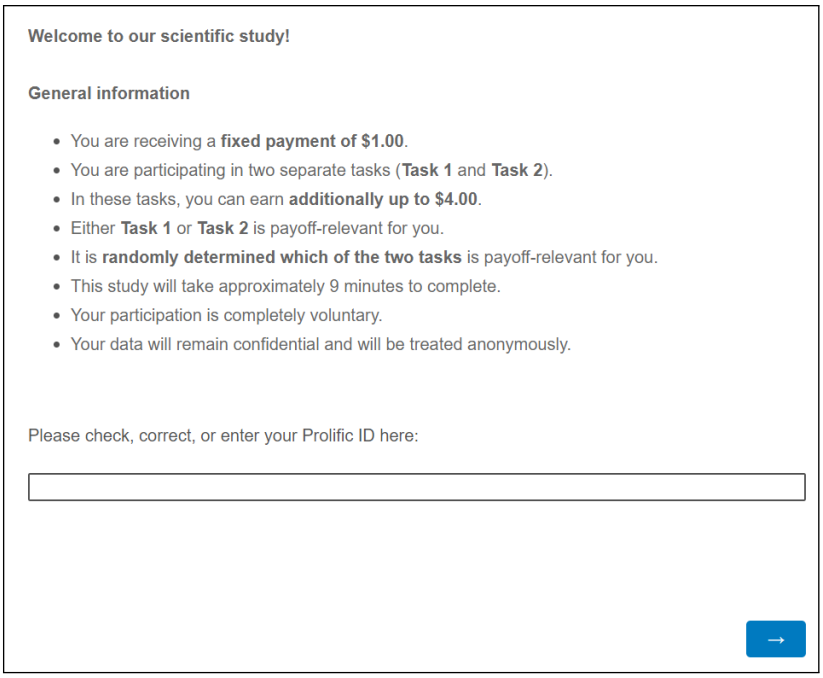
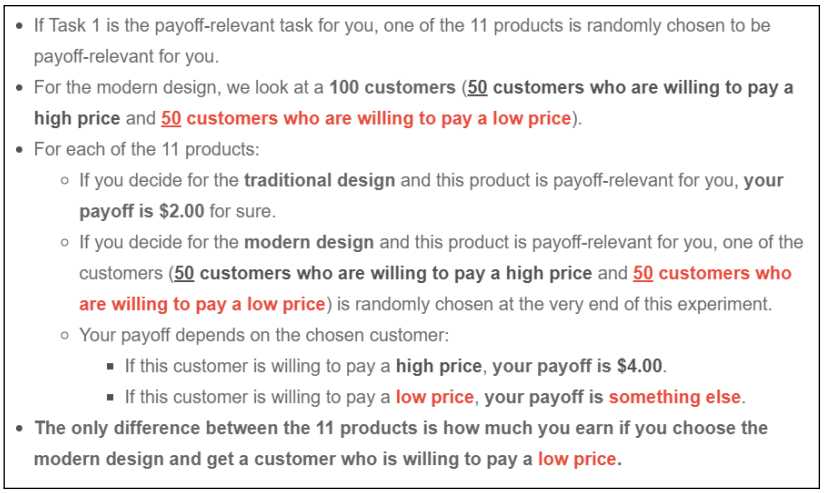


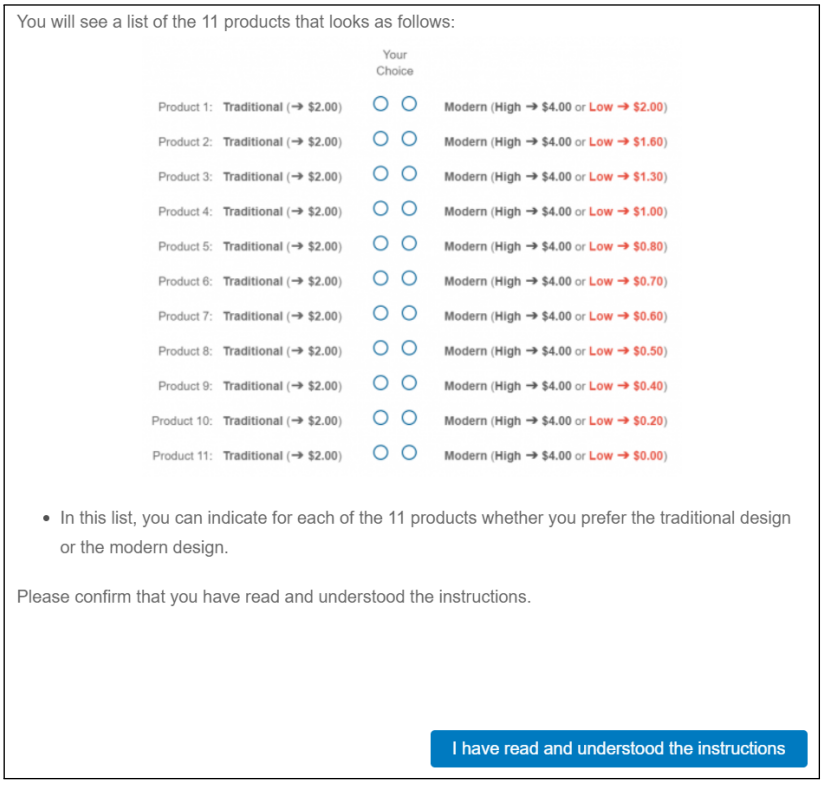
Figure 4.D.1: General Introduction for All Treatments

Figure 4.D.2: Task 1 Instructions for Baseline 1/3



- If Task 1 is the payoff-relevant task for you, one of the 11 products is randomly chosen to be payoff-relevant for you.
- For the modern design, we look at a **100 customers** (**50 customers who are willing to pay a high price** and **50 customers who are willing to pay a low price**).
- For each of the 11 products:
  - If you decide for the **traditional design** and this product is payoff-relevant for you, **your payoff is $2.00** for sure.
  - If you decide for the **modern design** and this product is payoff-relevant for you, one of the customers (**50 customers who are willing to pay a high price** and **50 customers who are willing to pay a low price**) is randomly chosen at the very end of this experiment.
  - Your payoff depends on the chosen customer:
    - If this customer is willing to pay a **high price**, **your payoff is $4.00**.
    - If this customer is willing to pay a **low price**, **your payoff is something else**.
- **The only difference between the 11 products is how much you earn if you choose the modern design and get a customer who is willing to pay a low price.**

Figure 4.D.3: Task 1 Instructions for Baseline 2/3



You will see a list of the 11 products that looks as follows:

	Your Choice	
Product 1:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $2.00)
Product 2:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $1.60)
Product 3:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $1.30)
Product 4:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $1.00)
Product 5:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.80)
Product 6:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.70)
Product 7:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.60)
Product 8:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.50)
Product 9:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.40)
Product 10:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.20)
Product 11:	Traditional (→ $2.00)	Modern (High → $4.00 or Low → $0.00)

- In this list, you can indicate for each of the 11 products whether you prefer the traditional design or the modern design.

Please confirm that you have read and understood the instructions.

I have read and understood the instructions

Figure 4.D.4: Task 1 Instructions for Baseline 3/3

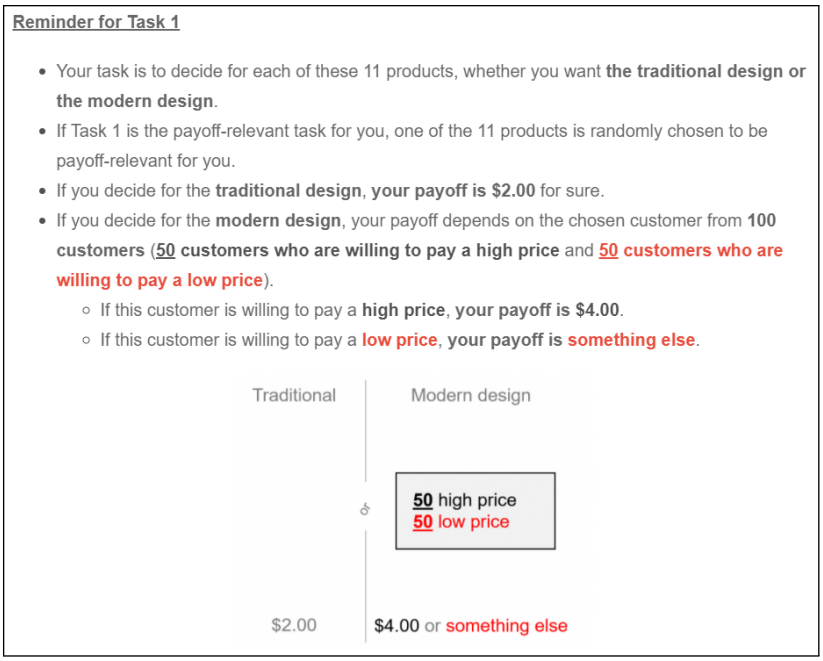


Figure 4.D.5: Task 1 for Baseline 1/2

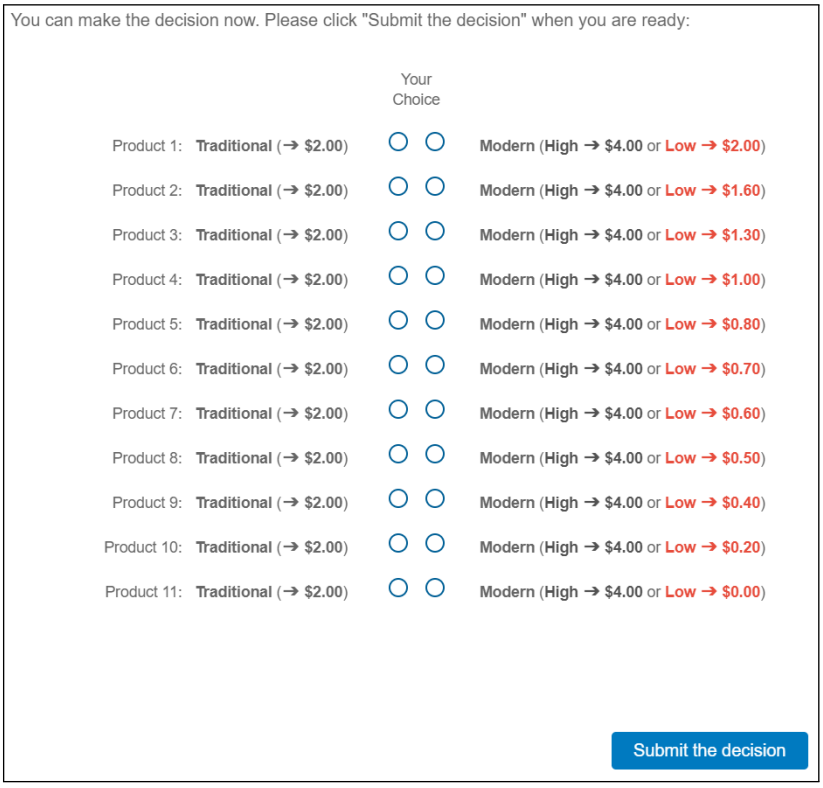


Figure 4.D.6: Task 1 for Baseline 2/2

Figure 4.D.7: Error Message in Task1 for Baseline

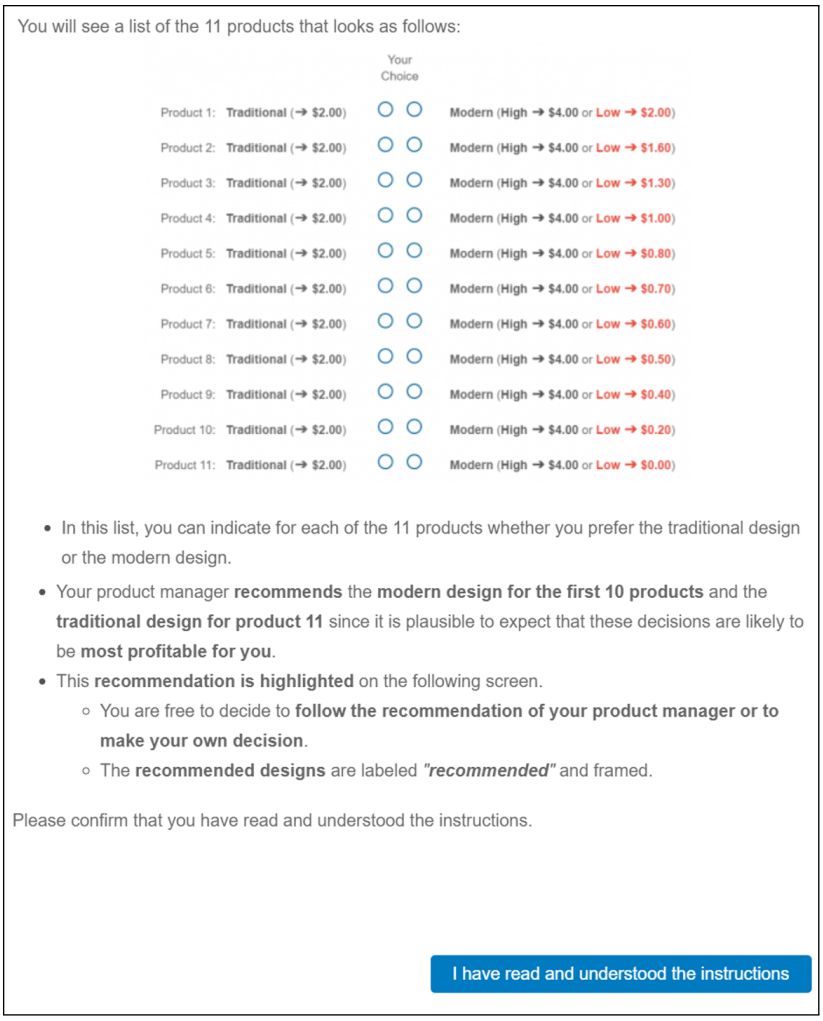


Figure 4.D.8: Task 1 Instructions for Pre-Recommendation 3/3

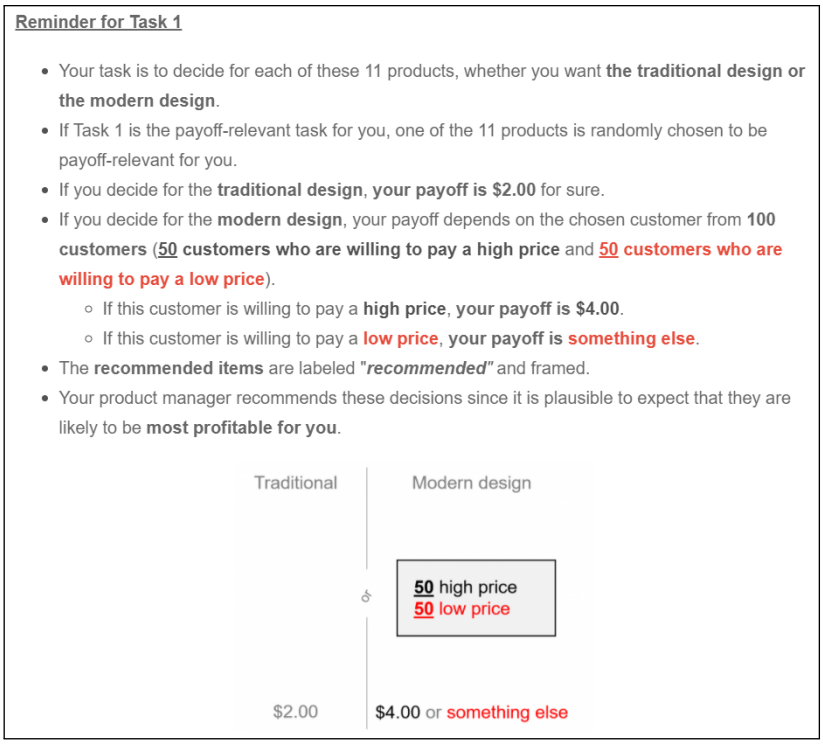


Figure 4.D.9: Task 1 for Pre-Recommendation 1/2

Figure 4.D.10: Task 1 for Pre-Recommendation 2/2

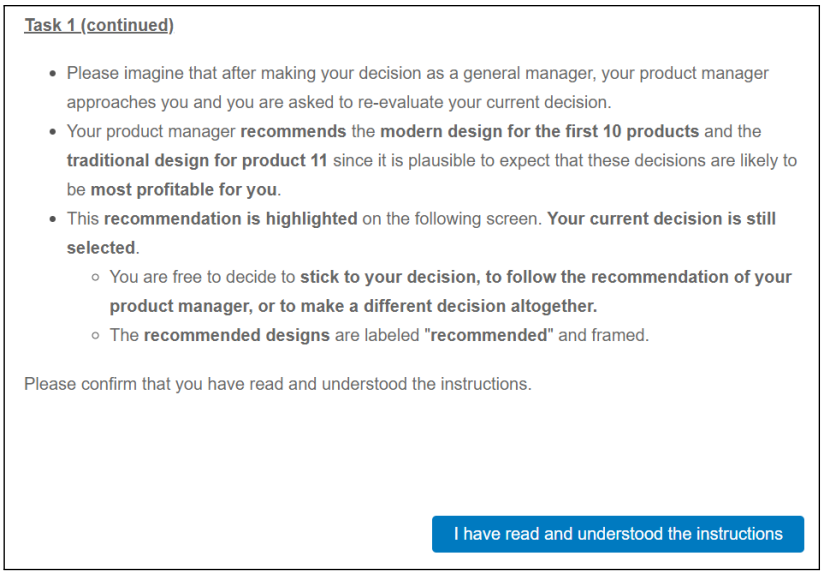


Figure 4.D.11: Task 1 Updated Instructions for Post-Recommendation

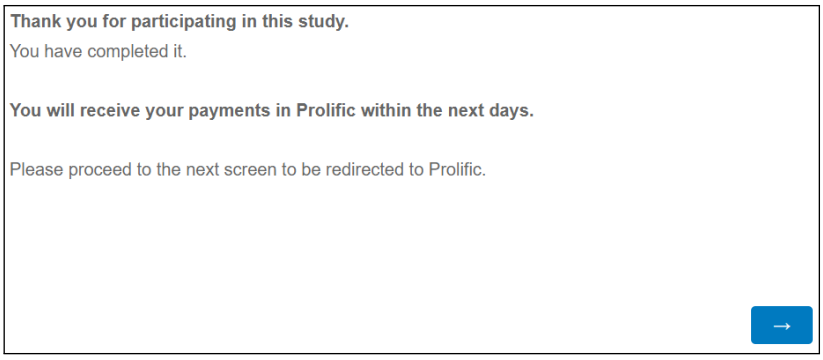


Figure 4.D.12: Last Screen for All Treatments

Appendix 4.E Summary of Hypotheses and Results

Number	Hypothesis	Result	Support
4.1	A pro-neutrality recommendation that is provided before making a decision promotes risk-neutral behavior in managerial situations.	A pro-neutrality recommendation that is provided before making a decision promotes risk-neutral behavior in managerial situations.	YES
4.2	A pro-neutrality recommendation that is provided after making an initial decision promotes risk-neutral behavior in managerial situations.	A pro-neutrality recommendation that is provided after making an initial decision promotes risk-neutral behavior in managerial situations.	YES
4.3	A pro-neutrality recommendation that is provided before making a decision has a stronger effect in promoting risk-neutral behavior than a pro-neutrality recommendation that is provided after making an initial decision in managerial situations.	A pro-neutrality recommendation that is provided before making a decision might have the same effect as a pro-neutrality recommendation that is provided after making an initial decision in regard to promoting risk-neutral behavior in managerial situations.	NO

