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Preface

This dissertation consists of four self-contained chapters in Behavioral and Experimental Economics. The chapters address distinct research questions about individual behavior in various domains, with a particular focus on prosocial behavior and the role of information in economic decision-making. Chapter 1 studies the demand for, and avoidance of, moral information and its impact on behavior in the context of meat consumption. Chapters 2 and 3 focus on charitable giving. In particular, Chapter 2 explores how asking individuals to fund charitable goods (instead of simply providing money) influences donations, while Chapter 3 complements this line of research by analyzing the effectiveness of subsidies in such a decision environment. Chapter 4 investigates whether beliefs about lobbying in the context of climate protection affect individuals' pro-environmental behavior.

Besides the common focus on individual behavior, a unifying element is that all chapters rely on experimental methods. Over the last decades, experiments have emerged as a valuable tool in economic research, offering major advantages such as clean and simple inference on causal effects. Chapter 1 employs a laboratory experiment (and complementary field data), which allows us to control information acquisition and its costs. Chapters 2 and 3 are based on online experiments where only specific characteristics of the decision environment are changed in order to identify their relevance. Chapter 4 makes use of an experiment in a survey panel to exogenously shift beliefs and study the consequences for individual behavior. In all of these cases, the experimental approach is crucial in establishing causality and addressing the considered research questions. The studied populations are diverse, including students (Chapter 1), crowd workers (chapters 2 and 3), and a large heterogeneous sample from the German population (Chapter 4). In the following, I provide a brief summary of each chapter.

Chapter 1 is joint work with Andreas Gerster. In this chapter, we investigate the presence and consequences of information avoidance in a moral context, i.e., a

situation where an individual's action might negatively affect others. Information about the consequences of consumption behavior can help consumers to reduce actions that are morally questionable or cause social costs. However, consumers might also avoid costless information in order to circumvent moral concerns from behaving selfishly. We explore this issue in the context of food choices, where the consumption of meat from intensive farming negatively affects animal welfare. We conduct a laboratory experiment and use complementary field data from university canteens to address three main research objectives. First, we elicit individuals' willingness-to-pay for information on the living conditions of animals in intensive farming and test whether information avoidance is present in this setting. Second, we estimate the effect of receiving such information on individuals' meat consumption in the laboratory and in the field. Third, we analyze how the effect of information differs between information seekers and avoiders, i.e., individuals with a non-negative and negative willingness-to-pay for information, respectively.

We find that about 30 percent of subjects avoid information on animals' living conditions in intensive farming. When receiving information, subjects significantly reduce their propensity to consume meat on average by about 12 percentage points in the laboratory and 6 to 9 percentage points in university canteens. The effect in the field is mainly driven by the first days after the experiment and is statistically indistinguishable from zero about one week after the experiment. In the laboratory, we also find suggestive evidence that individuals who select out of information are particularly responsive to it. This selection pattern impedes the effectiveness of information provision, even when information is provided for free.

Chapter 2 focuses on charitable giving and is joint work with Johannes Diederich and Timo Goeschl. Charities frequently deviate from the standard donation scheme in which potential donors are asked how much money they are willing to give. Instead, they ask donors to choose how many units of a charitable good (e.g., meals, bed nets, or trees) to fund at a given unit price. In an online donation experiment, we compare the performance of such a "unit donation" scheme with that of the standard "money donation" and investigate the factors that could explain differences. We find that despite the additional demands that it imposes on the charity, the unit donation does not outperform the money donation scheme in terms of overall donations. It significantly differs, however,

with respect to the propensity to give. The sign of the difference depends on the granularity of the scheme. When one unit of the charitable good is cheap, unit donation schemes increase the propensity to give and hence can serve as an effective tool for recruiting donors.

Chapter 3 is joint work with Johannes Diederich, Catherine C. Eckel, Timo Goeschl, and Philip J. Grossman. In this chapter, we complement the research on unit donation schemes presented in Chapter 2 by investigating the effectiveness of subsidies in such a decision context. An influential result in the literature on charitable giving is that matching subsidies – where a third party matches an individual's donation at a given rate – dominate rebate subsidies – where a third party refunds a fraction of the donation back to the donor – in raising funds. We investigate whether this result extends to unit donation schemes. In particular, we compare matches and rebates as well as simple discounts on the unit price in an online experiment in which donors' choices are over the number of units of a charitable good to fund (rather than the amount of money to give). In contrast to the standard result in the money donation literature, we find no evidence of dominance: The three subsidy types are equally effective overall. At a more disaggregated level, rebates lead to a higher likelihood of giving, while matching and discount subsidies lead to larger donations by donors. This suggests that charities using a unit donation scheme enjoy additional degrees of freedom in choosing a subsidy type. Rebates merit additional consideration if the primary goal is to attract donors.

Chapter 4 presents results from a survey experiment designed to identify the causal impact of beliefs about lobbying on individuals' pro-environmental behavior. In the context of environmental policy, lobbying is often expected to lower or prevent stricter regulations. A crucial question is whether such beliefs affect individuals' willingness to engage in pro-environmental behavior. I use a survey experiment with a heterogeneous sample from Germany that induces random variation in individuals' beliefs about the impact of lobbying. In the experiment, participants read three statements which either all provide reasons that lobbying increases or decreases climate protection. Afterward, participants have the opportunity to contribute to the reduction of carbon emissions by donating to a climate protection organization. The donation decision serves as a measure of pro-environmental behavior. A follow-up survey provides information on stated pro-environmental behaviors and the persistence of beliefs. While the exogenous

belief manipulation is successful, I find mixed evidence on whether expecting a more negative impact of lobbying on climate protection affects pro-environmental behavior. Being confronted with positive instead of negative statements significantly decreases stated pro-environmental behavior. Although the estimated treatment effect on the observed contribution to the reduction of carbon emissions points in the same direction, it is not statistically different from zero at any conventional level.

Chapter 1

Information Avoidance and Moral Behavior: Experimental Evidence from Food Choices*

with Andreas Gerster

1.1 Introduction

Information technologies, such as the Internet, give individuals access to comprehensive information about the consequences of their actions. Such information may help consumers to reduce behaviors that are morally questionable or cause social costs. Whether the availability of information induces behavioral change depends on consumers' willingness to acquire it, however. Evidence from laboratory experiments shows that individuals willfully ignore information about the

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consequences of their actions for others in order to behave more selfishly (e.g., Dana et al., 2007; Larson and Capra, 2009; Feiler, 2014). A crucial, yet little explored, question is whether individuals also avoid moral information that is relevant outside of a laboratory game. If they do, better access to moral information may only have a negligible effect on everyday behaviors.

In this paper, we investigate the presence and consequences of information avoidance in the context of food consumption. Expenditures for food account for a substantial share of GDP,[1] and farming practices have far reaching implications for land use, greenhouse gas emissions, and animal welfare. We focus on consumers' decision to eat meat from intensive farming. The production of such meat typically harms animals, for example, by substantially restricting their living space. Since many individuals consider inflicting harm on animals as morally questionable behavior (see Lin-Schilstra and Fischer, 2020, for a review), consuming meat from intensive farming involves a tradeoff between enjoying cheap meat and not harming or killing animals.[2] As a result, individuals might avoid information about animals' living conditions to circumvent moral concerns from meat consumption (Hestermann et al., 2020). Information avoidance could thereby contribute to elevated meat consumption levels that compromise animal welfare and exacerbate societal problems such as climate change (Gerber et al., 2013; Girod et al., 2014; Poore and Nemecek, 2018), fresh water scarcity (Mekonnen and Hoekstra, 2012; Jalava et al., 2014), and excessive land use (Machovina et al., 2015; Poore and Nemecek, 2018).

Our paper has three main objectives. First, we elicit individuals' willingness-to-pay for information on the living conditions of animals in intensive farming and test whether information avoidance is present in this setting. Second, we estimate the effect of receiving such information on individuals' meat consumption. Third, we analyze how the effect of information differs between information seekers and avoiders, i.e., individuals with a non-negative and negative willingness-to-pay for information, respectively. Based on these heterogeneous treatment effect estimates, we discuss how the effect of providing moral information depends on

[1] For example, food expenditures in the United States amounted to 1.77 trillion dollars in 2019 (https://www.ers.usda.gov/data-products/chart-gallery/gallery/chart-detail/?chartId=58364, accessed August 21, 2020), which corresponds to about 8 percent of U.S. GDP.

[2] It has also been documented that individuals are willing to make sacrifices when facing a tradeoff between their own payoff and animals' well-being (Falk and Szech, 2013; Albrecht et al., 2017; Falk et al., 2020).

the implied costs of information acquisition.

Our analysis builds on a laboratory experiment and complementary data on food consumption from university canteens. In the laboratory experiment, we give subjects the option to watch a 360° video about the living conditions of pigs in intensive farming via a virtual reality (VR) headset. The outside option is to watch a 360° video of the same duration on the German central bank, which all subjects have already watched at an earlier stage of the experiment. We elicit the willingness-to-pay (WTP) for information about intensive farming through an incentivized multiple price list approach and randomly implement one choice from that list. To measure meat consumption in the laboratory, we let subjects choose between a voucher for a meal with or without meat at the university canteen. Based on consumption data from the university canteens before and after the experiment, we additionally measure subjects' meat consumption in the field.[3] In our main analysis, we focus on the meat of any animal, including fish. To identify the effect of information on meat consumption, we exploit the fact that the likelihood to receive information depends merely on participants' WTP, which we elicit. Hence, we can use standard program evaluation methods, such as inverse probability weighting, to estimate average treatment effects (Imbens and Wooldridge, 2009).

We find that about 30 percent of subjects avoid information on intensive farming when it is costless. Furthermore, our results show that receiving such information decreases the likelihood to eat meat. In the laboratory, information reduces the likelihood to choose a voucher for a meal with meat by about 12 percentage points. In the field, the reduction of meat consumption amounts to 6 to 9 percentage points. The information effect is particularly pronounced immediately after the experiment and becomes indistinguishable from zero after about one week.

Furthermore, the laboratory data provides suggestive evidence that information avoiders, i.e., subjects with a negative WTP for information about intensive farming, are more responsive to information than subjects with a WTP of at least zero. In other words, particularly responsive individuals select out of information. By contrast, our field data does not show a clear pattern of selection based on treatment effects.[4] We also explore how the costs of acquiring moral

[3]The meat products offered at the university canteens are generally not declared to come from any special husbandry conditions that improve animal welfare beyond legal regulations.

[4]The outbreak of the coronavirus prevented us from reaching the pre-registered sample size

information influence the effectiveness of interventions that aim at disseminating such information. Our results suggest that providing free access to information without compensating individuals for their opportunity cost of time, for example, is unlikely to generate large effects, as many responsive individuals do not acquire information in that case.

Information avoidance has received considerable attention in the recent literature (see Hertwig and Engel, 2016; Golman et al., 2017; Handel and Schwartzstein, 2018, for reviews). The focus of our paper is on information avoidance in a moral context, i.e., a situation in which an individual's action might have consequences for others. Theoretically, a variety of mechanisms are able to explain why individuals avoid information in such a context. For example, individuals with self-image concerns may decide to avoid information about adverse consequences of their actions in order to sustain a positive self-image despite behaving selfishly (Grossman and van der Weele, 2017). Another explanation is that information avoidance facilitates motivated reasoning without costly self-deception, which may allow individuals to circumvent moral concerns from animal suffering caused by meat consumption (Hestermann et al., 2020). More generally, information avoidance could also occur because the information might trigger negative feelings (Golman et al., 2017), increase the attention paid to an unpleasant belief (Golman et al., 2021), or challenge the individual's current understanding of the world (Chater and Loewenstein, 2016).[5] Each of these mechanisms might contribute to the information avoidance observed in our experiment.

Empirically, a series of laboratory and online experiments have shown that a substantial percentage of subjects prefer to stay uninformed about the consequences of their action for another player (Dana et al., 2007; Larson and Capra, 2009; Matthey and Regner, 2011; Cain and Dana, 2012; Feiler, 2014; Grossman, 2014; Grossman and van der Weele, 2017; Exley and Kessler, 2021). The standard setting used is a binary choice dictator game in which a subject is initially unsure how a certain action affects the payoff of another player but can reveal all payoffs at no cost. Information avoidance has also been documented for laboratory games in which a subject's action might affect the donation to a charity (Kajackaite, 2015; Felgendreher, 2018; Lind et al., 2019; Momsen and Ohndorf,

of 500 subjects, which reduces statistical power to detect heterogeneity in the laboratory and the field.

[5]For a comprehensive overview of models to rationalize information avoidance, see Golman et al. (2017).

2020; Serra-Garcia and Szech, 2020). We contribute to this literature by testing whether individuals avoid information on the living conditions of animals in intensive farming. In contrast to most previous experiments in a moral context, this information is not only relevant to subjects' behavior within the laboratory but concerns consumption decisions in their everyday life. In that sense, our paper relates to Serra-Garcia and Szech (2020), who show that preferences for moral information in a laboratory game are predictive for the acquisition of information on the living conditions of cows in the dairy industry.[6]

While the presence of information avoidance in moral settings has been studied widely, its consequences on individual decision-making are only partly understood. Some studies find that the opportunity to avoid information leads to more selfish choices on average (Dana et al., 2007; Larson and Capra, 2009; Feiler, 2014; Exley and Kessler, 2021), but others do not detect a significant impact (Felgendreher, 2018; Lind et al., 2019; Momsen and Ohndorf, 2020). We contribute to that literature by exploring the consequences of information avoidance for an important everyday activity, food consumption. The use of complementary data from university canteens enables us to go beyond previous experimental studies that have analyzed subjects' behavior in the laboratory. In particular, we test whether behavioral changes in response to moral information extend from the laboratory to the field.

We implement an experimental design that allows us to estimate both the average response to information and the heterogeneity of information effects by subjects' information demand. Whether and how the selection into treatment interacts with the magnitude of the treatment effect is important for assessing policy implications of interventions (see, e.g., Heckman and Vytlacil, 2001, for a review and Maestas et al., 2013; Cornelissen et al., 2018; Carneiro et al., 2011; Kamhöfer et al., 2018, for applications). More specifically, if information avoiders do not respond to information, their avoidance decision does not reduce the effectiveness of providing information. The converse holds true, however, if avoiders react similarly or even more strongly than information seekers, i.e., individuals who acquire costless information.

How the selection into information relates to the heterogeneity in the effects

[6] In other contexts, d'Adda et al. (2018) show that a majority of subjects in an online survey prefer not to receive information about the environmental impacts of air conditioning usage and Freddi (forthcoming) finds that residents of regions with high refugee inflow access fewer newspaper articles about refugees.

of information is unclear, both empirically and theoretically. So far, empirical studies have found that selfish individuals tend to select out of information (Feiler, 2014; Kajackaite, 2015; Grossman and van der Weele, 2017; Serra-Garcia and Szech, 2020), but heterogeneity in the impact of information has remained unexplored. Theoretically, most information avoidance models that focus on a moral context do not unambiguously predict whether the response to information is stronger for information seekers or avoiders (see Appendix A.2 for a detailed discussion).[7] Our experimental design allows us to test whether information avoiders react to information on the living conditions of pigs in intensive farming and whether their reaction differs from that of information seekers. In addition, we use our elicitation of subjects' information demand and our estimates on treatment effect heterogeneity to explore how changes in the cost of information determine the impact of information provision. This analysis extends earlier studies that have highlighted the importance of information costs for ignorance (Cain and Dana, 2012; Grossman and van der Weele, 2017; Momsen and Ohndorf, 2020; Serra-Garcia and Szech, 2020) and selfish behavior (Cain and Dana, 2012).

We also contribute to a growing literature on meat consumption. Several studies investigate how people might try to mitigate moral concerns from eating meat (Loughnan et al., 2010; Kunst and Hohle, 2016; Bastian and Loughnan, 2017; Hestermann et al., 2020). In line with our results, survey studies document that people confess to avoiding information on animal welfare (Onwezen and van der Weele, 2016; Bell et al., 2017). The survey of Bell et al. (2017) also includes an information choice task which results in a similar share of information avoiders as in our experiment (31 to 33 percent). Furthermore, Tonsor and Olynk (2011) show that increased media coverage of animal welfare led to lower levels of meat consumption in the U.S. We extend this literature by using an experimental study to elicit the demand for information on animal welfare and to test whether

[7] For example, in the model by Hestermann et al. (2020), information avoiders might reduce their meat consumption in response to news about bad living conditions more or less than information seekers, depending on the cost of self-deception. If information avoiders face a low cost of self-deception, they fully suppress any arriving information. Hence, receiving information neither affects their beliefs about the state of the world nor their meat consumption. If instead, self-deception is rather costly, avoiders only partially suppress information and receiving information affects their beliefs as well as meat consumption. Depending on the shape of the utility function this can lead to a larger adjustment in the level of meat consumption than for information seekers (see Section A.2.1 for a concrete example).

information affects meat-eating behaviors. More broadly, our paper also relates to research on the effect of providing individuals with information about the consequences of their consumption behavior. Examples include labels informing about the environmental impact of products (see Yokessa and Marette, 2019, for a review) or the provision of information on carbon emissions from energy consumption (Newell and Siikamäki, 2014; Andor et al., 2020).

The remainder of the paper is structured as follows. In the next section, we describe the experiment. In Section 1.3, we explain the empirical strategy to identify the average effect of information on meat consumption. We present our results in Section 1.4 and discuss the relevance of information costs for the effectiveness of moral information provision in Section 1.5. Section 1.6 concludes.

1.2 Experiment

To investigate our research questions, we combine a laboratory experiment with field data from university canteens. The experiment provided subjects with the opportunity to receive information about the living conditions of pigs in intensive farming. It took place at the University of Bonn (BonnEconLab) and the University of Mannheim (Mannheim Laboratory for Experimental Economics). In the following, we introduce the information on intensive farming that subjects in our experiment chose to receive or to avoid (Section 1.2.1). We then explain our experimental design (Section 1.2.2) and procedure (Section 1.2.3). Afterwards, we provide details about the data (Section 1.2.4). We pre-registered our experiment at the AEA RCT Registry (Epperson and Gerster, 2020). A detailed mapping between our pre-analysis plan and the analyses in this paper can be found in Appendix A.1.

1.2.1 Information Signal About Intensive Farming

Following Arrow (1996), we refer to information as any signal that is correlated with the true state of the world and thus allows individuals to update their beliefs. In our setting, uncertainty about the state of the world – the living conditions of pigs in intensive farming – is multifaceted. For example, acquiring information about the living conditions requires not only to know about animal regulations in intensive farming but also to understand the implications of these

regulations for animal welfare.[8]

As the information signal in our experiment, we used a 360° video about the living conditions of pigs in intensive farming. The video allowed us to provide subjects with a comprehensive depiction of intensive farming practices. It is based on recordings from Germany, Great Britain, Italy, Mexico, and Spain. The original video is 7:27 minutes long and was published by the animal rights organization Animal Equality.[9] To keep the video within a 5-minute time frame, we presented a selection of scenes, which cover the life of a pig in intensive farming from birth until slaughter. In Appendix A.7, we provide a detailed description of the scenes (Table A.12) and show that they accord with German animal welfare regulations (Table A.13).

Subjects watched the video by using a virtual reality (VR) headset, which allowed them to adjust their line of vision within the video by turning their heads. A key advantage of this technology is that it creates a realistic three-dimensional picture of the scenes and thereby improves subjects' perception of spatial dimensions (Paes et al., 2017; Horvat et al., 2019). In our context, spatial perceptions are important for judging the implications of minimum floor surface requirements for animal welfare, for example. VR headsets are considered a powerful tool to convey information and have been widely used for educational training or data visualization (see Slater and Sanchez-Vives, 2016, for a review). They have also been applied to familiarize individuals with new perspectives (Banakou et al., 2013; Peck et al., 2013; Banakou et al., 2016; Seinfeld et al., 2018).[10] In the video shown to participants, the change of perspectives is implemented by two means. First, the recorded scenes create the impression of standing within a pig farm and seeing the scenes with one's own eyes. Second, the video is accompanied by a narrator who describes the conditions shown in the respective scenes from the "perspective of a pig" (see Table A.12 in Appendix A.7 for the translated transcript).

The information signal in our experiment closely resembles information sig-

[8]By contrast, uncertainty in previous laboratory experiments on information avoidance in a moral context has usually been limited to the financial payoffs for another player. This uncertainty can be resolved by simply revealing the payoffs.

[9]The original video was awarded with the German Web Video award for the best 360° video in 2016 and was also presented at an exhibition at the European Parliament. It is available online: https://www.youtube.com/watch?v=_pC0_mqmp6w (accessed March 19, 2020).

[10]Many of these applications create the illusion of inhabiting the body of someone else. This does not apply to the video in our experiment.

nals about intensive farming that are provided by animal protection organizations. Such signals often present information in a frame that is likely to trigger emphatic feelings towards animals, e.g., by describing scenes from the perspective of a pig. The fact that the sender of information provides a particular frame to an information signal is common in real-world settings. Importantly, in our case, the information signal is based on actual recordings from widespread intensive farming practices. Hence, in line with the broad definition by Arrow (1996), we refer to the video about intensive farming as providing *information*. This does not imply that different information signals about intensive farming will lead to the same response or avoidance behavior as the information signal in our experiment.

1.2.2 Design

Our experimental design has three core elements. First, we elicited subjects' willingness-to-pay (WTP) for information on the living conditions of pigs in intensive farming. Second, we introduced random variation in whether subjects receive the information or not. Third, we elicited two outcome variables: subjects' meat consumption in the laboratory and in the university canteens.

To quantify subjects' WTP for information on intensive farming, we followed a multiple price list approach. The key feature of this approach is that each subject chose whether to receive the information at several different prices. One of these prices was randomly drawn and the corresponding choice was implemented. This approach allowed us to elicit the demand curve for information at the individual level. Since each choice was implemented with a positive probability, answering truthfully was incentive-compatible.

To detect active information avoidance, our experimental design ensures that acquiring information did not involve effort, time, or unintended financial costs. In particular, subjects chose between the information about the living conditions of pigs in intensive farming and an (uninformative) outside option. The outside option was to watch another 360° video that subjects had already watched at an earlier stage of the experiment. This video gives a tour of the German central bank building.[11] We used this outside option for the following reasons. First, it

[11]The original video was published by the German central bank and is available online: https://www.youtube.com/watch?v=5S9nArmo_x4 (accessed March 19, 2020). We used a selection of scenes from the video that take exactly 5 minutes to watch.

eliminates the impact of opportunity costs of time that could otherwise prevent subjects from watching the video on intensive farming. Both videos are 5 minutes long, and hence subjects cannot save time by selecting the outside option. Second, by using a video that subjects have watched only minutes earlier, we ensure that the outside option does not convey novel information. In fact, when we test this conjecture based on our data, we find that most subjects considered watching the video again neither as informative nor as entertaining.[12] Third, the outside option also precludes that a desire to experiment with the virtual reality headset biases our WTP measure upwards: Subjects used the VR headset for the same time irrespective of their choice.

We let subjects choose between the two video options at 11 different relative prices of information about intensive farming, $p \in \{-8, -5, -3, -1, -0.5, 0, 0.5, 1, 3, 5, 8\}$ in euro. These prices were implemented by offering subjects varying payments from 0 to 8 euro for watching one of the two videos (see Appendix A.6 for the detailed instructions and a screenshot of the decision screen). The videos were labeled as "Option A" and "Option B" (randomized assignment), with Option B always being presented to the right of Option A. To limit the cognitive burden for subjects, prices for Option A decreased monotonically from the top to the bottom of the multiple price list (Andersen et al., 2006). We tested our price range in two pilot sessions with 22 participants and found that their WTP was always within that range.

Prior to the WTP elicitation task, subjects obtained detailed instructions and read a short description of both videos – the video about intensive farming and the video about the German central bank, which subjects had already watched before. To check whether they understood the WTP elicitation task, they answered a comprehension question and were only forwarded to the decision screen once they had answered the comprehension question correctly. We did not enforce consistency across decisions in the multiple price list. Similar to the procedure in Allcott and Taubinsky (2015), subjects with inconsistent choices

[12] After having watched the video about the German central bank for the first time, we asked subjects how informative and how entertaining it would be for them to watch the same video again. Answers were given on a five-point likert scale ranging from "1 – Very informative" to "5 – Not at all informative" and from "1 – Very entertaining" to "5 – Not at all entertaining." The percentage of subjects who selected at least 3 on the likert scale amounts to 90 and 85 percent, respectively. If for some reason watching the video about the German national bank again even creates disutility (e.g., through boredom), our WTP measure explained below provides an upper bound for the WTP for information.

received a hint and could revise their choices once.[13] When subjects preferred one option over the other at all prices, we asked them to state their hypothetical WTP using an open-ended elicitation format on a subsequent screen.

For each subject, we randomly selected one of the 11 prices from the multiple price list and implemented the subject's choice at this price. The random selection of one price introduced exogenous variation in whether subjects received the information. For example, a subject with a WTP of 6 euro watched the video about intensive farming if and only if the randomly selected price did not exceed 6 euro. We exploit this variation for identifying the average treatment effect of obtaining information (see Section 1.3). The probability of drawing the largest price (8 euro) and the probability of drawing the smallest price (-8 euro) was 27.5 percent. Each of the other prices was drawn with a probability of 5 percent. This design achieves a more balanced treatment assignment and hence improves the statistical power to identify treatment effects compared to drawing each price with the same probability.[14]

As outcome variables, we obtained revealed-preference measures of meat consumption from the laboratory and the field. In the laboratory, subjects had a 50 percent chance of winning a voucher for the university canteens. At the end of the experiment, each subject decided whether the voucher should be issued for a meal with or without meat. Both categories were offered daily at the university canteens and we used the standard terminology from the canteens when describing both options.[15] We also provided subjects with a description of all

[13]For example, if a subject selected Option A when the payment for Option A was 0 euro and the payment for Option B was 5 euro but switched to Option B when the payment for Option B decreased to 3 euro, the subject saw the following hint after submitting his choices: "Please consider the following hint. At decision E2 you have chosen Option A and at decision E3 you have chosen Option B, although the payment for Option B is higher at decision E2 than at decision E3. Please reexamine your choices. If you want to, you can revise your choices." If there were several inconsistent switching points, each was addressed in the hint. 2 out of 7 subjects who submitted inconsistent choices in the multiple price list, made consistent choices after receiving a hint.

[14]For example, someone whose WTP for information was -6 euro only received the information if a price of -8 euro was implemented. Given our selection probabilities, this happened with a probability of 27.5 percent. If instead, we had used a uniform distribution, the probability would have been about 9.1 percent. We announced the implementation probabilities to the subjects.

[15]In Mannheim, the vegetarian meal is called "Menü Vegetarisch" and the non-vegetarian meal is called "Menü 1". Both have a value of 3 euro and often contain a side dish, a soup, and a salad in addition to the main dish. The voucher was valid only in the main canteen. In Bonn, the meal categories are called "Hauptkomponente Vegetarisch/Vegan" and "Hauptkomponente Fleisch/Fisch", respectively. The prices of the two categories vary, usually between 1.35 and

meals offered under the two categories in a pre-defined sample week. To ensure that peer pressure does not affect voucher decisions, we informed subjects that the vouchers would be handed out in an envelope.

In the field, we observed subjects' food purchases at the university canteens before and after the experiment. These purchases were made with electronic payment cards, which are routinely used by students to purchase a variety of university-related services, including food at the university canteens.[16] We were able to track subjects' purchases because subjects provided the number of their electronic payment card during the experiment and agreed to the scientific use of the related data. We describe the detailed procedure in the next section. Details about the field data as well as the construction of our outcome variable(s) for the field are presented in Section 1.2.4.

1.2.3 Procedure

Subjects were invited via email to sign up for an experimental session. The emails included a link to a consent form which informed subjects about the use of VR glasses, data protection rules, and further participation requirements. In particular, we required subjects to be in good physical and psychological health. We collected the signed consent forms before the start of the experiment.

Subjects received a show-up fee of 5 euro. They had a 50 percent chance of winning a voucher for the university canteen and were able to earn up to 8 euro extra, conditional on the outcome of the WTP elicitation task. The voucher was handed out after the experiment and the payment was transferred to subjects' electronic payment card account. We informed subjects about the payment mode and the requirement to possess a valid electronic card in our invitation email.

At the beginning of the experiment, the experimenter instructed subjects in the use of the VR headsets. We used the "standalone" VR headset *Oculus Go*, which does not require a connection to a computer. In addition, every subject obtained headphones which were connected to the VR headset. After

2.05 euro and tend to be slightly cheaper for the vegetarian meal. In Bonn, the voucher only paid for the main dish and was valid in two different canteens. The vouchers were always valid for at least two weeks after the experimental session to give subjects enough time to use them.

[16] At the University of Mannheim, every enrolled student receives an ecUM (electronic card Universität Mannheim) which also serves as student ID. The MensaCard at the University of Bonn is optional and can be obtained against a deposit of 10 euro.

the instructions, subjects watched a 360° test video for five minutes.[17] The purpose of this video was to provide subjects with a first VR experience and to identify any problems that they may have when using the VR headset.

Throughout the experiment, the experimenter ensured that subjects started and finished videos at the same time, by giving instructions when to put on the headsets. VR headsets were handed out to subjects before they watched a video and collected as soon as the video was over. When subjects put on the VR headset, the video started automatically. Subjects were not able to continue with the experiment until their VR headset was collected.

The main experiment started after the VR instructions and consisted of three parts, which are illustrated in Figure 1.1. The first part was computer-based. Subjects read additional instructions and answered some questions about their familiarity with VR headsets. They were also prompted to enter the number of their electronic payment card on a separate sheet of paper with their participant label, which allowed us to pay out subjects and to link the laboratory and field data. Subsequently, subjects were informed that they were going to watch a 360° video about a virtual tour of the German central bank building and answered questions on that institution. As soon as all subjects had answered the questionnaire, they watched the video and, afterwards, answered questions about the video on the computer. We included "filler questions" about the German central bank and the corresponding video to obfuscate our research question, as for example proposed by Zizzo (2010).

In the second part of the experiment, we informed subjects about the opportunity to watch a video about intensive farming of pigs and asked questions about this topic. For example, we asked how well-informed subjects feel about the living conditions of pigs in intensive farming, how they assess these living conditions, and how the capacity of pigs to feel pain compares to that of humans. The questions were similar in structure to those about the German central bank. In a subsequent step, we elicited subjects' WTP for watching the video about intensive farming of pigs based on our multiple price list approach. After all subjects completed the WTP elicitation task, the computer randomly selected one price from the multiple price list for each subject and subjects watched the

[17] The video was a documentary about the tropical rain forest, published by the Federal Ministry for Economic Cooperation and Development, and is available online: https://www.youtube.com/watch?v=5S9nArmo_x4 (accessed March 19, 2020).

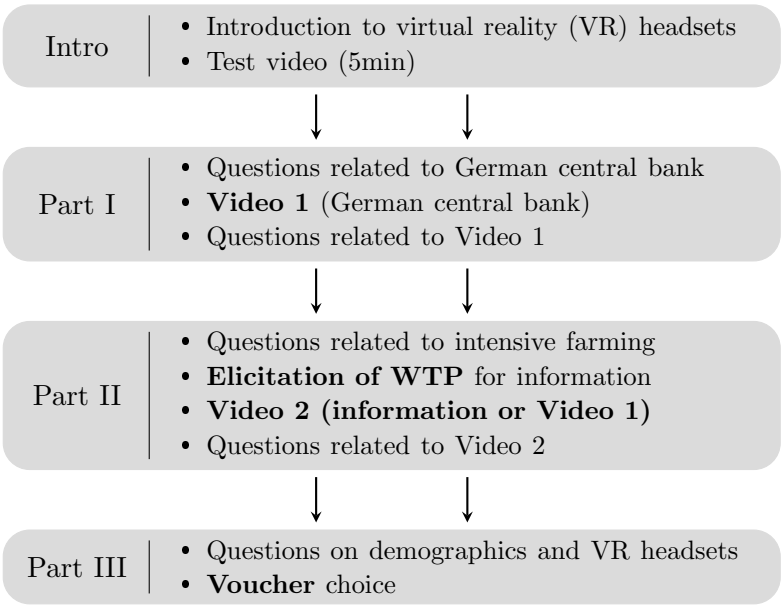

Figure 1.1: Illustration of Laboratory Procedure

Notes: The figure illustrates the different parts of the laboratory experiment.

video chosen at this price. Afterwards, subjects answered questions about the video on the computer.

The third part of the experiment consisted of questions on demographics and the VR headset as well as the voucher lottery. After all subjects chose among the vegetarian and the non-vegetarian voucher, the lottery outcome was randomly drawn and presented to each subject on a final computer screen. This screen asked students to come to the desk of the experimenter when their seat number was called. At the desk, the experimenter handed over an envelope with information about the subject's payment and a voucher, if applicable. To activate their payment, subjects had to put their card on an electronic card reader at the main university canteen.[18]

The experimental sessions were conducted between November 2019 and March 2020.[19] Each experimental session took about 1 hour and average payouts including the voucher were worth 11.3 euro. We used oTree (Chen et al., 2016) to

[18]In Mannheim, subjects could activate their payment on Tuesday in the week after the experiment. In Bonn, activation was possible on Monday two weeks after the experiment.

[19]Days with sessions in Bonn: January 15–18, 2020. Days with sessions in Mannheim: November 11–15, 2019; November 18–19, 2019; November 21–22, 2019; November 25, 2019; November 27, 2019; December 5–6, 2019; December 10, 2019; February 24–27, 2020; March 3, 2020.

program the experiment. Our final sample consists of 330 subjects: 126 students from the University of Bonn recruited via hroot (Bock et al., 2014) and 204 students from the University of Mannheim recruited via ORSEE (Greiner, 2015). Since we are interested in subjects' WTP for information and use this measure for our identification strategy of the information effect, our final sample excludes 8 subjects who made inconsistent choices in the WTP elicitation task.[20] Due to the coronavirus pandemic, we had to cancel planned sessions, which leaves us with fewer than the 500 participants that we pre-registered as our sample size. We intend to reach that number through additional sessions as soon as experimental laboratories reopen.

1.2.4 Data

As shown in Table 1.1, the average age of subjects in our experimental sample is 22 years. 46 percent of the subjects are female and 29 percent have obtained at least a Bachelor's degree. Furthermore, 62 percent of subjects took part in our experiment in Mannheim, while the remaining 38 percent took part in Bonn. The stated meat-eating habit varies across subjects: 13 percent state to never eat meat and can thus be classified as vegetarians. 51 percent eat meat at least several times per week, while the remaining 36 percent state to eat meat only occasionally, i.e., several times per month or several times per year.

Although vegetarians might be less likely to adjust their meat consumption in response to receiving information, we deliberately include them in our main analysis for two reasons. First, receiving the information could affect the likelihood that a vegetarian eats meat in the near future. Second, the share of vegetarians that (do not) avoid information might influence whether information avoiders are more or less responsive to information than information seekers. We explore how the response to information depends on the stated meat eating habit in Section 1.4.5.

About half of the subjects (173 out of 330) received the information on the

[20] In addition, 2 individuals accidentally participated without having student status at the respective university, which was a requirement for participation. They were compensated for taking part, but are excluded from our analysis. In our main analysis, we additionally exclude 4 senior students at age 61 or above. They are arguably very different from the rest of the sample. Including these subjects instead does not affect our main results. Another subject indicated an age of 223 years. We expect this to be a typo and impute an age of 23. The results are not sensitive to this observation.

living conditions of pigs in intensive farming. In Appendix A.4, we present the detailed responses to questions related to intensive farming and meat consumption, which were asked before the elicitation of subjects' WTP. On average, the living conditions of pigs were expected to be bad and subjects expected significantly worse living conditions after receiving the information.

We complement our laboratory experiment with data on subjects' food purchases at the university canteens. The observation period runs from August 1, 2019, until March 16, 2020, in Mannheim and from October 1, 2019, until March 19, 2020, in Bonn. The end of the observation periods was determined by the shut down of the university canteens to fight the spread of the coronavirus.[21] The data includes any food item that was purchased by a subject with his or her electronic payment card at a university canteen. The cards are routinely used by students to make such purchases.[22] For the purpose of our study, we are interested in food items that typically constitute a meal, such as a main and side dish, as well as bakery products like sandwiches. We thus exclude desserts, candy, and small snacks like crisps or fruits.[23] Food items purchased with a voucher from our experiment are generally not included in the data set as they were booked separately. In some rare cases, they nevertheless show up as a food item with a price of zero. We drop purchases with such a food item from our field data (19 observations).

As a meal, we consider all food items that are purchased by the same subject within a 20-minute time frame. We use the menus of the canteens to determine whether a meal includes at least one food item with meat. In some cases, we

[21] We planned to include two lecture periods at each university (August 1, 2019, until May 31, 2020, in Mannheim and October 1, 2019, until July 19, 2020, in Bonn). At the University of Mannheim, all canteens were closed as of March 17, 2020. At the University of Bonn, all canteens were closed as of March 20, 2020. Both university canteens established a takeaway service, but only 6 and 4 of our laboratory subjects made use of this service in Mannheim and Bonn, respectively. We exclude the period after canteens were closed, which affects less than 0.6 percent of all purchased food items in the data set.

[22] At the main canteen of the University of Mannheim, 97 percent of all transactions during the observation period were paid with an electronic card. At the two main canteens of the University of Bonn, 100 and 54 percent of all transactions during the observation period were paid with an electronic card. In Mannheim, paying with the electronic card is a prerequisite for receiving a student discount.

[23] These food categories typically do not contain meat and thus do not confront subjects with the choice of whether to eat meat. In Section 1.4.3, we show that receiving the information signal did not affect the likelihood to purchase food in the considered food categories, which suggests that there was no systematic substitution between included and excluded food categories.

Table 1.1: Descriptive Statistics

Variable	Mean	S.d.	*N*
Data from the laboratory			
WTP for information on intensive farming (euro)[a]	0.32	1.86	312
Age (years)	21.99	3.27	330
Female (1:yes, 0:no)	0.46	0.50	330
Bachelor's degree or higher (1:yes, 0:no)	0.29	0.45	330
Location: Mannheim (1:yes, 0:no)	0.62	0.49	330
Meat-eating habit (stated frequency)			
Never	0.13	0.33	330
Several times per year	0.11	0.31	330
Several times per month	0.25	0.44	330
Several times per week	0.38	0.49	330
Daily	0.13	0.33	330
Received information (1:yes, 0:no)	0.52	0.50	330
Data from the university canteens			
Number of purchases before experiment	19.15	18.07	330
Number of purchases after experiment	10.04	12.22	330
Observation period before experiment (days)	129.22	41.50	330
Observation period after experiment (days)	76.25	39.51	330
At least one purchase after experiment (1:yes, 0:no)	0.84	0.37	330
Share of meals with meat before experiment	0.42	0.31	292
Share of meals without meat before experiment	0.43	0.32	292
Share of ambiguous meals before experiment	0.15	0.22	292
Outcome variables			
Data from the laboratory			
Chose voucher for meal with meat (1:yes, 0:no)	0.37	0.48	330
Data from university canteens			
Share of meals with meat after experiment	0.42	0.35	276
Share of meals without meat after experiment	0.46	0.35	276

Notes: [a]The WTP for information is based on the midpoint of the corresponding WTP interval and only subjects with a WTP that is bounded by the prices in the multiple price list are considered.

are unable to determine with certainty whether a food item contains meat. This occurs, for example, when a food item exists in two varieties that are labeled identically in the data set. Therefore, we construct two different measures of meat consumption for each subject: (i) the share of meals with at least one food item that contains meat for sure and (ii) the share of meals without any food item that may contain meat. The residual category captures the share of meals with no food item that contains meat for sure and at least one food item that may contain meat.

In total, we observe 9634 meal purchases. This is equivalent to 29 meals per subjects, with 19 meals before and 10 meals after the experiment (see Table 1.1).[24] The average purchase price amounts to 3.11 euro and closely corresponds to typical price levels for meals at the university canteens (see Figure A.5 in Appendix A.9 for the full price distribution). Only less than 2 percent of purchase prices exceed 7 euro, which is the maximum price for a standard meal. Such rare high prices can occur when students purchase a meal that is priced by its weight (e.g., salads) or when students purchase multiple meals. Our results are robust to excluding purchases that are worth more than 7 euro from the analysis.

After our experiment, we observe at least one meal purchase for 276 subjects (84 percent).[25] Table A.14 in Appendix A.8 compares these subjects to those that did not purchase a meal in the period after the experiment. The latter are more likely to be female and to be located in Bonn. As expected, they also purchased fewer meals before the experiment and have a shorter (longer) observation period after (before) the experiment. All other pairwise comparisons, including the likelihood of having received the information about intensive farming, are not statistically significant. In Section 1.4, we conduct robustness checks to show that differences in sample composition across our laboratory and field data do not affect our findings.

[24]The larger number of meals before the experiment is partly driven by the longer time period. If we restrict our attention to the period from 13 days before to 13 days after the experiment (the shortest post-experimental period), the number of meals before and after the experiment amount to 2.9 and 3.1. Hence, we do not find evidence that the experiment reduced subjects' propensity to purchase meals in the university canteens (see also Figure A.7 in Appendix A.9).

[25]Five subjects did not provide a valid electronic card number. Additionally, there are two instances where the same electronic card number was used by two students. Since it is not possible to participate in the experiment twice, a subject might have entered the electronic card number of his or her friend. We link the data from the electronic card number to the subject who participated in an earlier session.

1.3 Empirical Strategy

To introduce our empirical strategy, we use the potential outcomes framework by Rubin (1974). Let Y_i^1 and Y_i^0 denote the potential outcome of individual i if she receives treatment ($T_i = 1$) or not ($T_i = 0$), respectively. In our case, treatment corresponds to watching the video about intensive farming of animals and the outcome to a measure of a subject's meat consumption.

We are interested in the average treatment effect, defined as $ATE = E(Y_i^1 - Y_i^0)$. Following the program evaluation literature (Imbens and Wooldridge, 2009), we can identify the ATE if two main assumptions are fulfilled: *unconfoundedness* and *overlap*. In our application, both assumptions are met as a consequence of our experimental design. Unconfoundedness requires that the potential outcomes Y_i^1 and Y_i^0 are as good as random, i.e., independent of treatment status T_i conditional on a covariate X_i: $(Y_i^1, Y_i^0) \perp T_i | X_i$. In our experiment, a subject receives the treatment only if her willingness to pay for information exceeds the randomly drawn price from the multiple price list. Hence, treatment is as good as random after conditioning on willingness-to-pay: $(Y_i^1, Y_i^0) \perp T_i | WTP_i$. Furthermore, overlap requires that, conditional on WTP_i, the treatment probabilities for individuals in the treatment and control group are larger than zero and smaller than one. This assumption holds for all subjects whose WTP is bounded by the prices in the multiple price list (95 percent of our sample). We know that their theoretical propensity of being treated lies between 27.5 and 72.5 percent. We exclude the remaining 5 percent of subjects with a WTP above 8 or below -8 from our estimation sample, as they are always or never treated, respectively. In these rare cases, treatment effects cannot be estimated without further restrictive identifying assumptions.

For our estimation sample of 312 subjects, we use an inverse probability weighting (IPW) estimator to obtain a consistent estimate of the average treatment effect of information. The idea of this approach is to eliminate differences between the treatment and control group by weighting the observations based on their propensity of being treated. To improve the statistical efficiency of the IPW estimator, we estimate the propensity scores by calculating the empirical treatment probability for each value of the WTP rather than using the true propensity scores that directly follow from the experimental design (Hirano et al., 2003).

The IPW estimator can be expressed as

$$\begin{aligned}\hat{\tau}_{IPW} &= \frac{1}{N}\sum_{i=1}^{N}\frac{T_i Y_i}{\hat{p}(WTP_i)} - \frac{1}{N}\sum_{i=1}^{N}\frac{(1-T_i)Y_i}{1-\hat{p}(WTP_i)} \\ &= \hat{\mu}_{T=1} - \hat{\mu}_{T=0},\end{aligned} \tag{1.1}$$

where WTP_i is the WTP of individual i, Y_i is the individual's propensity to eat meat, and $\hat{p}(.)$ is the estimated propensity of being treated, which only depends on the WTP in our experiment. The estimator can be understood as the difference of two terms. The first term estimates the average propensity to eat meat if everyone in the underlying population received the information ($\hat{\mu}_{T=1}$). For this purpose, it weights the outcomes of all treated subjects by their inverse probability of being treated. In particular, subjects who are treated despite having a low propensity of treatment receive a larger weight since they are underrepresented in the treatment group. The second term estimates the average propensity to eat meat if no one of the underlying population would receive the information ($\hat{\mu}_{T=0}$). Here, the weights are the inverse of the probability of not being treated, such that individuals who are underrepresented in the control group receive a larger weight. The propensity score is estimated based on the fraction of subjects with a particular WTP that received the information:

$$\hat{p}(WTP) = \frac{\sum_{j=1}^{N}\mathbb{1}\{WTP_j = WTP\}T_j}{\sum_{j=1}^{N}\mathbb{1}\{WTP_j = WTP\}}. \tag{1.2}$$

Given our empirical estimator of the propensity score, the IPW estimator presented in equation 1.1 is equivalent to averaging over the estimated conditional average treatment effects at each WTP level.

In total, 161 of the 312 subjects with a bounded WTP received the information in our experiment (see Figure A.6 of Appendix A.9 for the distribution conditional on WTP). Without weighting, the WTP for information is larger in the group of individuals that received information, compared to the group that did not receive information (for details, see Table A.15 in the Appendix). If individuals' WTP for information was correlated with their meat-eating behaviors, this imbalance could confound the comparison of the treatment and control group average. Weighting eliminates this imbalance in the WTP. As our design ensures that treatment probabilities merely depend on subjects' WTP, weight-

ing allows us to rule out the impact of other confounding factors. Nevertheless, small differences in group characteristics can arise from sampling variation. In fact, even after weighting, pairwise comparisons suggest that treated subjects tend to be less likely to hold a degree and tend to eat less meat, as measured by their stated meat consumption as well as their observed meat consumption in the canteens before the experiment.

To account for such differences, we also estimate the ATE based on weighted least squares (WLS) and augmented inverse probability weighting (AIPW). Both estimators allow us to control for individual characteristics, which serves two purposes. First, it improves the precision of our estimates and, second, controls for differences between the treatment and control group that are present even after weighting.[26] The WLS estimator regresses the outcome variable on a set of control variables, weighting observations by the inverse probability weights. It coincides with the IPW estimator if only a constant and a treatment dummy are included. The AIPW estimator "augments" the IPW estimator by using the control variables to predict outcome levels for each treatment group based on separate regressions (see, e.g., Lunceford and Davidian, 2004). Since our outcome variables of meat consumption are either binary or fractional, we use logistic regression models. More details of the AIPW estimator are presented in Appendix A.3.1.

The WLS and the AIPW estimators (as well as the IPW estimator) are consistent if the propensity score model is correctly specified (see, e.g., Lunceford and Davidian, 2004; Kang and Schafer, 2007). We can rule out any misspecification of the propensity score model as the treatment assignment is determined by our experimental design. To estimate heterogeneous treatment effects by different groups, we apply the IPW estimator to each group separately or include group-specific treatment and group dummies (WLS). Since allowing for heterogeneous effects is demanding in terms of sample size, we do not apply the more flexible AIPW estimator for that purpose.

For all estimators, we calculate asymptotic standard errors that account for the estimation of the propensity scores based on M-estimation methods (Stefanski and Boos, 2002). In Appendix A.5, we show that our results are robust to using

[26]The WLS estimator and the AIPW estimator can improve efficiency compared to the IPW estimator particularly in cases where both the propensity score model as well as the included outcome model are correctly specified (Lunceford and Davidian, 2004; Kang and Schafer, 2007).

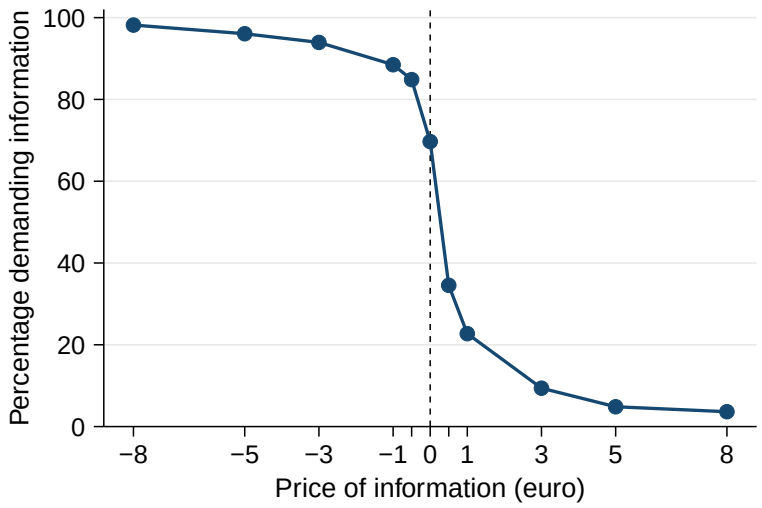

Figure 1.2: Demand for Information

Notes: The figure shows the percentage of subjects choosing to receive information about intensive farming at a given price of information from the multiple price list.

the true propensity scores instead. Details about the IPW estimator with the true propensity scores are presented in Appendix A.3.2. We also show that results are similar if we use simple OLS regressions to condition on the WTP, instead of weighting the observations based on the propensity scores.

1.4 Results

1.4.1 Demand for Information

Figure 1.2 presents the demand for information in our experiment. The share of subjects who choose to obtain information on intensive farming decreases from about 98 percent at a price of −8 euro to about 4 percent at a price of 8 euro. As hypothesized, we observe information avoidance: when information is costless, about 30 percent of subjects do not want to receive it.

While the demand curve for information is flat at high absolute prices, it is very steep, i.e., price elastic, around zero. When the price of information increases from 0 to 0.5 euro, the share of subjects who choose to obtain information decreases from 70 to 35 percent. At a price of −0.5 euro, i.e., a subsidy for information acquisition, the share of subjects not acquiring the information is cut in half, relative to providing information for free. These results are in line with Cain and Dana (2012), Momsen and Ohndorf (2020), and Serra-Garcia and

Szech (2020), who find that introducing a small monetary incentive or cost can have substantial effects on the level of information acquisition in experimental games. The large price elasticity around zero also emphasizes the importance of holding opportunity costs constant when eliciting the WTP for information.

The median WTP for information is in the interval $[0, 0.5)$ euro. When we use the midpoints of the WTP intervals as estimates for the WTP and focus on individuals with a WTP between -8 and 8 euro, the average WTP for information is 0.32 euro. In Table A.16 of Appendix A.8, we also present correlations between the WTP and observable characteristics. For example, a higher level of meat consumption is associated with a lower WTP for information about intensive farming.

1.4.2 Information Effect in the Laboratory

Table 1.2 presents the estimation results for the average effect of information on the likelihood to choose the voucher that contains meat. Based on the IPW estimator, we find that information on pigs' living conditions in intensive farming reduces the likelihood to choose meat by about 16 percentage points (column 1). Accounting for the meat-eating habit and further control variables reduces the effect size slightly, to 11 to 12 percentage points, but the effect remains economically and statistically significant (columns 2 to 5). Hence, obtaining information on pigs' living conditions significantly reduced meat consumption in the laboratory. The effect size is substantial given that the estimated average likelihood to choose a voucher for a meal with meat in the absence of information amounts to 42 to 45 percent (see Table 1.2). The finding is robust to focusing on the sample that is used to estimate the information effect in the field and controlling for the baseline meat consumption levels from the university canteens instead of the stated meat-eating habit (see Table A.19 in Appendix A.8).

1.4.3 Information Effect in the Field

To estimate the average effect of information on meat consumption in the field, we use (i) the share of purchased meals that certainly contain meat and (ii) the share of purchased meals that certainly do not contain meat as the outcome variable for each subject. These outcome variables measure the intensive margin of adjusting food choices when eating at the university canteens. We focus on the intensive

Table 1.2: Information Effect in the Laboratory

	Dependent variable: Choosing voucher for meal with meat				
	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
Info effect	-0.156***	-0.116**	-0.119**	-0.111**	-0.107**
	(0.055)	(0.054)	(0.050)	(0.046)	(0.045)
Mean (w/o info)	0.450	0.430	0.431	0.421	0.424
Meat-eating habit	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	312	312	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit in columns 2 to 5, we use indicator variables. Since never eating meat is a perfect predictor of choosing the vegetarian voucher, the expected likelihood for individuals with this meat-eating habit is not predicted via a logit model but directly set to zero for the models in columns 4 and 5. Additional controls are age, gender, degree, location, and WTP.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

margin as we do not detect any effects on the likelihood of purchasing food at the university canteens (see Table A.17 in Appendix A.8). In the main specifications of the WLS and AIPW estimators, we directly control for the baseline level of our outcome variable instead of subjects' stated meat-eating habits.[27]

Table 1.3 presents the estimation results for the average information effect in the field. Based on our IPW estimator, we find that receiving information reduces the propensity that a purchased meal contains meat (Panel A) by 12 percentage points on average. The estimated reduction amounts to 6 to 7 percentage points based our WLS and AIPW estimators that additionally control for covariates, such as baseline meat consumption, and is statistically significant at the 10 percent level throughout. Controlling for baseline levels allows us to account for the fact that, due to sampling variation, treated subjects were already slightly less likely to eat meat before the experiment (as discussed in Section 1.3).

Panel B of Table 1.3 shows our results for the alternative outcome variable, i.e., the propensity that a purchased meal contains no meat. The estimates mirror the results from Panel A: Receiving information significantly increases the

[27]Results are robust to using the stated meat-eating habit as control instead (see Table A.18 in Appendix A.8). For the few subjects who purchased a meal at the canteens after but not before the experiment ($N = 12$), we simply impute the average baseline level of subjects with the same stated meat-eating habit.

Table 1.3: Information Effect in the Field

	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
A. Share of meals with meat					
Info effect	-0.124***	-0.063*	-0.066**	-0.064**	-0.066**
	(0.042)	(0.033)	(0.032)	(0.032)	(0.031)
Mean (w/o info)	0.480	0.454	0.455	0.455	0.454
B. Share of meals without meat					
Info effect	0.147***	0.089**	0.089**	0.087***	0.087***
	(0.041)	(0.044)	(0.041)	(0.033)	(0.033)
Mean (w/o info)	0.374	0.399	0.399	0.398	0.401
Baseline level	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	261	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

propensity to purchase a meal that does not contain meat by 15 percentage points for our IPW estimator and about 9 percentage points for our WLS and AIPW estimators. Taken together, these estimates imply that the effect of information extends to the field.

Our data also allows us to explore the dynamics of the information effect in the field (not pre-specified). To account for the fact that most subjects did not purchase food in the canteens daily, we define our outcome variable as the percentage of purchased meals that certainly contain meat (or not) in a moving time window of seven days. We focus on the first 19 days after the experiment as the number of observations in the moving time window is stable during this period (see Figure A.7 in Appendix A.9). The decline of observations thereafter can be explained by the semester breaks and the fact that university canteens were closed in response to the outbreak of the coronavirus. We also estimate treatment effects in a moving 7-day window during the 14-day period before the experiment as a placebo test.

Figure 1.3 plots the treatment effects against the day that represents the midpoint of the 7-day window. The results are based on the WLS estimator and thus account for covariates and meat consumption before day -14. As expected, we do not observe any significant treatment effect before the experiment. In the first days after the experiment, the treatment effect is statistically significant

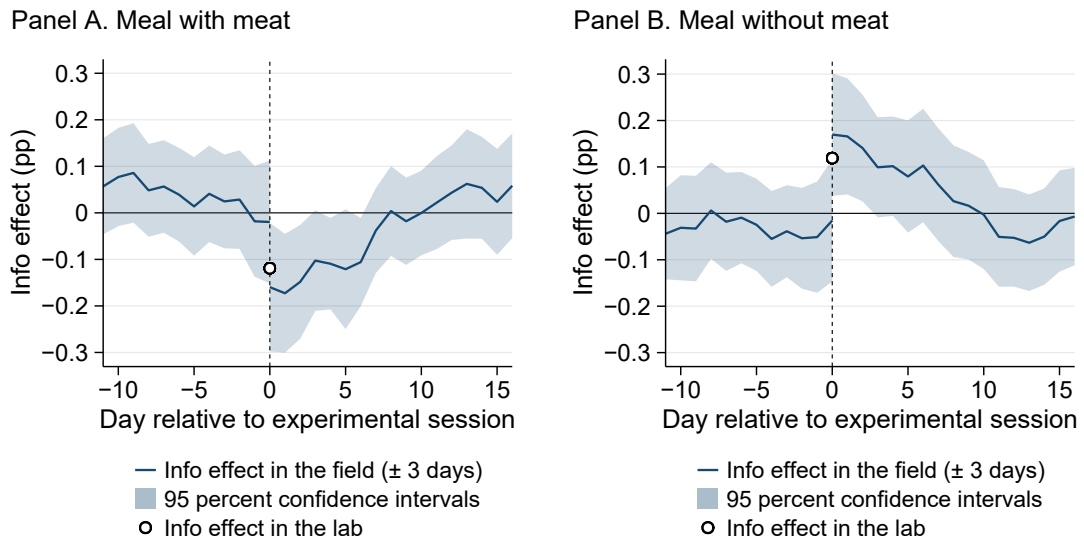

Figure 1.3: Effect of Information over Time

Notes: The figure shows the average effect of receiving information on the propensity to eat a meal with meat (Panel A) and a meal without meat (Panel B) over time. Day 0 represents the day of the experimental session. The effect is estimated based on a moving time window of seven days with the day on the x-axis as the midpoint (±3 days). Around day 0, the time window captures only those days before or after the experiment. We use the WLS specification and control for covariates and the meat consumption before day −14 (missing values are imputed as described in Section 1.4.3). To estimate propensity scores, we merge WTP groups only consisting of treated or untreated subjects with neighboring WTP groups.

and comparable to the effect observed in the laboratory. However, the effect diminishes rapidly and about one week after the experiment, the point estimates are close to zero and no longer statistically significant at any conventional level. As the number of observations within the moving 7-day window is stable during the considered period, the decline in effect sizes is unlikely to reflect changes in sample composition.