Table 4.E.1: Summary of Hypotheses and Results

Appendix 4.F Random Effects Ordered Probit Regressions

	Model 1	Model 2	Model 3
Pre-Recommendation	-0.968^{**}	-1.044^{**}	-1.047^{**}
	(0.485)	(0.444)	(0.446)
Post-Recommendation	-0.925^{***}	-0.988^{***}	-0.962^{***}
	(0.193)	(0.193)	(0.196)
Constant	3.946^{***}	8.197^{***}	8.515^{***}
	(0.772)	(2.595)	(2.598)
Additional Questionaires	NO	YES	YES
Sociodemographics	NO	NO	YES
Wald-χ^2	25.04	46.10	48.65
$p(\chi^2)$	< 0.001	< 0.001	< 0.001
Number of observations	198	198	195
Number of groups	149	149	147

Note: The dependent variable for all three models is the level of aversive behavior measured by the individual-specific reversed switching point. Clustered standard errors are presented in parentheses. For all models, the reference group is Baseline. In Model 2, additional controls are added. These controls include numeracy (Cokely et al., 2012), decision confidence, preference for reasoning (Butler et al., 2014), general risk aversion (Dohmen et al., 2011), tolerance for ambiguity (Herman et al., 2010), personal attitude towards entrepreneurship and entrepreneurial capacity (Liñán & Chen, 2009), and resistance to change (Oreg, 2003). In Model 3, sociodemographic characteristics are added as additional control variables. These characteristics are gender, age, employment status, educational level, household income, own contribution to household income, and the number of adults and children in the household. Model 3 includes a slightly lower number of observations than Model 2 due to the exclusion of one subject who identified as neither female nor male. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4.F.1: Results of Random Effects Ordered Probit Regressions

Chapter 5

How Does Unethical Behavior Spread? – Gender Matters![*]

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Abstract

Using an online experiment with two distinct honesty games, we analyze how dishonesty in men and women is "inspired" by merely thinking about or by learning about the dishonesty of others in a related, but different, ethically loaded situation. Thinking is induced by eliciting a belief about others' dishonesty in a different game. We find that such belief elicitation (1) increases males' (but not females') dishonesty and (2) has no influence on participants' beliefs about the dishonesty of others in the game that they themselves play. Learning is induced by receiving a signal about the actual honest or dishonest choices of others in a different game. We find that the level of unethicality provided in such a signal (1) increases females' (but not males') dishonesty and (2) is positively correlated with participants' beliefs about the dishonesty of others in the game that they themselves play. We conclude that gender matters when examining how unethical behavior spreads. Both genders update their beliefs about others' dishonesty in the same way when presented with information about others' choices, but dishonesty in men is triggered by merely thinking about others' dishonesty. A potential channel is the feeling of competition introduced by wanting to earn more than others. Women adapt their dishonesty to the level of unethicality presented to them. Potential channels include their higher responsiveness to social cues and willingness to comply and adapt their behavior accordingly.

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5.1 Introduction

Many researchers investigate unethical behavior in the form of lying (Fischbacher & Föllmi-Heusi, 2013). While most research focuses on individual decisions (for an overview, see Abeler et al., 2019), some try to understand group dynamics (Kocher et al., 2018) or leadership effects (Bush et al., 2021; Grosch et al., 2021). There seems to be a consensus that moral costs play a vital role in explaining lying behavior (Abeler et al., 2019). While learning about others' unethical acts might be connected to one's moral costs, only very few papers investigate the spread of unethicality due to signals about others' behavior (Bicchieri & Xiao, 2009; Gino et al., 2009; Innes & Mitra, 2013). Moreover, even fewer papers focus on whether this process might be gender-specific (Fosgaard et al., 2013; Greene & Paxton, 2009). However, for companies that wish to avoid the decay of norms and the spread of unethicality, understanding how people adapt to surrounding unethical acts and whether gender plays a role in it is essential. One form of unethicality in the workplace is counterproductive work behavior. This includes theft and other unethical acts (Gruys & Sackett, 2003) and can be caused by (perceived) unfairness (Cohen-Charash & Mueller, 2007; Schweitzer & Gibson, 2008) – a concept not far removed from thinking about or observing unethical choices by others for their own benefit. Understanding how unethical behavior spreads is of the utmost importance, as counterproductive work behavior is linked to a decline in business performance (Abdullah et al., 2021), and, even more drastically, to the wide spread of unethicality, which can lead to extreme situations, such as bankruptcy. Cohn et al. (2014) describe a spread of high levels of unethicality within the banking industry. In this industry and, relatedly, the commodities industry, two prominent examples that filed for bankruptcy are Wirecard (Davies, 2020) and Enron (Emshwiller & Smith, 2001). Another example in the manufacturing industry is Volkswagen (Boston, 2017). In all three cases, reports describe widespread unethical acts at all levels of the hierarchy. Each of them became what O'Brien (2003, p. 79) describes as a "corrupted company." Therefore, failing to understand the dynamics of how unethical acts spread is costly for companies, their employees and shareholders, and our society as a whole.

In this paper, we present an experimental study on how merely thinking about the potential unethical behavior of others and learning about the actual unethical behavior of others affects one's own unethical behavior in another moral situation. Previous research on unethicality focuses on spreads based on one situation (Gino et al., 2009; Innes & Mitra, 2013), while previous research on spillovers differentiates two domains (Belot & Schröder, 2016; Dolan & Galizzi, 2015) rather than two situations rooted in the same domain. To the best of our knowledge, we are the first to investigate the effects of thinking and learning about others based on two distinct situations both settled in the domain of unethicality, rather than mixing the domains (as it is the case in previous research on spillovers) or using the same situation twice (as it is the case in previous research on unethicality). Additionally, the distinction between *thinking* and *learning* about others' behavior has, to the best of our knowledge, also not yet been investigated for lying behavior. Related to this distinction, Krupka and Weber (2009, p. 307) investigate and find that a norm affects behavior in

the pro-social domain by means of a "focusing and (an) informational" influence. Hence, such a norm can draw one's attention to pro-social acts and exerts a stronger impact when one observes higher levels of pro-social behavior in others.

Our experiments starts with a belief elicitation about others' behavior in a dishonesty game (dot task: Gino et al., 2010). This elicitation is used to induce thinking about others' behavior. Subsequently, a signal about others' behavior containing different levels of unethicality is provided. Therefore, participants learn the actual choices of others (in the dot task). As our main behavioral measure, we use another dishonesty game (modified version of the die-rolling task: Fischbacher & Föllmi-Heusi, 2013), in which subjects are asked to report the outcome of a die roll. Finally, as a manipulation check, we ask subjects to state their belief about the dishonesty of others' in the game they themselves just played (i.e., the die-rolling task). The treatment Belief-Signal (B-S) features both, an initial belief elicitation and a signal about others' behavior. In this treatment, the level of unethicality in the signal varies. We use the corresponding sub-treatments based on the level of unethicality to investigate the effect of learning about others' unethical behavior. To test for the effect of merely thinking about others' behavior, we use two additional treatments: First, we omit the signal about others' behavior. Hence, Belief-NoSignal (B-NS) still features the initial belief elicitation as well as the main behavioral measure and the manipulation check, but omits the signal about others' dishonesty. Second, we omit both the initial belief elicitation and the signal in NoBelief-NoSignal (NB-NS), while, again, keeping the main behavioral measure and the manipulation check. By comparing the two treatments (B-NS vs. NB-NS), we investigate the effect of merely thinking about the potential unethicality of others.

We find support for our proposal that gender is a significant factor for how people adapt their unethical behavior.[61] Men increase their lying behavior as soon as they are asked to think about other people's potential dishonesty in a different game. The level of unethicality provided in a signal about others' behavior does not increase or decrease males' dishonesty. In contrast, women do not change their behavior when asked to think about others' potential dishonesty. However, the level of unethicality in the signal has an unethicality-promoting effect on women. Hence, females' dishonesty increases with the level of unethicality provided in the signal. Our findings are in line with a previous finding of a gender-related difference in adapting to signals about unethicality (Fosgaard et al., 2013) and gender differences in preferences found in a variety of domains (Croson & Gneezy, 2009). In addition, our results support previous reports of gender differences in cooperation (Furtner et al., 2021; Molina et al., 2013), willingness to comply (Crawford et al., 1995; Eagly, 1978; Maccoby, 1974; Minton et al., 1971), generosity (Eckel & Grossman, 1998), technology adaptation (Venkatesh & Morris, 2000), competitiveness (Gneezy et al., 2003; Gneezy & Rustichini, 2004), selection into competition (Datta Gupta et al., 2013; Niederle & Vesterlund, 2007; Vandegrift & Brown, 2005), and risk-taking (Holt & Laury, 2002). Our results indicate that gender does not affect how people form beliefs about the unethical behavior of others. Hence, our findings strengthen the

[61] We recognize gender as a non-binary variable. However, only 24 of 1,351 participants in our sample identified as neither male nor female (1.78%). Therefore, we had to exclude this group from our analysis, in which we compare the decisions of subjects based on their gender.

case that men and women differ in how responsive they are to social cues (Gilligan, 1982/2016; Kahn et al., 1971; Roberts, 1991) rather than in how attentive they are to such cues (Bales & Parsons, 1956/2014; Garai, 1968; Williams & Best, 1982).

The remainder of the paper is organized as follows: In Section 5.2, we develop the relevant hypotheses for the two effects (*thinking* about others' behavior and *learning* about the actual behavior of others). Section 5.3 describes the experimental design. Section 5.4 presents our non-parametric and parametric analyses, which we split by gender. Finally, in Section 5.5, we offer a discussion of our results.

5.2 Hypothesis Development

In this paper, we investigate how merely thinking about and learning about others' behavior affects a person's decision to act ethically or unethically. In the relevant literature, the evidence predominantly supports the idea that men are more dishonest than women in economic experiments (in the laboratory: Conrads et al., 2014; Dreber & Johannesson, 2008; Grosch & Rau, 2017; Houser et al., 2012; and in the field: Azar et al., 2013; Bucciol et al., 2013). However, some researchers find no difference (Aoki et al., 2013; Childs, 2012). Moreover, DePaulo et al. (1996) and Tyler et al. (2006) provide opposing reports of more unethical behavior in females when considering all types of social interactions instead of limiting the observations to economic experiments. Still, it seems fair to assume that women are associated with less dishonesty than men. For example, Swamy et al. (2001) report a negative correlation between the level of unethicality and the percentage of women present in various situations, using multinational datasets that feature different levels of observability. Still, this gender difference seems quite complex and results from various factors. Previous findings report gender-related differences in preferences (Croson & Gneezy, 2009; Eckel & Grossman, 2008), in behavior related to lying (e.g., generosity: Eckel & Grossman, 1998), and in how men and women react to signals and social cues (Fosgaard et al., 2013; Kahn et al., 1971; Roberts, 1991). Therefore, we hypothesize different effects for men and women regarding the two influences we investigate in our treatments.

Thinking about others' behavior by eliciting an initial belief about it in an ethically loaded situation might increase the willingness to lie in a different moral situation.[62] Krupka and Weber (2009, p. 314) find a corresponding "focusing effect" of a norm in the domain of pro-social behavior. Likewise, Fosgaard et al. (2013) test a manipulation aimed at increasing awareness of cheating being a viable option. This is closely related to the idea that honesty occurs due to the absence of temptation (The "Grace hypothesis"; for weak support, see Greene & Paxton, 2009, p. 12506). Krupka and Weber (2009) do not report any gender differences in their paper. However, Fosgaard et al. (2013) report that the effect of their manipulation for cheating awareness holds for women only. While one might expect the same for the intervention used in our experiment (i.e., prior belief elicitation), we hypothesize the opposite outcome. In contrast to Fosgaard et al. (2013), our

[62] In our experimental design, we use the treatments NB-NS and B-NS to test for the aforementioned effect for men and women. Section 5.3 describes the design and treatment manipulations.

treatment manipulation introduces a) another group of subjects, b) a moral situation (faced by this second group of subjects), and c) the interplay between the second group and the moral situation, which offered increasing payoffs for unethicality.

Introducing another group and promoting thinking about other participants' behavior in a situation rooted in the domain of unethicality might prompt our participants to compare themselves to the other group and their situation. Two consequences are evident. On the one hand, this might remind participants of the social norm, that is being honest as the socially desirable choice (Abeler et al., 2019). On the other hand, they might feel a sense of competition because a group has been introduced. Now, participants can compare themselves and try to outdo the others in terms of payoff received. In principle, both consequences (the reminder of the social norm and the increased sense of competition) might be valid for all subjects. However, as stated above, there are reasons to suspect a gender difference. Previous studies have shown that men are more willing to compete (Datta Gupta et al., 2013; Niederle & Vesterlund, 2007; Vandegrift & Brown, 2005) and are potentially better in competitive situations (Gneezy et al., 2003; Gneezy & Rustichini, 2004) than women. Hence, we assume that men are more likely to see the belief elicitation as a form of getting into competition. Consequently, men might adapt their behavior towards more self-interest and higher payoffs in order to be better than the other group. We capture this hypothesized effect in Hypothesis 5.1. For women, previous findings indicate that their intentions are more often formed based on the currently apparent norm (Crawford et al., 1995; Eagly, 1978; Maccoby, 1974; Minton et al., 1971). Assuming that ethicality, rather than unethicality, is the predominant social norm when thinking about a moral situation, we hypothesize that a prior belief elicitation will decrease females' lying behavior (see Hypothesis 5.2). To sum up, we expect an unethicality-promoting effect from eliciting an initial belief about others' unethical behavior for men and the opposite for women.

Hypothesis 5.1. *Men lie more when an initial belief about others' unethical behavior is elicited than when it is not elicited.*

Hypothesis 5.2. *Women lie less when an initial belief about others' unethical behavior is elicited than when it is not elicited.*

There is some evidence that a signal about others' behavior influences one's own behavior. For pro-social behavior, Krupka and Weber (2009, p. 309) report an "informational effect" caused by a norm, stating that higher levels of the specific behavior in the norm are associated with more similar behavior. In addition, Desmet and Engel (2021) report that people are conditional rule followers. This implies that the extent to which people follow (or violate) a moral norm might depend on the number of rule followers (or violators) observed. Most closely related to our experiment, Innes and Mitra (2013) report in a series of experiments that higher levels of unethical behavior in such a signal are associated with more unethical behavior. However, in contrast to our experiment, Innes and Mitra (2013) use one situation rather than two distinct situations that both feature dishonesty. In the same domain and also using the same situation twice, Gino et al. (2009) show that one

participant acting as a cheating agent in a laboratory can affect whether other participants choose to behave unethically. Therefore, both results are related to what we investigate; however, both papers might observe imitation rather than the spread of unethicality we want to examine. All these findings are plausible, and it seems straightforward to hypothesize an unethicality-promoting effect of higher levels of unethicality in a behavioral signal. However, again, we know that the story might be more complex. We expect to find a gender-related difference in our experiment.

Based on our argumentation for Hypothesis 5.1 and Hypothesis 5.2 regarding the elicitation of a prior belief (without a signal being provided), we hypothesize a higher level of dishonesty in men than women. Given that women are, indeed, more likely to act in accordance with the evident moral norm (see Hypothesis 5.2), a signal about others' behavior containing unethicality might free them from feeling the immediate pressure to follow the norm.[63] We relate this line of thinking to previous research on self-depletion (Gino et al., 2011; Mead et al., 2009). Honesty decreases when the power to resist fades. Observing other participants violating the apparent norm in another situation might trigger women to also "forget" about the norm and go for the money on the table. In addition, women have a higher willingness to comply with observed norms (Crawford et al., 1995; Minton et al., 1971) and to confirm with the majority (Eagly, 1978; Maccoby, 1974). Therefore, if the signal contains high levels of unethicality, women might adapt their behavior according to what becomes the apparent norm. An underlying reason for this might be that women are more attentive and/or sensitive to social cues (Bales & Parsons, 1956/2014; Garai, 1968; Gilligan, 1982/2016; Kahn et al., 1971; Roberts, 1991; Williams & Best, 1982). This line of argument is strengthened by a finding that women are more likely to act as conditional cooperators (Furtner et al., 2021). In addition, when in leadership positions, women align their dishonest behavior with their belief on group members' honesty preferences (Grosch et al., 2021). Putting these points together, we argue that a higher level of unethicality provided in a signal about others' behavior in a moral situation increases dishonesty in women in a different, ethically loaded situation (see Hypothesis 5.3).

Hypothesis 5.3. *Women lie more when a higher level of unethicality by others is signaled to them.*

For men, some of the effects discussed above are less relevant or irrelevant. More precisely, we argue that a prior belief elicitation increases rather than decreases males' dishonesty (see Hypothesis 5.1). Hence, a signal cannot help them avoid following the apparent norm for honesty. Also, previous findings imply that women behave according to what they can observe about others' behavior, but men do not. In addition, men do not adapt their decisions to act unethically based on their belief on group members' preferences. In other words, men seem to not care about

[63] In our experimental design, we use the treatment B-S and its five sub-treatments (B-S-0%, B-S-25%, B-S-50%, B-S-75%, and B-S-100%) based on the level of unethicality in the signal to test for the aforementioned effect for men and women. Section 5.3 describes the design and treatment manipulations.

what others do or want. Therefore, we argue that the unethicality-promoting effect of higher levels of unethicality in a behavioral signal is not present for men.[64]

Hypothesis 5.4. *Men are not affected by the level of unethicality by others signaled to them.*

In summary, we hypothesize that both genders are affected by eliciting a prior belief about the dishonesty of others. However, we propose an unethicality-promoting effect for men (see Hypothesis 5.1) and an unethicality-reducing effect for women (see Hypothesis 5.2). In addition, we hypothesize that women are affected by the level of unethical behavior provided in the behavioral signal about others (see Hypothesis 5.3). In contrast, for men, we propose that this effect is not present (see Hypothesis 5.4).

5.3 Design and Procedure

The main experiment consists, in some treatments, of four tasks (Task 1, Task 2, Task 3, and Task 4). The tasks are always in the same order. In some treatments, tasks are featured in which we ask our subjects to form beliefs about the behavior of other subjects and, additionally, signal the choices of other subjects to them. To avoid deceiving our subjects, we conducted a pre-experiment with a small sample of subjects ($N = 81$). These subjects first perform several versions of the dot task by Gino et al. (2010) and then a variant of the die-rolling task by Fischbacher and Föllmi-Heusi (2013). Both games are explained in detail below, as they are also used in the main experiment. We use the collected data to calculate the payoffs for the belief elicitation and provide an accurate signal about others' behavior. Therefore, we refrain from analyzing this data in the current paper.[65]

5.3.1 Treatment with an Initial Belief Elicitation and a Signal About Others' Behavior (B-S)

We start by describing the treatment B-S of our main experiment, as it is the one in which all tasks are included. Subsequently, we describe the other treatments by pointing out the difference compared to this treatment. Table 5.2 provides an overview of how the three treatments differ regarding the four tasks. In B-S, participants face all four tasks (Task 1, Task 2, Task 3, and Task 4). This treatment consists of five sub-treatments called B-S-0%, B-S-25%, B-S-50%, B-S-75%, and B-S-100%. In order to explain how these sub-treatments differ, it is useful to describe

[64] Greene and Paxton (2009, p. 12506) propose the "Will hypothesis," arguing that dishonesty increases as a result of observing dishonesty in others. They find no support for this. In contrast, Fosgaard et al. (2013) find support for the Will hypothesis for men but not women. This appears not to be in line with our argumentation. However, the manipulation used by Fosgaard et al. (2013) can also be interpreted as a trigger of competition. More precisely, the observability in their experiment implies that ten out of ten other, previously paid participants of the current session acted dishonestly. Moreover, in the experiment by Fosgaard et al. (2013, p. 280), "subjects in each session know each other" because they are part of one "class [and are] ... forced to spend a lot time together." Hence, a male subject might perceive himself as being in competition with his fellow peers and adapt his behavior accordingly; this is in line with what we argue in Hypothesis 5.1. Their experiment does not feature different levels of unethicality.

[65] In Appendix 5.A, the screens of the main experiment are presented. Screens of the pre-experiment are available upon request.

the four tasks first. Note that all participants in all sub-treatments of B-S participate in all four tasks. The only difference between the sub-treatments is what is signaled to them at the end of Task 2.

Task 1: Initial Belief About Others in the Dot Task

In Task 1, we elicit an initial, incentivized belief about the behavior of others in a specific ethically loaded situation.[66] In the pre-experiment, subjects were shown an easy-to-judge picture of the dot task (Gino et al., 2010). In this dishonesty game, participants are asked to report on which half of a quadratic area ("left" or "right") more dots are located. Reporting "right," and therefore misreporting, leads to a higher payoff than reporting "left," which is a truthful report. In the main experiment, participants see the exact task previous participants faced and are asked to guess the proportion of the previous participants who reported "right." In other words, they are asked to think about the proportion of the previous participants that was willing to misreport to receive a higher payoff. The elicitation uses a five-item Likert scale (0% to 20%, ..., 81% to 100%). We refrain from using words like lying, misreporting, or high payoff but simply show the dot task and the corresponding incentives to avoid an interfering effect from the wording. However, it seems easy to understand that a misreport with a high outcome is an unethical choice in this case.[67] Therefore, the corresponding belief indicates how much unethical behavior the participants in our main experiment expect from another group of participants. We incentivize this belief with US$0.20 for a correct guess.

Task 2: Signal About Others' Behavior in the Dot Task

In Task 2, we inform our participants that they are going to learn what four participants of the other group have chosen for the specific example of the dot task. This is the same version of the dot task and the same group of other participants previously introduced in Task 1 of the main experiment. Following Innes and Mitra (2013), we let participants choose which participants' choices they want to observe. This is done to mitigate the experimenter demand effect (Zizzo, 2010). More precisely, we ask participants to select four out of 12 participants (named anonymously "Participant 1" to "Participant 12") from the other group.

Afterward, the choice of these four participants in the aforementioned dot task is revealed as either "right" or "left." Therefore, this signal about the others' behavior contains either zero, one, two, three, or four out of four observations of unethical behavior ("right"), which makes the specific levels of unethicality 0%, 25%, 50%, 75%, or 100%, respectively. Hence, the treatment splits into

[66] We do not ever inform the subjects in our experiment about the sample size of our pre-experiment. We only inform them that subjects recruited via the same platform have participated in it. In addition, we inform our subjects about the incentives in the pre-experiment.

[67] In the original experiment by Gino et al. (2010), some screens feature easy-to-judge pictures, while others present a picture for which a subject has to diligently count the dots in each half to be able to respond with certainty (truthfully or untruthfully). In our main experiment, the belief elicitation is done over an easy-to-judge version. Hence, it becomes quite clear which choice ("left" or "right") is a misreport.

five sub-treatments according to the level of unethical behavior (B-S-0%, B-S-25%, B-S-50%, B-S-75%, and B-S-100%).[68] There are no financial incentives associated with Task 2, but subjects have to make their choices in order to proceed to the subsequent task.

Task 3: Report of a Die Roll (Die-Rolling Task)

In Task 3, we use an adapted version of the die-rolling task by Fischbacher and Föllmi-Heusi (2013). Similar to how Kocher et al. (2018) present the die roll outcome, a video of a ten-sided die roll is presented to the participants. In our experiment, the outcome of the throw is random, but not all outcomes are equally likely. There is a 91% probability of seeing the die roll outcome "2" and a 9% probability of seeing a different outcome, with equal probabilities for each of the nine other outcomes. Therefore, we were able to communicate to our participants that the outcome is random. This also enables us to limit the observations to participants with the same die roll outcome ("2"). In the instructions, we do not state the underlying probabilities. We ask participants to watch the video and report the outcome of the die roll. This report is incentivized from US$0.30 to US$3.00, as presented in Table 5.1.

Report in Task 3	1	2	3	4	5	6	7	8	9	10
Payoff in US$	0.30	0.60	0.90	1.20	1.50	1.80	2.10	2.40	2.70	3.00

Table 5.1: Monetary Incentives of Task 3

In contrast to previous research by Fosgaard et al. (2013), Gino et al. (2009), and Innes and Mitra (2013), we use two distinctly different, ethically loaded situations: On the one hand, the initial belief elicitation (Task 1) and the signal about others' behavior (Task 2) use the dot task (Gino et al., 2010). On the other hand, the main behavioral measure in our experiment (Task 3) is a modified version of the die-rolling task (Fischbacher & Föllmi-Heusi, 2013; Kocher et al., 2018).

This combination comes with three advantages: First, providing a signal about others' unethicality (Task 2) in a situation different from the following behavioral measure (Task 3) does not contaminate the effect with pure imitation of observed behavior but allows us to investigate the spread of unethicality based on observed unethical behavior. Second, the first situation (the dot task) is easy to understand and allows for a clear judgment of what is (un)ethical. Hence, it is a good game for eliciting the level of unethicality participants expect from another group (Task 1) and signaling a certain level of unethicality to our participants (Task 2). Third, while the initial belief and the signal are easy to understand, because both build on a binary outcome of either

[68] To collect an approximately equal number of observations for each of the five sub-treatments of B-S, we varied which participants from the other group are associated with "Participant 1" to "Participant 12." By doing so, we create different distributions of unethicality in the 12 participants from which participants in the main experiment can choose four. This is not communicated to our participants.

definitely ethical or definitely unethical, the main behavioral measure features a decision space for the participants, enabling us to potentially detect an adaptive increase in unethical behavior. However, it turns out that we could not make use of this last advantage, as the behavior in our samples is predominately binary in Task 3.[69]

Task 4: Belief About Others in the Die-Rolling Task

So far, we have described three tasks (Task 1, Task 2, and Task 3) for which we use two different dishonesty games (the dot task for Task 1 and Task 2; the die-rolling task for Task 3). In Task 4, we elicit a second, incentivized belief about the behavior of others in the same ethically loaded situation the participants in our main experiment face as Task 3. The first belief elicitation in Task 1 measures the participants' initial belief about the behavior of others in another ethically loaded situation. In contrast, this second belief elicitation in Task 4 measures the participants' induced belief about the behavior of others in the same ethically loaded situation they have decided on moments prior (i.e., the die-rolling task). This second elicitation also takes place after participants have received a signal (Task 2) about four participants and how they have decided in the dot task. To be more precise, we inform the participants in our main experiment that the other group had also been tasked with Task 3. We then ask our participants to guess what most participants in the other group reported for the same die roll outcome they have just seen. A correct guess (for example, "2" if the majority in the other group reported "2") earns the participant US$0.20; a guess one away from the correct answer (either "1" or "3" in the example above) pays US$0.10. This last task serves to check whether the signal provided in Task 2 induces an update in the belief about the unethicality of the other group. Hence, we can differentiate such an update (captured in Task 4) from a behavioral reaction (captured in Task 3).

5.3.2 Treatment With an Initial Belief Elicitation but Without a Signal About Others' Behavior (B-NS)

The second treatment of our experiment has the same structure and order of tasks as the B-S treatment. The only difference is that in B-NS, Task 2 is omitted, that is, the treatment B-NS has no signal about others' behavior in the dot task. More precisely, we left out the elements of selecting participants whose choices are disclosed (the first part of Task 2) and receiving a signal (the second part of Task 2). The initial belief elicitation about others' behavior in the dot task (Task 1) is still included.

[69] More precisely, we use a ten-sided die to allow for more steps between the ethical option (truthful report) and the most unethical option (highest possible report). However, we find that participants do not adapt their behavior within this space but rather opt for the ethical or most unethical choice. Only 39 of 1,351 participants in our sample (2.89%) produce reports that are neither truthful nor the highest possible untruthful report (i.e., reports corresponding to the die roll and reports of "10," respectively). Therefore, we excluded this small group of partial liars from our analysis and proceed with the analysis of a binary variable.

5.3.3 Treatment With Neither an Initial Belief Elicitation nor a Signal About Others' Behavior (NB-NS)

The third and last treatment, NB-NS, has the same structure and order of tasks as the B-S treatment. The difference is that in NB-NS, both Task 1 and Task 2 are omitted. Thus, the subjects neither form beliefs about others in the dot task (Task 1) nor receive a signal about others' behavior in the dot task (Task 2). We compare this treatment (NB-NS) against B-NS to investigate the effect of an initial belief elicitation introducing prior thinking about others' behavior. It also serves as the baseline for our parametric analysis in Section 5.4. Table 5.2 summarizes which tasks are included in each of the three treatments.

	Treatments		
	B-S	B-NS	NB-NS
Task 1: Initial belief	Yes	Yes	**No**
Task 2: Signal	Yes	**No**	**No**
Task 3: Report	Yes	Yes	Yes
Task 4: Control belief	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes

Note: The four tasks are described in Subsection 5.3.1. In Task 2, different levels of unethicality are included in the signal. Hence, the treatment B-S splits into B-S-0%, B-S-25%, B-S-50%, B-S-75%, and B-S-100%. Details regarding the additional controls are described in Subsection 5.3.4.

Table 5.2: Overview of Included Tasks

5.3.4 Additional Controls

In all three treatments, we include a set of additional controls in the form of questionnaires aimed at measuring personality traits connected to unethicality. To control for the participants' general preference for honesty and dispositional greed, we use the honesty-humility dimension of the HEXACO-60 (Ashton & Lee, 2009) and the dispositional greed and materialism scale (Seuntjens et al., 2015). In our study, we use treatment manipulations affecting the position of the individual decision relative to the morally right and morally still acceptable choices. Therefore, we also control for a concept called moral disengagement (Shu et al., 2011). In addition, we use a battery of sociodemographic questions, including gender and age, to control for some of the heterogeneity in a broad online subject pool. As we do not derive hypotheses based on correlations between these controls and our main outcome variable, we only add them to our parametric analysis as additional

explanatory variables to test the robustness of our results. While we show that the inclusion of these controls does not change our results, we do not discuss the coefficients and p-values associated with these control variables.

5.3.5 Procedure and Subjects

For the main experiment, we recruited 1,351 subjects using Prolific (Palan & Schitter, 2018). The online experiment took place from June 2019 to June 2021 and was realized using Qualtrics. The sample is limited to US citizens with a high school degree or a higher educational level who were born in the US and lived there at the time of the experiment. Of the 1,351 participants, 1,245 saw a die roll outcome of "2." We focus our analysis on observations of these participants. As previously stated, while we recognize gender as a non-binary variable, we exclude observations of participants who identified as neither male nor female ($N = 22$; 1.77%). Likewise, we exclude observations of participants who decided to lie partially ($N = 33$; 2.65%). Partial lying is defined as not reporting either truthfully ("2") or completely untruthfully ("10"). At the end of the experiment but before participants are informed about their payoff, we ask them whether they are technically able to watch a video. If they confirmed that they were, we ask them to watch a four-second video displaying the word "dog" and report its content. With this simple technical check, we are able to filter out 11 participants (0.88%) who could not watch videos for technical reasons. With some of these exclusions overlapping, we end up with 1,182 participants whose data we analyze.[70] Of these, 50.93% identified as female (Binominal probability test: $p = 0.541$). The average participant was 34.08 years old ($SD = 11.88$). The average time spent on the experiment was 8.72 minutes ($SD = 5.36$). Participants earned US$2.24 ($SD = 0.95$) on average. The minimum payoff was US$1.60, and the maximum was US$4.40, including a show-up fee of US$1.00.

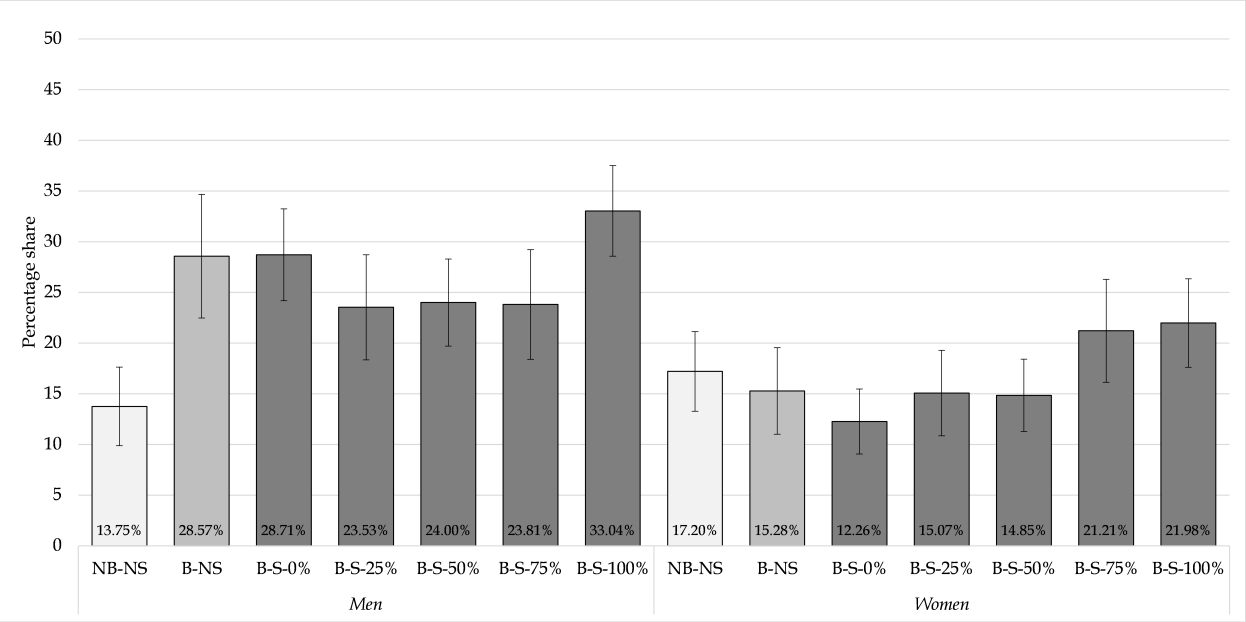
5.4 Results

As explained in Subsection 5.3.5, the following analysis is based on 1,182 participants in our sample. We excluded participants if they identified as neither male nor female, failed to pass a technical check for being able to watch the video of the die roll outcome, received a die roll outcome other than "2," or lied partially. We use a binary variable (*untruthful report = 1/truthful report = 0*) for our analysis of participants' unethical behavior (Task 3). Here, we start by looking at general gender differences in lying behavior. Subsection 5.4.1 covers the effect of thinking about others' behavior by experiencing an initial Belief elicitation (Task 1). Subsection 5.4.2 sheds light on the effect of receiving a Signal about others' behavior containing a certain Level of unethicality (Task 2). Lastly, Subsection 5.4.3 follows up with an investigation of the second belief elicitation (Task 4) to ensure that our manipulation worked as intended.[71]

[70] The total number of included observations (1,182) splits over treatments (and sub-treatments) as follows: B-S: 881, (B-S-0%: 207, B-S-25%: 141, B-S-50%: 201, B-S-75%: 129, B-S-100%: 203), B-NS: 128, and NB-NS: 173. Variations in the number of participants per (sub-)treatment are a result of randomization.

[71] Appendix 5.B provides an overview of the hypotheses and results discussed in the following subsections.

We find support for a gender difference (Fisher's exact test over all treatments: $p < 0.001$).[72] This finding is in line with previous findings showing more unethical behavior by men than women in economic situations. Regarding the three different treatments (B-S, B-NS, and NB-NS), this finding holds for B-S ($p < 0.001$) and more weakly for B-NS ($p = 0.082$). However, we do not find a significant difference between males' and females' lying behavior in NB-NS ($p = 0.675$). This mixed finding, ranging from a highly significant difference to no significance for such a difference, might indicate why some researchers find no difference (Childs, 2012), while others do (Azar et al., 2013; Dreber & Johannesson, 2008; Grosch & Rau, 2017). However, taken together, there is more evidence in our data that men are, indeed, more willing to lie in order to generate a benefit for themselves. In the following two subsections, we will delve into how (and why) lying behavior differs between the genders.



Note: Error bars represent the standard errors.

Figure 5.1: Lying Behavior Over Treatments and Gender

5.4.1 Behavioral Reaction to Thinking About Others' Behavior

The difference in participants' behavior between NB-NS and B-NS can be attributed to thinking about the behavior of others without actually learning something about their choices. Design-wise, the only difference between the two treatments is whether Task 1 is omitted. Task 1 is the initial Belief elicitation in which we ask participants to guess the share of unethically behaving participants in another game (i.e., the dot task).

For men, we observe an increase from 13.75% (NB-NS) to 28.75% (B-NS) in lies, which is a

[72]Presented results of Fisher's exact tests do not differ relevantly when using χ^2 tests throughout the paper.

strong and significant difference (Fisher's exact test: $p = 0.048$). In contrast, no such increase can be observed for women (17.20% to 15.28%; $p = 0.833$). While there was no difference between men and women in NB-NS ($p = 0.675$), men lie significantly more than women in B-NS ($p = 0.082$). Figure 5.1 shows the corresponding means over treatments. In addition to the non-parametric testing, we also apply parametric testing in the form of probit regressions with the binary lying variable as the dependent variable split by gender.[73] Table 5.3 shows the results of six models: Models 1 to 3 feature male participants, while Models 4 to 6 represent the observations from female participants. Models 1 and 4 feature the effect of eliciting an initial BELIEF, providing a SIGNAL, and the LEVEL of unethicality contained in the signal on lying behavior. In Models 2 and 5, we add additional sociodemographic controls. In Models 3 and 6, we additionally add controls in the form of questionnaires about honesty-humility, dispositional greed, and moral disengagement. Our parametric analysis supports the above-mentioned findings. Eliciting an initial BELIEF (i.e., introducing another group and encouraging consideration of their unethicality in another ethically loaded situation) significantly increases the likelihood of observing a man lying (Model 1: $p_{\text{BELIEF}} = 0.035$) but not a woman lying (Model 4: $p_{\text{BELIEF}} = 0.740$). This effect persists even when additional controls are added.