The dynamics of the information effect look similar if we extend the considered post-experimental period (see Figure A.8 in Appendix A.9), directly compare the evolution of the average meat consumption by information status (see Figure A.9 in Appendix A.9), or estimate the effect of receiving information for the first three days after the experiment, the first seven days after the experiment, and the period more than one week after the experiment (see Table A.24 in Appendix A.8).[28] Hence, our results suggest that the effect of receiving information is particularly strong in the first days after the experiment but does

[28]We do not detect any statistically or economically significant treatment effect on the extensive margin (see Figure A.10 in Appendix A.9 and Table A.25 in Appendix A.8). Furthermore, Table A.26 in Appendix A.8 shows that the pattern of a diminishing effect over time becomes stronger if we restrict our attention to subjects who are observed to purchase food during as well as after the first week of the post-experimental period.

not persist over time. A potential explanation for the lack of persistence is that individuals gradually suppress information, as found by Zimmermann (2020) for negative feedback on relative IQ-test performance.

1.4.4 Heterogeneity by Information Demand

The results presented in the previous sections show that (i) a substantial percentage of subjects avoid information on the living conditions of pigs in intensive farming and that (ii) receiving information significantly reduces the likelihood to consume meat, in the laboratory as well as in the field. To assess the consequences of information avoidance, we now investigate whether the information effect varies with the demand for information. We allow the information effect to differ between individuals who avoid information when it is costless ($WTP < 0$) and those who acquire costless information ($WTP \geq 0$).

Table 1.4 presents the estimation results for the laboratory. We find that information has a large impact on the meat consumption of avoiders. Receiving the information reduces their likelihood to choose the meat voucher by about 23 percentage points (see columns 2 and 3). In contrast, our point estimates for information seekers are considerably smaller in magnitude. Whether the difference between the information effects is statistically significant depends on the model specification. While we can reject the null hypothesis of homogeneous effects for the IPW estimator, the p-value for the comparison based on the WLS specification is just above 10 percent ($p \approx 0.12$).[29] A stronger effect for avoiders corresponds to a selection pattern "out of responsiveness." It implies that interventions that merely provide free access to moral information will only have modest consequences: Those individuals most susceptible to changing their behavior upon receiving this information will choose to avoid it.

Results from the field are less conclusive, which may be explained by the fact that the field sample is smaller than the one from the laboratory. We only find a significant information effect for avoiders in the case of the IPW estimator (see column 1 of Table A.20 in Appendix A.8). When we control for baseline levels the significance vanishes and differences in the information effect between avoiders and seekers are not statistically significant. The sensitivity to the inclusion of control variables is mainly driven by the fact that even after weighting,

[29]The lack of significance might be driven by the fact that the outbreak of the coronavirus prevented us from reaching the pre-registered sample size of 500 participants.

Table 1.4: Information Effect in the Laboratory – Avoider and Seeker

	Dependent variable: Choosing voucher for meal with meat		
	IPW	WLS	
	(1)	(2)	(3)
Avoider			
Info effect	-0.341***	-0.231***	-0.233***
	(0.100)	(0.086)	(0.082)
Mean (w/o info)	0.581	0.532	0.528
Seeker			
Info effect	-0.077	-0.065	-0.071
	(0.065)	(0.064)	(0.062)
Mean (w/o info)	0.394	0.386	0.389
Comparison of effects (p-value)	0.03	0.12	0.12
Meat-eating habit	No	Yes	Yes
Additional controls	No	No	Yes
Observations	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Indicator variables are used to control for the meat-eating habit in columns 2 to 5. Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

information avoiders who receive information have a lower baseline tendency to eat meat than information avoiders who do not receive information.[30]

Taken together, the patterns observed in the field and the laboratory suggest that avoiders are not less affected by information on intensive farming than information seekers.

1.4.5 Exploratory Heterogeneity Analyses

Beyond the analyses that we have pre-specified, our comprehensive data set allows us to conduct further exploratory analyses along two dimensions. First, we explore whether the information effect depends on the meat-eating habit of the subjects. Similar to Hestermann et al. (2020), we divide subjects into three different categories based on their reported frequency to eat meat. *Vegetarians*

[30]We also check whether the results from the laboratory change if we only consider subjects who are considered for the estimation of the information effect in the field and use the baseline levels from the field instead of the stated meat-eating habit as a control variable. The difference between information avoiders and seekers becomes less pronounced but the point estimate for information avoiders remains larger in absolute terms (see Table A.21 in Appendix A.8).

Table 1.5: Information Effect by Meat-Eating Habit

	Laboratory		Field			
	Meal with meat		Meal with meat		Meal w/o meat	
	(1)	(2)	(3)	(4)	(5)	(6)
Omnivores						
Info effect	-0.207**	-0.201**	-0.087*	-0.106**	0.104**	0.119***
	(0.086)	(0.082)	(0.049)	(0.048)	(0.044)	(0.044)
Mean (w/o info)	0.726	0.724	0.658	0.666	0.224	0.219
Flexitarians						
Info effect	-0.027	-0.041	-0.023	-0.026	0.062	0.060
	(0.074)	(0.070)	(0.063)	(0.062)	(0.094)	(0.086)
Mean (w/o info)	0.158	0.162	0.278	0.281	0.548	0.545
Vegetarians						
Info effect	0.000	-0.016	-0.051	-0.032	0.120	0.114
	(0.000)	(0.028)	(0.039)	(0.040)	(0.105)	(0.109)
Mean (w/o info)	0.000	0.008	0.082	0.070	0.698	0.701
Meat-eating habit	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes
Observations	312	312	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit, we use indicator variables. Additional controls are age, gender, degree, location, and WTP. All estimates are based on the WLS estimator. In columns 1 and 2, the dependent variable is whether the individual chose the voucher for the meal with meat. In column 3 and 4, the dependent variable is the share of meals with meat. In columns 5 and 6, the dependent variable is the share of meals without meat.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

never eat meat (13 percent of subjects), *flexitarians* eat meat several times per month or year (37 percent of subjects), and *omnivores* eat meat several times per week or on a daily basis (51 percent of subjects). Table 1.5 presents the estimation results from WLS regressions for the laboratory and the field. The results suggest that the information effect is primarily driven by omnivores. The absolute information effect for this group ranges from about 21 percent in the laboratory to about 9 percent in the field and is always statistically significant at the 10 percent level or lower. The information effects for the other two groups are usually much smaller and never statistically significant at any conventional level. Furthermore, we can reject the null hypothesis that omnivores and vegetarians have the same information effect in the laboratory ($p < 0.05$).[31]

Second, we investigate the responses to information on the living conditions

[31] We cannot reject the null hypothesis that the information effect for omnivores is different from that for flexitarians at any conventional significance level.

of pigs for two separate meat categories: meat from terrestrial animals, such as pork and chicken, and meat from aquatic animals, such as fish and calamari. Our results show that the point estimates for both categories are almost indistinguishable (see Table A.22 in Appendix A.8), which suggests that subjects adjust their consumption of meat from terrestrial as well as aquatic animals. Hence, subjects may update their beliefs about animal welfare in meat production more generally rather than only for terrestrial animals or pigs. To further disentangle such effects, we divide the meat from terrestrial animals in pork and meat from other terrestrial animals (see Table A.23 in Appendix A.8). The point estimates support the impression that the change in meat consumption is not limited to pork.[32] Hence, our findings offer suggestive evidence that subjects do not only change the consumption of the meat on which information was provided.

1.5 Discussion

In this section, we discuss the relevance of information avoidance for decision-making and explore the consequences of information provision policies that change the cost of information. In practice, policy makers can change the cost of information by providing free access to information or by subsidizing information acquisition, for example. The two crucial factors for this exercise are the percentage of subjects who select into information at a given cost and the response of these subjects to information.

Our experimental design allows us to identify both. We first estimate separate effects of receiving information for subjects that do and those that do not select into information at a given cost. For this purpose, we use the WLS estimator and control for the stated meat-eating habit (laboratory data) or baseline consumption (field data). The estimation results are shown in Figure A.11 and Figure A.13 in Appendix A.9. We then multiply the information effect for subjects who select into information with their share in our study population to obtain the effect of providing information at a given cost on the average level of meat consumption.

We discuss the average treatment effect of information on meat consumption

[32] One caveat to these heterogeneity analyses is that analyzing the effect for each meat category reduces the statistical power to detect significant effects if the aggregate behavioral adjustment actually comes from several categories (as it seems to be the case).

in our study population for various scenarios that differ in the cost of information. The reference scenario for the effects is always that the information is not available or available only at a prohibitive cost. As before, we focus on subjects whose WTP for information is bounded in the range of −8 to 8 euro. For meal purchases at university canteens, results are based on subjects who purchased a meal at the university canteens after the experiment. We focus on the laboratory results in the main discussion and emphasize differences compared to the field results at the end of the section.

The first scenario that we consider is characterized by no information acquisition cost, except for the opportunity cost that subjects face from acquiring information. This scenario allows us to assess the consequences of information technologies, such as the Internet, that make information available at no direct cost. Using the hourly average payout in our experiment (11.3 euro) as a measure of subjects' value of time, the opportunity cost of watching the 5-minute video is about 1 euro. At a cost of 1 euro, only about one-fifth of all individuals watch the video (bottom panel of Figure 1.4). Furthermore, we find that the average reduction in our study population's meat consumption is negligible and amounts to about 2 percentage points (top panel of Figure 1.4). These results are a consequence of the steep information demand curve that prevents about 80 percent of the study population from demanding information at this cost.

Our next scenario explores the implications of compensating individuals for their opportunity cost of time, e.g., through a 1 euro subsidy for watching the video. In this case, the cost of information is zero and the percentage of subjects who choose to receive information increases strongly to 70 percent while the average reduction in meat consumption reaches 5 percentage points. This finding demonstrates that opportunity costs are a major obstacle for individuals to access moral information.

Next, we consider the consequences of reducing the cost of information slightly to −0.5 euro. Such reductions might result from a small financial reward for acquiring information or from requiring individuals to incur some effort to avoid information. In the context of a real-world information campaign, such effort could consist of taking another (more distant) entry to a supermarket or avoiding contact to information campaign staff more generally. Compared to the previous scenario, a decrease in the cost of information to −0.5 euro increases the percentage of subjects obtaining information to about 86 percent. More im-

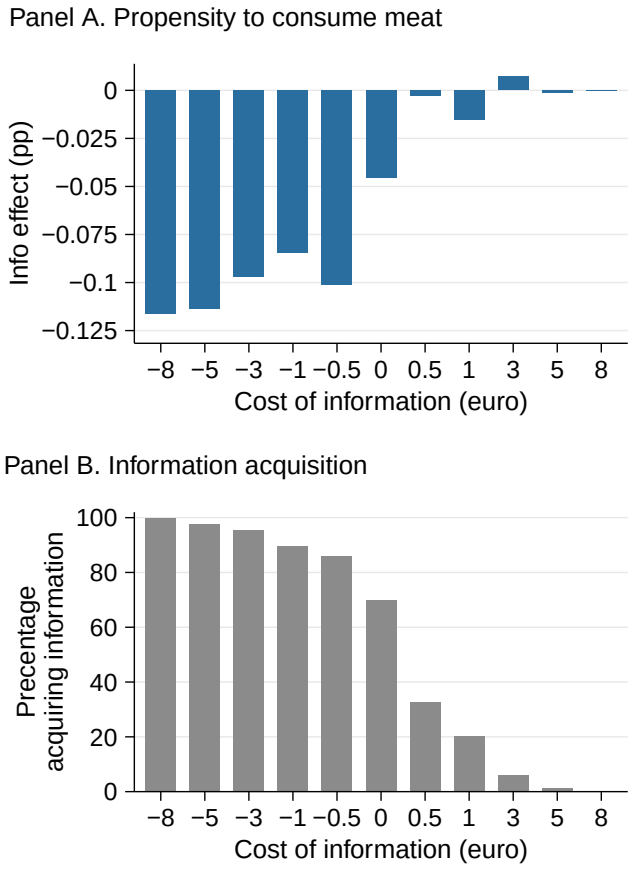

Figure 1.4: Effect of Information Provision on Meat Consumption in the Laboratory

Notes: Panel A shows the average effect of making information available at a given cost. The outcome variable is whether a subject chooses the voucher for a meal with meat. The effect at a given cost c is calculated as $ATE(WTP \geq c) \frac{1}{N} \sum_{i=1}^{N} \mathbb{1}\{WTP_i \geq c\}$, where WTP_i denotes the willingness-to-pay of subject i, $ATE(WTP \geq c)$ denotes the average treatment effect of receiving information for subjects with a willingness-to-pay for information at least as large as the cost, $\mathbb{1}\{\cdot\}$ denotes the indicator function, and N denotes the number of subjects. Estimates of $ATE(WTP \geq c)$ are based on the WLS estimator, allowing for heterogeneous information effects based on the selection into information at a given cost and controlling for the stated meat-eating habit. Panel B shows the fraction of subjects obtaining information at a given cost, $\frac{1}{N} \sum_{i=1}^{N} \mathbb{1}\{WTP_i \geq c\}$. Only individuals whose WTP for information is bounded by -8 and 8 euro are considered.

portantly, we find that the average treatment effect from access to information at that cost rises sharply in magnitude and amounts to −10 percentage points. This increase reflects our finding that information avoiders tend to have larger treatment effects than information seekers. Hence, subtle encouragements, such as small financial incentives, can be highly effective in changing behaviors as they push highly responsive individuals into receiving information.

To gauge the impact of strong encouragements, we consider a scenario where the cost of information reduces to −8 euro. At this cost, all subjects choose to obtain information and the average information effect amounts to −12 percentage points. Very low costs of information could be achieved by large subsidies, but also by interventions that substantially increase the cost for individuals to not obtain information, e.g., through salient labels on food products or the inclusion of the respective information into school curricula. Despite the large change in the cost of information, the reduction in meat consumption is only slightly more pronounced than in the previous scenario with a much smaller encouragement (−12 instead of −10 percentage points). This finding suggests that strong interventions that mandate information acquisition may not be substantially more effective than subtle encouragements.

Our results remain similar when we analyze the scenarios based on our field data. A minor difference is that reducing the cost of information below zero has only little or no additional impact on the average treatment effect (see Figure A.12 in Appendix A.9).

1.6 Conclusion

Our results shed light on the role of moral information for everyday choices. We consider the context of food consumption, where the decision to eat meat from intensive farming entails negative consequences for animal welfare. In our experiment, about 30 percent of subjects avoid information on pigs' living conditions in intensive farming when it is costless. This level of information avoidance is substantial but lies within the range that has been observed in laboratory and online experiments where the information is about the payoff to another subject or a charity (e.g., Dana et al., 2007; Larson and Capra, 2009; Feiler, 2014; Kajackaite, 2015; Grossman and van der Weele, 2017; Serra-Garcia and Szech, 2020; Exley and Kessler, 2021). In particular, these studies have found avoidance rates

from 9 to 72 percent.

To analyze the impact of information provision on consumption choices, we combine our laboratory experiment with data from university canteens. We find that receiving information on the living conditions of pigs reduces the level of meat consumption in the laboratory as well as in the field. The reduction amounts to about 12 percentage points in the laboratory and 6 to 9 percentage points in university canteens. These magnitudes are large, given that the treatment only consists of a 5-minute video about intensive farming practices. The effect of information reduces considerably over the course of the first week after the experiment, which could reflect that individuals suppress information over time (Zimmermann, 2020). Hence, a single informational intervention may not lead to the formation of a new habit and additional interventions may be necessary to achieve persistent behavioral change (see, e.g., Ito et al. 2018 for a discussion of the persistence of moral suasion).

Furthermore, our research design enables us to show that information avoiders, i.e., subjects who avoid information when it is costless, are at least as responsive to information as information seekers, who demand costless information. In a subsequent step, we use our information demand measure and heterogeneous treatment effect estimates to assess the consequences of information provision under varying information acquisition costs. We find that merely providing free access to moral information is likely to only have a small effect on behaviors. The reason for this finding is that a substantial percentage of individuals do not acquire costless information even though they would change their behavior if they received it. However, our results also show that small encouragements to receive information can be sufficient to motivate many responsive individuals to acquire information and thereby strongly increase the effectiveness of information provision.

Our findings have immediate policy implications. On the one hand, they demonstrate that information provision about the consequences of one's actions for others can effectively alter everyday behaviors but that providing information once might be insufficient to create persistent change. On the other hand, they show that an important policy challenge is to target those individuals who choose to remain uninformed when information is costless. Hence, the mere deployment of information technologies that reduce information acquisition costs, such as the Internet, is not a panacea for fostering prosocial behaviors. Instead,

additional incentives to acquire information might be crucial. In practice, such encouragements may take the form of small financial rewards for obtaining information. For example, many health insurance companies reward their customers for taking up pre-emptive medical screening and getting informed about their health status. Rewarding individuals for acquiring information in the context of prosocial behavior might be more difficult, however. An alternative strategy for policy makers would be to increase the cost of information avoidance, e.g., by displaying information prominently on product packages.

More broadly, our findings demonstrate the importance of assessing how the responsiveness to information relates to individuals' information preferences. If not accounted for, the selection of responsive individuals out of information could undermine the effectiveness of a wide range of policies that inform about the consequences of own behavior in order to foster prosocial actions, including policies to fight climate change, poverty, or a pandemic.

Chapter 2

How to Design the Ask? Funding Units vs. Giving Money*

with Johannes Diederich and Timo Goeschl

2.1 Introduction

Donation calls in which the potential donors are asked how many units of a charitable good they wish to fund are a frequently used solicitation scheme among fundraising practitioners. A prominent example that has attracted about one million donors from all over the world is ShareTheMeal, a smartphone app and initiative of the United Nations World Food Programme which is used to provide food to children in need. Donors for ShareTheMeal are informed that feeding one child for a day costs €0.70 and are then asked to indicate the number of feeding days ("meals") that they would like to fund ("share"). Over 88 million meals have been provided through the organization's app so far. Unit donation schemes are not only implemented in food programs. Development aid agencies, for example, promote child sponsorships by fixing the monthly donation for the sponsorship – usually around $35 – and prospective donors choose the number of

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child-months to sponsor rather than the amount of money to donate. UNICEF Canada goes beyond this and provides a whole online shop for specific charitable goods, such as a set of measles vaccines for $16, a teacher training for $114, or a water pump for $492. Similarly, fundraising drives for biodiversity conservation or reforestation programs let donors indicate the number of acres or trees to fund. For instance, in the Monarch Butterfly Habitat Exchange program of the Environmental Defense Fund, donors sponsored acres of milkweed habitat for $35 per acre. In the Plant A Tree program of the Jewish National Fund, donors were asked to choose the number of trees to be planted at $18 a tree.

The prevalence of such schemes in fundraising must reflect a belief among practitioners that they can outperform alternative schemes in particular circumstances. This difference in expected performance needs to outweigh the need for the charity to make more information available to the donor and the restriction on how the raised funds can be used. The belief in better performance may well be justified: Research has shown that seemingly small changes in the choice architecture of a decision problem can lead to substantial changes in donor behavior. Examples are the announcement of seed money (List and Lucking-Reiley, 2002; Huck et al., 2015), specifying default amounts (Goswami and Urminsky, 2016; Altmann et al., 2019), suggesting donation levels (Edwards and List, 2014; Reiley and Samek, 2019), and providing information about the efficacy of the donation (Latour and Manrai, 1989; Cryder et al., 2013). However, to the best of our knowledge, no previous study has systematically compared a solicitation scheme in which the charity asks potential donors to fund units – which we refer to as a "unit donation" scheme – to the traditional scheme of "simply asking for money" (Landry et al., 2010) – which we refer to as a "money donation" scheme.

This paper seeks to close this gap in the literature by comparing both schemes under controlled conditions in an online donation experiment with real money at stake. In their purest forms, the two schemes differ along three dimensions: Unit donation schemes (i) frame the choice in terms of physical units of the charitable good instead of money, (ii) restrict the choice to complete units (i.e. the donation to multiples of the price per unit), and (iii) provide information about the effectiveness of a donation (i.e. the price per unit). The restriction to complete units reflects that some charitable goods are indivisible as a matter of nature (planting half a tree or donating half a coat) or of choice (offering half a meal). Stating the price per unit is necessary for the potential donor to calculate her

expenses. Our experimental variations reflect these three dimensions: Subjects in the money donation treatment are simply asked how much money they would like to donate for the provision of food to malnourished children. Subjects in the unit donation treatments are asked how many nutritional rations they would like to fund at a given price, without the possibility to fund fractions. We compare two different unit sizes: A smaller sized unit of the ration that feeds one child for one day (price of \$0.50) and a larger sized unit that feeds one child for one week (price of \$3.50). In addition to these pure implementations of the money and unit donation schemes, we implement three intermediate forms that selectively activate one or two of the three dimensions that distinguish the two schemes.

Our experiment delivers three main findings. The first is that the money and unit donation schemes were, on average, equally effective in raising donations: There is no statistically significant difference in average donations between the two schemes. This is surprising in light of the higher demands that the unit donation scheme places on the charity. If our results hold more generally, this means that practitioners of unit donation schemes either hold erroneous beliefs about their fundraising effectiveness or use it to pursue other objectives than maximizing the size of the average donation.

Our second finding highlights a plausible alternative objective implicit in unit donation schemes. In our experiment, the unit donation scheme increased the propensity to become a donor when the unit size was small: For the one-day ration at a price of \$0.5, the share of donors was about 13 percentage points higher than for the baseline money donation scheme. This is a statistically and quantitatively significant increase in the propensity to give. An appropriately designed unit donation scheme has therefore the potential to recruit more donors than a money donation scheme. Such a recruitment is likely to be valuable to fundraisers in its own right: Previous research has shown that it is easier to reactivate prior or lapsed donors compared to "cold calling" an unselected sample (Eckel and Grossman, 2008a; Landry et al., 2010). Unit donation schemes therefore have a plausible role in growing a charity's donor base.

Our third finding is that unit size matters: In our experiment, the larger-sized unit (one-week ration at \$3.50) reduced the propensity to become a donor by 22 percentage points compared to the pure money donation scheme, a statistically and quantitatively significant amount. Larger unit-sizes therefore deter donors at the extensive margin. Our intermediate treatments indicate that this decrease

can be traced back to discretizing the donation choice rather than to framing donations in units or to informing about the effectiveness of a donation. In lights of these results, we conclude with the hypothesis that unit donation schemes with small unit sizes decrease a possible stigma associated with "penny donations" or similarly small donation sizes.

2.2 Experimental Design

2.2.1 Donation Appeal

The experiment consisted of a real donation ask administered to subjects during an unrelated online survey. Designing the ask as a pure money donation scheme, a pure unit donation scheme, and various intermediate schemes, requires a charitable good or service readily divisible into discrete and meaningful units. We partenered with a relief organization, Sign of Hope e.V., which frequently uses intermediate schemes of unit donation calls in their own fundraising campaigns. Among their activities, we chose the treatment of malnourished children with a special nutritional paste and high energy cookies in a bush clinic in South Sudan. This service offered practicable units and prices for our experiment. The associated expenses amounted to \$0.50 per day or \$3.50 per week. These benchmarks provided the two different unit sizes for the experiment: (i) a *one-week* nutritional ration per child at a price of \$3.50 and (ii) a *one-day* nutritional ration per child at a price of \$0.50.

The first part of the donation appeal in the experiment was uniform across all treatments. It introduced the charity, the charitable good (treatment of malnourished children with special nutrition in a hospital in South Sudan), and the charitable cause (a high need due to decades of civil war in the country and hence, a high incidence of malnourishment).[1]

The second part of the donation appeal was treatment specific. The six experimental treatments (see Table 2.1) were designed to (i) compare contributions under the unit and money donation scheme, (ii) investigate whether the size of a unit matters for this comparison and (iii) disentangle the channels through which differences may arise. The three treatments of type A ("pure schemes") address

[1]We also provided a link to the charity's web page and informed about a transparency certificate the charity holds to increase trust in the charity (Adena et al., 2019).

aspects (i) and (ii). They consist of a treatment with a pure money donation scheme, in which subjects are simply asked how much money they would like to give, and two treatments with a pure unit donation scheme. In the latter, subjects were asked how many rations they would like to fund (one-week and one-day rations, respectively), had their choice restricted to whole units, and learned the price of a unit ($3.50 and $0.50, respectively). Subjects entered their desired amount of money or number of nutritional rations in an input field at the bottom of the solicitation screen. We provide the exact wording of the donation appeal for each treatment in the Appendix.

The three treatments of type B ("intermediate schemes") address aspect (iii). They include a strict subset of the three characteristics that jointly make up a unit donation scheme (unit framing, restricted choice set, price information). This allows us to identify the channels through which potential differences in giving behavior between the pure schemes arise. Specifically, we conduct an unrestricted money donation scheme that provides unit price information (Info), a money donation scheme restricted to multiples of a disclosed unit price (Info + Restricted), and a unit donation scheme with perfectly divisible units (Info + Frame). The three intermediate schemes are implemented only for the case of the one-week ration because we expect larger treatment effects for the larger-sized unit. The three intermediate schemes deliberately leave out three additional possible combinations of the three characteristics because they do not have a meaningful real-world counterpart. There are, for obvious reasons, no schemes that ask for units to fund without also informing the donor about the unit price; and schemes do not arbitrarily restrict the choices of monetary amounts to multiples of some unit price unless information on the unit price is given.[2]

2.2.2 Experimental Protocol

We conducted the experiment online recruiting U.S. residents from the online labor market Amazon Mechanical Turk (AMT).[3] In the posted task, we informed

[2]To illustrate, imagine a donor who decides how much money to give. If the donor is informed that for $3.50 the charity can provide a nutritional ration that feeds one child for one week, a restriction of donations to multiples of $3.50 will most likely seem reasonable to the donor. However, if this information is not given such a restriction will probably appear arbitrary to the donor.

[3]AMT is known to provide several benefits to researchers, among them fast and easy access to subjects, a diverse subject pool, and low costs (Paolacci et al., 2010; Mason and Suri,

Table 2.1: Treatments

Treatment	Framing	Choice set	Price info	Unit size
A. Pure schemes				
Money Donation	Money	Unrestricted	No	—
Unit Donation – Large	Units	Restricted	Yes	Large
Unit Donation – Small	Units	Restricted	Yes	Small
B. Intermediate schemes				
Info	Money	Unrestricted	Yes	Large
Info + Frame	Units	Unrestricted	Yes	Large
Info + Restricted	Money	Restricted	Yes	Large

Notes: The table provides an overview of the different treatments in our experiment. Framing: whether the ask is framed in terms of money or physical units. Choice set: whether the choice set is restricted to complete units of the charitable good (multiples of the unit price when the donation is framed in money). Price info: whether information about the unit price of the charitable good is provided. Unit size: Whether one physical unit is a one-day ration of food (small) or a one-week ration of food (large).

workers that they would earn \$7 for answering a 20-minute academic survey on several topics, including demographics, occupational background, religion, and opinions about some political and societal challenges. Interested workers followed a link to the survey on LimeSurvey. Before the start of the survey, workers read and confirmed a consent form about the research study.

The experimental survey consisted of 22 questions on sociodemographics, employment, religious beliefs, and political attitude before subjects encountered the donation ask, and 12 unrelated questions after the call. One of the treatments was drawn at random and presented to the subject (between-subjects design). The survey ended with five manipulation check questions. After completing the survey, subjects received a unique code that had to be entered into the survey task window on AMT for payment.

In total, 900 subjects completed the survey experiment. We chose the sample size to be able to pick up significant differences in mean donations starting at about 5% of the endowment, according to power calculations based on data from a pilot experiment (80% power and 5% significance level). The concern that some

2012). Regarding data quality, several papers highlight a high internal consistency of self-reported demographics, an incentive-compatibility of earnings, and a "spammer"-free workforce from the built-in reputation system (Ross et al., 2199; Mason and Suri, 2012). They also provide evidence that results from standard experimental games successfully replicate on AMT (e.g. Paolacci et al., 2010; Rand, 2012). In implementing our experiment, we followed the suggestions for researchers in that literature and the Guidelines for Academic Requesters on AMT (WeAreDynamo, 2014).