Overall, we conclude two things: First, men adapt their behavior in an ethically loaded situation when they are asked beforehand to think about others' behavior in another ethically loaded situation. This implies support for Hypothesis 5.1 and is in line with previous research regarding selection into competition and competitiveness (Datta Gupta et al., 2013; Gneezy et al., 2003; Gneezy & Rustichini, 2004; Niederle & Vesterlund, 2007; Vandegrift & Brown, 2005). Second, women seem not to be affected by this, which is not in line with our hypothesis. In Hypothesis 5.2, we argue that women are likely to reduce their lying behavior as a consequence. While the mean does, indeed, decrease (from 17.20% to 15.28%), this turns out not to be significantly different. Therefore, we reject this hypothesis.

Result 5.1. *Men lie more when asked to think about others' behavior in another ethically loaded situation.*

Result 5.2. *Women's lying behavior is not affected by thinking about others' behavior in another ethically loaded situation.*

5.4.2 Behavioral Reaction to Receiving a Signal About Others' Behavior

In the previous subsection, we present evidence in favor of supporting a gender-specific effect of thinking about others' behavior in another moral situation. Regarding an actual SIGNAL containing a certain LEVEL of unethicality by others in another moral situation, we also hypothesize a gender-specific effect. As stated above, we find that men lie more often than women (all treatments: $p < 0.001$). This holds for treatments in which participants are asked to think about others' behavior (B-NS: $p = 0.082$) and when others' behavior is signaled to them (B-S: $p < 0.001$).

[73] In Appendix 5.C, we present a version in which the main variables of interest are interacted with gender rather than splitting the observations by gender. The results for thinking about others' behavior hold as discussed here.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
		Men			*Women*	
BELIEF (1 if provided)	0.526**	0.566**	0.453*	−0.078	−0.118	−0.184
	(0.249)	(0.254)	(0.260)	(0.236)	(0.243)	(0.252)
SIGNAL (1 if provided)	−0.167	−0.186	−0.140	−0.345	−0.487*	−0.340
	(0.256)	(0.261)	(0.267)	(0.279)	(0.292)	(0.303)
LEVEL (of Unethicality in SIGNAL)	0.031	0.024	0.030	0.101**	0.133***	0.110**
	(0.043)	(0.043)	(0.045)	(0.049)	(0.051)	(0.053)
Constant	−1.092***	−0.992**	−1.277**	−0.946***	−1.429**	−1.790**
	(0.175)	(0.474)	(0.567)	(0.153)	(0.604)	(0.695)
Sociodemographics	NO	YES	YES	NO	YES	YES
Additional Questionnaires	NO	NO	YES	NO	NO	YES
Pseudo R^2	0.012	0.024	0.088	0.008	0.018	0.084
Number of observations	580	577	577	602	594	594

Note: The dependent variable for all six models is the participants' decision to lie (*untruthful report = 1/truthful report = 0*) in Task 3 (see Subsection 5.3.1). NOBELIEF-NOSIGNAL (NB-NS) is the reference group in all models. Standard errors are in parentheses. Models 1 to 3 include observations of male participants, and Models 4 to 6 include those of female participants. In Models 2 and 5, sociodemographic characteristics are added as additional control variables. These characteristics are age, employment status, college education, household income, own contribution to household income, English native speaker status, and race. Model 2 includes a slightly smaller number of observations than Model 1 due to the exclusion of two subjects reporting unreasonably low ages and one subject not saying which race he belonged to. Likewise, Model 5 includes a slightly smaller number of observations than Model 4 due to the exclusion of eight subjects not saying which race they belonged to. In Models 3 and 6, additional controls are added. These controls include honesty-humility (dimension of the HEXACO-60: Ashton & Lee, 2009), dispositional greed (Seuntjens et al., 2015), and moral disengagement (Shu et al., 2011). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.3: Results of Probit Regressions

When investigating the effect of such a SIGNAL, regardless of the LEVEL of unethicality contained in it, we find no differences in male and female lying behavior (B-NS vs. B-S for men: $p = 0.874$; and for women: $p = 0.865$). While there seems to be no general effect of such a SIGNAL for both genders, there is a major difference in how men and women react to the LEVEL of unethicality provided. Bear in mind that in our experimental design, the LEVEL of unethicality in the SIGNAL could be 0%, 25%, 50%, 75%, or 100%. The average rate of female liars is lower in the sub-treatments B-S-0%, B-S-25%, and B-S-50% but higher in B-S-75% and B-S-100% when compared with the treatment's average (B-S: 16.70%). We find evidence that the female level of lying behavior is correlated with the LEVEL of unethicality provided in the SIGNAL (Spearman's rank correlation: $\rho = 0.098$, $p = 0.042$).[74] In contrast, testing for a correlation between the LEVEL

[74]Presented results of Spearman's rank correlation tests do not differ relevantly when using Point-Biserial or Somer's D correlation tests, if feasible, throughout the paper.

of unethicality and male lying behavior yields no significant relationship ($\rho = 0.036$, $p = 0.454$). All these findings (i.e., no general effect of the SIGNAL for both genders and a LEVEL-dependent effect for women but not men) are supported by our parametric analysis. More precisely, we find a significant effect of the LEVEL of unethicality for women (Model 4: $p_{\text{LEVEL}} = 0.042$). However, there is no such effect in men (Model 1: $p_{\text{LEVEL}} = 0.460$). Again, this holds when adding additional controls.[75] In summary, we find an effect of increased levels of unethicality affecting females' dishonesty, as hypothesized (see Hypothesis 5.3). In addition, we argue that men are unaffected affected by this. Indeed, we find no effect for men and, therefore, find support for Hypothesis 5.4. The behavioral reaction by women (but not men) is in line with previous research arguing that women are more attentive or responsive to social cues than men (Bales & Parsons, 1956/2014; Garai, 1968; Gilligan, 1982/2016; Kahn et al., 1971; Roberts, 1991; Williams & Best, 1982). Such a reaction might also be caused by females' higher willingness to adapt their behaviors based on the choices or believed preferences of others (Furtner et al., 2021; Grosch et al., 2021; Venkatesh & Morris, 2000) and comply with the apparent norms (Crawford et al., 1995; Eagly, 1978; Maccoby, 1974; Minton et al., 1971).

Result 5.3. *Women lie more when a higher level of unethicality by others is signaled to them.*

Result 5.4. *Men's lying behavior is not affected by receiving a signal or the level of unethicality contained in it.*

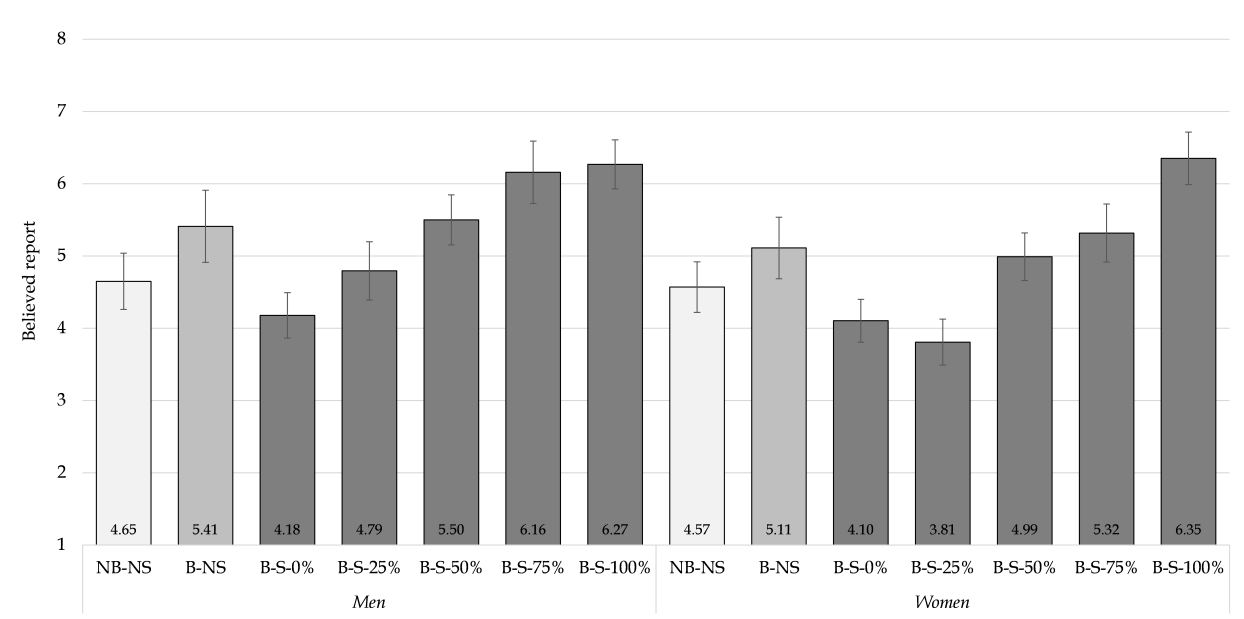
5.4.3 Manipulation Check

To delve deeper into how our treatment manipulations work, we present the outcome of the second belief elicitation in this subsection. In Task 4, we ask participants to guess which report the majority of the other group provided for the die roll outcome they have just seen (in Task 3). All treatments include Task 4.

Figure 5.2 shows the corresponding means of the second belief. There are two points[76] that stand out: First, we find that men and women adapt their beliefs in accordance with the level of unethicality provided in the signal (Spearman's rank correlation for men: $\rho = 0.228$, $p < 0.001$;

[75] In Appendix 5.C, we present a version in which the main variables of interest are interacted with gender rather than splitting the observations by gender. While the results for thinking about others' behavior hold as discussed, we lack finding clear supporting evidence for the discussed results regarding the effect of learning about others' behavior. More precisely, the regressions using an interaction show a significant effect of the LEVEL of unethicality for men and women. Meanwhile, the additional, negative effect for men remains insignificant. We attribute this to the low value and somewhat noisy behavior by men rather than a general problem with our analysis presented in the main text. Moreover, at least in Model 2 in Table 5.C.1, the 10% significance level for the additional, negative effect of the LEVEL for men (hence, the lack of such an effect when taken together with the aforementioned positive effect for both subgroups) is just barely missed ($p = 0.110$).

[76] There is a third point we want to mention: Previous research discusses whether men and women differ in their attentiveness or responsiveness to social cues (for arguments in favor of attentiveness, see Bales & Parsons, 1956/2014; Garai, 1968; Williams & Best, 1982; for arguments in favor of responsiveness, see Gilligan, 1982/2016; Kahn et al., 1971; Roberts, 1991). Our experimental results indicate that men and women follow the same pattern when adapting their beliefs based on our treatment manipulations. In other words, we find support for both genders being equally attentive to the social cues we implement. However, we find different behavioral reactions to these cues. Hence, men and women seem to differ in terms of their responsiveness rather than their attentiveness to social cues.



Note: Error bars represent the standard errors.

Figure 5.2: Induced Belief Over Treatments and Gender

and for women: $\rho = 0.257$, $p < 0.001$). Therefore, our manipulation of providing a specific level of unethicality is apparently successful for both genders. Put somewhat more simply, both men and women understand the signal and its content and adapt their beliefs about others accordingly. However, only women react with a change in behavior. Second, we could not find a significant difference in the second belief between NB-NS and B-NS for either gender (Mann-Whitney test for men: $|z| = 1.259$, $p = 0.208$; and for women: $|z| = 0.807$, $p = 0.420$). Introducing an initial belief elicitation (Task 1) and thereby encouraging consideration of another group of participants and their unethicality (and corresponding profits) does not lead to an updated belief about their unethicality. This result seems to be straightforward. However, it becomes relevant when considering that men (but not women) react with a change in behavior. Hence, while men do not adapt their beliefs about the other group (and, indeed, there is no valid reason to do so), men consider the other group when deciding to misreport for their own good. We argue that the most likely cause is an increase in the perceived competitiveness of the situation – an opportunity for males' unethicality to thrive while women do not react significantly.

5.5 Discussion and Further Research

In this paper, we present an experimental study with two distinct moral situations. We investigate how merely thinking about and learning about the dishonesty of others affect one's unethical behavior in another ethically loaded situation. We find evidence for gender-specific effects.

Men adapt their lying behavior in an ethically loaded situation when they are asked beforehand

to think about others' behavior in another ethically loaded situation. However, we find no evidence that this effect is present in women. We argue that the effect in men is most likely triggered by a change in the decision environment, allowing the decision to be seen in the light of competition (Gneezy et al., 2003; Gneezy & Rustichini, 2004). Men willingly accept and appreciate this competition (Datta Gupta et al., 2013; Niederle & Vesterlund, 2007; Vandegrift & Brown, 2005), and, consequently, males' unethicality thrives. In contrast to men, women adapt their lying behavior in an ethically loaded situation according to different levels of unethicality provided beforehand in a signal about others' behavior in another ethically loaded situation. We find no evidence that this effect is present in men. We conclude that our findings are in line with previous research reporting that women are more responsive to social cues (Gilligan, 1982/2016; Kahn et al., 1971; Roberts, 1991), more willing to comply with apparent norms (Crawford et al., 1995; Eagly, 1978; Maccoby, 1974; Minton et al., 1971), and more likely to adapt their behavior accordingly (Furtner et al., 2021; Molina et al., 2013; Venkatesh & Morris, 2000). Krupka and Weber (2009) report a focusing and an informational influence from norms in the pro-social domain. For the domain of unethicality, we suggest that related effects of merely thinking about or learning about others' behavior are gender specific. However, more research is needed to pinpoint the exact triggers.

Unethicality and its spread can lead to devastating outcomes (Abdullah et al., 2021; Cohn et al., 2014; O'Brien, 2003) and threaten the economic and social wellbeing of societies. Therefore, it is important that managerial-focused research follows up on our research in trying to understand the spread of unethical acts and how this process is gender specific. In addition, leadership may play a vital role in real-life economic situations. While men stay equally dishonest when deciding for themselves or as leaders of a group, dishonesty in women increases as they align their unethical behavior with their belief on group members' honesty preferences (Grosch et al., 2021) – an alarming finding for practitioners. Promotion-focused instead of prevention-focused ethical leadership might be one way to decrease counterproductive work behavior and the spread of unethicality (Bush et al., 2021). Future research might benefit from investigating the effect of leadership as well as thinking and learning about others' behavior.

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Appendix 5.A Screens of the Online Experiment

In the Belief-Signal (B-S) treatment, participants are welcomed with the information that today's online experiment consists of three tasks (see Figure 5.A.1). In Section 5.3, we described four tasks (Task 1, Task 2, Task 3, and Task 4). Indeed, Task 2 is part of what participants received as their first task. Hence, participants' first task in B-S is Tasks 1 and 2, while participants' second task corresponds to what we describe as Task 3, and the third task corresponds to what we describe as Task 4.

After the general introduction, participants are instructed for their first task (see Figure 5.A.2) and complete it (see Figure 5.A.3). This part corresponds to what we describe as Task 1 in this paper. Therefore, it is what we use to introduce another group and the moral situation they faced to the participants in our experiment.

Following the first task (but not labeled as the second task) is the selection of other participants whose behavior the participant learns about (see Figure 5.A.4). Based on the selection, the corresponding information is provided (see Figure 5.A.5), making the level of unethicality zero, one, two, three, or four out of four. This part corresponds to what we describe as Task 2 in this paper.

In the participants' second task, the die-rolling task is presented, as described as Task 3 above. The instructions are shown in Figure 5.A.6. The decision screen, with an unviewed and a viewed video, is shown in Figure 5.A.7 and Figure 5.A.8, respectively.

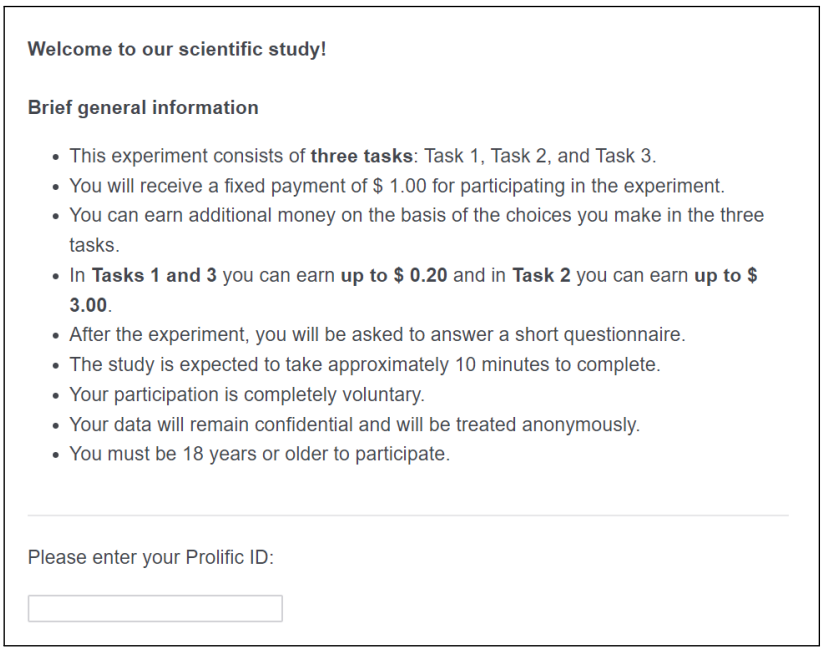
In this paper, we have given the name Task 4 to what is the third task for the participants. The corresponding instructions can be found in Figure 5.A.9. These are continued in Figure 5.A.10 and Figure 5.A.11, again with the video being either unviewed or already viewed, respectively. Figure 5.A.12 shows the decision screen of the main measure.

In the Belief-NoSignal (B-NS) treatment, Task 2 (hence, the screens between the participants' first and second task) are left out. Therefore, the screens presented in Figure 5.A.4 and Figure 5.A.5 are not included in this treatment. All other screens remain unchanged.

In the NoBelief-NoSignal (NB-NS) treatment, Tasks 1 and 2 (hence, the participants' first task and the part between their first and second task) are left out. Consequently, the screens presented in Figure 5.A.2, Figure 5.A.3, Figure 5.A.4, and Figure 5.A.5) are not included in this treatment. Therefore, participants in this treatment start with the second task. The enumeration is adapted accordingly. Likewise, the general introduction and the last screen are slightly modified. Additional screens for this treatment are available upon request.

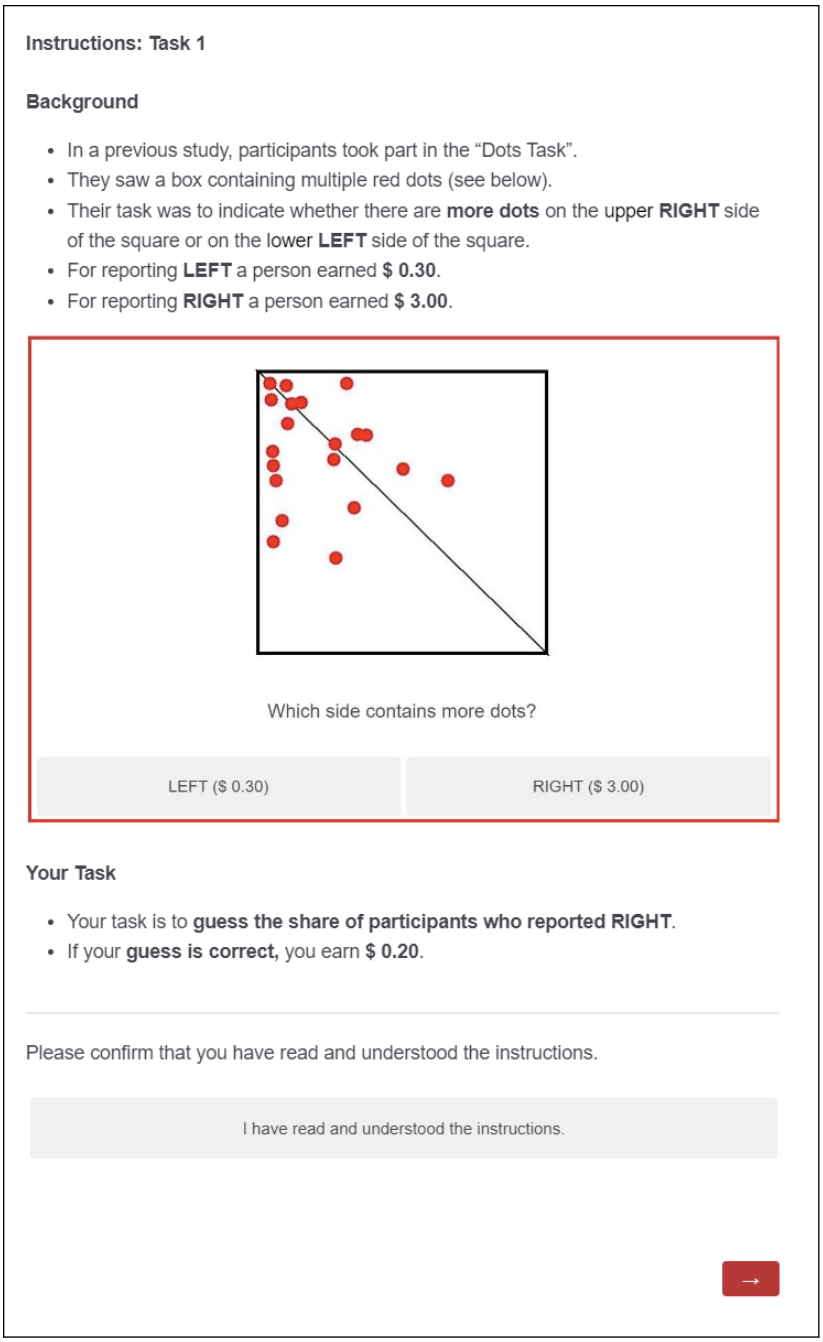
The subsequent parts of the online experiment is identical in all treatments. First, participants transition to the questionnaires (see Figure 5.A.13). The first questionnaire is the honesty-humility dimension of the HEXACO-60 (Ashton & Lee, 2009, see Figure 5.A.14 and Figure 5.A.15). Subsequently, the second questionnaire (dispositional greed Seuntjens et al., 2015, see Figure 5.A.16 and Figure 5.A.17) and third questionnaire (moral disengagement Shu et al., 2011, see Figure 5.A.18 and Figure 5.A.19) are provided. On the last screen in the questionnaire section, the sociodemographic questions are asked (see Figure 5.A.20, Figure 5.A.21, and Figure 5.A.22). Figure 5.A.23 and Figure 5.A.24 show the technical check with the video unviewed and viewed, respectively.

All treatments end with payoff information on the last screen (see Figure 5.A.25).



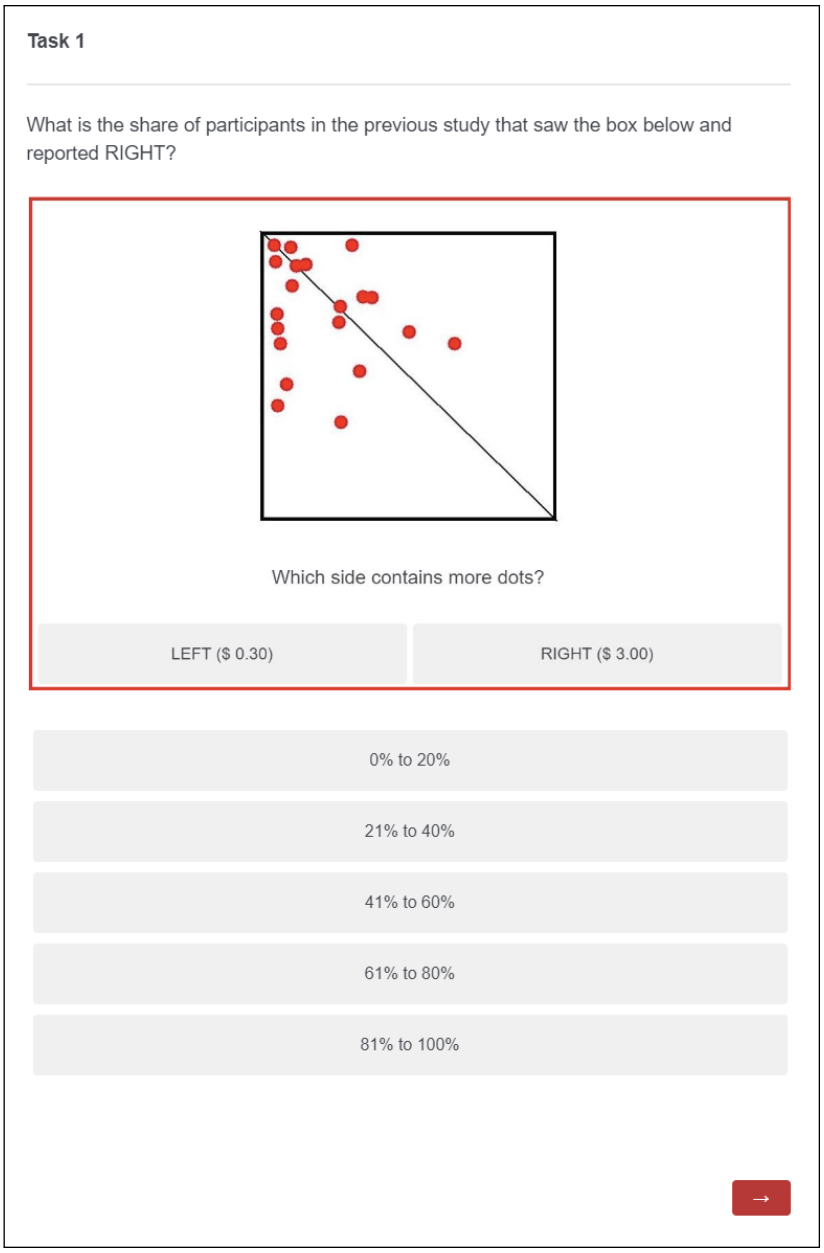
Note: In NB-NS subjects are informed that the experiments consists of two (rather than three) tasks as shown here. Subjects in NB-NS do not participate in the belief elicitation (first task in B-NS) and do not receive a signal about others (jointly with the belief elicitation; first task in the B-S). The information about payment is modified accordingly.

Figure 5.A.1: General Introduction



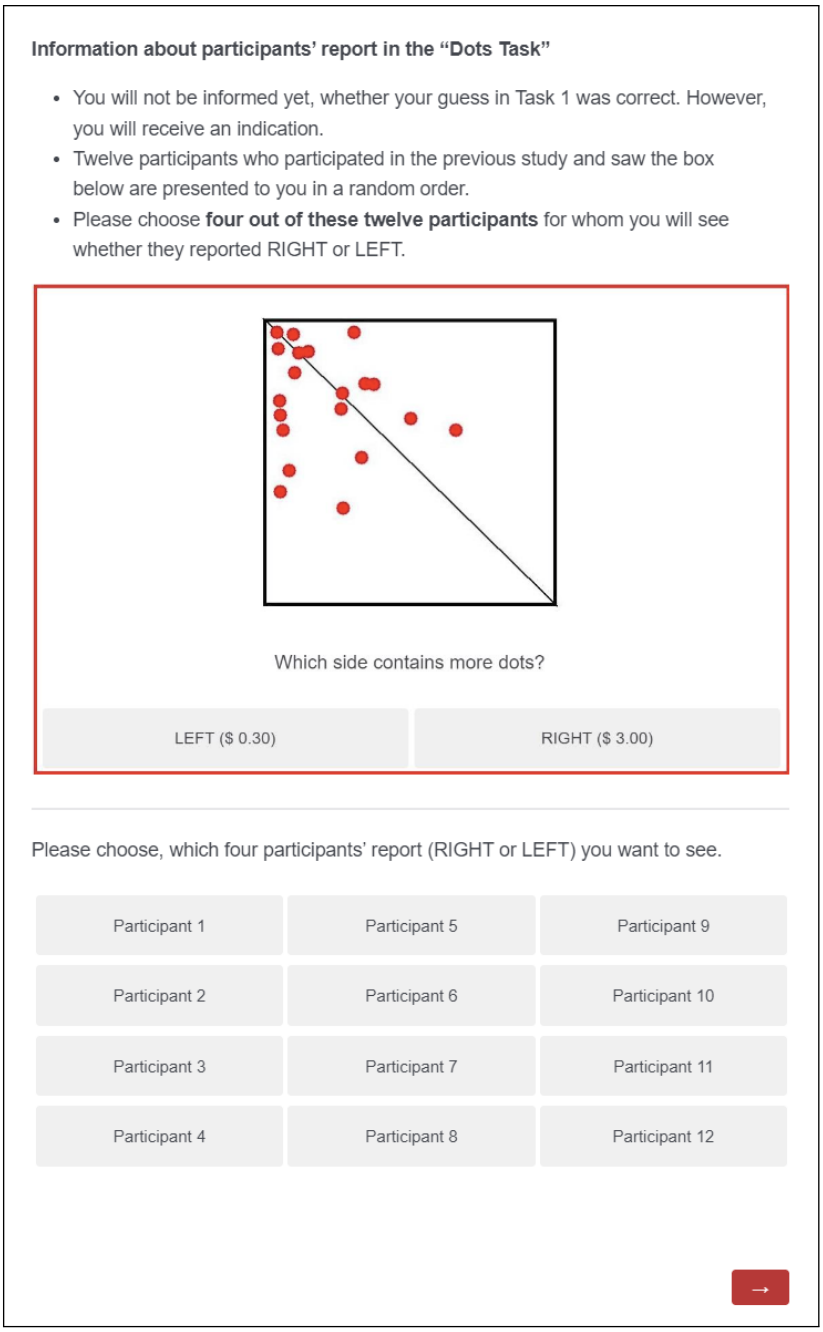
Note: This screen is not provided in NB-NS.

Figure 5.A.2: Instructions for the First Task (Task 1)



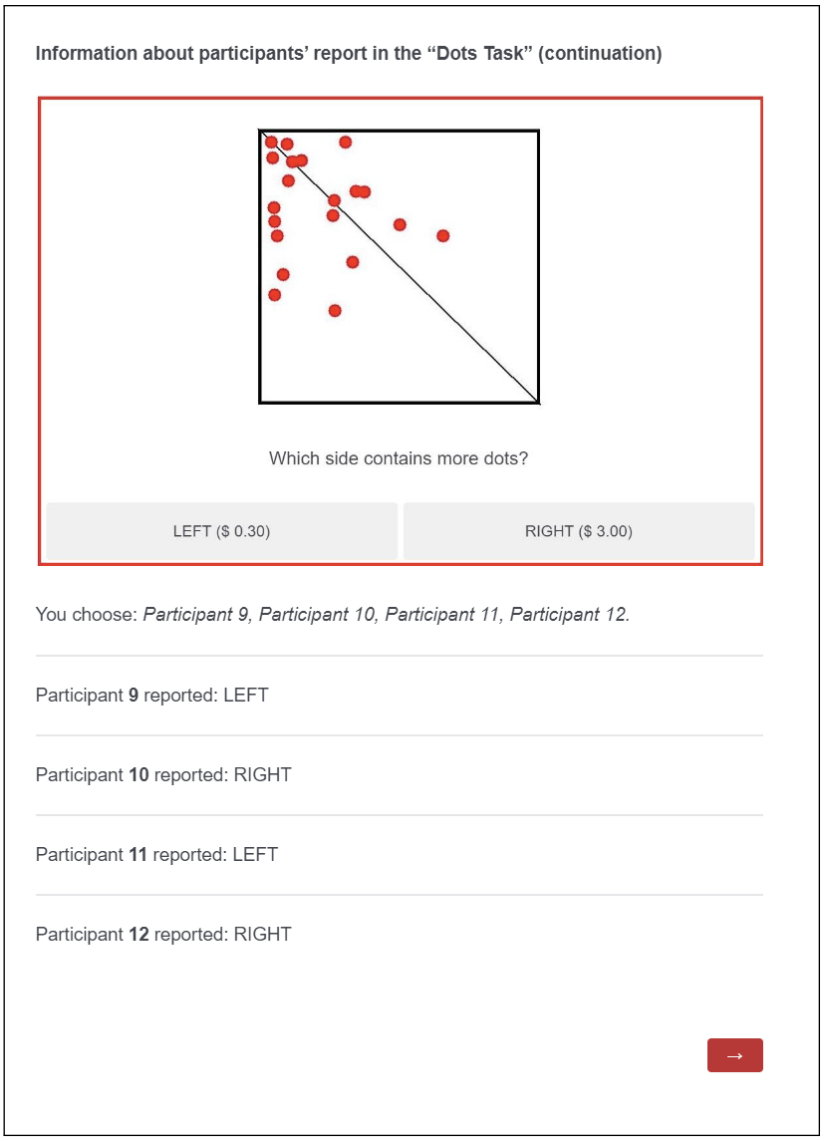
Note: This screen is not provided in NB-NS.

Figure 5.A.3: Decision Screen for the First Task (Task 1)



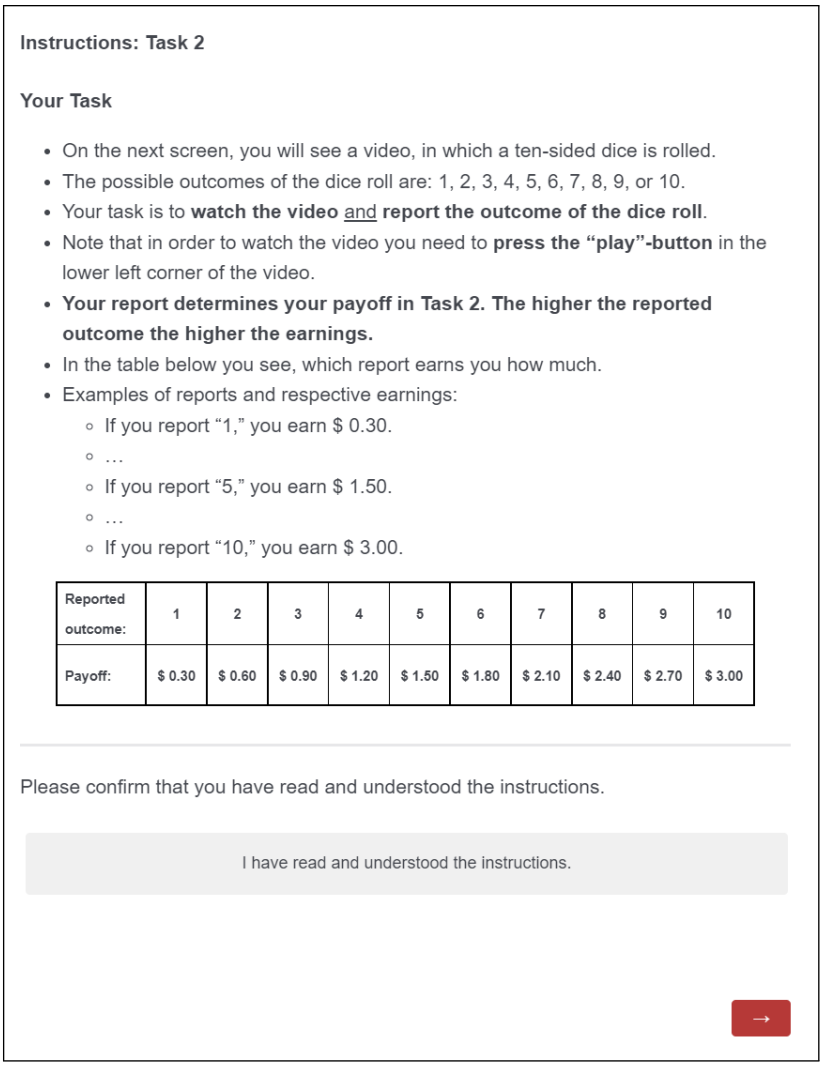
Note: This screen is provided in B-S only.

Figure 5.A.4: Selection Screen for the Information (Task 2)



Note: This screen is provided in B-S only.

Figure 5.A.5: Screen With the Information (Task 2)



Instructions: Task 2

Your Task

- On the next screen, you will see a video, in which a ten-sided dice is rolled.
- The possible outcomes of the dice roll are: 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10.
- Your task is to **watch the video** and **report the outcome of the dice roll**.
- Note that in order to watch the video you need to **press the "play"-button** in the lower left corner of the video.
- **Your report determines your payoff in Task 2. The higher the reported outcome the higher the earnings.**
- In the table below you see, which report earns you how much.
- Examples of reports and respective earnings:
  - If you report "1," you earn $ 0.30.
  - …
  - If you report "5," you earn $ 1.50.
  - …
  - If you report "10," you earn $ 3.00.

Reported outcome:	1	2	3	4	5	6	7	8	9	10
Payoff:	$ 0.30	$ 0.60	$ 0.90	$ 1.20	$ 1.50	$ 1.80	$ 2.10	$ 2.40	$ 2.70	$ 3.00

Please confirm that you have read and understood the instructions.

I have read and understood the instructions.