Table 2.2: Summary Statistics

Variable	Mean	SD	N
Female	0.54	0.50	842
Age (years)	37.11	10.59	847
Has children	0.46	0.50	844
College graduate	0.49	0.50	845
Crowdworker	0.25	0.43	834

Notes: The total sample consists of 848 observations. We did not force subjects to answer the sociodemographic questions in the survey. The variable *crowdworker* indicates whether online crowdworking is the subject's primary source of income.

subjects may fraudulently use multiple accounts to participate more than once is generally seen as a minor problem in online experiments (Horton et al., 2011; Paolacci et al., 2010).[4] We nevertheless follow the common approach to exclude subjects with duplicate Internet Protocol addresses from the analysis. Including them does not change the results. This leaves us with a sample of 848 subjects. Average payouts were \$5.87 excluding donations. Subjects took on average 10.1 minutes to complete the experiment.

2.3 Results

Table 2.2 shows summary statistics for the sample that participated in our experiment. Our average subject is slightly more likely to be female than male, has an average age of 37.1 years, and has children and a college degree with a probability of 46% and 49%, respectively. About 25% of subjects state that online crowdworking is their primary source of income. To check the balance of these sociodemgraphic variables across the experimental groups, we regress each variable on treatment dummies and conduct F-tests for the joint significance of the coefficients. The corresponding p-values are reported together with the group means in Appendix Table B.1. The test results suggest that the randomization led to experimental groups that are balanced on all sociodemgraphic variables (p-values between 0.25 and 0.93).

Table 2.3 reports the share of donors and the mean donation (including non-

[4] In the case of AMT, having multiple accounts is forbidden by Amazon's Terms of Service (Mason and Suri, 2012) and creating an account requires a unique credit card number (Paolacci et al., 2010).

Table 2.3: Donations

Treatment	Share of donors	Mean donation	N
A. Pure schemes			
Money Donation	0.47 (0.50)	0.95 (1.53)	152
Unit Donation - Large ($p = \$3.50$)	0.26 (0.44)	1.16 (2.14)	121
Unit Donation - Small ($p = \$0.50$)	0.60 (0.49)	1.07 (1.62)	146
B. Intermediate schemes			
Info ($p = \$3.50$)	0.43 (0.50)	1.12 (1.86)	150
Info + Unit frame ($p = \$3.50$)	0.38 (0.49)	1.15 (2.06)	132
Info + Restricted ($p = \$3.50$)	0.27 (0.44)	1.34 (2.10)	146

Note: Standard deviations in parentheses.

donors) in each of the six treatment groups. For the treatments with unit framing, the mean dollar donation corresponds to the average number of nutritional rations donated times the unit price. Mean donations vary between \$0.95 for the money donation scheme and \$1.34 for the money donation with unit cost information and a restricted choice set. The variance within each treatment is considerable and donations range from the lower limit of \$0.00 (no donation) to the the upper limit of \$7.00. The share of donors varies between 26% for the large-sized and 60% for the small-sized unit donation scheme. The benchmark of the standard money donation is at 47%. These descriptive statistics point to possibly significant extensive-margin effects across solicitation schemes.

Figure 2.1 presents the cumulative distribution of donations for the three pure scheme treatments. The solid black line refers to the 152 donation decisions under the standard money donation scheme. As reported in Table 2.3, we see that slightly more than half of the subjects chose not to donate to the charity. For positive donations, there are clear focal points of contributions at full dollar amounts and, less pronounced, at half dollars. In other words, donors do not make use of the unrestricted nature of the donation space, with some exceptions between \$0.00 and \$1.00. Unsurprisingly, lower money donations are more frequently observed than higher ones.

The dashed blue line shows the cumulative distribution of 121 donation decisions under the large-sized unit donation scheme. There are only three possible donation levels under this scheme: No donation (\$0.00), one week of nutrition (\$3.50), or two weeks of nutrition (\$7.00). 74% of subjects chose not to donate under this scheme while 18% chose to provide one week of nutrition and about

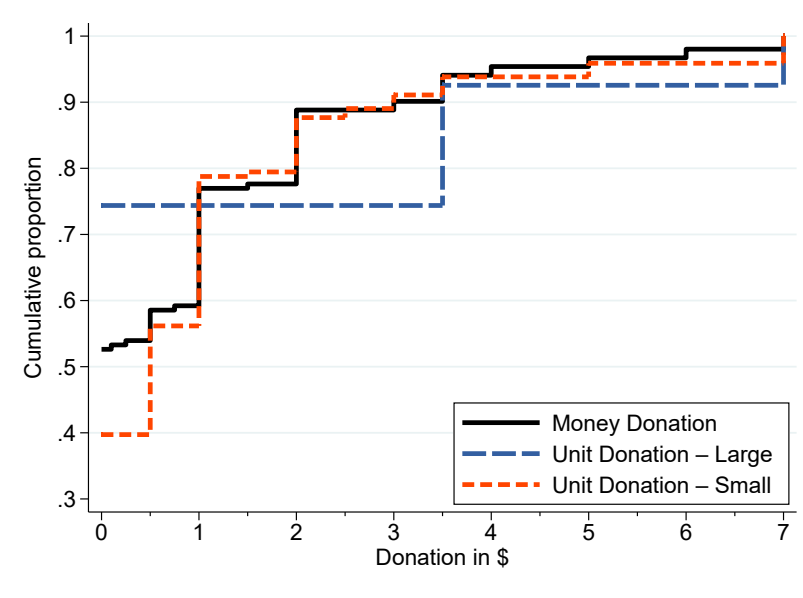

Figure 2.1: Cumulative Distribution of Donations in the Pure Scheme Treatments

7% to provide two weeks. The dashed orange line presents the cumulative distribution of 146 donation decisions under the small-sized unit donation scheme. There are fifteen possible donation levels for subjects, ranging from zero to fourteen days of nutrition. Here, around 40% of subjects chose not to donate. For positive donations, slightly more than 16% chose to provide a single day of nutrition at a cost of \$0.50 and almost 23% chose two days. More days of nutrition are less common and their relative frequency under the small-sized unit donation scheme visually does not differ much from that under the money donation scheme.

We proceed in Section 2.3.1 by first comparing the mean donations (including non-donors) across treatments. Afterwards, we investigate the behavior of potential donors at the extensive margin (Section 2.3.2).

2.3.1 Donations

We observed in Table 2.3 that the differences in the mean donations between treatments are small (at most 5.6% of the endowment of \$7) and that the within-treatment variances are high. More specifically, the differences between the pure money and pure unit donation treatments amount to \$0.21 for the one-week and to \$0.12 for the one-day rations. Both differences are insignificant in a two-sided t-test ($p = 0.380$ and $p = 0.546$, respectively).

Table 2.4: Effect on Donations (OLS Regression)

	Pure schemes		Pure and intermediate schemes with large unit size			
	(1)	(2)	(3)	(4)	(5)	(6)
Unit Donation – Large	0.202	0.217	–	–	–	–
	(0.231)	(0.232)				
Unit Donation – Small	0.110	0.104	–	–	–	–
	(0.183)	(0.182)				
Price information	–	–	0.216	0.237	0.167	0.180
			(0.185)	(0.186)	(0.196)	(0.197)
Unit frame	–	–	0.117	0.149	0.221	0.270
			(0.175)	(0.177)	(0.237)	(0.239)
Restricted choice set	–	–	-0.070	-0.111	0.029	0.005
			(0.174)	(0.176)	(0.228)	(0.232)
Unit frame × restricted	–	–	–	–	-0.215	-0.251
					(0.351)	(0.357)
Controls[a]	No	Yes	No	Yes	No	Yes
Observations	419	408	701	686	701	686
R^2	0.002	0.032	0.004	0.018	0.004	0.019

Notes: Robust standard errors are in parentheses, $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The dependent variable is the amount of money the charity receives and the pure money donation scheme always serves as baseline. In columns 1 and 2, only the treatements with pure solicitation schemes are considered. In columns 3 to 6, all treatments except the unit donation scheme with a small unit size are considered.
[a]Controls include gender, age, whether the individual has a college degree, whether the individual has children and whether online crowdworking is the individual's primary source of income.

In columns 1 and 2 of Table 2.4, we report estimation results from regressing the monetary amount donated on the treatment dummies, using the pure money donation scheme as baseline and only considering the pure solicitation schemes. Even after controlling for available covariates, the differences between the pure solicitation schemes remain very small and insignificant (see column 2). Thus, we do not find evidence that applying a unit donation scheme instead of a money donation scheme significantly affects the average amount of money received, irrespective of whether a small or large unit size is employed.

For the large unit size, we use our intermediate treatments to identify how specific characteristics of a unit donation scheme affect giving. In particular, we regress individuals' donations on a dummy for each of the three characteristics of a unit donation scheme (price information, unit framing, and a restricted choice set). This allows us to test whether the insignificant difference between the unit and money donation scheme masks countervailing effects of single characteristics. The estimation results are reported in columns 3 and 4 of Table 2.5. We do not find any evidence for such countervailing effects: All coefficients are small

and insignificantly different from zero. The same holds true if we additionally include the interaction of using a unit frame and restricting the choice set (see columns 5 and 6), which can be identified due to the selection of our intermediate treatments.

2.3.2 Propensity to Give

As already noted, the differences at the extensive margin merit attention. While in the pure money scheme, about 47% of subjects donate, only about 26% of subjects decide to give under the pure unit scheme with a one-week ration at price of \$3.50 as single unit ($p < 0.001$, χ^2-test). If the unit presented to subjects is instead a one-day nutritional ration at a unit price of \$0.50, the propensity to donate is about 13 percentage points higher than under the pure money donation scheme ($p = 0.026$, χ^2-test).

Regression results from a linear probability model are presented in Table 2.5 and confirm these findings.[5] In columns 1 and 2, we regress the binary variable of whether an individual donated on the type of the solicitation scheme, only considering pure schemes and using the pure money donation scheme as baseline. A unit donation scheme with a large unit size (a one-week ration at a price of \$3.50) is estimated to decrease the propensity to give by about 22 percentage points compared to a pure money donation scheme, whereas a unit donation scheme with a small unit size (a one-day ration at a price of \$0.50) is estimated to increase the propensity to give by 11 percentage points when including controls. Hence, applying a unit donation scheme affects the propensity to give, but the direction of the effect depends on the unit size.

Why do we find such substantial effects on the extensive margin but no significant differences in mean donations? As Figure 2.1 reveals, the large-sized unit donation scheme decreases the share of individuals who donate but also encourages individuals to choose a higher donation level than they would have chosen under a money donation scheme. For example, the mass of individuals who give more than \$1.00 but less than \$3.50 under the money donation scheme seems to entirely shift to the donation level of \$3.50. In the case of the small-sized unit donation scheme, the positive impact on the extensive margin does not translate into substantially higher mean donations since the increase is mainly driven by

[5] Results are robust to using a probit model instead.

Table 2.5: Effect on the Propensity to Give (Linear Probability Model)

	Pure schemes		Pure and intermediate schemes with large unit size			
	(1)	(2)	(3)	(4)	(5)	(6)
Unit Donation – Large	-0.217***	-0.219***	–	–	–	–
	(0.057)	(0.057)				
Unit Donation – Small	0.129**	0.113**	–	–	–	–
	(0.057)	(0.057)				
Price information	–	–	-0.050	-0.050	-0.040	-0.041
			(0.054)	(0.054)	(0.057)	(0.058)
Unit frame	–	–	-0.033	-0.027	-0.055	-0.047
			(0.040)	(0.041)	(0.059)	(0.059)
Restricted choice set	–	–	-0.146***	-0.157***	-0.166***	-0.177***
			(0.040)	(0.040)	(0.055)	(0.056)
Unit frame × restricted	–	–	–	–	0.044	0.043
					(0.080)	(0.081)
Controls[a]	No	Yes	No	Yes	No	Yes
Observations	419	408	701	686	701	686
R^2	0.077	0.109	0.032	0.045	0.033	0.046

Notes: Robust standard errors are in parentheses, $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The dependent variable is whether a subject has donated and the pure money donation scheme always serves as baseline. In columns 1 and 2, only the treatements with pure solicitation schemes are considered. In columns 3 to 6, all treatments except the unit donation scheme with a small unit size are considered.
[a]Controls include gender, age, whether the individual has a college degree, whether the individual has children and whether online crowdworking is the individual's primary source of income.

additional small donations of one unit, i.e. \$0.50. Beyond donations of \$0.50, the cumulative distribution function looks similar to that of the money donation treatment.

Due to the inclusion of the intermediate treatments, we are able to attribute differences in the propensity to give between the pure money and the large-sized unit donation scheme to a particular characteristic. Analogously to the procedure in Section 2.3.1, we regress the binary variable of whether a subject donated on a dummy for each of the three characteristics. The estimation results are reported in columns 3 and 4 of Table 2.5. Neither the unit frame nor the information on the effectiveness of a donation significantly affects the propensity to give. By contrast, restricting the choice set significantly reduces the propensity to give by about 15 percentage points. In columns 5 and 6, we additionally allow for an interaction between the restriction of the choice set and the framing in units. The impact of restricting choices is slightly but not significantly smaller in absolute terms if a unit frame is already in place ($p = 0.59$). These results are robust to controlling for sociodemographic characteristics and are consistent with pairwise comparisons.

2.4 Discussion and Conclusion

Donation schemes can be designed in terms of physical units to fund rather than the amount of money to give. Does this design of the ask affect individuals' giving behavior? The popularity of unit donation schemes among fundraisers suggests that it should, and that the scheme's performance justifies the complications of the design, such as additional information provision and the reduced freedom in how the funds can be used. To address this research question, we conducted an online experiment in which we tested different solicitation schemes. While we do not find evidence that unit donation schemes affect the amount of money raised, we show that they alter the propensity to give. The direction of this effect depends on the unit size of the charitable good. If the unit size is small, a unit donation scheme attracts more donors than a competing money donation scheme. If the unit size is large, unit donations can deter donors. A unit donation scheme with a small unit size can therefore be an effective strategy for a charity to expand its donor base.

An interesting question is why the effect on the extensive margin reverses when the unit size becomes small. The negative impact of larger-sized units on the extensive margin of giving, relative to the money donation scheme, is unsurprising. In fact, from the intermediate treatments, we can pinpoint that it is the restriction of the choice set that is mostly responsible for the drop in donors. Since the price of becoming a donor increases from almost zero ($0.01) in a virtually continuous choice set to $3.50 in a restricted choice set, this demand side response is expected. However, the same logic would apply to the smaller-sized unit. There, the price of becoming a donor increases from almost zero ($0.01) in a virtually continuous choice set to $0.50 (the price of a one-day nutritional ration). As a result, we would expect the magnitude of the effect at the extensive margin to be smaller, but the predicted direction would be the same.

While we do not have intermediate treatments for the small unit size that would allow us to disentangle the exact characteristic responsible for that increase, a closer look at the distributions of the donations under the pure solicitation schemes (see Figure 2.1) offers a plausible explanation. As observed earlier, the distribution of donation amounts under the money donation scheme has focal points at $0, $1.00, and $2.00. This suggests that a substantial share of subjects

behaves as if the range of donations available is restricted to integers of dollar amounts. In such a world, the minimum donation, and hence the *perceived* price of becoming a donor, is \$1.00. Offering to fund a charitable good with a price below the smallest focal point of the unrestricted distribution of giving reduces the perceived price of becoming a donor, resulting in a higher propensity to give. Another possible explanation is that the small unit size acts like a low suggested donation amount. Edwards and List (2014) show that suggesting an amount that is below the average donation can increase the propensity to give. Finally, in a world in which "penny donations" carry a stigma or are considered unproductive, a smaller-sized unit justifies small donation amounts. These explanations are in line with the observation that the positive effect on the extensive margin is mainly driven by additional small donations of \$0.5 (as discussed in Section 2.3.2).

Our results from the intermediate treatments (for the large unit size) offer interesting insights into the relevance of framing, choice restrictions, and information provision. First, simply rephrasing the ask from giving money to funding units does not affect giving behavior. Second, we show that restrictions of the choice set can have large behavioral consequences that should be taken into account when designing experiments or fundraising campaigns. This evidence is in line with the finding that a minimum donation amount reduces the propensity to give in the absence of extrinsic incentives to give (Cartwright and Mirza, 2019). It also matches well with the result that large suggested donation amounts discourage giving (Adena et al., 2014). Finally, providing explicit information about the per unit price of the charitable good did not significantly affect the propensity to give or overall donations in our experiment. Previous experiments have used different formats when providing information about the effectiveness of a donation and either did not find an impact on the aggregate (Karlan and Wood, 2017) or identified a significant increase in donations (Latour and Manrai, 1989; Cryder et al., 2013).

An interesting avenue for future research is to explore the role of the unit size in more detail. While we show that the unit size matters for the impact of applying a unit donation scheme, it is unclear whether the effect on the extensive margin monotonically increases with the unit size. Furthermore, it would be interesting to know at which point the effect reverses and to which extent it depends on the distribution of donations under unrestricted choices.

Chapter 3

Subsidizing Unit Donations: Matches, Rebates, and Discounts Compared*

with Johannes Diederich, Catherine C. Eckel, Timo Goeschl, and Philip J. Grossman

3.1 Introduction

Subsidies are a common way of incentivizing charitable giving. They typically take the shape of rebates, in which a third party (e.g., the government) refunds a fraction r of the gift back to the donor; or the shape of matches, in which a third party (e.g., a generous donor) supplements each donation at a rate m, such that the charity receives a total of $(1 + m)$ times the original donation. Both rebates and matches have been extensively studied and several key findings have emerged in the literature (see Vesterlund, 2016; Epperson and Reif, 2019,

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for comprehensive reviews). Probably the most notable result is that although rebates and matches imply the same price of giving if the corresponding subsidy rates r and m satisfy $r = \frac{m}{m+1}$, overall donations received by the charity are higher under matches than under equivalent rebates (Eckel and Grossman, 2003, 2006a,b, 2008a, 2017; Davis et al., 2005; Lukas et al., 2010; Bekkers, 2015; Gandullia and Lezzi, 2018; Gandullia, 2019).[1] Another finding is that matching subsidies often significantly increase private contributions net of the subsidy compared to a no subsidy condition without a lead donor (Eckel et al., 2007; Karlan and List, 2007; Gneezy et al., 2014; Huck et al., 2015; Eckel and Grossman, 2017).[2]

The literature has established these findings in a setting in which individuals are asked to decide how much money to give to a charity, arguably the most common scheme for soliciting donations. We refer to this as a *money donation* scheme. Yet another frequently applied strategy is to frame the donor's choice variable not in terms of money, but in terms of physical units of a charitable good awaiting funding. A prominent example that has attracted donors from all over the world is *ShareTheMeal*, a smartphone app and initiative of the UN World Food Programme, which is used to provide food to children in need. Donors for ShareTheMeal do not simply choose an amount of money to give. Instead, they are informed that feeding one child for a day costs \$0.80 and are then asked to indicate the number of feeding days ("meals") that they would like to fund.[3] We refer to this alternative scheme as a *unit donation* scheme.

Do the key findings about the effects of matches and rebates in money donations generalize to the alternative unit donation scheme? In this paper, we examine the effect of subsidies on unit donations by conducting an online field experiment. We asked 558 subjects how many units of a charitable good they

[1] In this literature, rebates are realized without any delay. If a delay is involved (as is the case for tax deductions), time preferences need to be considered. Furthermore, we are aware of only one paper that finds the same level of charity receipts between rebates and matches, but it does so under a choice architecture that does not resemble a typical donation decision: In Davis (2006), subjects do not decide how much to donate (checkbook giving) but how much the charity receives (charity receipts). This choice architecture, motivated by an investigation of the causal mechanism that underpins the standard result of non-equivalence, makes it difficult to compare his results with ours.

[2] There are counterexamples to this finding, however (e.g., Karlan et al., 2011; Helms McCarty et al., 2018).

[3] Similar food provision campaigns are the "100 Thousand Meals" appeal of the Salvation Army Australia or the "Help with €2" campaign of Misereor, the German Catholic Bishops' Organisation for Development Cooperation.

would like to provide to a predetermined charity (Sign of Hope e.V.), funded out of their reward for answering an unrelated online survey. The decision variable is framed in quantities of nutritional packages provided for malnourished children. The unsubsidized price is \$0.50 per package. In the baseline, no subsidy is offered. The main treatments differ across three subsidy types and two subsidy rates. The first type, the rebate, is offered at a rate of either 33% or 50% such that a third party refunds to the subject about one third or one half of the reward she spent on nutritional packages. The second type, the match, is offered at a rate of 0.5 (1:2) or 1 (1:1) such that a third party adds a nutritional package for either every two or each package donated. The third subsidy type is novel for charitable giving and takes the form of a price discount of either 33% or 50%. This subsidy type is without a direct parallel in money donations.

The contribution of this paper is threefold. First, we define unit donations as a separate class of charitable donations distinct from money donations. Second, we investigate how rebates and matches perform in a setting of unit donations and compare the results to the established literature on money donations. Based on between-subjects evidence, our core result is that matches and rebates are equally effective in incentivizing private net donations and in generating total charity receipts. In other words, we do not replicate the superiority of matching subsidies observed in the case of money donations. Third, we check whether, in a setting of unit donations, the discount subsidy offers an attractive alternative to these subsidy types. We find that discounts are equally effective as matches and rebates when considering net donations or charity receipts. This may well be good news for charities: In a world in which subsidy types perform equally well, charities enjoy additional degrees of freedom in campaign design. At the same time, the different subsidy types perform differently when disaggregated into the extensive and the intensive margin of giving: Rebates are more effective than matches in attracting donors, but matches result in larger donations. Under discounts, the likelihood to give is lower than under rebates, and on both margins, behavior corresponds to that under matches. We conclude that if attracting donors is a secondary objective of a fundraising campaign that uses unit donations, rebates merit additional consideration. New donors offer the possibility of an ongoing income stream for charities, since previous donors are more likely to give in the future (Eckel and Grossman, 2008a; Landry et al., 2010).

The remainder of the paper is organized as follows. In a background section

(Section 3.2), we contrast money and unit donations, explain the mechanics of subsidizing the latter, and review the relevant literature. Section 3.3 describes our experimental design, followed by a presentation of our main results (Section 3.4) and a discussion of potential explanations (Section 3.5). Section 3.6 concludes.

3.2 Background

3.2.1 Unit vs. Money Donations

For our purposes, we define a *money donation* scheme as a solicitation scheme in which potential donors are asked to decide how much money to give to a charity. It is arguably the most common scheme for solicting donations. Academic papers in the lineage of the now classic donation models (Bergstrom et al., 1986; Andreoni, 1988, 1989) capture its main features by generally assuming a linear production technology for the charitable public good and normalizing the per-unit price of both the private and the public good to one. In such models, the prospective donor i's choice is to divide her endowment w_i (in dollars) between private consumption x_i (in dollars) and giving g_i (in dollars) to the charitable good, G. Under a money donation scheme, therefore, the donor's choice variable g_i is denominated in terms of monetary expenditures.

By contrast, we define a *unit donation* scheme as a solicitation scheme that frames the donor's choice variable in terms of physical units of a charitable good awaiting funding. Unit donation schemes have a popularity that extends beyond the food programs mentioned above. Development aid agencies, for example, promote child sponsorships by fixing the monthly donation for the sponsorship – usually around $35 – and prospective donors choose the number of child-months to sponsor rather than the amount of money to donate. Similarly, fundraising drives for biodiversity conservation or reforestation programs let donors indicate the number of acres or trees to fund.[4] In unit donation schemes, the price of a unit of the charitable good G is no longer implicit. Instead, the fundraiser states an explicit price p and asks how many discrete units g_i the potential donor would

[4]For instance, in the *Monarch Butterfly Habitat Exchange* program of the Environmental Defense Fund, donors sponsor acres of milkweed habitat for $35 per acre. In the *Plant A Tree* program of the Jewish National Fund, donors choose the number of trees to be planted at $18 a tree.

like to fund. In this respect, the setting resembles early models of the private provision of public goods that are explicit about units and prices (e.g., Warr, 1983). Under a unit donation scheme, therefore, the donor's choice variable g_i is denominated in terms of the quantity of the charitable good funded.

Although under both schemes donors eventually provide money, there are important differences between unit and money donations. First, donors' choice sets differ. Under a unit donation scheme, the units of the charitable good to be provided are typically indivisible, which introduces an element of discreteness that is largely absent in the virtually continuous money donations. Second, the information provided to prospective donors differs. By stating the per-unit price of the charitable good, unit donation schemes make statements about the charity's marginal cost of production, whereas money donation schemes frequently provide little information on the cost structure of producing the charitable good. While information on the share of fundraising and overhead costs is increasingly available to donors (Ribar and Wilhelm, 2002; Meer, 2014), information on the impact of a contribution (or the absence thereof) can substantially affect donations (Bekkers and Wiepking, 2011; Lewis and Small, 2019). Third, the framing of the choice differs. By asking for the number of physical units of the charitable good, unit donation schemes emphasize how a donation generates specific outcomes for recipients. As a result, the motive of giving to create an impact (Duncan, 2004) might become more relevant for the donation decision.

Diederich et al. (2020) compare the two donation schemes in an experimental study and show that the choice of the donation scheme significantly affects the likelihood of receiving donations. The direction of the effect depends on the size of a physical unit: A unit donation scheme attracts more donors than the equivalent money donation scheme if the unit size is small (daily nutritional rations at a price of \$0.50) but fewer donors if the unit size is large (weekly rations at a price of \$3.50). For the large unit size, the difference is primarily driven by the restricted choice set under the unit donation scheme.

3.2.2 Subsidizing Unit Donations

Subsidizing unit donations involves some small but important differences compared to subsidizing money donations. In unit donations, rebates can be applied by refunding a fraction of the donor's provision costs back to the donor. If, for

example, a unit of the charitable good costs \$0.50 and a 50% rebate is offered, the donor receives \$0.25 back for each unit funded. Matches can be applied to unit donations by providing supplementary units of the charitable good. If, for example, a 1:1 match is offered for tree plantings, the third party funds one additional tree for each tree funded by the donor. Due to the indivisibility of units, matching payments by the third party are restricted to complete units of the charitable good. This introduces some discontinuity in the matching payment if the matching rate is not an integer: For example, at a matching rate of 0.5 (1:2) every second tree funded by the donor induces one tree funded by the third party. However, for a donation of only one tree, there is no additional funding by the third party. This is in contrast to the continuous choice in money donations, in which the matching rate typically applies to any arbitrary amount in the same way (i.e., at a matching rate of 0.5, a donation of any dollar amount induces a matching payment of 0.5 times this amount).

The transferability of results from money to unit donations is therefore not only a matter of framing effects: When matches consist of supplementary units and rebates are refunded costs, rebates and matches are also no longer theoretically equivalent. This is particularly evident at the extensive margin of becoming a donor: The smallest positive donation is to fund one unit of the charitable good. Given a unit price p, this implies a minimum expense of p required under matches. In contrast, rebates provide a refund on the donation given and, at subsidy rate of r, the cost of becoming a donor is $p(1-r) < p$. As a result, rebates are potentially more effective in attracting donors. An additional difference comes into play when subsidy rates take non-integer values: The change in the matching payment due to a one unit increase in the donation depends on the donation level. In contrast, under rebates any increase in the donation proportionally increases the subsidy payment, as is the case for both subsidy types under money donations. In sum, there are not only structural differences between money and unit donations; there are also reasons to expect that subsidies perform differently under the two schemes.

In a way, unit donation schemes resemble the shopping experience for private goods. For example, WorldVision provides a comprehensive gift catalog where donors can choose the number of units of various gifts that are associated with explicit prices.[5] In this regard, a matching subsidy is similar to bonus packs or

[5]https://donate.worldvision.org/giftcatalog (accessed on March 31, 2021)

"buy one get two" offers, whereas a rebate is comparable to coupons that provide an instant refund at checkout or cash-back programs (although the latter usually involve a time delay between the payment and the refund). However, with a well-defined good and an explicit price, there is a third option available, which is a direct price discount. Similar to stores that advertise price reductions, a charity can announce the discounted price of a gift and reveal that the gap to the original price is provided by a large donor or a governmental grant. Hence, if a unit of the charitable good costs \$0.50 and a 50% discount is offered, the donor can fund one unit at a price of \$0.25 while being informed that the remaining \$0.25 are funded by an external party.

A discount at rate d is theoretically equivalent to a rebate at rate r. However, two small differences exist that could cause different behavior.[6] First, rebate subsidies are paid to the donor whereas discount subsidies are paid to the charity. Second, in comparison to rebates (and matches), discounts obviate the need for donors to calculate the effective price of giving. In an online charity gift shop like the one by WorldVision, the rebate would take effect as instant refund upon checkout whereas the discount is applied to the advertised prices during "shopping." Furthermore, evidence for private goods shows that consumers may indeed respond different to rebates and direct price discounts (Davis and Millner, 2005), which makes it crucial to distinguish between both subsidy types in our research.

3.2.3 Related Literature

We are not aware of any previous study that conducts a clean comparison between subsidy types under a pure unit donation scheme. At the same time, there are parallels with a number of papers studying charitable giving. Like our study, Meier (2007) and Gneezy et al. (2014), for example, feature discrete choice sets. However, both frame donations in money, rather than physical quantities, and focus on matches only, yielding results that align with the wider money donation literature. A different parallel is with Lewis and Small (2019) who also provide subjects with information about the cost of a unit of impact and test

[6] If rebates are realized with a delay, a third difference comes into play: The expenses at the time of the donation are larger under a rebate than under a discount. In this paper, rebates are realized without delay as it allows us to compare our rebate to its money donation counterpart in the experimental literature that brought about the seminal result of matches outperforming rebates (e.g., Eckel and Grossman, 2003, 2006b; Davis et al., 2005).

different framings of the information. They find that a cheaper unit price leads to lower donations, an effect that is eliminated or reversed if the price is framed in units-per-dollar rather than dollars-per-unit. Yet, donations in their study are again framed in terms of money, rather than physical quantities, and the authors do not compare different subsidy types. Also relevant is a literature in marketing that experimentally compares product promotion strategies such as bonus packs (which are similar to matching subsidies) and price promotions (which are similar to our discount subsidy if they explicitly state the effective price). The papers in this literature provide mixed evidence (see, e.g., Sinha and Smith, 2000; Mishra and Mishra, 2011; Chen et al., 2012), with bonus packs either being superior, equivalent, or inferior to price promotions. More in line with the money donations literature are Davis and Millner (2005), who find that matches outperform rebates also for private goods and that direct price discounts have an effect in between. While our focus on charitable giving sets our paper apart from this literature, its setting of unit donations offers the opportunity to study price discounts, a tool from private product promotion, in the context of charitable donations.

The paper probably closest to the focus of ours is Kesternich et al. (2016). The authors compare the effectiveness of rebate and matching subsidies in the context of carbon offsetting: When buying their ticket(s) online, clients of a long distance bus operator decide whether to offset the carbon emissions from their travel at a given price per kilogram of emissions. Rebates are found to increase the likelihood to offset while matches only do so at certain matching rates and to a lesser extent. However, the overall contributions net of the subsidy are higher under matches. Key differences to our study are the binary decision format and the use of an impure public good for which the size of giving is tied to the private good. Both limit the comparability to our setup. A few other studies implicitly employ an experimental design soliciting unit donations to an environmental public good (Löschel et al., 2013; Diederich and Goeschl, 2014, 2017, 2018), but they do not compare subsidy types.[7]

[7]Weakly related to a unit donation scheme are so called "buy-one give-one" business models (see, e.g., Marquis and Park, 2014; Hamby, 2016) where for each product purchased the selling company donates a similar product. However, in these models, the donation is tied to the consumption of a private good. We are not aware of any paper introducing or comparing subsidies in that context.

3.3 Experimental Design

3.3.1 Donation Appeal

We adapt the real-donation dictator game introduced by Eckel and Grossman (1996) and subsequently applied to compare subsidy types (Eckel and Grossman, 2003; Davis and Millner, 2005; Davis, 2006; Eckel and Grossman, 2006a,b). In the standard version of the game, subjects decide how much of their money endowment to hold and how much to pass to a charity. In our variant of the game, subjects decide how many units of the charitable good to fund at a given nominal price.

Our variant of the game requires a charitable good or service that is easily quantifiable. We approached a relief organization, Sign of Hope e.V., which frequently uses various forms of unit donation schemes in their fundraising campaigns. Among their activities, we chose the treatment of malnourished children in a certain area of South Sudan as this service offered practical units and prices for our experiment. The children were treated in two "bush clinics" operated by the relief organization at the time of the experiment. Treating one child for one month using a special nutritional paste and high energy cookies requires a donation of US$15. We divided this number into practical units of nutritional packages per child and day, which implies a "price" of $0.50 per package.

The donation appeal was part of an online survey and participants used their reward for completing the survey ($2) to make any donations. The donation appeal introduced the charity, the charitable good, and its marginal provision cost to the charity. We also provided a link to the charity's web page and informed subjects about a transparency award the charity had won to increase trust in the charity (Adena et al., 2019). The final part of the donation appeal was treatment specific. Table 3.1 shows the seven treatment conditions. In the control condition, no subsidy was applied and subjects chose how many packages to fund at a price of $0.50. The remaining six treatment conditions follow a 3×2 factorial design with one factor being the subsidy type (rebate, match, or discount) and the other factor being the effective price ($0.33 or $0.25) implied by the level of the subsidy. In the instructions, we framed the rebate conditions as 33% (50%) rebate and stated that while providing packages would cost the subject $0.50 apiece, a rebate of $0.17 ($0.25) per package would be added to the subject's final reward at the end of the experiment. For the matching conditions,

Table 3.1: Treatment Conditions

Subsidy type	Subsidy rate	Nominal unit price	Effective unit price	N
No subsidy	–	\$0.50	\$0.50	83
Rebate	33%	\$0.50	\$0.33	71
Match	1:2	\$0.50	\$0.33	85
Discount	33%	\$0.33	\$0.33	90
Rebate	50%	\$0.50	\$0.25	58
Match	1:1	\$0.50	\$0.25	80
Discount	50%	\$0.25	\$0.25	91

instructions stated that for every two packages (each package) that the subject provided at a nominal cost of \$0.50 apiece, one package would be matched at no additional cost to the subject. As a result, the charity would receive the combined number of packages. For the discount conditions, instructions stated that the subject would be able to provide packages for \$0.33 (\$0.25) instead of \$0.50 apiece. Hence, the nominal price corresponded to the effective price. For all subsidy types, instructions noted that the subsidy, i.e., the rebate, the matched units, or the money needed to reduce the nominal price, was provided by "a third party." This was a truthful yet indefinite reference to the research budget involved. Subjects chose the desired number of packages from a drop-down menu. The exact wording of each treatment can be found in Table 3.2.[8]

3.3.2 Experimental Protocol

We conduct the experiment online recruiting U.S. residents from the online labor market, Amazon's Mechanical Turk (AMT).[9] In the case of money donations, online field experiments based on AMT (Gandullia and Lezzi, 2018; Gandullia,

[8]Figure C.1 in Appendix C.3 shows a screenshot of the complete donation appeal.

[9]AMT provides several benefits to researchers, among them fast and easy access to subjects, a diverse subject pool, and low costs (Paolacci et al., 2010; Mason and Suri, 2012). Several papers have examined the suitability of AMT for experimental research and have found encouraging results (Paolacci et al., 2010; Ross et al., 2199; Mason and Suri, 2012; Rand, 2012). Results in these papers highlight a high internal consistency of self-reported demographics, an incentive-compatibility of earnings, and a "spammer"-free workforce from the built-in reputation system. They also present and review results from successful replications of standard experimental games in AMT (e.g., Paolacci et al., 2010; Rand, 2012). In implementing our experiment, we followed the suggestions for researchers in that literature and the Guidelines for Academic Requesters on AMT (WeAreDynamo, 2014).

Table 3.2: Final Part of Donation Appeal Wording by Treatment

Treatment	Wording
No subsidy	*In this survey, you may use all, part, or none of your reward of $2.00 for this HIT to provide these nutrition packages. Thus, you may choose any number between 0 and 4 packages. $0.50 per package will be subtracted from your reward.*
33% rebate	[Same text as in no subsidy condition] *Upon completion of the survey, a third party has agreed to fund a **33% rebate** for each package you provide. The rebate ($0.17 per package provided) will be added to your reward.*
1:2 match	[Same text as in no subsidy condition] *A third party has agreed to **match every two packages you provide**, at no additional cost to you. So, for example, if you choose to provide 2 packages, Sign of Hope will receive 3.*
33% discount	*In this survey, you will be able to provide these nutritional packages for **$0.33** apiece (a third party will fund the remaining $0.17). You may use all, part, or none of your reward of $2.00 for this HIT to provide packages. Thus, you may choose any number between 0 and 6 packages. $0.33 per package will be subtracted from your reward.*
50% rebate	[Same text as in no subsidy condition] *Upon completion of the survey, a third party has agreed to fund a **50% rebate** for each package you provide. The rebate ($0.25 per package provided) will be added to your reward.*
1:1 match	[Same text as in no subsidy condition] *A third party has agreed to **match each package you provide**, at no additional cost to you. So, for example, if you choose to provide 2 packages, Sign of Hope will receive 4.*
50% discount	*In this survey, you will be able to provide these nutritional packages for **$0.25** apiece (a third party will fund the remaining $0.25). You may use all, part, or none of your reward of $2.00 for this HIT to provide packages. Thus, you may choose any number between 0 and 8 packages. $0.25 per package will be subtracted from your reward.*

2019) and not based on AMT (Bekkers, 2015) have been successfully used to replicate the superiority of matches over rebates. Gandullia and Lezzi (2018) and Gandullia (2019) use the same endowment level and subsidy rates as we do. Our task was posted five times on the MTurk task queue between July and October 2015, resulting in five online sessions. Interested workers were informed that they would earn \$2 for answering a 20-minutes academic survey on several topics. The payment is rather high when compared to the average hourly wage of about \$3.1 to \$3.5 per worker on AMT (Hara et al., 2018). Each worker was only able to particpate once. Donations were mentioned as one of the topics, but the real-donation dictator game was not particularly salient compared to other survey elements. As a result, it is unlikely that subjects considered the donation task as the main subject of investigation. Interested workers followed a link which directed them to the survey containing the experiment on Qualtrics. Having followed the link to the survey platform, interested workers read and confirmed an informed consent page about the research study.

The experimental survey consisted of four parts: (1) the donation appeal, (2) a questionnaire on various topics, (3) a low-stake version of the Eckel-Grossman risk task (Eckel and Grossman, 2002, 2008b),[10] and (4) a 5-item manipulation check questionnaire comparable to Eckel and Grossman (2003, 2006b) and Davis and Millner (2005). Parts (1) and (2) were presented in random order. Hence, a subject encountered the donation appeal either before or after the questionnaire. One of the treatment conditions was drawn at random and presented to the subject (between-subjects design).[11] The questionnaire of part (2) consisted of questions on sociodemographics, employment, and religious beliefs, as well as

[10]We opt for the Eckel-Grossman Risk Task because of its simplicity and quickness. A sample of AMT workers is likely to exhibit larger heterogeneity in numeracy than a standard laboratory sample of students. The Eckel-Grossman task has been shown to produce better results with people with low mathematical skills (Dave et al., 2010). Stakes start out at \$0.28 for the sure option and end up at \$0.02 and \$0.70 for the most risky gamble.

[11]A different sample of 113 subjects received the treatment conditions in a within-subjects (WS) design to investigate how the results differ if individuals are forced to directly compare different subsidy types. Unlike in the between-subjects design, matches and discounts are more effective in providing the charitable good than rebates, including a significant crowding-in of net donations for those two subsidy types. Our analysis suggests that those different results for the WS sample are mostly an artifact of the WS design, which we consider less externally valid: When confronted with all possible subsidies, subjects seem to make a single decision at the extensive margin of giving across all subsidy types with a similar rate and then respond to the subsidy type mostly at the intensive margin. We provide more details about the WS design and the associated results in Appendix C.4.

current ambient environmental conditions and the Ten Item Personality Inventory (TIPI), which is a standard one-minute version of more extensive multi-item instruments to assess the Big Five personality dimensions (Gosling et al., 2003; Ehrhart et al., 2009). After completion of all survey parts, a unique code was shown that the subject had to enter into the survey task window on AMT to receive payment.

In total, we have 613 observations of participants starting the survey and 599 completed records. Incomplete records were dropped from the analysis.[12] The obvious concern that some subjects may fraudulently use multiple accounts to participate more than once is generally seen as a minor problem in online experiments (Horton et al., 2011; Paolacci et al., 2010).[13] We nevertheless follow the common approach to exclude 40 subjects with duplicate Internet Protocol addresses from the analysis. Including them does not change the results. We also dropped one subject who indicated an age below 18 in the questionnaire, despite having confirmed an age above 18 when agreeing to the informed consent statement. This leaves us with a sample of 558 subjects (see Table 3.1 for the allocation of subjects across treatments). Average payouts were \$1.79 (net of donations and including an average of \$0.30 additional payment for the risk task). Subjects took on average 8.38 minutes to complete the experiment.

3.4 Results

Variables elicited in the questionnaire suggest a diverse sample of subjects (see Table C.2 in Appendix C.2): Slightly less than half of subjects are female, and slightly less than half graduated from college. About one-third of subjects are married, and about the same share has children under age 16 living in the household. Both age and income are well spread, with the median age in category 26–34 and the median yearly income in category US\$40,000–49,999. Separate F-tests suggest that the characteristics are well-balanced across our treatment groups. Only one out of 35 comparisons is significant, and two additional ones

[12] Among the complete observations, three subjects had restarted the survey and hence created an incomplete duplicate record. We kept the complete observations of these three subjects after making sure that they had not encountered a treatment condition in their first attempt and gave the same answers in the survey.

[13] In the case of AMT, having multiple accounts is forbidden by Amazon's Terms of Service (Mason and Suri, 2012) and creating an account requires a unique credit card number (Paolacci et al., 2010).

reach marginal significance.

Answers to the manipulation check questions indicate that on average, subjects clearly understood instructions and procedures, felt that their anonymity was preserved, trusted the experimenters and the charity, and found the recipients of the donations worth supporting (Table C.2 in Appendix C.2). With the exception of the last item, the answers to the manipulation check questions do not significantly differ across treatments.

In Table 3.3, we present descriptive results for donations observed in the experimental treatments. Panel A reports mean values and standard deviations. Column 1 shows the average number of nutritional packages that subjects selected to donate in their version of the donation appeal. Hence, column 1 represents the units purchased before any rebate or matching subsidy while accounting for subsidized nominal prices in the discount conditions. If multiplied by the nominal price, column 1 would correspond to out-of-pocket expenses that are frequently denoted as "checkbook giving" in standard money donation experiments. Column 2 reports individual net donations in dollars that result from subjects' choices after all subsidies are accounted for. That is, column 2 is column 1 evaluated at the (discounted) nominal price minus any rebates. Column 3 refers to the mean number of nutritional packages the charity "receives," based on subjects' choices, that is, column 1 plus any matched packages. If we multiplied column 3 by \$0.50 for all treatments, we would obtain gross charity receipts in dollars, a common focus in money donation experiments. Because of perfect collinearity of both receipts measures, we only use charity receipts expressed in units in the following analysis, with the exception of Figure 3.1 where we use charity receipts expressed in dollars to illustrate its composition. Columns 4 and 5 show the intensive and the extensive margin of giving, respectively. Column 4 reports mean charity receipts conditional on being a donor while column 5 reports the fraction of donors. For each variable, panels B to D report p-values of pairwise comparison tests between treatments.

In the remainder of this section, we present the results from our analysis. We first focus on rebates and matches (Section 3.4.1) and subsequently turn to the effectiveness of discount subsidies (Section 3.4.2). We discuss potential explanations for our results in Section 3.5.

Table 3.3: Descriptive Results

Treatments			Donation variable				
Condition	Nominal unit price ($)	Effective unit price ($)	Individual choice (units)	Net donation ($)	Charity receipt, uncond. (units)	Charity receipt, cond. (units)	Prob. of donation
			(1)	(2)	(3)	(4)	(5)
A. Mean values (S.D.)							
No subsidy	0.50	0.50	1.169	0.584	1.169	2.256	0.518
			(1.413)	(0.706)	(1.413)	(1.177)	(0.503)
33% rebate	0.50	0.33	1.690	0.558	1.690	2.400	0.704
			(1.545)	(0.510)	(1.545)	(1.294)	(0.460)
1:2 match	0.50	0.33	1.059	0.529	1.506	3.048	0.494
			(1.339)	(0.670)	(2.021)	(1.886)	(0.503)
33% discount	0.33	0.33	1.478	0.488	1.478	2.771	0.533
			(1.973)	(0.651)	(1.973)	1.927	(0.502)
50% rebate	0.50	0.25	1.931	0.483	1.931	2.732	0.707
			(1.705)	(0.426)	(1.705)	(1.379)	(0.459)
1:1 match	0.50	0.25	1.113	0.556	2.225	3.787	0.588
			(1.253)	(0.626)	(2.506)	(2.176)	(0.495)
50% discount	0.25	0.25	2.143	0.536	2.143	3.545	0.604
			(2.831)	(0.708)	(2.831)	(2.879)	(0.492)
B. Tests of subsidy types: p-values							
B1. At effective price of $0.33							
33% rebate vs. 1:2 match			0.01	0.76	0.52	0.06	0.01
33% rebate vs. 33% discount			0.44	0.44	0.44	0.27	0.03
1:2 match vs. 33% discount			0.10	0.68	0.93	0.49	0.60
B2. At effective price of $0.25							
50% rebate vs. 1:1 match			0.00	0.41	0.41	0.01	0.15
50% rebate vs. 50% discount			0.57	0.57	0.57	0.07	0.20
1:1 match vs. 50% discount			0.00	0.84	0.84	0.63	0.82
C. Tests of subsidized prices: p-values							
50% vs. 33% rebate			0.41	0.36	0.41	0.24	0.97
1:1 vs. 1:2 match			0.79	0.79	0.05	0.09	0.23
50% vs. 33% discount			0.07	0.64	0.07	0.11	0.34
D. Tests of subsidized vs. unsubsidized prices: p-values							
D1. Low subsidy rate							
33% rebate vs. no subsidy			0.03	0.79	0.03	0.58	0.02
1:2 match vs. no subsidy			0.61	0.61	0.21	0.02	0.76
33% discount vs. no subsidy			0.24	0.35	0.24	0.12	0.84
D2. High subsidy rate							
50% rebate vs. no subsidy			0.01	0.29	0.01	0.09	0.03
1:1 match vs. no subsidy			0.79	0.79	0.00	0.00	0.37
50% discount vs. no subsidy			0.00	0.65	0.00	0.00	0.25

Notes: Panel A shows mean values of the donation variables for each treatment (standard deviations in parentheses). Column 1 reports the number of packages that subjects selected to give at the nominal price. Column 2 shows the net dollar contribution implied by subjects' choices, i.e., column 1 evaluated at the nominal price minus the rebate (if any). Column 3 reports the overall number of packages received by the charity, i.e., column 1 plus matched units (if any). Column 4 reports the same measure as column 3 but conditional on giving (intensive margin). Column 5 reports the share of subjects who donated at least one package (extensive margin). Panels B to D show pairwise tests between treatment conditions. Panel B compares subsidy types conditional on the effective price. Panel C compares the two subsidized prices, $0.25 and $0.33, conditional on subsidy type. Panel D compares the unsubsidized price with the subsidized price arising from the low subsidy rate for each subsidy type. Columns 1 to 4 in panels B to D report p-values of two-tailed t-tests with unequal variances. Column 5 report p-values of Pearson χ^2 tests for binary data.

3.4.1 Rebates versus Matches

Three main results follow from columns 1 to 3 of Table 3.3. First, column 3 shows that the charity received an average of about 1.7 (1.9) units per subject in the 33% (50%) rebate condition and about 1.5 (2.2) units per subject in the 1:2 (1:1) matching condition. At both effective prices, the levels of charity receipts do not significantly differ between the two subsidy types ($p = 0.52$ and $p = 0.41$).

Result 1. *(Charity receipts) Charity receipts do not significantly differ between rebate and matching subsidies.*

Second, column 3 also shows that charity receipts significantly increase in the subsidy level, either from introducing a subsidy ($p < 0.05$ for a 33% rebate, 50% rebate, and 1:1 match) or from increasing the subsidy rate ($p = 0.05$ in case of the match).

Result 2. *(Law of demand) Charity receipts significantly decrease in the price.*

Third, column 2 indicates that net donations exhibit a roughly constant share of around one quarter of the endowment across all treatment conditions. Thus, neither the introduction of a subsidy at any rate nor an increase in the subsidy rate results in significant changes of subjects' own contributions net of the subsidy ($p \geq 0.29$ for pairwise comparisons). Note that in order to achieve the same level of charity receipts and own net donations, subjects need to select more units to donate under a rebate than under a match (since the match is paid on top of the units selected). This is exactly what we observe in column 1: The average number of units selected is at least 0.5 units larger ($p \leq 0.01$ at both effective prices).

Result 3. *(Net donations) There is no evidence for crowding-in or crowding-out of net donations by rebate or matching subsidies of any level.*

Result 3 has an important implication: It implies that the increase in charity receipts (Result 2) is entirely driven by the additional money provided as subsidy payment by the third party, instead of being driven by individuals actually giving more (see Figure 3.1 for an illustration). This finding does not generally hold in the money donation literature. Several papers find that matches significantly increase net donations compared to a no subsidy condition without lead donor (Eckel et al., 2007; Karlan and List, 2007; Gneezy et al., 2014; Huck et al., 2015;

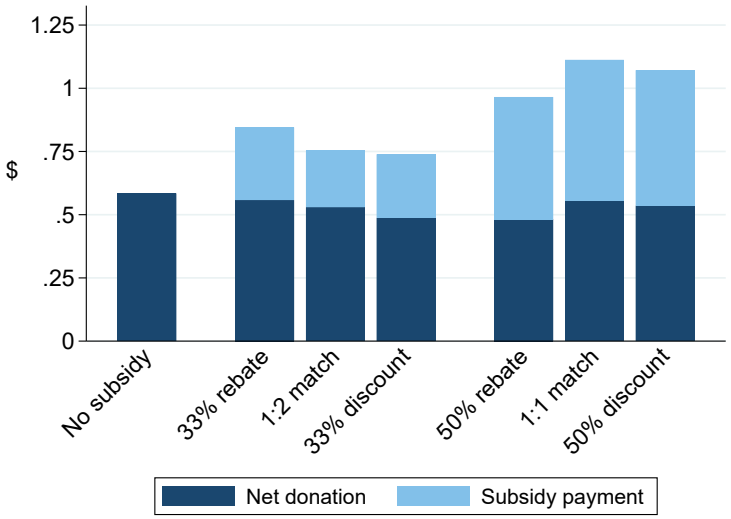

Figure 3.1: Charity Receipts (Divided into Net Donations and Subsidy Payments) by Treatment

Notes: The figure shows the average charity receipts in dollars (divided into average net donations and corresponding subsidy payments) by treatment. This corresponds to charity receipts in units (column 3 of Table 3.3) multiplied by the (unsubsidized) unit price of \$0.50.

Eckel and Grossman, 2017). However, there are also some papers that do not find a significant effect on net donations (Lukas et al., 2010; Karlan et al., 2011; Helms McCarty et al., 2018). Evidence for rebates is scarcer and rather points in the opposite direction, i.e., that rebates crowd out net donations (Eckel and Grossman, 2003, 2008a; Gandullia and Lezzi, 2018; Gandullia, 2019).

The most substantial difference between our findings and that of the money donation literature is the equality of rebates and matches regarding charity receipts (Result 1). The well-established finding in the context of money donations is that charity receipts under matches exceed those under rebates (Eckel and Grossman, 2003, 2006a,b, 2008a, 2017; Davis et al., 2005; Lukas et al., 2010; Bekkers, 2015; Gandullia and Lezzi, 2018; Gandullia, 2019), while "checkbook giving," which corresponds to column 1 of Table 3.3 multiplied by the nominal price, is often roughly the same under both subsidy types. We therefore now examine whether our results are robust to controlling for available covariates. For this purpose, we estimate an Ordered Probit Model with the individual choice as dependent variable and use it to analyze the effect of the different subsidies on the level of charity receipts. The details of the model are presented in Appendix

C.1.[14]

In Panel A of Table 3.4, columns 1 and 2, we present the results in the form of the average marginal effects on charity receipts without and with controlling for covariates. For example, offering a 33% rebate is estimated to increase average charity receipts per individual by about 0.5 packages compared to not offering any subsidy (column 1, Rebate), whereas increasing the subsidy rate from 33% to 50% has no significant effect in the case of the rebate (column 1, Rebate × low price). Analogously to Table 3.3, the predicted levels of charity receipts are compared pairwise across subsidy types in Panel B, holding the effective price constant. The estimates confirm Result 1 and Result 2. We repeat the same exercise for predicted levels of net donations. In line with Result 3, we neither find significant differences in net donations between subsidy types at the same effective price nor any evidence for crowding-in or crowding-out at any conventional significance level when changing the price of giving due to a specific subsidy type.[15]

Having observed that charity receipts do not differ between rebates and matches, we ask whether this result masks heterogeneities in the "conversion rates" of the experimental donation call (the extensive margin of giving) and the conditional level of charity receipts demanded by donors (the intensive margin of giving). As discussed in Section 3.1, rebates decrease the minimum net expense required to become a donor, making them potentially more effective at the extensive margin than matches. Indeed, results in column 5 of Table 3.3 confirm that rebates attract a larger share of donors than matches. In particular, the differences amount to roughly 21 percentage points (70.4% vs. 49.4%) and 12 percentage points (70.7% vs. 58.8%) in the case of the high and the low effective price, respectively. The difference is significant at the high effective price ($p = 0.01$) but not at the low effective price ($p = 0.15$).[16] We take this as evi-

[14]Whereas a common approach in the literature is to estimate a Tobit Model with the monetary value that the individual has chosen to give, charity receipts, or their logarithmized value as dependent variable, the discrete nature of our donation decision makes it an unsuitable choice to model our data. This is supported by conditional moment tests significantly rejecting the assumption of normally distributed error terms for the Tobit Model with charity receipts or logarithmized charity receipts as dependent variable ($p < 0.01$). We nevertheless run different Tobit specifications and OLS regressions as robustness checks and find similar results (available from the authors upon request).

[15]Results are available from the authors upon request.

[16]The fact that the difference in the extensive margin is more pronounced for the low subsidy rate is not surprising, since for the 1:2 match the first unit donated does not result in a matching

Table 3.4: Estimation Results

	Charity Receipts, unconditional (units)		Probability of donation (binary)		Charity Receipts, conditional (units)	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Marginal effects						
Rebate	0.546**	0.533**	0.186**	0.220***	0.091	-0.039
	(0.237)	(0.257)	(0.077)	(0.081)	(0.256)	(0.27)
Match	0.324	0.248	-0.024	-0.011	0.799**	0.488
	(0.261)	(0.291)	(0.077)	(0.086)	(0.359)	(0.392)
Discount	0.306	0.231	0.015	0.012	0.537	0.316
	(0.247)	(0.272)	(0.076)	(0.084)	(0.332)	(0.357)
Rebate × low price	0.218	0.218	0.003	-0.018	0.326	0.342
	(0.277)	(0.302)	(0.081)	(0.088)	(0.266)	(0.279)
Match × low price	0.888**	0.992**	0.093	0.104	0.783*	0.782*
	(0.373)	(0.405)	(0.077)	(0.084)	(0.448)	(0.466)
Discount × low price	0.585*	0.613*	0.071	0.057	0.571	0.807
	(0.326)	(0.366)	(0.073)	(0.084)	(0.448)	(0.503)
B. Tests of subsidy types: p-values						
B1. At effective price of $0.33						
33% rebate vs. 1:2 match	0.43	0.35	0.01	0.00	0.05	0.17
33% rebate vs. 33% discount	0.37	0.30	0.02	0.01	0.17	0.31
1:2 match vs. 33% discount	0.95	0.96	0.60	0.79	0.53	0.69
B2. At effective price of $0.25						
50% rebate vs. 1:1 match	0.23	0.22	0.14	0.21	0.00	0.01
50% rebate vs. 50% discount	0.71	0.81	0.19	0.13	0.09	0.08
1:1 match vs. 50% discount	0.43	0.37	0.82	0.77	0.33	0.78
Covariates[a]	No	Yes	No	Yes	No	Yes
Log likelihood	-801.28	-592.51	-372.07	-250.92	-427.37	-314.94
Observations	558	428	558	428	326	256

Notes: (1)–(2): Ordered Probit with the number of packages selected by the individual as dependent variable. (3)–(4): Probit for whether or not a donation was made. (5)–(6): Ordered Probit conditional on being a donor, with the number of packages selected by the individual as dependent variable. (1)–(2) and (5)–(6) treat a single observation with 5 selected packages as if it were 6 selected packages.
Panel A presents average marginal effects. Standard errors reported in parentheses, $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. For (1)–(2) and (5)–(6), *marginal effects* refer to the average change in expected charity receipts over all individuals or donors only, respectively. For each individual considered, the change is calculated by taking the difference in expected charity receipts between receiving a particular subsidy at the low rate (rebate, match, discount) and not receiving any subsidy or between receiving a particular subsidy at the high rate (rebate × low price, match × low price, discount × low price) and receiving the same subsidy at the low rate.
Panel B presents p-values for the pairwise comparison of treatment effects (subsidy treatment vs. no subsidy) between subsidy types, based on the average marginal effects.
[a]Covariates include gender, marital status, the Big Five personality dimensions, risk preferences, categorical variables for age, income, residential environment, and religion, and dummies for whether the individual holds a college degree, whether children under the age of 16 live in the household, whether the individual is a registered voter, whether the individual frequently attends religious services, whether the individual works for a not-for-profit organization and task order. Likelihood ratio tests reject that their coefficients in model (2), (4) and (6) are jointly zero ($p < 0.01$, $p < 0.01$ and $p < 0.05$, respectively).

dence that the equality result for the level of charity receipts is partly driven by the fact that rebate subsidies are more effective at the extensive margin.