→

Figure 5.A.6: Instructions for the Second Task (Task 3)

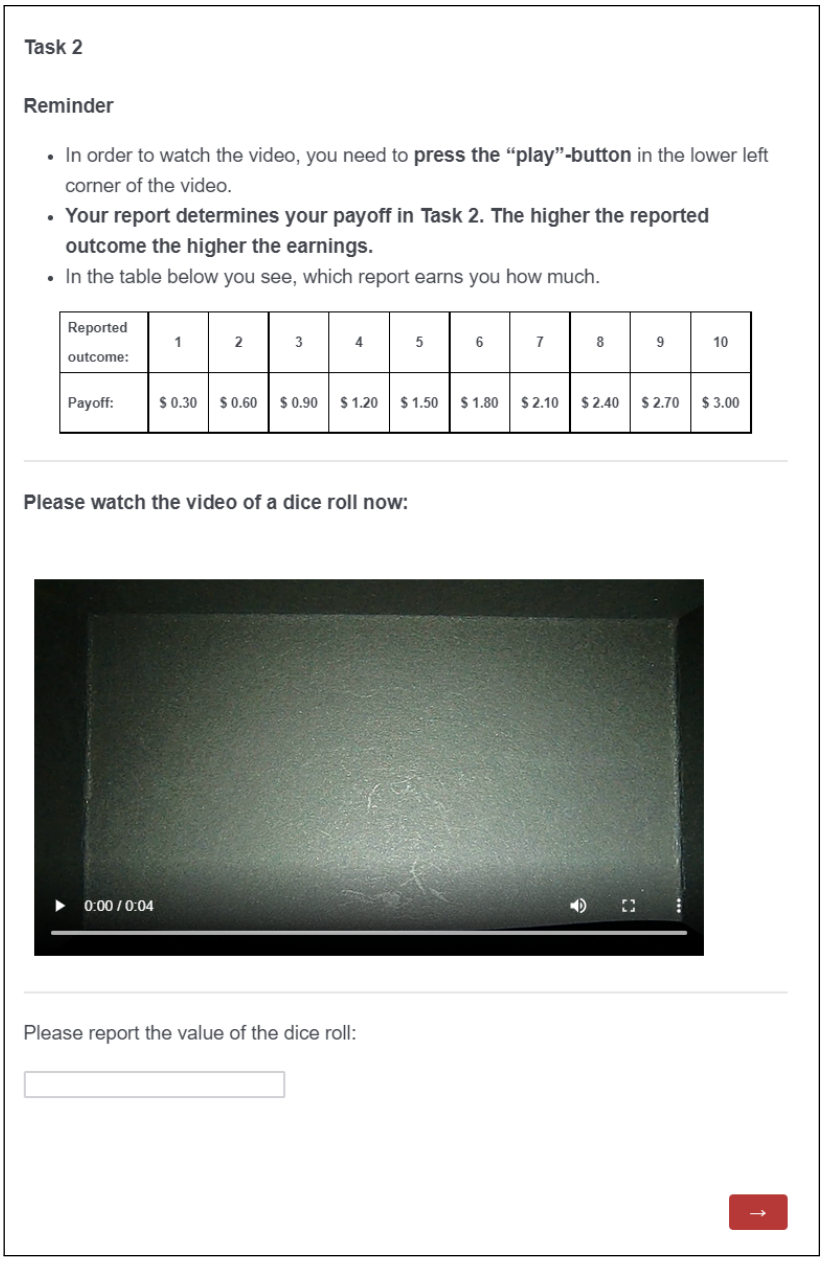


Figure 5.A.7: Decision Screen for the Second Task (Task 3) With Unviewed Video

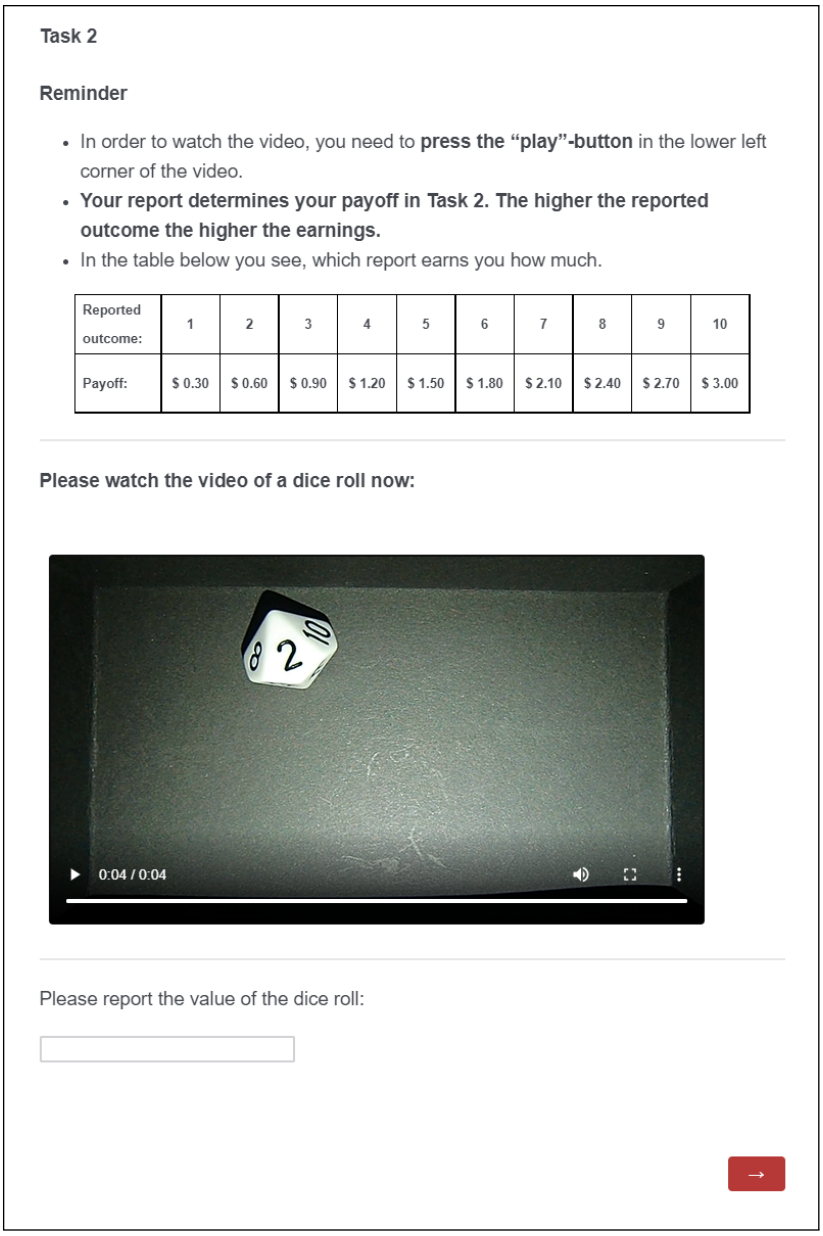


Figure 5.A.8: Decision Screen for the Second Task (Task 3) With Viewed Video

Instructions: Task 3

Background

- In a previous study, other participants also saw a video of a ten-sided dice roll.
- The possible outcomes of the dice roll were also: 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10.
- Furthermore, their reports determined their earnings from the task in the same way as your report in Task 2. (Below you see again the table that informs you about which report earned them how much.)

Reported outcome:	1	2	3	4	5	6	7	8	9	10
Payoff:	$ 0.30	$ 0.60	$ 0.90	$ 1.20	$ 1.50	$ 1.80	$ 2.10	$ 2.40	$ 2.70	$ 3.00

Your Task

- Your task is to **guess, which report most of the participants who saw the same outcome of the dice roll as you, made**.
- If your **guess is correct,** you earn **$ 0.20**.
- If your **guess is almost correct** you earn **$ 0.10**.
- Examples of almost correct guesses:
  - You guess is “1” or “3,” while the correct guess is “2;”
  - …
  - or your guess is “5” or “7,” while the correct guess is “6;”
  - …
  - or your guess is “8” or “10,” while the correct guess is “9.”
- If you don’t remember the outcome you saw, you can watch the video again (see below).

Figure 5.A.9: Instructions for the Third Task (Task 4) 1/2

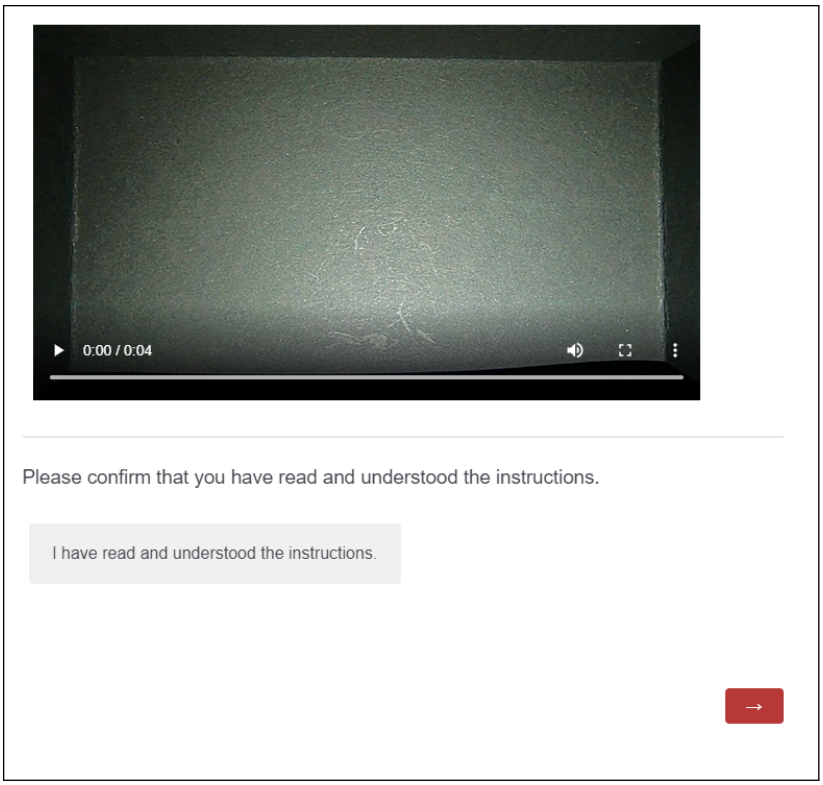


Figure 5.A.10: Instructions for the Third Task (Task 4) 2/2 With Unviewed Video

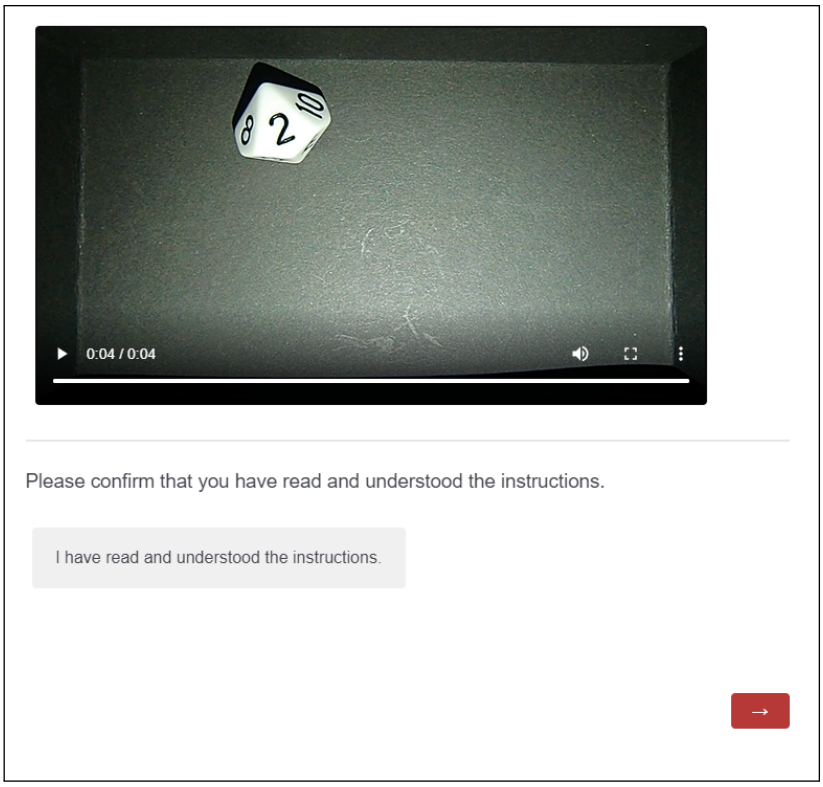


Figure 5.A.11: Instructions for the Third Task (Task 4) 2/2 With Viewed Video

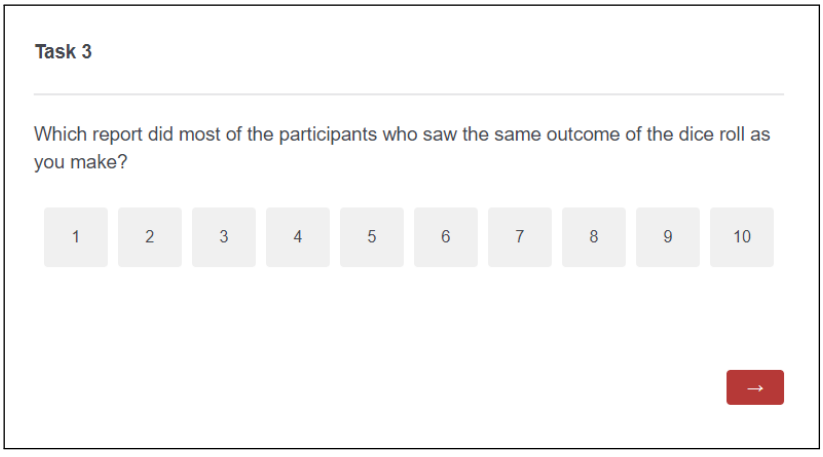


Figure 5.A.12: Decision Screen for the Third Task (Task 4)

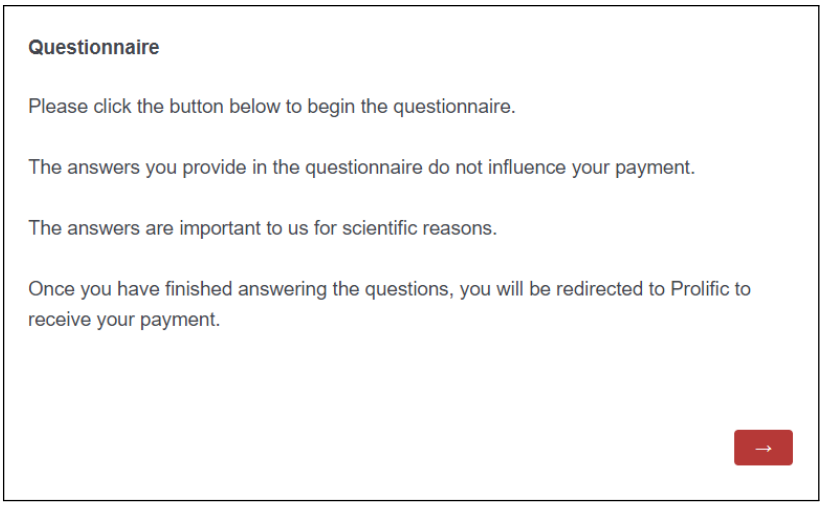
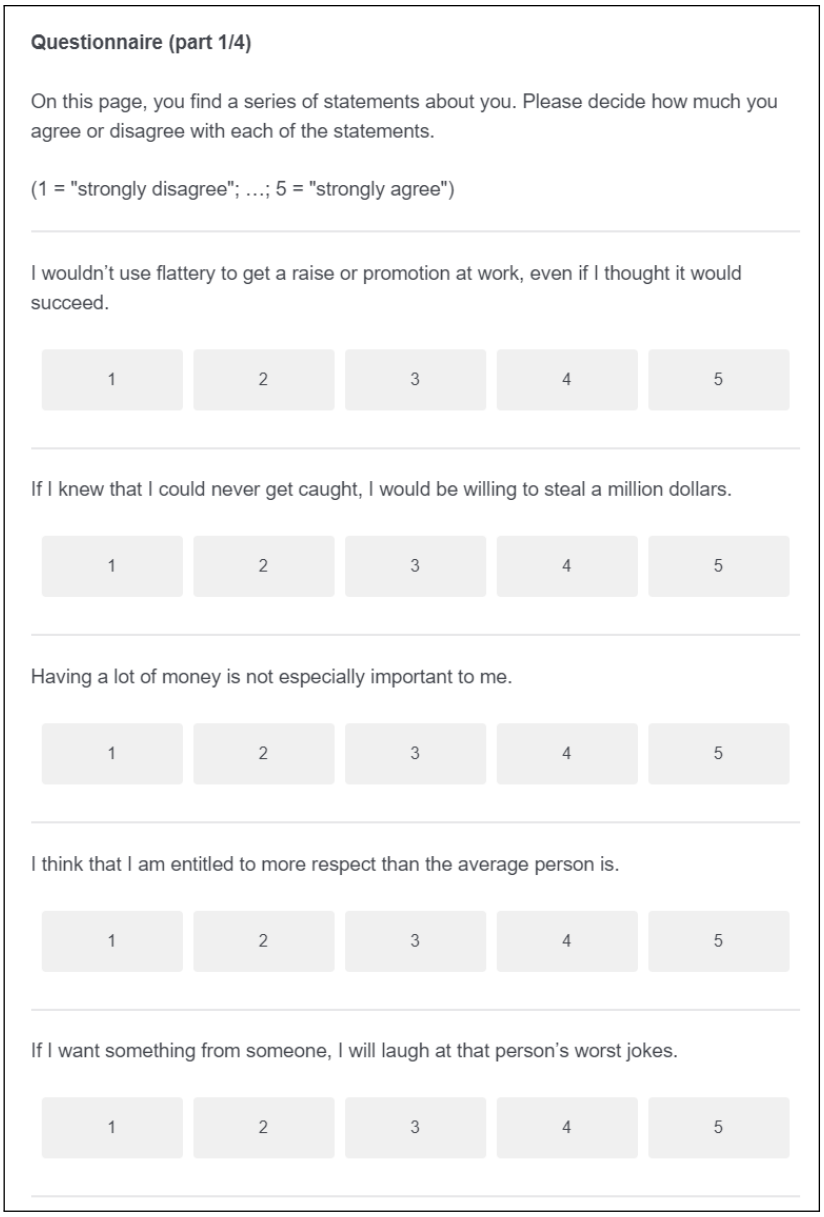


Figure 5.A.13: Introduction to the Questionnaire Section



Questionnaire (part 1/4)

On this page, you find a series of statements about you. Please decide how much you agree or disagree with each of the statements.

(1 = "strongly disagree"; …; 5 = "strongly agree")

I wouldn't use flattery to get a raise or promotion at work, even if I thought it would succeed.

1 2 3 4 5

If I knew that I could never get caught, I would be willing to steal a million dollars.

1 2 3 4 5

Having a lot of money is not especially important to me.

1 2 3 4 5

I think that I am entitled to more respect than the average person is.

1 2 3 4 5

If I want something from someone, I will laugh at that person's worst jokes.