Result 4. *(Extensive margin) Rebates are more effective in attracting donors than matches.*

Turning to the intensive margin, column 4 of Table 3.3, shows that conditional charity receipts under both match conditions significantly exceed the corresponding values in the rebate conditions (3.0 vs. 2.4 units and 3.8 vs. 2.7 units, respectively; $p = 0.06$ and $p = 0.01$).

Result 5. *(Intensive margin) Charity receipts per donor are higher under matching than under rebate subsidies.*

Comparing matching and rebate treatments in column 4 and 5 of Table 3.3 to the control reinforces the view that the channel through which rebates raise unconditional charity receipts primarily is the extensive margin whereas matches unfold their impact through the intensive margin. For the rebate, introducing the low subsidy rate increases the share of donors in column 5 from 51.8% to 70.4% ($p = 0.02$) compared to the no subsidy condition, while the intensive margin is not significantly affected ($p = 0.58$). In contrast, for the match, introducing the low subsidy rate increases mean conditional charity receipts in column 4 from 2.3 to 3.0 units ($p = 0.02$) compared to the no subsidy condition, while the extensive margin is unaffected ($p = 0.76$).

Again, we supplement the descriptive results by estimating appropriate parametric models. Columns 3 and 4 of Table 3.4 refer to a Probit without and with covariates, respectively, while columns 5 and 6 capture the intensive margin by estimating an Ordered Probit Model for donors only. The latter is set up analogously to the Ordered Probit Model used above. If we assumed that after controlling for observable characteristics, the error terms between the decisions to donate and how much to donate are uncorrelated, we could interpret these two models jointly as a Two-Part model. The parametric estimation confirms results 4 and 5, but for the intensive margin, only the difference between matches and rebates at the high subsidy rate remains significant when covariates are included.

payment. Consequently, the minimum expense required to become a donor is larger than for the equivalent rebate while the impact of the action is the same: a single nutritional package received by the charity. As a result, not only the costs but also the effective prices at the margin of becoming a donor differ, further decreasing the relative attractiveness of the match.

One concern regarding the comparison of rebate and matching subsidies in our experiment might be that differences in the budget constraints for charity receipts could drive some of the results. Under a rebate, the highest possible number of packages received by the charity is always four, since the donor must fully fund each selected unit at a nominal price of \$0.5 before receiving the refund. In contrast, the matching subsidy applies on top of the selected packages: If under a 1:1 match a donor decides to spend her whole endowment of \$2 to fund four packages, then the charity receives eight packages.[17] Similar differences apply to almost all laboratory experiments comparing rebates and matches in the money donation literature, as they also endow subjects with a limited amount of money. In the money donation literature, it is shown that the higher effectiveness of matches observed in laboratory studies also holds in field experiments where subjects use their own income (Eckel and Grossman, 2008a, 2017). If the budget constraint mattered in our design, the results could understate the effectiveness of rebates compared to matches for situations in which the budget constraint is more loose or non-binding. This implies that rebates might be even more effective than matches in such situations.

To provide a robustness check on this matter, we revisit Result 1 and Result 5 by recoding subjects' choices in order to equalize budget constraints. In our data, a total of 29.5% of subjects give the maximum amount under rebates, compared to 10.9% in the matching conditions. For each condition, we set all charity receipts above four packages to four packages (the maximum level of charity receipts under rebates). Detailed results are presented in Table C.4 in Appendix C.2. Although rebates now provide the highest average number of packages received by the charity, the difference to matches is not significant at the high subsidy rate ($p = 0.65$) and only marginally significant at the low subsidy rate ($p = 0.09$). Hence, Result 1 survives the robustness check. In contrast, the difference on the intensive margin (Result 5) vanishes after censoring charity receipt at four packages. A possible explanation is that matches create larger conditional donations only in settings where the budget constraint is binding for a sufficiently large share of individuals.

[17] See Table C.3 in Appendix C.2 for the detailed choice set by treatment.

3.4.2 Discount Subsidies

Subsidies that put a simple price reduction on nominal prices turn out to be as effective as rebate and matching subsidies that produce equivalent effective prices. Charity receipts in column 3 and net donations in column 2 of Table 3.3 do not significantly differ from those under the other two subsidy types ($p \geq 0.44$ for each pairwise comparison). Hence, the increased salience of the effective price under discounts does not seem to affect demand, and this alternative subsidy type does not lend itself to a more effective subsidy. Instead, the selected number of units in column 1 under a discount is statistically indistinguishable from that selected under a rebate ($p = 0.44$ and $p = 0.57$) but by an amount higher than under the corresponding match ($p = 0.10$ and $p < 0.005$), which approximately makes up for the additional units provided as matching payment. In line with the law of demand, charity receipts increase in the subsidy level by applying the 50% discount instead of the no subsidy condition ($p < 0.01$) or the %33 discount ($p = 0.07$). There is again no evidence for crowding-in or -out. Our Ordered Probit estimates in Table 3.4, columns 1–2, confirm these findings.

Result 6. *(Discounts) The discount subsidy produces the same level of charity receipts and net donations as rebates and matches. Increasing the subsidy rate increases charity receipts, without crowding-in or crowding-out net donations.*

As in the previous section, we can differentiate the behavior into the extensive and the intensive margin. At the low subsidy rate, the likelihood to give under the discount is significantly lower than under the rebate ($p \leq 0.03$, column 5 of Table 3.3 and columns 3–4 of Table 3.4). Since we would expect the responses to rebates and discounts to be similar at the extensive margin, this difference may hint towards a behavioral bias in the response to an equivalent decrease in the cost of becoming a donor. At the intensive margin, there is only some marginally significant difference between discounts and rebates at the high subsidy rate, shown in column 4 of Table 3.3 and columns 5–6 of Table 3.4. In comparison to the matching subsidies, there are neither significant differences at the extensive nor at the intensive margin.

3.5 Discussion

In our experiment, we find equivalence between matches and rebates as subsidy-based incentives to donors and an equivalence with price discounts. This equivalence under a unit donation scheme contrasts with the existing literature that has examined subsidy types under a money donation scheme and has generally found matches outperforming rebates. This includes papers that also use an online experimental methodology (Bekkers, 2015; Gandullia and Lezzi, 2018; Gandullia, 2019) of which two recruit from a similar subject pool and use the same endowment level as we do (Gandullia and Lezzi, 2018; Gandullia, 2019). A closer parallel exists with experimental evidence comparing product promotions in the marketing literature. For private goods, matches (bonus packs) and price promotions sometimes perform equally well (Sinha and Smith, 2000; Hardesty and Bearden, 2003; Chen et al., 2012). For charitable goods, however, our finding is unusual.

To guide our intuition about this result, note that as discussed in Section 3.2.2, rebates and matches are no longer theoretically equivalent under a unit donation scheme. When donors face the choice architecture of a unit donation scheme, the minimum net expense required to become a donor is lower under rebates than under matches. This does not hold for a money donation scheme. As a result, the behavior on the extensive margin might be a crucial factor to explain why matches do not outperform rebates in our setting. In line with this reasoning, our results on the extensive margin differ from those obtained in the context of money donations schemes. While we find that rebates attract more donors than matches, Bekkers (2015) finds the opposite by using similar subsidy rates in a standard money donation choice architecture. Furthermore, Gandullia and Lezzi (2018) use the same online population, subsidy rates, and endowment levels as we do but focus on a standard money donation scheme. In their experiment, both rebates and matches increase the fraction of donors compared to a no-subsidy condition and effect sizes between the different subsidy types are similar.

Additional evidence that the lower cost of becoming a donor under rebates might drive the results comes from a simple recoding exercise. In our experiment, the minimum positive net donation under a match amounts to \$0.50. Under a 33% (50%) rebate, 25% (22%) of donors give less than \$0.50. Recoding those

subjects as non-donors eliminates any significant difference at the extensive margin, and matches now lead to higher charity receipts than rebates (1.506 vs. 1.437 units at the low subsidy rate and 2.225 vs. 1.707 units at the high subsidy rate), yet differences between the two subsidy types remain statistically insignificant ($p = 0.817$ and $p = 0.167$). Hence, equalizing the cost of becoming a donor ex post moves the subsidy comparison towards the standard result from money donations, namely that matches outperform rebates.

The importance of the extensive margin to explain our results is also in line with findings by Diederich et al. (2020). The authors show that using unit instead of money donation schemes affects the propensity to give, with the primary driver being the discrete choice set under unit donation schemes. This characteristic is also responsible for the different cost of becoming a donor between rebates and matches in our experiment. While the discreteness thus appears to be an important factor to explain our results, we cannot exclude the possibility that the other distinct characteristics of unit donation schemes discussed in Section 3.2.1 – i.e., the additional information on the effectiveness of a donation and the framing in terms of physical units – may also play a role.[18] Additional research is required to quantify the relative effects of the different characteristics on the responsiveness to subsidies. One possible approach would be to introduce one characteristic at a time, similar to Diederich et al. (2020), and investigate whether and how the effectiveness of certain subsidy types changes.

Regarding the comparison of the rebate and discount subsidies, the equivalence on the aggregate level accords with our expectations. However, our data offers some evidence that rebates are more effective in attracting donors. Although we should await future research to confirm the robustness of this finding, such a difference is surprising given that both subsidies imply the same price of becoming a donor. One speculative explanation is that a donor has the feeling of providing the whole unit of the charitable good herself under the rebate but only a fraction of the unit under the discount. In this case, the donor might derive lower warm glow utility from becoming a donor under the discount than under the rebate.

An interesting question for future research is how the effectiveness of the

[18] As pointed out by an anonymous referee, a potential explanation for a different response to rebates under a unit instead of a money donation scheme is fungibility: Due to the unit framing, the donation is in terms of physical units whereas the rebate is in terms of money, which might abate the feeling that money is taken away from the recipient.

different subsidy types under a unit donation scheme depends on the level of the unsubsidized unit price. At a given subsidy rate, a larger unsubsidized price increases the absolute difference in the minimum expense required to become a donor between rebates and matches. As a result, the differences on the extensive margin might become more pronounced, which in turn might be sufficient to make the rebate raise more money than the match. In contrast, reducing the unit size might move results closer to what has been found for money donation schemes.

Future research could also help to verify the generalizability of our results, for example, with respect to the absolute size of the earned endowment and the relative sizes of unit price and endowment. At \$2, the earned endowment is small in absolute terms, even though it is large in the context of the experimental population we recruit. Gandullia and Lezzi (2018) and Gandullia (2019) use the same endowment level with a similar subject pool and replicate the standard finding of matches outperforming rebates under a money donation scheme. These results hint at generalizability, but more research is needed for the specific case examined in the present paper. At \$0.50, the unsubsidized unit price of the charitable good is a quarter of the earned endowment and therefore restricts the room for variation in the donation decision, potentially limiting the scope for identifying differences. Although we still find significant differences between the subsidy and no subsidy conditions as well as between the subsidy types on the extensive and intensive margin, the equivalence of rebates and matches regarding charity receipts merits further examination.

3.6 Conclusion

In this paper, we define a class of donations in which donors are asked to choose the number of discrete units of the charitable good to fund instead of the amount of money to give. We call the former a unit donation and the latter a money donation. We present empirical evidence from an online field experiment designed to analyze how different subsidy types affect unit donations. In doing so, we focus on the two prevalent subsidy types, rebates and matches, as well as a subsidy type that is novel to charitable giving and framed as a simple price discount. The latter can be applied since for unit donations, each physical unit has a well-defined price that can be explicitly reduced.

The results remarkably differ from the well-established findings for money donations. Matching subsidies do not outperform rebates but are equally effective in raising funds. Yet matching and rebate subsidies create different responses at the extensive and intensive margin of giving. While rebates significantly increase the fraction of donors, matches produce larger donations. The significantly higher likelihood to give under rebates compared to matches is in contrast to the money donation literature and appears to be one reason why rebates catch up with matches in the unit donation setting of our experiment. Price discounts raise similar levels of funds as rebates and matches. None of the subsidy types significantly affects net donations.

Our results underline the relevance of the decision environment when soliciting donations and, thus, have important implications for practitioners. First, charities that employ unit donations in their fundraising efforts cannot rely on the insights from the existing literature on subsidizing money donations. Second, whether it is useful to apply a certain type of subsidy to unit donations depends on a charity's objectives. Previous research has shown that individuals that donated once are more likely to give in the future. If the charity desires to maximize the set of donors, our evidence suggests that a rebate is preferable over a match. If the charity instead seeks to maximize charity receipts, the choice of the subsidy type seems to be irrelevant, offering some additional degrees of freedom to charities in their campaign design. Third, in cases where funds are not tied to being used as a subsidy, subsidizing unit donations is not necessarily beneficial as on the aggregate it may not crowd in private giving.

Chapter 4

Do Beliefs About Lobbying Affect Pro-environmental Behavior? Experimental Evidence*

4.1 Introduction

Lobbying is an established part of the political process and builds on different strategies that are applied by interest groups to influence political decision-making. Previous research provides evidence that lobbying can be effective in exerting influence (e.g., Gawande et al., 2006; Markussen and Svendsen, 2005; Igan and Mishra, 2014; Giger and Klüver, 2016) and in securing substantial returns for the lobbying party (de Figueiredo and Silverman, 2006; Kang, 2016). Such achievements are paid for by the billions of dollars spent on lobbying activities (OECD, 2012). Given this evidence, it is not surprising that people's perception of lobbying is rather negative. An often held belief is that it negatively affects policy-making and primarily benefits businesses and politicians instead of citizens (Epperson et al., 2019).

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The goal of this paper is to investigate whether such a negative perception of lobbying has direct consequences for individual behavior. I focus on the context of environmental protection, where lobbying is often associated with the prevention of stricter regulations (Grey, 2018; Meng and Rode, 2019). On the one hand, a negative perception might discourage individuals from engaging in pro-environmental behavior. Experimental studies have found that a large share of individuals conditionally cooperate in public good games (e.g., Keser and Van Winden, 2000; Fischbacher et al., 2001; Croson and Shang, 2008; Fischbacher and Gächter, 2010), i.e., they contribute less if others contribute less. As environmental protection is a public good, this suggests that the more lobbying is expected to decrease the environmental contributions of other actors, the lower an individual's willingness to behave pro-environmentally. On the other hand, expecting lobbying to prevent environmental regulations could also increase individuals' contributions, e.g., if an individual tries to make up for what others fail to provide. This behavior could be rationalized with a marginal utility from contributing that is decreasing in the size of the public good.

Since it is unlikely that individuals are perfectly informed about the impact of lobbying, it is their *belief* about the impact and not the impact itself that is of primary importance for the behavioral response to lobbying. In general, beliefs are endogenous and are likely to be correlated with a large set of other factors that also affect individual behavior. I use an experimental approach to randomly shift beliefs about lobbying and identify their causal impact on pro-environmental behavior. The experiment is implemented in a survey with a heterogeneous sample from Germany. It mainly builds on the following two treatment groups. Participants in the first group (Group Positive) read three statements that suggest lobbying promotes climate protection, whereas participants in the second group (Group Negative) read three statements that suggest lobbying hinders climate protection. Afterward, participants answer questions about the impact of lobbying on climate protection and have the opportunity to contribute to the reduction of carbon emissions via a donation to the climate protection organization atmosfair. The donation decision serves as a measure of individuals' pro-environmental behavior. A follow-up survey about seven months after the experiment offers additional information on the persistence of beliefs and stated pro-environmental behaviors.

While the experiment successfully induces a gap in the beliefs of the two

groups, I find mixed evidence on whether this difference in beliefs affects pro-environmental behavior. Only the measure based on the stated behaviors shows a significant impact of being confronted with negative instead of positive statements, suggesting that negative beliefs reduce pro-environmental behavior. The estimated treatment effect on the observed contribution to the reduction of carbon emissions points in the same direction but is not significant at any conventional level.

This paper contributes to a large literature on the determinants of pro-environmental behavior and, more generally, the voluntary contribution to public goods. Most closely related is a strand of this literature that investigates the effect of others' contributions on own behavior. For example, Schultz et al. (2007) and Goldstein et al. (2008) show how providing information on the descriptive norm (i.e., what others do) induces individuals to adjust their conservation behavior (reusing towels in hotel rooms) towards the presented norm. Similarly, descriptive norms have been successfully used in fostering energy or water conservation (e.g., Allcott, 2011; Ferraro et al., 2011; Ferraro and Price, 2013; Allcott and Rogers, 2014).[1]

In the context of laboratory experiments, a common finding is that a substantial share of individuals conditionally cooperate in public good games (e.g., Keser and Van Winden, 2000; Fischbacher et al., 2001; Croson and Shang, 2008): Individuals give more if others give more. Some of these experiments analyze the role of beliefs in explaining observed behaviors (Fischbacher and Gächter, 2010; Smith, 2013). In this paper, the considered belief does not directly concern the contribution decision of other individuals but the impact of lobbying on climate protection. Nevertheless, the underlying mechanism might be similar: Expecting lobbying to decrease the level of climate protection (e.g., from firms, governments, or individuals) might discourage contributions on the individual level.

Fairness concerns could play a crucial role in such a behavioral response: An unfair distribution of costs has been found to be associated with a lower acceptance of environmental policies (Maestre-Andrés et al., 2019) and a reduction in the willingness to contribute (Andor et al., 2018). In particular, Andor et al. (2018) show that households' (hypothetical) willingness-to-pay for increasing the share of renewable energy is higher if energy-intensive companies are not exempt

[1] See Farrow et al. (2017) for a literature review on the impact of norms on pro-environmental behavior.

from bearing the costs.

My paper also extends the literature on the consequences of lobbying by investigating a potential spillover effect that has received little attention so far. Several studies investigate whether and how lobbying influences political decision-making (e.g., Igan and Mishra, 2014; Giger and Klüver, 2016; Bertrand et al., 2020; for a review, see de Figueiredo and Richter, 2014), including the context of environmental policy (Markussen and Svendsen, 2005; Gullberg, 2008; Kang, 2016; Meng and Rode, 2019). For example, Kang (2016) estimates that lobbying by the energy sector during the 110th Congress had a small but significant impact on the enactment of policies. However, little is known about whether – in addition to a direct impact of lobbying – the public perception of lobbying might cause behavioral adjustments on the individual level. This spillover can be a relevant factor in assessing the welfare effects of lobbying, particularly in the context of environmental protection, where massive action of all actors is needed to tackle global challenges.

The paper proceeds as follows. Section 4.2 describes the experiment and provides detailed information about the sample. Section 4.3 presents the results by first focusing on the effectiveness of the belief manipulation and subsequently turning to the impact on pro-environmental behavior. Section 4.4 concludes.

4.2 Experiment

4.2.1 The German Internet Panel

The experiment was implemented in the German Internet Panel (GIP), a probability-based online panel of the general population in Germany aged 16–75.[2] The panel includes both the online and the offline population by recruiting participants face-to-face and providing equipment to households that do not have access to the internet or a computer. Surveys are conducted on a bi-monthly basis and take 20 to 25 minutes. Participants usually receive a reward of 4 euro for each completed survey and an annual bonus of up to 10 euro if all surveys within a one-year period have been answered. A detailed description of the GIP, including the recruitment process, can be found in Blom et al. (2015).

[2]The German Internet Panel is funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) – Project-ID 139943784 – Collaborative Research Center 884 "Political Economy of Reforms" (SFB 884).

In this paper, I use data from the German Internet Panel waves 38, 41, 43, 44, and 48 (Blom et al., 2019, 2020a, 2020b, 2021a, 2021b). The main experiment was implemented in wave 44, which took place in November 2019. The data sets of wave 38 (November 2018), 41 (May 2019), and 43 (September 2019) are used to obtain baseline beliefs and sociodemographic characteristics, while wave 48 (July 2020) contains a follow-up survey, which is described in more detail in Section 4.2.3.

4.2.2 Experimental Design

The experiment in wave 44 consisted of three main parts. The goal of the first part was to shift individuals' beliefs about the impact of lobbying. After reading a short general introduction about the topic of lobbying, participants were randomly assigned to one of three experimental groups. The first group (Control) directly continued with the second part of the experiment described below. The second group (Group Positive) was confronted with three statements arguing that lobbying (i) improves environmental policy by providing crucial information, (ii) allows nonprofit organizations to achieve a stricter climate policy, and (iii) is sufficiently transparent. The third group (Group Negative) was confronted with three statements arguing that lobbying (i) prevents environmental regulations, (ii) leads to a climate policy in the interest of companies, and (iii) lacks transparency. The wording of these statements is presented in Appendix Table D.1. Participants were asked to read all presented statements and mark those they agree with. Afterward, they indicated how many of the statements they had heard of before. The instructions did not discuss whether the statements are true.

In the second part of the experiment, participants answered up to five questions about the impact of lobbying.[3] The last of these questions elicited the belief about the impact of lobbying on the level of climate protection and hence is the focus of this paper.[4] The translated question reads as follows:

"How does lobbying affect the level of climate protection in the European Union? Lobbying leads to ..."

[3]The number of questions varied between 4 and 5 due to the conditioning on previous answers.

[4]The other questions focused on the actual or potential impact of lobbying on the European Union's climate policy.

The answer categories were (2) "much more climate protection," (1) "rather more," (0) "neither more nor less," (−1) "rather less," and (−2) "much less climate protection." Alternatively, participants could select the answer "I don't know." The same question had been included in a survey one year before the experiment (wave 38, November 2018),[5] which allows me to control for baseline beliefs when investigating whether the treatment had the intended effect.

The final part of the experiment was separated from the belief elicitation by six unrelated questions and was restricted to participants from Group Positive and Group Negative. It informed participants that they receive an additional payment of 4 euro for answering the survey, with the opportunity to donate all or part of this extra money to the climate protection organization *atmosfair*. atmosfair allows individuals to reduce carbon emissions by donating to climate projects, which typically focus on renewable energies in third world countries.[6] Participants received information about the organization and entered the amount they would like to give on a separate screen.[7] For simplicity, only integer amounts between 0 euro and 4 euro were possible. Any money not donated to atmosfair was transferred onto the participants' study accounts and paid out together with the cumulative earnings since November 2019 in April 2020.[8] In line with previous research (Löschel et al., 2013; Diederich and Goeschl, 2014, 2017, 2018; Goeschl et al., 2020), the donation to reduce carbon emissions is used as a measure of pro-environmental, or more specifically, pro-climate behavior.

Participants of the GIP can generally choose from three different modes of payment for the cumulative earnings on their study account. The earnings are either (i) transferred to their bank account, (ii) provided in the form of an Amazon voucher, or (iii) donated in equal portions to three charities, i.e., the German Red Cross, SOS-Kinderdorf e.V., and the World Wildlife Fund For Nature (WWF). Participants have to make this decision when being recruited into the GIP but can change the mode of payment at any time.[9] As donating to WWF might be

[5]The wording of the question in wave 38 was the same except that the phrase "Lobbying leads to ..." was not included. This phrase was added in wave 44 to avoid any possible confusion about the interpretation of the answer categories.

[6]See https://www.atmosfair.de/en/.

[7]See Appendix Table D.2 for the detailed instructions.

[8]To eliminate the impact of time preference on the donation decision, donations to *atmosfair* were paid out in April 2020 as well.

[9]In October 2019 (the last payout of cumulative earnings before the experiment), 15 percent of GIP participants with a positive balance on their study account donated their earnings, 32 percent got an Amazon voucher, and 53 percent received a bank transfer.

a substitute for a contribution to climate protection via atmosfair,[10] I use the sum of money donated to atmosfair and WWF in April 2020 as an alternative measure of pro-environmental behavior. The data on the payouts of the cumulative earnings is only available via the On-Site Data Access (ODA) facilities of the GIP for data protection reasons.

4.2.3 Follow-Up

A follow-up survey was implemented in the GIP about seven months after the main experiment (wave 48, July 2020). The survey did not differ across treatments and served two main purposes. First, by including the belief elicitation question from wave 44, it allows me to test whether a potential difference in beliefs induced by the experiment persists over time. Second, it provides additional measures of pro-environmental behavior in a stated format. This is relevant as a donation to atmosfair does not capture all forms of pro-environmental behavior. The follow-up survey elicited how often individuals use a car in a typical week and whether they had conducted the following actions at least once in the past six months: (i) taking a plane, (ii) purchasing local, organic products, (iii) bringing their own bag for shopping, (iv) considering a product's sustainability in the purchasing decision, (v) donating to an environmental organization, (vi) volunteering for an environmental cause, (vii) protesting for more environmental or climate protection, and (viii) signing a petition for more environmental or climate protection. These questions were inspired by previous studies that rely on pro-environmental behaviors (e.g., Videras et al., 2012; Meyer, 2015; Binder et al., 2020).

Kormos and Gifford (2014) and Lange and Dewitte (2019) discuss the advantages and disadvantages of stated measures in the context of pro-environmental behavior. Major advantages are the low costs of data collection and the great flexibility, which makes it relatively easy to consider a wide range of behaviors. Major disadvantages are the risk of over-reporting and limited memory.[11] In

[10] Grieder et al. (2020), for example, use a donation to WWF as a proxy for pro-environmental behavior.

[11] Two other disadvantages frequently discussed are social desirability bias and the ambiguity of answer categories or questions. Empirical research has found only very limited support for the former (see, e.g., Milfont, 2009; Chao and Lam, 2011). To avoid the latter, the answer categories of the questions used in this paper have a simple yes-no format or refer to a precise unit of measurement (the number of days per week). Furthermore, each question asks about a

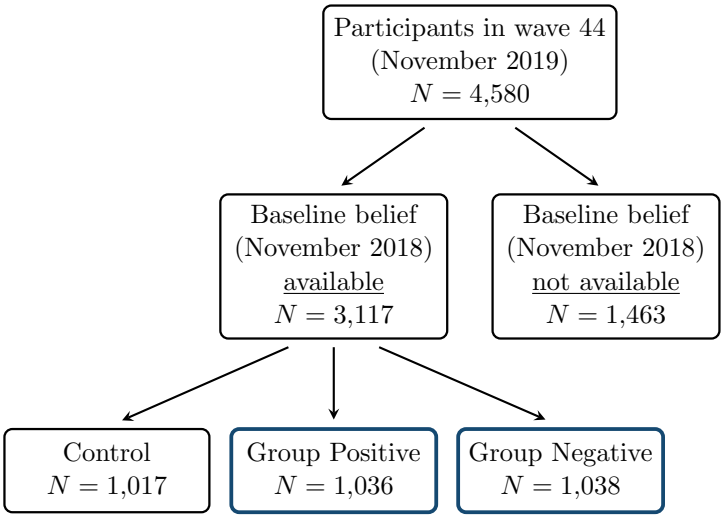


Figure 4.1: Treatment Assignment in Wave 44 (November 2019)

Notes: The figure illustrates the assignment of participants in wave 44. The numbers include incomplete survey responses which are subsequently removed. The final sample consists of 3,071 individuals (1,014 in Control, 1,031 in Group Positive, and 1,026 in Group Negative). Throughout the paper, I mainly focus on participants in Group Positive and Group Negative, who completed all three parts of the experiment described in Section 4.2.2.

their meta-analysis, Kormos and Gifford (2014) find a strong correlation between stated and observed pro-environmental behavior. However, the authors also emphasize that a large fraction of variance remains unexplained when using stated behavior as a predictor of observed behavior. Although this might be less of a concern when the main focus is on the difference between two groups rather than the level of pro-environmental behavior, the weaknesses of stated measures should be kept in mind when interpreting the results.

4.2.4 Sample

The sample for this survey experiment is a subset of the individuals who participated in wave 44 (November 2019) of the GIP. The assignment procedure was as follows (see Figure 4.1 for an illustration). Participants were eligible for the experiment if they indicated a well-defined belief about the impact of lobbying in wave 38 (November 2018). Hence, participants who did not complete the survey of wave 38 (909 individuals), did not answer the belief elicitation question in wave 38 (3 individuals), or selected the answer "I don't know" (551 individuals) were excluded from the survey experiment. Three thousand ninety-one from the

specific behavior.