1 2 3 4 5

Figure 5.A.14: Honesty-Humility Items of the HEXACO-60 Questionnaire 1/2

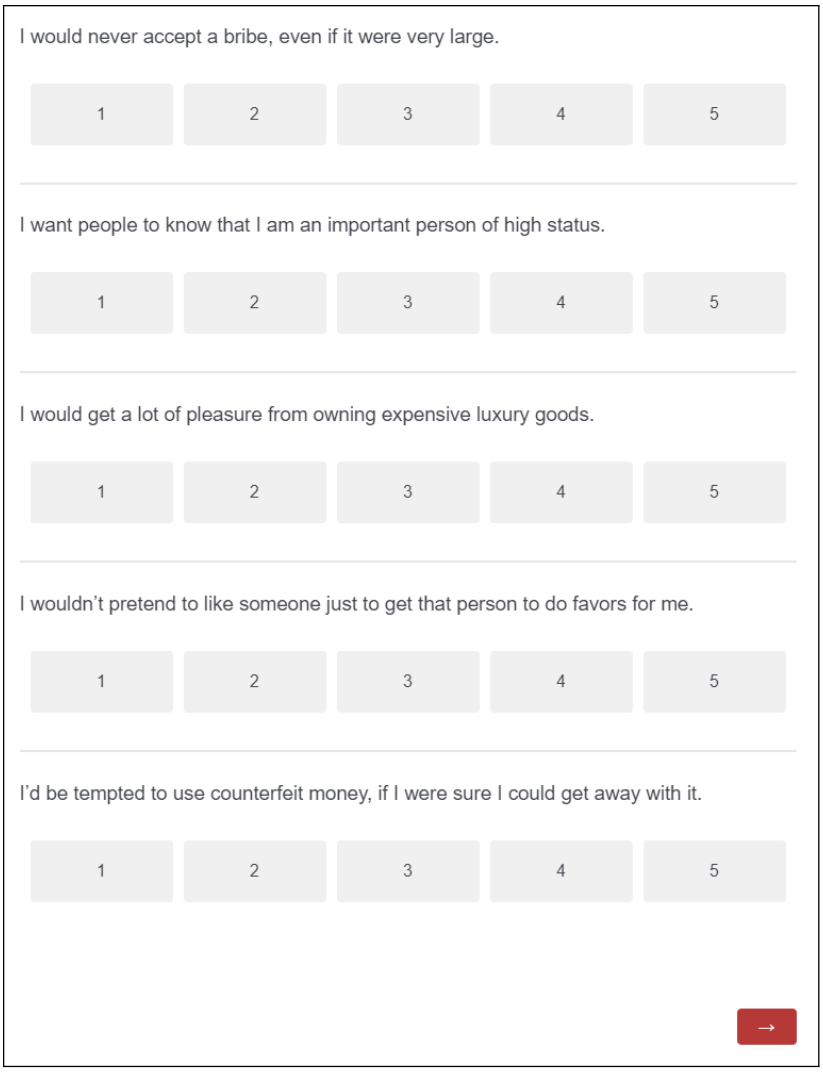


Figure 5.A.15: Honesty-Humility Items of the HEXACO-60 Questionnaire 2/2

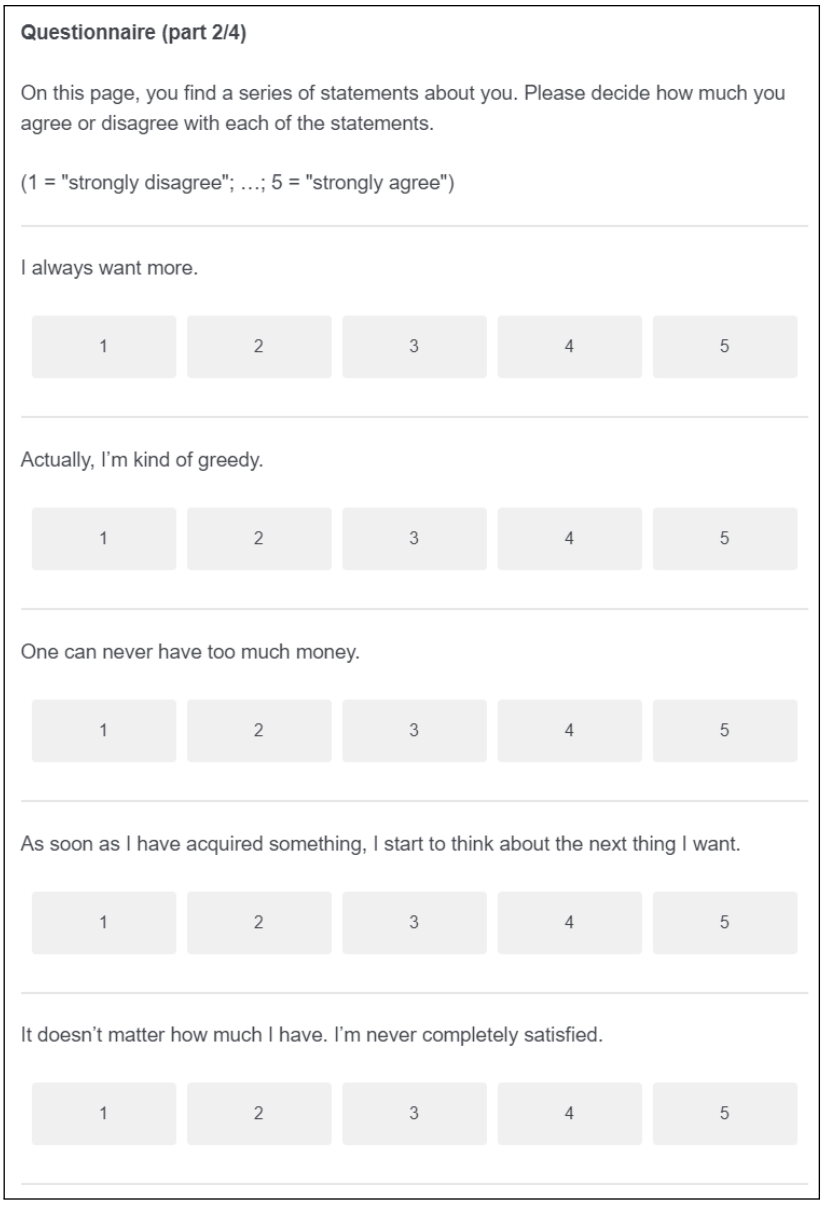


Figure 5.A.16: Dispositional Greed Questionnaire 1/2

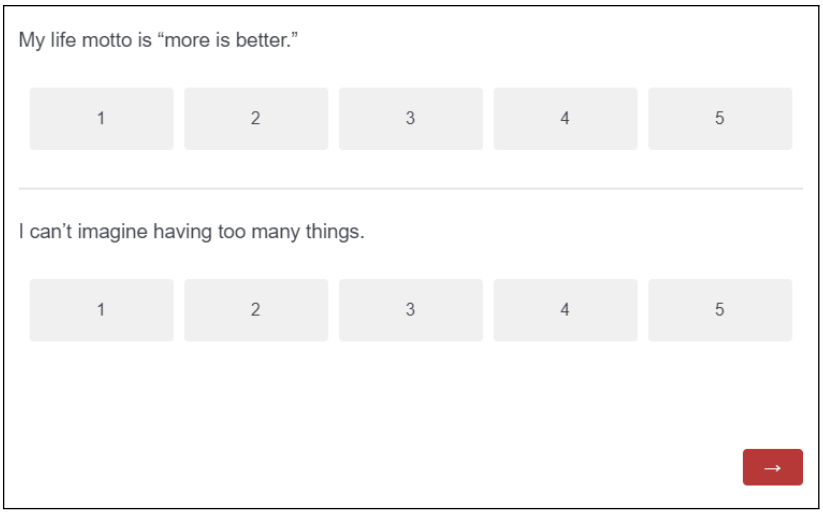


Figure 5.A.17: Dispositional Greed Questionnaire 2/2

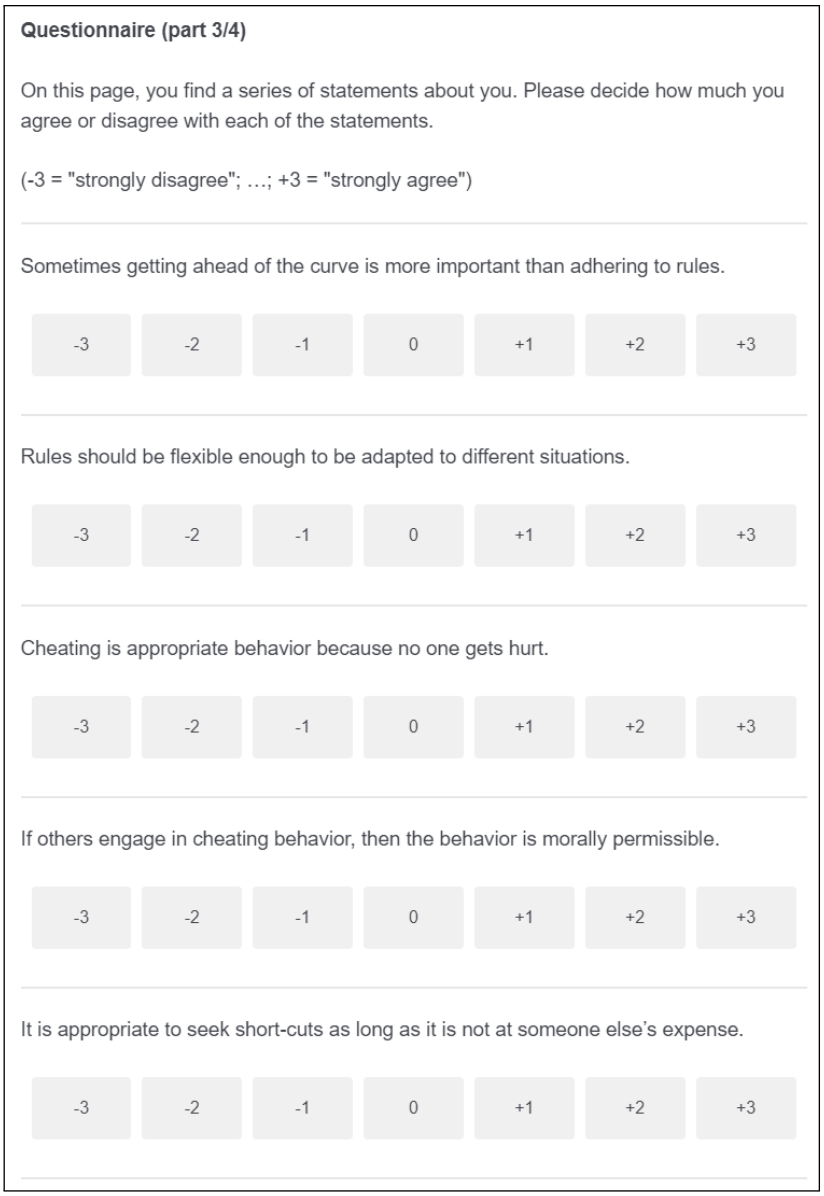


Figure 5.A.18: Moral Disengagement Questionnaire 1/2

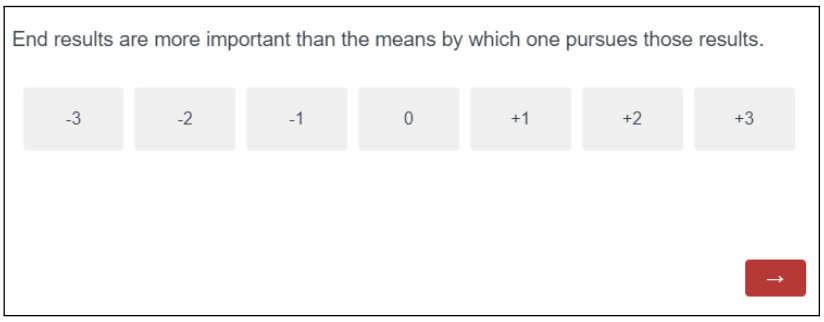


Figure 5.A.19: Moral Disengagement Questionnaire 2/2

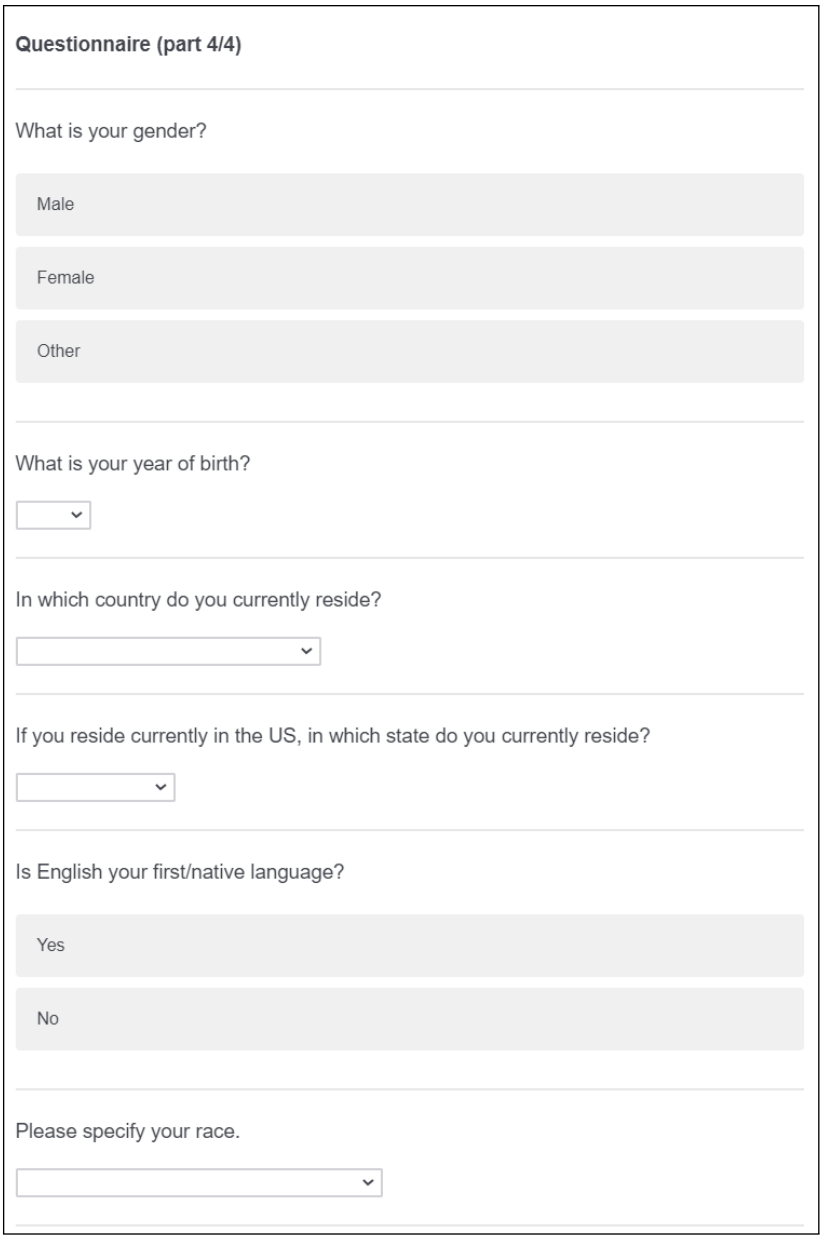


Figure 5.A.20: Sociodemographic Questions 1/3

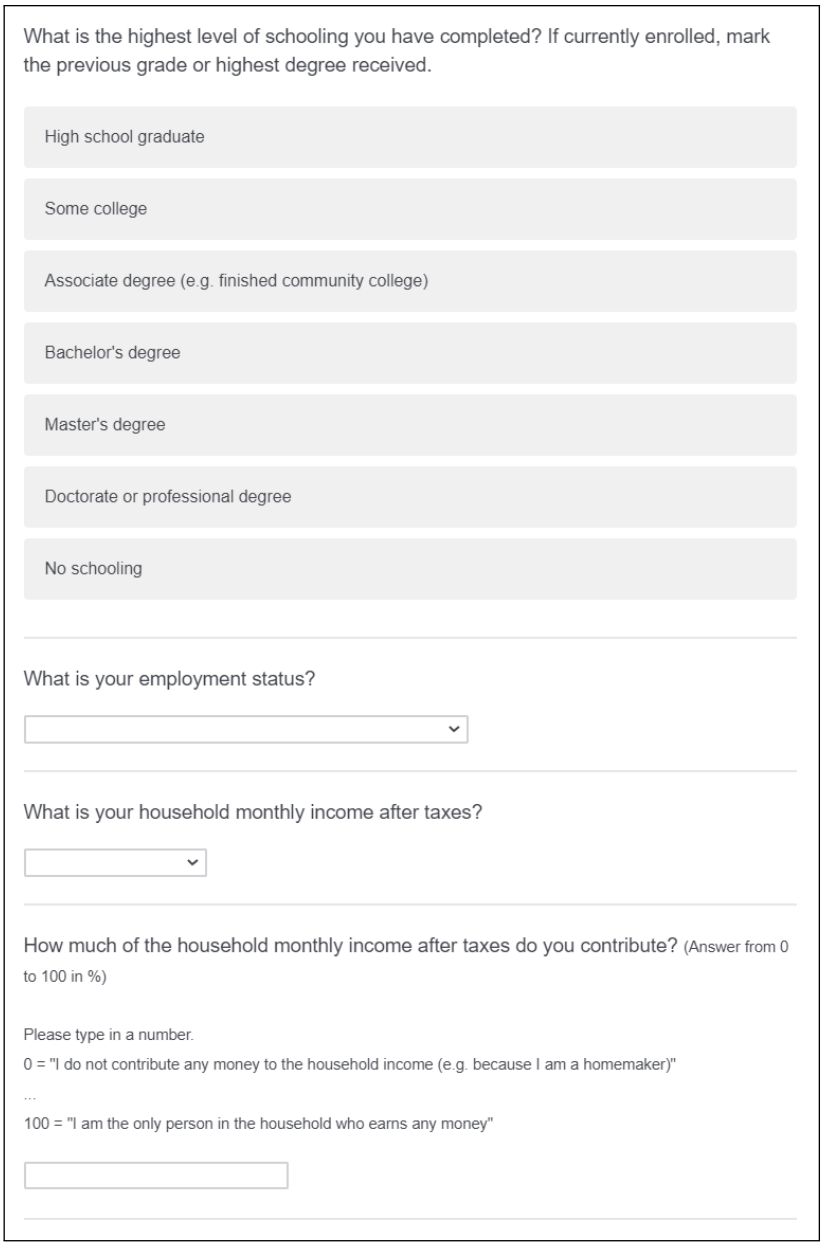


Figure 5.A.21: Sociodemographic Questions 2/3

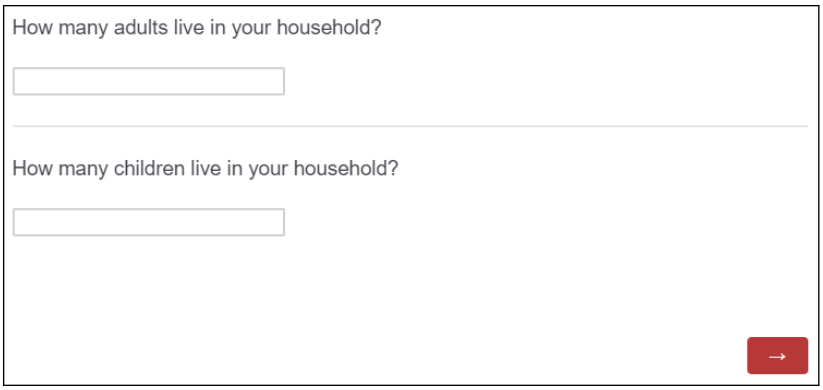


Figure 5.A.22: Sociodemographic Questions 3/3

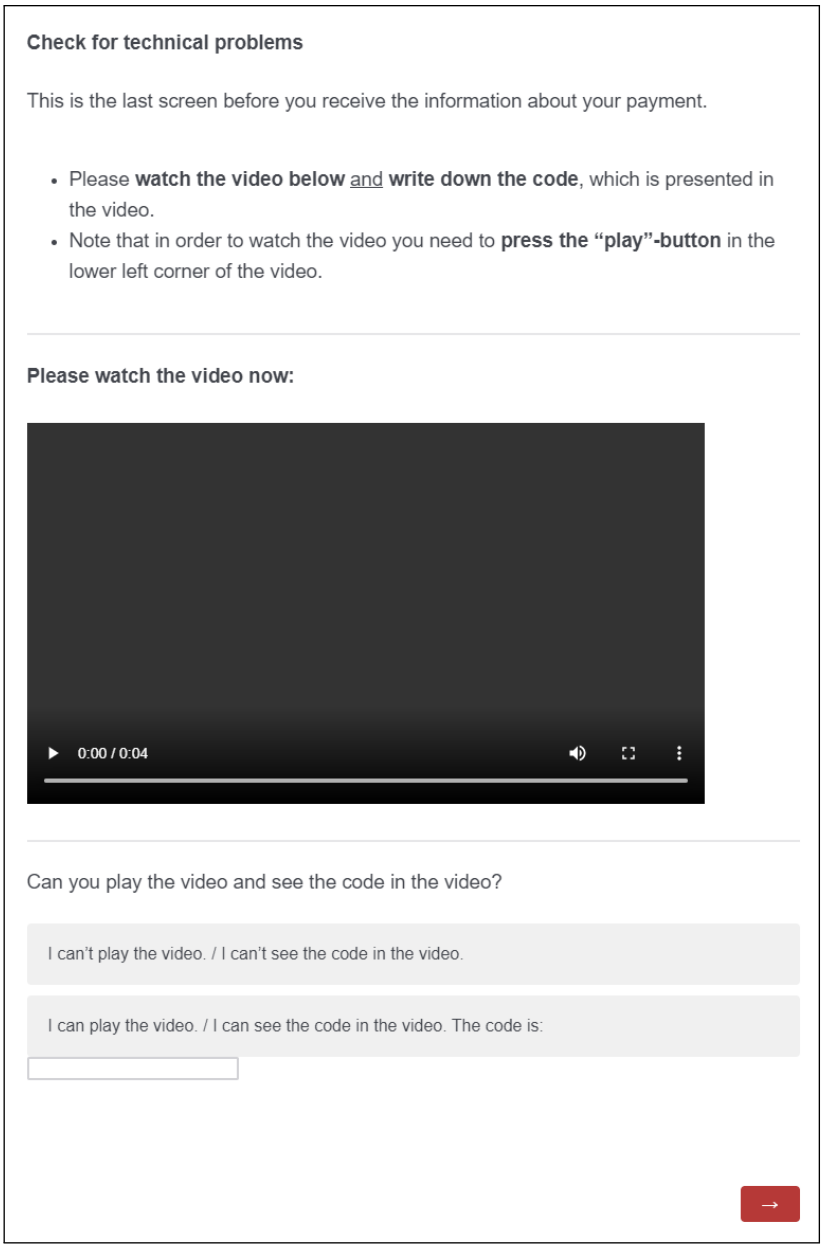


Figure 5.A.23: Technical Check With Unviewed Video

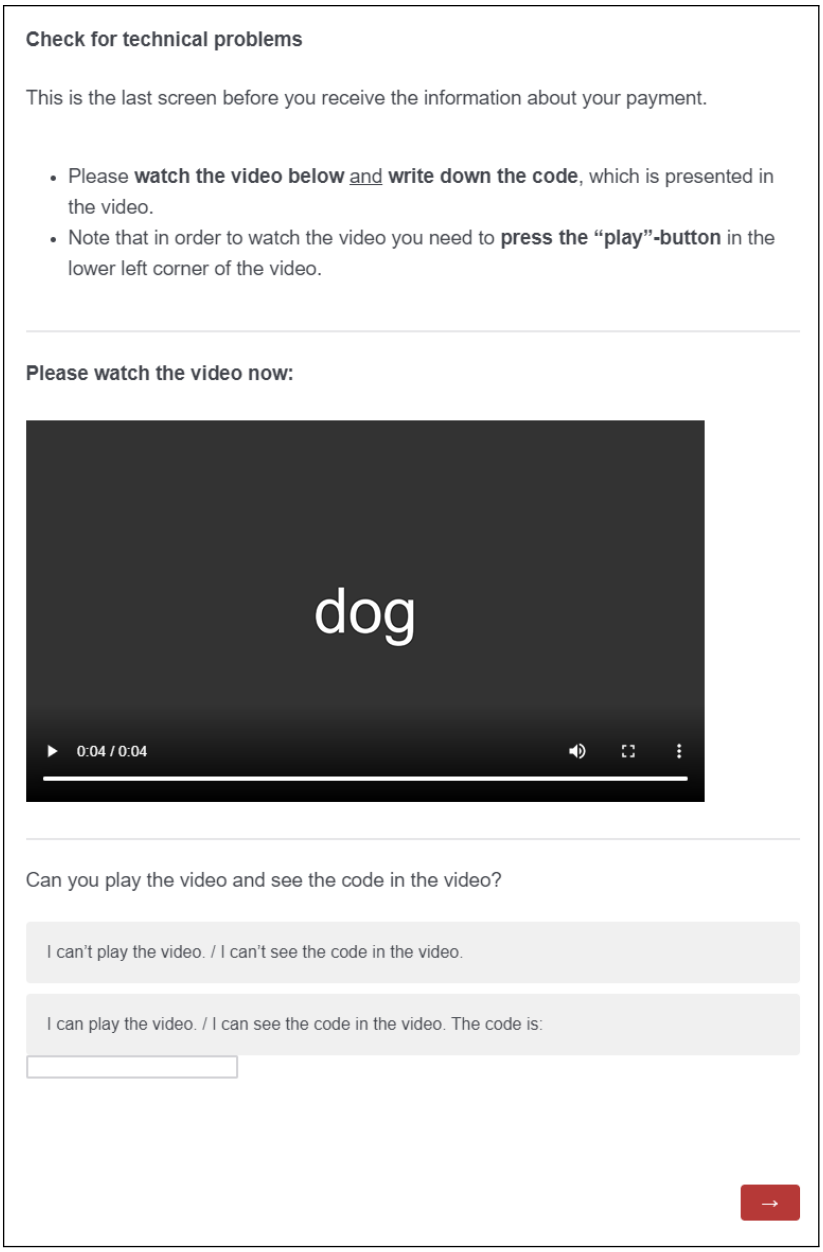
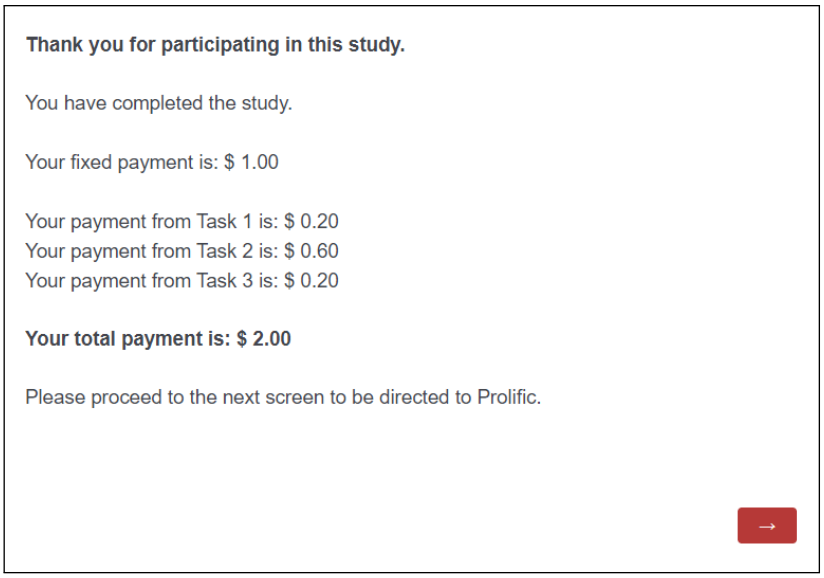


Figure 5.A.24: Technical Check With Viewed Video



Note: In NB-NS, the experiments consists of two (rather than three) tasks as shown here. Subjects in NB-NS do not participate in the belief elicitation (first task in B-NS) and do not receive a signal about others (jointly with the belief elicitation; first task in the B-S). The general introduction is modified accordingly.

Figure 5.A.25: Last Screen

Appendix 5.B Summary of Hypotheses and Results

Number	Hypothesis	Result	Support
5.1	Men lie more when an initial belief about others' unethical behavior is elicited than when it is not elicited.	Men lie more when asked to think about others' behavior in another ethically loaded situation.	YES
5.2	Women lie less when an initial belief about others' unethical behavior is elicited than when it is not elicited.	Women's lying behavior is not affected by thinking about others' behavior in another ethically loaded situation.	NO
5.3	Women lie more when a higher level of unethicality by others is signaled to them.	Women lie more when a higher level of unethicality by others is signaled to them.	YES
5.4	Men are not affected by the level of unethicality by others signaled to them.	Men's lying behavior is not affected by receiving a signal or the level of unethicality contained in it.	YES

Table 5.B.1: Summary of Hypotheses and Results

Appendix 5.C Probit Regressions With Interaction

	Model 1	Model 2	Model 3
BELIEF (1 if provided)	−0.078	−0.146	−0.206
	(0.236)	(0.242)	(0.250)
SIGNAL (1 if provided)	−0.345	−0.469	−0.356
	(0.279)	(0.290)	(0.300)
LEVEL (of Unethicality in SIGNAL)	0.101**	0.132***	0.114**
	(0.049)	(0.051)	(0.052)
Men×BELIEF (1 if provided and male)	0.604*	0.681*	0.642*
	(0.343)	(0.350)	(0.359)
Men×SIGNAL (1 if provided and male)	0.178	0.292	0.228
	(0.378)	(0.387)	(0.398)
Men×LEVEL (of Unethicality in SIGNAL)	−0.069	−0.106	−0.083
	(0.065)	(0.066)	(0.069)
Men (1 if male)	−0.145	−0.157	−0.235
	(0.233)	(0.237)	(0.244)
Constant	−0.946***	−1.117***	−1.410***
	(0.153)	(0.380)	(0.444)
Sociodemographics	NO	YES	YES
Additional Questionnaires	NO	NO	YES
Pseudo R^2	0.022	0.030	0.091
Number of observations	1,182	1,171	1,171

Note: The dependent variable for all three models is the participants' decision to lie (*untruthful report = 1/truthful report = 0*) in Task 3 (see Subsection 5.3.1). Female subjects in NOBELIEF-NOSIGNAL (NB-NS) form the reference group in all models. Standard errors are in parentheses. In Model 2, sociodemographic characteristics are added as additional control variables. These characteristics are age, employment status, college education, household income, own contribution to household income, English native speaker status, and race. Model 2 includes a slightly smaller number of observations than Model 1 due to the exclusion of two subjects reporting unreasonably low age and nine subjects not answering which race they belonged to. In Model 3, additional controls are added. These controls include honesty-humility (dimension of the HEXACO-60: Ashton & Lee, 2009), dispositional greed (Seuntjens et al., 2015), and moral disengagement (Shu et al., 2011). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.C.1: Results of Probit Regressions With Interaction

Chapter 6

Conclusion

This dissertation features four distinct papers addressing different but related issues. The results presented here are aimed at either helping to inform practitioners, allowing them to apply presented insights, or informing other researchers to allow them to build on the generated insights. Each paper provides unique insights into how to improve managerial effectiveness or increase business performance for both a company and the society it is part of.

In the first paper (by Böhm et al., 2021a, see Chapter 2), we use a threshold public good game (Palfrey & Rosenthal, 1984) with a labor market frame and two roles (employer and employee) to mimic the case of an organizational change. With it, we test whether a pro-change default and a pro-change recommendation are effective in fostering employee support for organizational change. We find evidence that a pro-change default is effective while a pro-change recommendation lacks a corresponding effect. We compare the default to a deliberate wage choice and find that in our experimental setup, this nudge has a cost allowance of approximately 70% of the wage increase. Hence, we deliver a lab-tested intervention aimed at increasing employees' supportive behaviors for organizational change. A default nudge is better suited than costly interventions, such as wage increases, due to increased cost pressure when facing the need for change. Overall, our results are in line with previous findings for the high effectiveness of default rules (Jachimowicz et al., 2019) and cost-efficiency of nudging in general (Benartzi et al., 2017). Additionally, our results strengthen previous findings by Thaler and Benartzi (2004) and Venema et al. (2018), reporting on the effectiveness of default rules in the organizational context. In summary, our results indicate that nudging and, to a greater extent, default rules can be tools with economically relevant and significant effects in change management scenarios. Thus, we contribute to the nudging and change management literature, as, to the best of our knowledge, previous research on organizational change has lacked a focus on nudging, and previous research on nudging has lacked a specific focus on organizational change. Future researchers could use our insights to further understand the dynamics within groups as potential moderators of the effectiveness of nudging (Hauser et al., 2018). Additionally, further (field) research is needed in order to make practical use of our findings.

In the second paper (by Böhm, 2021, see Chapter 3), I use a multiple price list (Holt & Laury, 2002) with a managerial frame and an additional adaptation to mimic uncertainty (Moore & Eckel, 2006; Ross et al., 2012) in order to measure managerial decision-making under risk and uncertainty.