3,117 eligible participants reached the first part of the survey experiment and were, with equal probabilities, assigned either to Control, Group Positive, or Group Negative. The randomization was stratified by the answer to the belief elicitation question in wave 38.[12] Twenty participants (3 out of 1,017 in Control, 5 out of 1,036 in Group Positive, and 12 out of 1,038 in Group Negative) are subsequently removed since they did not complete the survey or did not receive the belief elicitation question due to a technical problem. Hence, the final sample of the survey experiment consists of 3,071 individuals from the general population of Germany. About 91 percent of these individuals also provide complete information on their pro-environmental behaviors and beliefs in the follow-up survey. The attrition does not significantly differ across the experimental groups (χ^2-test, $p = 0.67$).

Throughout the paper, I mainly focus on observations from Group Positive and Group Negative since participants who were assigned to the control group did not have the opportunity to donate money to atmosfair. I will only use these observations to learn more about how beliefs adjusted in response to the positive and negative statements compared to not reading any statement.

Table 4.1 presents summary statistics by experimental group and p-values from testing the balance of each covariate (see Appendix Table D.3 for a detailed description of each variable used throughout this study). Overall, the covariates are well-balanced, including the distribution of baseline beliefs (Belef 2018). The only significant differences are in the percentage of females ($p = 0.02$), full-time employed individuals ($p = 0.03$) and participants with an income below 1000 euro ($p = 0.09$). Although the experimental design prevents systematic selection and hence the differences are by chance, I will show that controlling for covariates does not affect the results. An interesting insight from Table 4.1 is that at baseline, the majority tends to believe that lobbying decreases the level of climate protection.

[12] A participant was assigned to the experimental group that, at the time of the assignment, had the fewest individuals with the same baseline belief as the participant. In case of a tie, the assignment was randomized with equal probabilities for the respective groups. Note that for technical reasons, the number of individuals with a given baseline belief still differs by more than one across the different groups. Such inaccuracies could occur, for example, when several individuals were assigned at the same time.

Table 4.1: Summary Statistics

Variable	Control	Group Positive	Group Negative	F-test p-value
Belief 2018				
Much more climate protection	0.01	0.02	0.01	0.98
	(0.12)	(0.12)	(0.12)	
Rather more	0.25	0.25	0.25	0.99
	(0.43)	(0.43)	(0.43)	
Neither more nor less	0.17	0.16	0.16	0.97
	(0.37)	(0.37)	(0.37)	
Rather less	0.35	0.35	0.36	0.97
	(0.48)	(0.48)	(0.48)	
Much less climate protection	0.22	0.22	0.22	0.94
	(0.41)	(0.42)	(0.41)	
Female	0.42	0.48	0.44	0.02
	(0.49)	(0.50)	(0.50)	
Age[a]	51.35	52.32	51.82	0.37
	(15.14)	(15.83)	(15.31)	
Married[a]	0.61	0.64	0.62	0.42
	(0.49)	(0.48)	(0.48)	
Employment				
Full time	0.47	0.43	0.48	0.03
	(0.50)	(0.50)	(0.50)	
Part time	0.14	0.14	0.12	0.54
	(0.34)	(0.35)	(0.33)	
Retired	0.24	0.25	0.23	0.54
	(0.43)	(0.44)	(0.42)	
Other	0.16	0.18	0.16	0.36
	(0.36)	(0.38)	(0.36)	
Internet usage[a]	0.91	0.91	0.91	0.90
	(0.28)	(0.28)	(0.29)	
Recruited 2018	0.41	0.42	0.43	0.84
	(0.49)	(0.49)	(0.49)	
Payment mode (October 2019)[a]				
Donation	0.13	0.14	0.14	0.60
	(0.33)	(0.34)	(0.35)	
Amazon voucher	0.31	0.33	0.30	0.45
	(0.46)	(0.47)	(0.46)	
Bank transfer	0.56	0.54	0.55	0.49
	(0.50)	(0.50)	(0.50)	
Pro-environmental[a]	0.24	0.24	0.24	0.90
	(0.43)	(0.42)	(0.42)	
Income[a]				
$<$ €1000	0.16	0.19	0.15	0.09
	(0.37)	(0.39)	(0.36)	
[€1000, €2000)	0.31	0.32	0.32	0.68
	(0.46)	(0.47)	(0.47)	
[€2000, €3000)	0.28	0.26	0.29	0.35
	(0.45)	(0.44)	(0.45)	
$\geq$ €3000	0.25	0.23	0.25	0.54
	(0.43)	(0.42)	(0.43)	
Observations	1014	1031	1026	

Notes: The table reports the averages by treatment group with standard deviations in parentheses. The last column presents p-values from regressing each variable on treatment dummies and conducting an F-test for the joint significance of the regressors.
[a]Not available for all participants. In particular, the number of missing observations amounts to 4 (age), 1 (married), 8 (internet usage), 16 (payment mode), 132 (pro-environmental), and 479 (income).

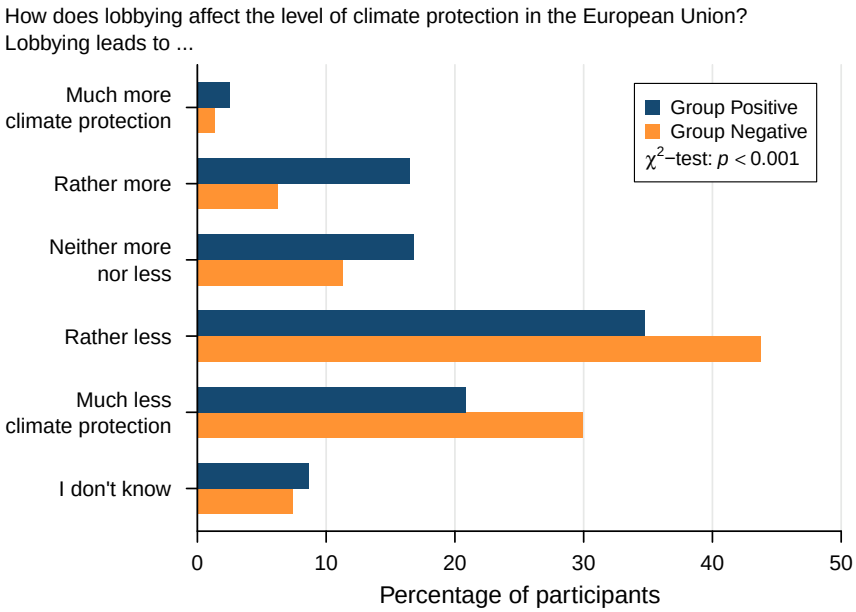


Figure 4.2: Distribution of Beliefs in 2019

Notes: The figure shows the distribution of responses to the belief elicitation question in the survey experiment (November 2019) by treatment group. The (translated) wording of the belief elicitation question is presented at the top of the figure. A χ^2-test confirms that the distributions of the responses significantly differ between the two treatment groups (p-value < 0.001).

4.3 Results

4.3.1 Beliefs

As intended, the experiment induced a substantial gap in beliefs. In particular, participants in Group Negative assessed the impact of lobbying on climate protection more negatively[13] than participants in Group Positive. Figure 4.2 presents the distributions of beliefs elicited as part of the survey experiment (November 2019) for each of the two treatment groups. In both groups, the majority believed that lobbying has a negative impact. However, this is much more pronounced in Group Negative (74 percent) than in Group Positive (55 percent). The percentage of participants who expected lobbying to increase climate protection is instead larger in Group Positive (19 percent) than in Group Negative (8 percent). A χ^2-test confirms that the distributions of beliefs significantly differ between the two treatment groups (p-value < 0.001).

[13]Throughout the paper, the terms "negative" and "positive" refer to the direction of the impact of lobbying on climate protection when used in this context. They do not express whether an individual thinks that a certain impact is desirable or not.

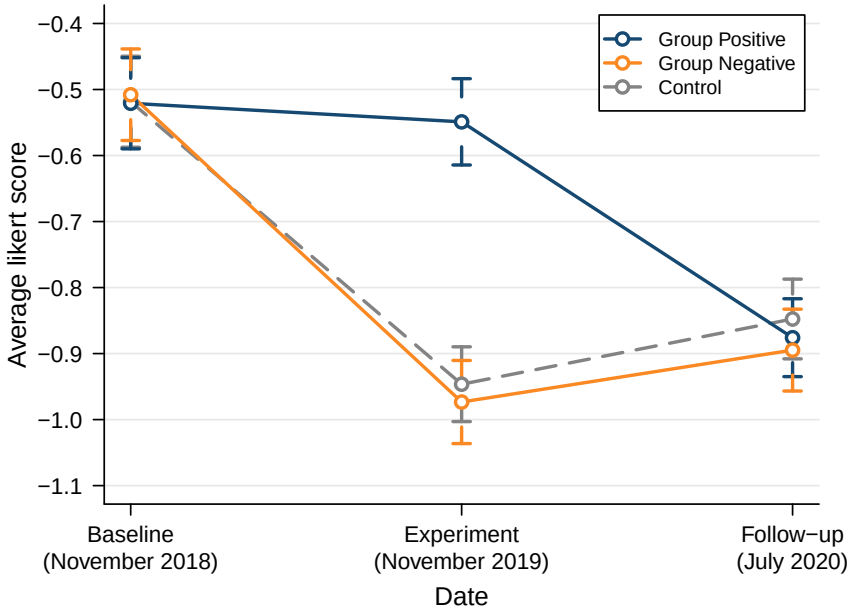


Figure 4.3: Beliefs over Time

Notes: The figure shows the average likert score for the response to the belief elicitation question by experimental group over time, with the likert scale ranging from −2 (lobbying leads to much less climate protection) to 2 (lobbying leads to much more climate protection). The response "I don't know" is treated as neither expecting a positive nor a negative impact. The error bars represent 95 percent confidence intervals.

Figure 4.3 visualizes the beliefs across the two treatment groups over time and in comparison to the control group. For this purpose, I treat the response "I don't know" as neither expecting a positive nor a negative impact and report the average likert score for each experimental group. This procedure simplifies the presentation of the data but does not affect the conclusions (see Figure D.1 and Figure D.2 in the Appendix for the comparison of the full distributions). In line with Figure 4.2, the average likert score from the response to the belief elicitation question in the survey experiment (November 2019) is significantly higher for individuals in Group Positive than in Group Negative. When we compare these averages to the control group, we observe that reading positive statements significantly increased the average likert score compared to not receiving any statement ($p < 0.001$, t-test). In contrast, the average likert score is not significantly different between Group Negative and the control group ($p = 0.53$, t-test). This asymmetric belief adjustment might be explained by the fact that participants were less familiar with the positive than with the negative statements ($p < 0.001$, t-test). In particular, participants in Group Negative were familiar with on average 2.1 of the three presented statements, while in Group

Positive the average only amounts to 1.5 statements.

In contrast to the strong short-term impact of the belief manipulation, beliefs elicited in the follow-up survey seven months after the experiment do not significantly differ across the experimental groups. Hence, the belief manipulation did not create a persistent effect.

The successful manipulation of beliefs in the short term and the lack of persistence are robust to controlling for available covariates (see Appendix Table D.4 for estimation results from ordered logit regressions). Furthermore, Appendix Figure D.3 illustrates that the impact of the belief manipulation is similar across the different baseline beliefs.

4.3.2 Pro-environmental Behavior

Since the treatment affected beliefs, the next question is whether the exogenous shift in beliefs translated into differences in participants' pro-environmental behavior. I first focus on the main outcome variable, i.e., the contribution to the reduction of carbon emissions via atmosfair. This contribution decision is arguably the most reliable measure available as it constitutes an observed behavior at the time when beliefs differed. Afterward, I turn to the other available outcome variables, which mostly consist of stated behaviors (see Appendix Table D.5 for an overview of the outcome variables and their summary statistics by treatment group). Since these variables concern behaviors in the months after the experimental intervention, the lack of persistence in the belief manipulation might limit the scope to identify differences, particularly if beliefs across the treatment groups converged quickly.

In the survey experiment, about 66 percent of participants contributed to the climate protection organization atmosfair, and the average contribution over all participants was 2.35 euro. In general, the correlation between contributions and individuals' baseline beliefs about the impact of lobbying on climate protection is significantly negative ($r = -0.12$). However, the correlation might be misleading as it does not account for factors that jointly determine beliefs and pro-environmental behavior.

Figure 4.4 shows the distribution of contributions to reducing carbon emissions by treatment group. In each group, more than 50 percent of the participants contributed the maximum amount of 4 euro, and another large percentage

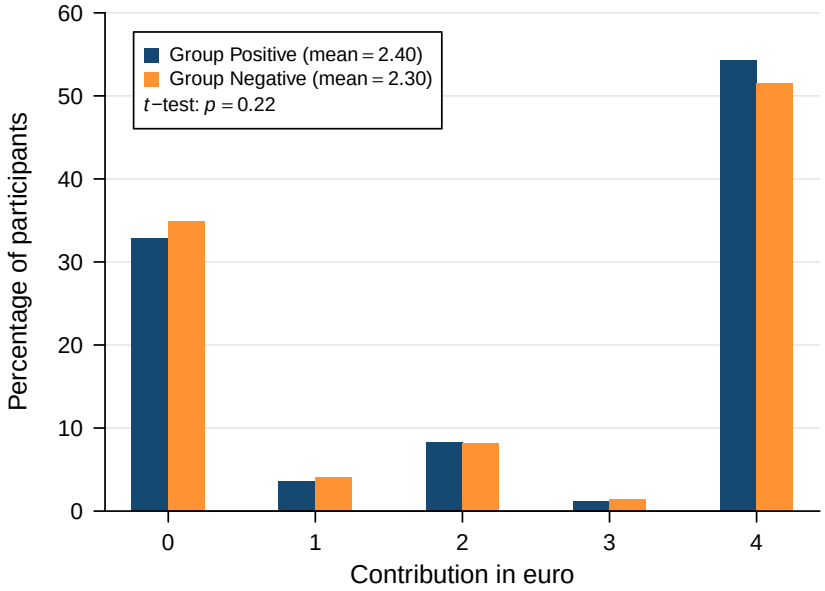


Figure 4.4: Contributions to CO_2 Reduction via atmosfair

Notes: The figure shows the percentage of subjects who chose a particular contribution to atmosfair in the survey experiment (November 2019) by treatment group. The average contribution does not significantly differ across the two treatment groups ($p = 0.22$, t-test).

of more than 30 percent did not contribute at all. Less extreme contributions between 1 and 3 euro are rare. The average contribution amounts to 2.40 euro in Group Positive and 2.30 euro in Group Negative. Although individuals in Group Negative gave slightly less, the difference between the two groups is not significantly different ($p = 0.22$, t-test).

Table 4.2 reports estimation results from regressing individual contributions on a dummy for whether the participant was assigned to Group Negative and different sets of covariates. The estimated coefficient of receiving negative (instead of positive) statements is always slightly negative but never significantly different from zero. Hence, based on the main outcome variable, there is little evidence that the exogenous shift in beliefs affected individuals' pro-environmental behavior.

Next, I consider the other measures of pro-environmental behavior, starting with the sum of donations to environmental charities in the context of the GIP. This outcome variable considers the contribution to the reduction of carbon emissions via atmosfair and the money transferred to WWF (which is one-third of the participant's cumulative earnings if the participants decided to donate the cumulative earnings since November 2019 in April 2020). The average donation

Table 4.2: Estimation Results – Pro-environmental Behavior

	Contribution		
Variable	(1)	(2)	(3)
Group Negative	-0.10	-0.09	-0.09
	(0.08)	(0.08)	(0.09)
Belief 2018	Yes	Yes	Yes
Female		Yes	Yes
Age		Yes	Yes
Married		Yes	Yes
Employment		Yes	Yes
Internet usage		Yes	Yes
Recruited 2018		Yes	Yes
Payment mode		Yes	Yes
Income			Yes
Pro-environmental			Yes
Observations	2057	2042	1687

Notes: Robust standard errors are in parentheses. Each column corresponds to a separate OLS regression with the contribution to the reduction of carbon emissions via atmosfair as dependent variable.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

to both charities amounts to 3.05 euro in Group Negative and 3.06 euro in Group Positive. Hence, the absolute difference in the behavior between the two groups becomes smaller and remains insignificant after accounting for a potential substitution between the donation to atmosfair and the donation to WWF. For both outcome variables, the standardized effect of being assigned to Group Negative is illustrated in the upper part of Figure 4.5.

The lower part of Figure 4.5 shows the standardized effects for the stated pro-environmental behaviors. Since I measure a wide range of behaviors, I create an index from the separate measures following the procedure proposed by Kling et al. (2007). In particular, I normalize each variable such that it has a mean of zero and a standard deviation of 1 in Group Positive. Afterward, I code each variable such that a higher value represents more pro-environmental behavior and calculate the average of the normalized variables for each individual to obtain the stated behavior index. The major advantage of using the index is that it increases the power to detect differences if each outcome variable is affected in the same direction (Kling et al., 2007).

As shown in Figure 4.5, negative beliefs (caused by reading negative statements) significantly reduced the stated behavior index by about 0.13 standard

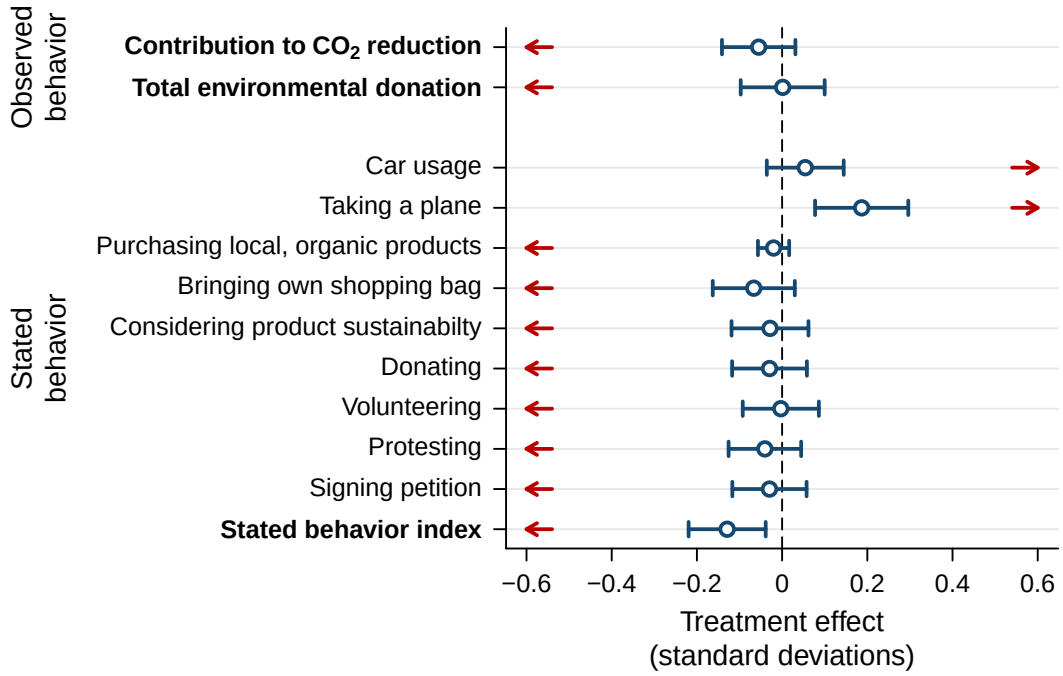


Figure 4.5: Standardized Treatment Effects

Notes: The figure shows the standardized treatment effects of being assigned to Group Negative instead of Group Positive for different outcome variables. The effects are standardized by the standard deviation of the respective outcome variable in Group Positive and are obtained from regressing the outcome variable on treatment assignment and indicator variables to control for baseline beliefs. The error bars represent 95 percent confidence intervals (based on heteroskedasticity robust standard errors). The red arrows indicate the directions that represent less pro-environmental behavior.

deviations, suggesting that a more negative belief leads to less pro-environmental behavior. A look at the separate behaviors included in the index reveals that the effect is substantially driven by a large and significant standardized effect on the likelihood of taking a plane. In non-standardized terms, the treatment is estimated to double the likelihood of taking a plane from 3.5 to 7 percent. However, all stated behaviors point into the same direction (i.e., less pro-environmental behavior in Group Negative). The effect on the stated behavior index and the effect on taking a plane remain significant after controlling for the number of considered outcome variables in Figure 4.5 via the Holm multiple hypothesis correction (Holm, 1979). In particular, the corrected p-values amount to 0.06 and 0.01, respectively. Hence, in contrast to the observed behavior, I do find evidence of a causal impact of beliefs about lobbying on stated pro-environmental behavior.

4.4 Concluding Remarks

In this paper, I have investigated whether individuals' beliefs about the impact of lobbying on climate protection affect pro-environmental behavior. To identify the causal effect of beliefs, I have employed a survey experiment with a large heterogeneous sample from Germany. In the experiment, individuals were randomly induced to expect a more negative or more positive impact of lobbying on the level of climate protection.

The results offer some evidence of a causal impact of beliefs on pro-environmental behavior. Individuals who received a negative (instead of a positive) shock to their beliefs reported significantly less pro-environmental behavior. However, the significance is mainly driven by a single behavior (taking a plane). Furthermore, I do not find an effect on observed pro-environmental behavior, i.e., the contribution to the reduction of carbon emissions: The corresponding estimate points in the same direction but is insignificant.

One concern might be that the difference in beliefs caused by the experimental treatments was insufficient to trigger a strong behavioral response. Figure 4.3 reveals that the difference in beliefs between Group Positive and Group Negative after the treamtent is similar to the shift in beliefs that naturally occurred in the control group within the course of one year (November 2018 until November 2019). In this regard, the manipulation appears to be well suited to assess the consequence of a realistic belief change. Nevertheless, it is possible that a more extreme difference in beliefs would have resulted in a significant effect even for the observed pro-environmental behavior.

The above considerations suggest that additional research is needed before concluding about whether the perception of lobbying affects individual behavior. If a causal link exists, the question of why individuals hold such negative beliefs gains importance. The experimental data shows that in November 2018, about 57 percent of the participants in the control group believed that lobbying decreases the level of climate protection in the European Union, and this share has increased over time (in the absence of any belief manipulation). If there is a lack of information that leads to overly pessimistic beliefs, increasing the transparency of lobbying activities could help correct beliefs and mitigate adverse spillover effects. For example, the European Parliament, Council, and Commission have recently agreed upon establishing a reinforced Transparency

Register in order to "strengthen transparent and ethical interest representation" (European Comission, 2020).

Appendices

A Appendix to Chapter 1

A.1 Mapping of Paper and Pre-analysis Plan

We conducted the experiment as described in our pre-analysis plan. As the corona pandemic prevented us from running some planned experimental sessions, our sample size of 330 subjects is smaller than the planned sample of 500 subjects. Similarly, the observation periods used in our main specification (August 1, 2019, until March 16, 2020, in Mannheim and October 1, 2019, until March 19, 2020, in Bonn) are shorter than pre-specified due to the shut down of the university canteens to fight the spread of the coronavirus (see Section 1.2.4 for details). Table A.1 maps the hypotheses in our pre-analysis plan to the analyses in the paper.[14]

[14]Note that in the pre-analysis plan, we used the term *meat* to refer to the meat from terrestrial animals.

Table A.1: Mapping of Paper and Pre-analysis Plan

Description	Pre-analysis plan	Paper
Info avoidance	Hypothesis 3	Results are presented in Section 1.4.1 of the paper.
Average info effect in lab	Hypothesis 1a	Results are presented in Section 1.4.2 of the paper.
Average info effect in field	Hypothesis 1b	We pre-specified to test this hypothesis with the propensity to eat meat as well as the propensity to eat meat from terrestrial animals as outcome variable. While the former was planned as robustness check, we use it in our main analysis as it is directly comparable to the outcome variable in the lab. Results for the propensity to eat meat are presented in Section 1.4.3. Results for the propensity to eat meat from terrestrial animals are presented in Table A.22 (Appendix A.8) and discussed in Section 1.4.5. In cases where the data is insufficient to determine whether a meal contains meat for sure, we planned to code it as containing meat if it usually does. This turned out to be difficult, however, as ambiguous food items were almost always offered with and without meat. Therefore, we consider two outcome variables: the share of meals that certainly contain meat and the share of meals that certainly do not contain meat.
Average info effect for avoiders in lab	Hypothesis 4a	Results are presented in Section 1.4.4 of the paper.
Average info effect for avoiders in field	Hypothesis 4b	We code outcome variables as for Hypothesis 1b. Results for the propensity to eat meat are presented in Section 1.4.4. Results for the propensity to eat meat from terrestrial animals are presented in Table A.29 (Appendix A.8).
Heterogeneous info effect by info demand in lab	Hypothesis 2a	We pre-specified to test the heterogeneity of the info effect by splitting subjects into two groups based on the median of the WTP distribution. The median WTP interval is $[0, 0.5)$ euro. We planned to allocate subjects with the median WTP such that the groups are as balanced as possible in terms of sample size. This implies a split of subjects into those with WTP < 0.5 (67 percent) and those with WTP ≥ 0.5 (33 percent). The alternative allocation of subjects with the median WTP leads to the split into information avoiders (WTP < 0, 30 percent) and seekers (WTP ≥ 0, 70 percent). Since the alternative split has almost the same balance but a clearer interpretation (avoiders vs. seekers), we use it in our main analysis (see Section 1.4.4). Results for the other split are presented in Table A.27 (Appendix A.8).
Heterogeneous info effect by info demand in field	Hypothesis 2b	We code outcome variables as for Hypothesis 1b and split subjects as for Hypothesis 2a. Results for the comparison of the effect for information avoiders and seekers using the propensity to eat meat as outcome variable are presented in Section 1.4.4. Results for the propensity to eat meat from terrestrial animals as outcome variable are presented in Table A.29 (Appendix A.8) and those for the alternative split in Table A.28 (Appendix A.8).

A.2 Theoretical Predictions about Heterogeneity

A variety of theoretical models are able to explain information avoidance in a moral context (see, e.g., Rabin, 1995; Feiler, 2014; Grossman and van der Weele, 2017; Hestermann et al., 2020; Serra-Garcia and Szech, 2020). In the following, we use two examples, Hestermann et al. (2020) and Grossman and van der Weele (2017), to illustrate that these models make ambiguous predictions about treatment effect heterogeneity. In particular, we illustrate that the effect of information on individuals who avoid costless information (information avoiders) and those who do not avoid costless information (information seekers) depends on the model parameters. Similar ambiguity can also be shown for other theoretical models (Rabin, 1995; Feiler, 2014; Serra-Garcia and Szech, 2020).[15]

A.2.1 Self-Deception Model by Hestermann et al. (2020)

We start with the theoretical model by Hestermann et al. (2020), who explicitly focus on the context of meat consumption. In their model, an individual has two selves: Self 0 can obtain an information signal about the state of the world. The information signal is either *bad news* and suggests that animals in the meat production suffer much (x_H) or *good news* suggesting that animals suffer little (x_L). After receiving the information signal, Self 0 decides what information to transmit to Self 1. Based on the transmitted information, Self 1 forms a belief $\tilde{x}$ about the true state of the world and chooses a level of meat consumption c that maximizes the utility function

$$\max_{c \in \mathbb{R}_+} U(c) - (p + w\tilde{x})c,$$

where $U(\cdot)$ represents the taste for meat, p is the price of meat, and w is the individual's feeling of empathy towards animals. In general, an individual faces the tradeoff between consuming meat and not harming animals. The key feature of the model is to allow for self-deception. In particular, Self 0's utility from meat consumption depends on Self 1's belief and can be expressed as $U(c^*(\tilde{x})) - (p + w\tilde{x})c^*(\tilde{x})$, where $c^*(\tilde{x})$ denotes the optimal consumption level of Self 1 given

[15] In the case of the basic model by Rabin (1995) and a binary perfect information signal, avoiders always respond to the information that the action creates social harm and seekers either respond in the same way or less (in absolute terms). Under the assumption of fixed-belief behaviorally equivalent utility functions (see Definition 3 in Rabin, 1995), information avoiders and seekers react in exactly the same way to receiving information.

belief $\tilde{x}$. As a result, Self 0 does not necessarily transmit the actual information received but might distort the information to lower the moral costs from meat consumption. However, distorting information creates some cognitive cost, k, and Self 1 takes into account Self 0's tendency to engage in self-deception when forming beliefs (see equations 4 and 5 in Hestermann et al., 2020). The avoidance of information allows Self 0 to keep Self 1 uninformed without engaging in costly self-deception.