I test whether a pro-neutrality default rule is effective in fostering neutral behavior under risk and under uncertainty. The paper presents supporting evidence for the effectiveness of the default rule in risky situations but not in uncertain situations. Overall, these findings resemble previous, mixed results relating to defaults. While there is predominantly evidence of their high effectiveness (see previous paragraph; Böhm et al., 2021a; Jachimowicz et al., 2019; Thaler & Benartzi, 2004; Venema et al., 2018), there have also been reports of no effect (Reiter et al., 2012) and even backfiring effects (Bolton et al., 2020; Krijnen et al., 2017; Sunstein, 2017). I argue that the underlying domain of the managerial situation might be the crucial reason for a pro-neutrality default working as hypothesized or failing to be impactful. My argumentation is built on the work by Dinner et al. (2011), who find three channels by which a default impacts behavior. These channels should be affected in different ways by the underlying domain (Hauser et al., 2018; van Kleef & van Trijp, 2018). However, while I find a significant effect under risk but not under uncertainty and this, in turn, may indicate support for the moderating effect, I actually do not find a significance of the moderating effect of the domain in my analysis. This may be, in part, due to the measure used rather than to an actual lack of this moderating effect. Additional research might benefit from using a more nuanced measure to avoid this problem. In addition, in future research, the role of the origin of the default could be investigated. Researchers should direct their attention to finding tools to guide managerial behavior in the domain of uncertainty. My results indicate that a pro-neutrality default nudge is useless in uncertain situations and may only prove impactful for decision-making under risk.

In the third paper (by Böhm et al., 2021b, see Chapter 4), we use a framed multiple price list (Holt & Laury, 2002) to measure risk aversion in a managerial situation. We place a pro-neutrality recommendation either before or after starting the decision-making process and find that both a pre-recommendation (placed prior to starting) and a post-recommendation (placed after starting the decision-making process) are effective in fostering neutral behavior. In opposition to our hypothesis, we find no supporting evidence for the post-recommendation's effectiveness suffering as a result of it materializing later in the decision-making process. Therefore, we conclude that such a pro-neutrality recommendation is impactful regardless of when it materializes. However, as stated by Böhm (2021), this result, or lack thereof, may also emerge partly due to the measure used. Overall, our findings are in line with previous research reporting effective interventions based on recommendations provided prior to starting the decision-making process (Barron & Nurminen, 2020; Keller et al., 2011; Newell & Siikamäki, 2014) as well as afterward (Bertrand & Morse, 2011; Cadario & Chandon, 2020; Thorndike et al., 2012). Taking into consideration arguments for System 2 nudges[77] being more transparent and, therefore, preferred by many (Jung & Mellers, 2016; Sunstein, 2016), a pro-neutrality recommendation (tested in the third paper) might be even better suited than a pro-neutrality default (tested in the second paper) in promoting risk-neutral managerial behavior. Future research should try to generate more insights into how and when

[77]System 1 and System 2 describe two systems of thinking (Kahneman, 2011; Tversky & Kahneman, 1983). System 1 thinking is fast, automatic, and unconscious, allowing for rapid decisions. In contrast, System 2 thinking is slow, effortful, and conscious and follows a more logical approach than System 1 thinking.

a recommendation suffers from "coming late" to the decision-making process. While we find no such evidence, there is a strong case for suspecting it. Additionally, field experiments that put our findings to work might prove fruitful.

In the fourth paper (by Böhm et al., 2022, see Chapter 5), we use two distinct honesty games. The first game is the dot task (Gino et al., 2010), and the second is a modified version of the die-rolling task (Fischbacher & Föllmi-Heusi, 2013; Kocher et al., 2018). By introducing an initial belief elicitation and providing a signal about others' dishonesty, we test for the effects of merely thinking and learning about the unethical behavior of others. We find that thinking about others' dishonesty increases males' (but not females') lying in a subsequent, different situation. This finding is in line with previous research reporting that men are better in competitive situations (Gneezy et al., 2003; Gneezy & Rustichini, 2004) and more willing to engage in competition (Datta Gupta et al., 2013; Niederle & Vesterlund, 2007; Vandegrift & Brown, 2005) than women. In contrast, we find evidence that females' (rather than males') lying increases when learning about about higher levels of others' unethicality. Our result is in line with previous research reporting that women are more responsive to social cues (Gilligan, 1982/2016; Kahn et al., 1971; Roberts, 1991), more willing to comply with apparent norms (Crawford et al., 1995; Eagly, 1978; Maccoby, 1974; Minton et al., 1971), and more likely to adapt their behaviors accordingly (Furtner et al., 2021; Molina et al., 2013; Venkatesh & Morris, 2000) than men. It is of the utmost importance for companies to understand how unethicality is spreading and how gender matters in this process. Allowing the spread of unethicality can lead to devastating outcomes (Abdullah et al., 2021; Cohn et al., 2014; O'Brien, 2003) and threaten the economic and social wellbeing of societies. Our paper sheds light on how such spreads take place, and gender does, indeed, matter enormously in this process. Future research could build on the insights we generated here and should aim to find avenues for interventions to slow down or stop the spread of unethicality and, potentially, even reverse it. Moreover, further research might address the exact triggers we assumed to be present in our experiment: a sense of competition for males' sudden increase in dishonesty and compliance with norms for females' adaptive increase in dishonesty.

In summary, this dissertation offers a number of interesting findings to improve managerial effectiveness and increase the performance of companies. The first paper starts by suggesting nudging to increase the likelihood of succeeding with a change project. Indeed, it offers supporting evidence for a default rule being a viable tool. The second paper takes over by testing whether a default rule can be effective in fostering neutral behavior in risky and uncertain situations. It offers support for the effectiveness of the default rule under risk only. This finding may improve managerial guidance in the future. The third paper follows up on this result by investigating the effect of a recommendation prior to and after starting the decision-making process in a risky endeavor. It offers supporting evidence for a recommendation being effective regardless of when it materializes. Again, this finding may aid research on managerial guidance. Finally, in the fourth paper, we provide new insights into how unethicality spreads and the gender-related contribution to this spread. We find that men adapt their unethical behavior based merely thinking about

others' dishonesty, while women adapt their unethical behavior when actually learning about more dishonesty. Our findings may prove valuable when additional research tries to find avenues for supporting companies in stopping or even reversing the spread of unethicality.

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