In the following, we investigate how information avoiders and information seekers react to receiving bad news, i.e., the information that animals suffer.[16] We use examples of different parameter values to show that the model does not provide unanimous conclusions about treatment effect heterogeneity by information demand. In particular, we show that information avoiders have larger treatment effects than information seekers for some parameter values, while the reverse holds true for other parameter values.

Assume $U(c) = \sqrt{c}$, $p = 0.1$, $x_L = 0$ and $x_H = 0.9$. Under these assumptions, an individual with a moderate empathy towards animals ($w = 0.3$) and a high cost of self-deception ($k = 0.8$) engages in partial self-deception. Nevertheless, one can show that the individual prefers to receive costless information over not receiving it and can hence be classified as an information seeker. Bad news decrease the individual's meat consumption compared to the case without information (by about 0.3 units).

Compare this to an individual with the same cost of self-deception ($k = 0.8$) but a lower empathy towards animals ($w = 0.1$). This individual also engages in partial self-deception (with a slightly lower tendency of being self-deceptive). However, the individual does not prefer to receive costless information over not receiving it and is thus an information avoider. Comparing the consumption level without information to the consumption level under bad news suggests a negative effect of information on consumption (by about 2.2 units). Hence, in this example, the information avoider responds more strongly to information than the information seeker.[17]

[16] In our experiment, bad news correspond to watching the video about intensive farming, which induced subjects to adjust their beliefs towards worse living conditions of pigs on average (see Section A.4).

[17] At first sight, it might be counterintuitive that an individual with a lower empathy towards animals is more responsive to information on animal welfare. The main reason is that a lower empathy leads to generally higher levels of meat consumption. Given the concavity of the utility function, this can result in a larger absolute change in meat consumption when beliefs

However, for other parameter values, information avoiders are less responsive to information than information seekers. Consider an individual with a low empathy towards animals ($w = 0.1$) and a low cost of self-deception ($k = 0.1$). The equilibrium strategy of this individual is to engage in full self-deception. The individual also avoids costless information about the true state of the world as this allows to stay uninformed without incurring the cost of self-deception. Since the individual engages in full self-deception, receiving bad news does not affect beliefs and thus does not have any impact on meat consumption.

A.2.2 Self-Image Model by Grossman and van der Weele (2017)

Another model of information avoidance is proposed by Grossman and van der Weele (2017). In their model, individuals avoid information to protect their self-image despite behaving selfishly. Individuals have again two selves: A "decision maker self" decides whether to receive information about the state of the world and whether to behave prosocially. Depending on the state of the world, taking a costly action a improves social welfare or not. An "observer self" observes the equilibrium choices under the endogenous information state and uses them to infer the social preferences of the decision self. The utility function maximized by the decision self is given by

$$u(\theta, a, I, \sigma) = a(\theta E[W|\sigma] - c) - kI + \mu E[\theta|\sigma, a],$$

where a is the binary choice to behave prosocially or not, $\theta E[W|\sigma]$ and c are the expected benefit and the cost of behaving prosocially, respectively, μ is the self-image concern, and $E[\theta|\sigma, a]$ is the expected social preference based on the chosen actions. In our setting, the preference parameter θ could represent the empathy towards animals, $E[W|\sigma]$ could capture the expected increase in animal welfare from not eating meat, a the decision to eat meat and c the utility from meat consumption. In the following, we use the terminology of the original model.

The authors assume that individuals differ in their preferences as follows. A fraction ε of the population does not have social preferences ($\theta = 0$) and does not care about self-image ($\mu = 0$). These *nonsocial* individuals will attend to costless information and behave selfishly if $c \geq 0$. All other individuals care about their self-image ($\mu > 0$) and have a social preference θ which is drawn

change.

from a distribution with positive support on $[0, 1]$. For individuals with a low θ, it can be attractive to avoid information as it allows them to differentiate themselves from the nonsocial individuals without choosing the prosocial action.

The authors show the existence of a semiseparating equilibrium with $k = 0$ in which (i) nonsocial types attend to information and choose $a = 0$, (ii) individuals with a social preference parameter below some cutoff θ^* avoid information and behave selfishly, and (iii) individuals with a social preference parameter above the cutoff attend to information and choose the prosocial action if it improves social welfare. Several parameter combinations are possible to establish this equilibrium. In the following, we show that within this set of possible parameter combinations the prediction on how information seekers (which includes nonsocial agents) and information avoiders react to information is ambiguous.

To identify the information effect, we compare the behavior of each type if no one receives information and if everyone receives *bad news* in the sense that a costly action a improves welfare. We focus on semiseparating equilibria with some cutoff value θ^*, where all individuals with θ equal or above the cutoff choose $a = 1$ and all individuals with θ below the cutoff choose $a = 0$. Since the self-image from an action does not only depend on the own action but also on the behavior of all others, we extend the simulation code published by Grossman and van der Weele (2017) to calculate the heterogeneous information effects under different parameter values.

As a starting point, we use the parameter values introduced in Section 5.3 of the paper by Grossman and van der Weele (2017), i.e., $\varepsilon = 0.1$, $p = 0.5$, $k = 0$, $w = 1.65$, $\mu = 0.95$, $c = 1$, θ has a truncated normal distribution on $[0, 1]$ with a mean of 0.5 and standard deviation of 0.1. Under these assumptions, receiving bad news reduces the share of selfish behavior by 0.46 percentage points for information avoiders and 0.82 percentage points for information seekers. If we instead assume a slightly larger share of nonsocial agents ($\varepsilon = 0.2$), bad news reduce the share of selfish behavior by 0.88 percentage points for information avoiders and only 0.61 for information seekers. The effect sizes change mainly because the nonsocial agents do not react to information and now make up a larger share of the information seekers. Furthermore, the larger share of nonsocial agents increases the self-image cost of choosing the selfish action under full information and thus lowers the cutoff value of θ at which a social agent will choose the prosocial action when informed about a welfare impact.

It is also possible to select parameter values such that information avoiders are predicted to not react to bad news at all (initial parameter values but with $c = 1.25$). In this case, the costs of behaving prosocially are too high for information avoiders. Hence, they all prefer a lower self-image from behaving selfishly under full information instead of taking the costly prosocial action.

A.3 Estimation Details

A.3.1 AIPW Estimator

Let T_i denote whether individual i receives treatment ($T_i = 1$) or not ($T_i = 0$). The AIPW estimator can be expressed as:

$$\begin{aligned}\hat{\tau}_{AIPW} = \frac{1}{N}\sum_{i=1}^{N}\frac{T_iY_i - (T_i - \hat{p}(WTP_i))m_1(\boldsymbol{X_i}, \hat{\boldsymbol{\beta}}_{\mathbf{1}})}{\hat{p}(WTP_i)} \\ - \frac{1}{N}\sum_{i=1}^{N}\frac{(1-T_i)Y_i + (T_i - \hat{p}(WTP_i))m_0(\boldsymbol{X_i}, \hat{\boldsymbol{\beta}}_{\mathbf{0}})}{1-\hat{p}(WTP_i)},\end{aligned} \tag{A.1}$$

where WTP_i is the WTP of the individual, Y_i is the individual's propensity to eat meat, and $\hat{p}(.)$ is the estimated propensity of being treated which we know only depends on the WTP. $m_0(\boldsymbol{X_i}, \hat{\boldsymbol{\beta}}_{\mathbf{0}})$ and $m_1(\boldsymbol{X_i}, \hat{\boldsymbol{\beta}}_{\mathbf{1}})$ are the predicted levels of the outcome variable for being in the control group and being treated, respectively. When the outcome variable is binary or fractional, we use a logit model to obtain predicted outcome levels. Otherwise, we apply OLS. The parameters $\hat{\boldsymbol{\beta}}_{\boldsymbol{t}}$ are estimated based on the data from subjects with $T = t$. The "augmentation" of the inverse probability weighting estimator makes the AIPW estimator doubly robust (Lunceford and Davidian, 2004), which means that it is consistent if at least the propensity score model or the outcome models are correctly specified.

A.3.2 IPW Estimator Based on True Propensity Scores

The IPW estimator based on the true propensity scores can be expressed as

$$\begin{aligned}\hat{\tau}_{IPW} &= \Big(\sum_{i=1}^{N}\frac{T_i}{p(WTP_i)}\Big)^{-1}\sum_{i=1}^{N}\frac{T_iY_i}{p(WTP_i)} \\ &\quad - \Big(\sum_{i=1}^{N}\frac{1-T_i}{1-p(WTP_i)}\Big)^{-1}\sum_{i=1}^{N}\frac{(1-T_i)Y_i}{1-p(WTP_i)} \\ &= \hat{\mu}_{T=1} - \hat{\mu}_{T=0},\end{aligned} \tag{A.2}$$

where $p(.)$ is the true propensity of being treated that follows directly from the experimental design. In contrast to the estimator presented in equation 1.1, we replace the factor $\frac{1}{N}$ by the terms $\Big(\sum_{i=1}^{N}\frac{T_i}{p(WTP_i)}\Big)^{-1}$ and $\Big(\sum_{i=1}^{N}\frac{1-T_i}{1-p(WTP_i)}\Big)^{-1}$ to normalize the weights, which ensures that they add up to one. When using our

empirical propensity score estimates, this normalization becomes redundant.

A.4 Beliefs

We elicited beliefs about the living conditions of pigs in intensive farming as part of the questionnaire before subjects completed the multiple price list. The answer categories range from (1) *very good* to (5) *very bad*. On average, living conditions were expected to be bad with a likert score of about 4.62. 70 percent of subjects (230 out of 330) selected the most negative answer and less than 2 percent (6 out of 330) assessed the conditions as *good* or *very good* (see top left graph of Figure A.1). To evaluate whether the information signal had an impact on beliefs, we asked subjects who received the information to assess the living conditions again after watching the video about intensive farming. For these subjects, the average likert score increased from 4.66 to 4.86 and a rank sum test confirms that the change in beliefs is statistically significant ($p < 0.001$).

In addition, we also elicited beliefs about the living conditions of chicken to investigate whether individuals used the information to make inference about the living conditions of other animals. The conditions were again expected to be bad ex ante (average likert score of 4.66, distribution of responses presented in top right graph of Figure A.1), but we do not find a significant adjustment of beliefs after watching the video about intensive farming ($p = 0.64$, rank sum test). Figures A.1 and A.2 also show the distributions of responses to other questions which were related to intensive farming and meat consumption but were only asked before eliciting subjects' WTP for information.

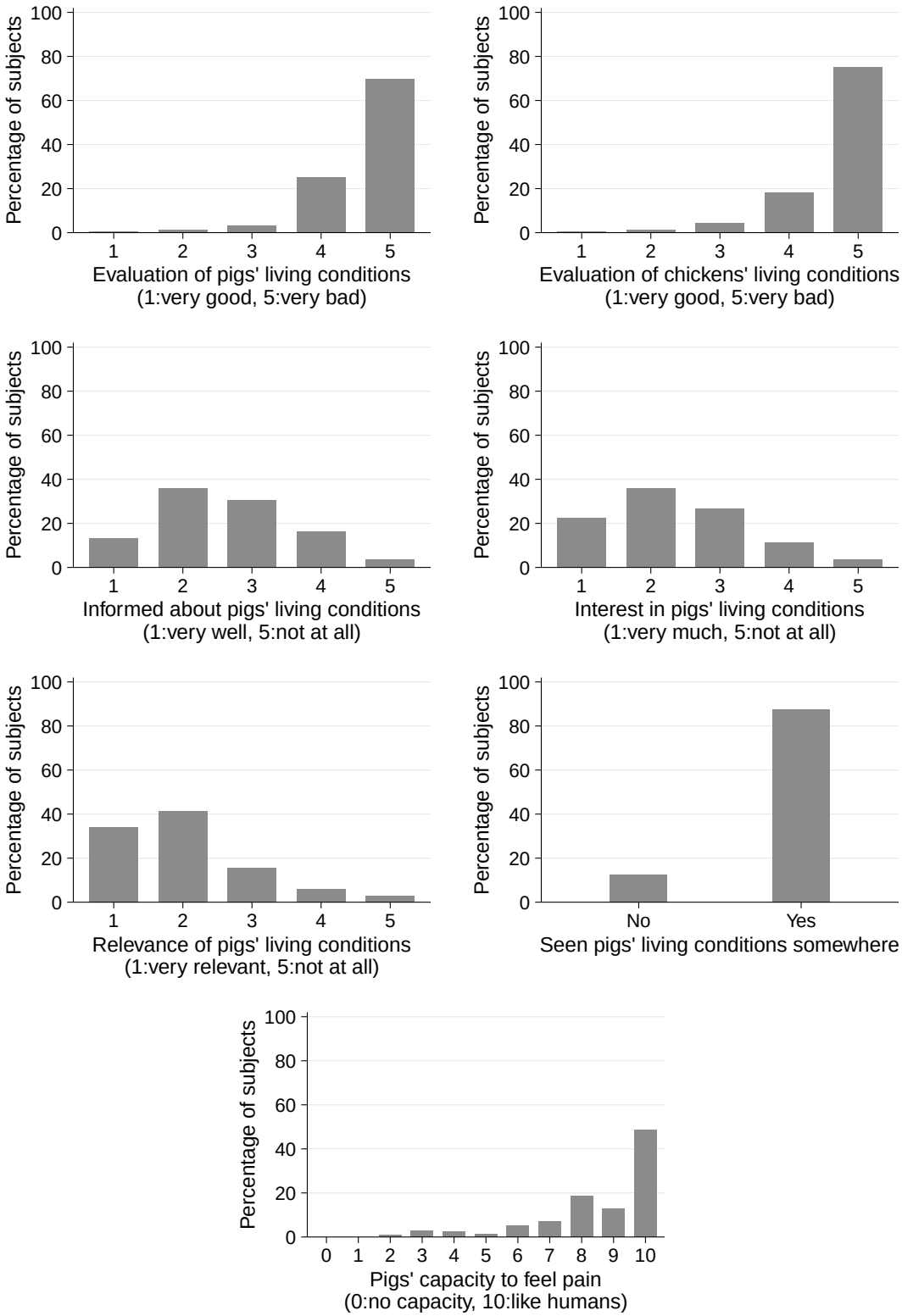


Figure A.1: Responses Related to Intensive Farming

Notes: The figure shows the distributions of responses for different questions related to intensive farming. All questions were asked before eliciting subjects' WTP for information. Each graph is based on 330 observations.

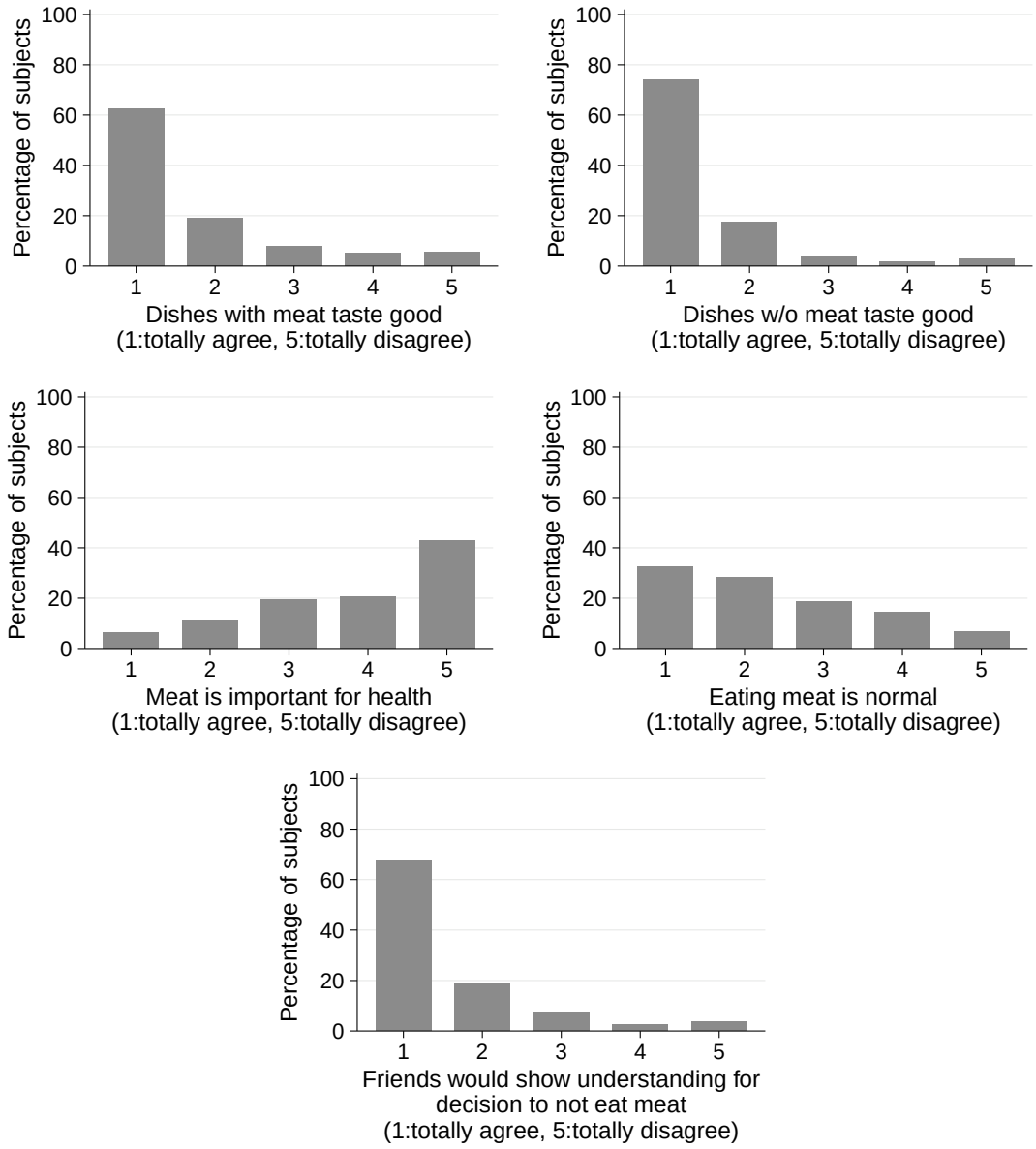


Figure A.2: Responses Related to Meat Consumption

Notes: The figure shows the distributions of responses for different questions related to meat consumption. All questions were asked before eliciting subjects' WTP for information. Each graph is based on 330 observations.

A.5 Robustness Checks

Since we conduct a controlled experiment, we know the probability of receiving the information on intensive farming for each subject and can use the true instead of the empirical propensity scores when estimating the effect of information (see equation A.2 in Appendix A.3.2). Tables A.2 to A.5 show that the estimation results from this alternative strategy are very similar to our main findings presented in the paper. We also ensure that our results are robust to directly specifying a model for the outcome variable via OLS regressions (see Table A.6 and Table A.7).

Furthermore, we investigate whether there are important differences in the results for the two locations, Bonn and Mannheim. Figure A.3 shows that the demand curves for information are almost exactly the same across the two locations. To estimate location-specific information effects, we apply the IPW and WLS estimator separately to the data from Mannheim and Bonn, respectively. Since this is demanding in terms of sample size, results should only be taken as suggestive evidence. The estimates from the field do not significantly differ between the locations (see Table A.10 and Table A.11). In the laboratory, it seems that information seekers ($WTP \geq 0$) in Mannheim respond less than information seekers in Bonn (see Table A.9) and the difference between the estimates is weakly significant for each specification ($p < 0.1$). The response of information avoiders ($WTP < 0$) is instead very similar. This pattern leads to a smaller point estimate for the average information effect in Mannheim (see Table A.8), which results in a marginally significant difference compared to the point estimate from Bonn in the case of the WLS estimator ($p < 0.1$). Hence, there is suggestive evidence of a difference between locations for a subgroup of individuals in the laboratory, but this difference does not show up in the field.

Table A.2: Information Effect in the Laboratory Based on True Propensity Scores

	Dependent variable: Choosing voucher for meal with meat				
	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
Info effect	-0.168***	-0.156***	-0.143***	-0.153***	-0.135***
	(0.055)	(0.045)	(0.045)	(0.044)	(0.043)
Mean (w/o info)	0.461	0.454	0.447	0.449	0.441
Meat-eating habit	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	312	312	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit in columns 2 to 5, we use indicator variables. Since never eating meat is a perfect predictor of choosing the vegetarian voucher, the expected likelihood for individuals with this meat-eating habit is not predicted via a logit model but directly set to zero for the models in columns 4 and 5. Additional controls are age, gender, degree, location, and WTP. All estimates are based on the true instead of the empirical propensity scores.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.3: Information Effect in the Field Based on True Propensity Scores

	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
A. Share of meals with meat					
Info effect	-0.086**	-0.059*	-0.067**	-0.060*	-0.066**
	(0.044)	(0.033)	(0.032)	(0.033)	(0.032)
Mean (w/o info)	0.457	0.440	0.444	0.440	0.450
B. Share of meals without meat					
Info effect	0.099**	0.054	0.090***	0.053	0.083**
	(0.043)	(0.036)	(0.034)	(0.036)	(0.035)
Mean (w/o info)	0.402	0.430	0.411	0.429	0.410
Baseline level	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	261	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP. All estimates are based on the true instead of the empirical propensity scores.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.4: Information Effect in the Laboratory Based on True Propensity Scores – Avoider and Seeker

	Dependent variable: Choosing voucher for meal with meat		
	IPW	WLS	
	(1)	(2)	(3)
Avoider			
Info effect	-0.309***	-0.236***	-0.229***
	(0.097)	(0.077)	(0.073)
Mean (w/o info)	0.530	0.503	0.500
Seeker			
Info effect	-0.095	-0.100*	-0.102*
	(0.066)	(0.054)	(0.055)
Mean (w/o info)	0.403	0.408	0.409
Comparison of effects (p-value)	0.07	0.15	0.16
Meat-eating habit	No	Yes	Yes
Additional controls	No	No	Yes
Observations	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit in columns 2 to 5, we use indicator variables. Additional controls are age, gender, degree, location, and WTP. All estimates are based on the true instead of the empirical propensity scores.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.5: Information Effect in the Field Based on True Propensity Scores – Avoider and Seeker

	IPW	WLS	
	(1)	(2)	(3)
A. Share of meals with meat			
Avoider			
Info effect	-0.106	-0.001	-0.006
	(0.086)	(0.066)	(0.065)
Mean (w/o info)	0.453	0.412	0.415
Seeker			
Info effect	-0.084	-0.084**	-0.089**
	(0.052)	(0.037)	(0.037)
Mean (w/o info)	0.460	0.460	0.463
Comparison of effects (p-value)	0.82	0.27	0.27
B. Share of meals without meat			
Avoider			
Info effect	0.198**	0.048	0.069
	(0.085)	(0.073)	(0.071)
Mean (w/o info)	0.377	0.436	0.424
Seeker			
Info effect	0.062	0.073*	0.095**
	(0.052)	(0.039)	(0.039)
Mean (w/o info)	0.423	0.417	0.406
Comparison of effects (p-value)	0.17	0.76	0.75
Baseline level	No	Yes	Yes
Additional controls	No	No	Yes
Observations	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP. All estimates are based on the true instead of the empirical propensity scores.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.6: Information Effect Based on OLS

	Laboratory		Field			
	Meal with meat		Meal with meat		Meal w/o meat	
	(1)	(2)	(3)	(4)	(5)	(6)
Info effect	-0.130***	-0.128***	-0.064**	-0.067**	0.088**	0.088***
	(0.047)	(0.047)	(0.032)	(0.032)	(0.034)	(0.034)
Mean (w/o info)	0.439	0.438	0.446	0.448	0.410	0.410
Meat-eating habit	Yes	Yes	No	No	No	No
Baseline level	No	No	Yes	Yes	Yes	Yes
WTP	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes
Observations	312	312	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit and WTP, we use indicator variables. Additional controls are age, gender, degree, and location. All estimates are based on OLS regressions. In columns 1 and 2, the dependent variable is whether the individuals chose the voucher for the meal with meat. In column 3 and 4, the dependent variable is the share of meals with meat. In columns 5 and 6, the dependent variable is the share of meals without meat.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.7: Information Effect Based on OLS – Avoider and Seeker

	Laboratory		Field			
	Meal with meat		Meal with meat		Meal w/o meat	
	(1)	(2)	(3)	(4)	(5)	(6)
Avoider						
Info effect	-0.271***	-0.260***	-0.022	-0.025	0.101	0.102
	(0.082)	(0.079)	(0.062)	(0.063)	(0.066)	(0.067)
Mean (w/o info)	0.522	0.517	0.422	0.423	0.407	0.406
Seeker						
Info effect	-0.071	-0.074	-0.081**	-0.083**	0.083**	0.083**
	(0.057)	(0.056)	(0.037)	(0.036)	(0.039)	(0.039)
Mean (w/o info)	0.393	0.395	0.460	0.460	0.413	0.412
Comparison of effects (p-value)	0.04	0.05	0.41	0.42	0.81	0.80
Meat-eating habit	Yes	Yes	No	No	No	No
Baseline level	No	No	Yes	Yes	Yes	Yes
WTP	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes
Observations	312	312	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit and WTP, we use indicator variables. Additional controls are age, gender, degree, and location. All estimates are based on OLS regressions. In columns 1 and 2, the dependent variable is whether the individuals chose the voucher for the meal with meat. In column 3 and 4, the dependent variable is the share of meals with meat. In columns 5 and 6, the dependent variable is the share of meals without meat.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

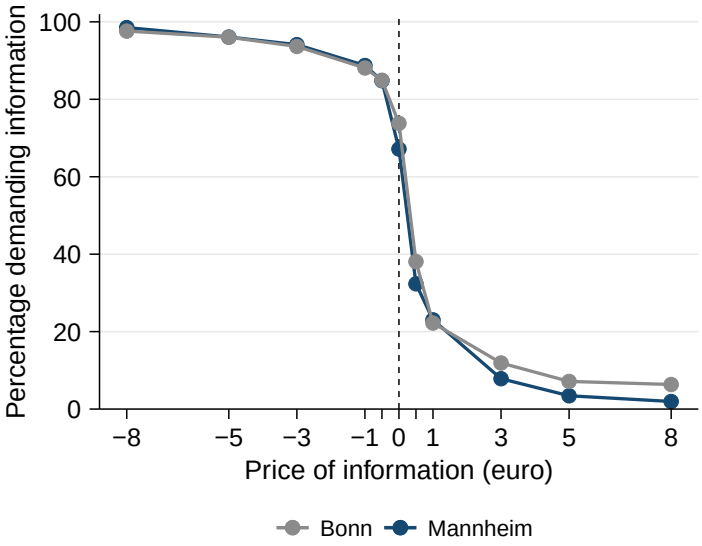


Figure A.3: Demand for Information by Location

Notes: For each location, the figure shows the percentage of subjects choosing to receive information about intensive farming at a given price from the multiple price list.

Table A.8: Information Effect in the Laboratory by Location

	Dependent variable: Choosing voucher for meal with meat					
	Mannheim			Bonn		
	IPW	WLS		IPW	WLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Info effect	-0.104	-0.049	-0.052	-0.248***	-0.252***	-0.216***
	(0.067)	(0.070)	(0.067)	(0.088)	(0.080)	(0.073)
Mean (w/o info)	0.424	0.388	0.390	0.495	0.501	0.483
Meat-eating habit	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes
Observations	197	197	197	115	115	115

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit in columns 2, 3, 5, and 6, we use indicator variables. Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.9: Information Effect in the Laboratory by Location – Avoider and Seeker

	Dependent variable: Choosing voucher for meal with meat					
	Mannheim			Bonn		
	IPW	WLS		IPW	WLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Avoider						
Info effect	-0.322***	-0.238***	-0.248***	-0.359**	-0.278*	-0.168
	(0.108)	(0.091)	(0.093)	(0.156)	(0.143)	(0.120)
Mean (w/o info)	0.377	0.436	0.424	0.377	0.436	0.424
Seeker						
Info effect	0.001	0.044	0.040	-0.209**	-0.243***	-0.232***
	(0.083)	(0.091)	(0.091)	(0.105)	(0.093)	(0.088)
Mean (w/o info)	0.346	0.313	0.316	0.476	0.498	0.493
Comparison of effects (p-value)	0.02	0.03	0.04	0.42	0.84	0.67
Meat-eating habit	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes
Observations	197	197	197	115	115	115

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit in columns 2, 3, 5, and 6, we use indicator variables. Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.10: Information Effect in the Field by Location

	Mannheim			Bonn		
	IPW	WLS		IPW	WLS	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Share of meals with meat						
Info effect	-0.105**	-0.061	-0.059	-0.182***	-0.087	-0.086
	(0.050)	(0.050)	(0.049)	(0.069)	(0.053)	(0.055)
Mean (w/o info)	0.487	0.469	0.467	0.486	0.443	0.442
B. Share of meals without meat						
Info effect	0.127***	0.075	0.071	0.184***	0.120*	0.112
	(0.049)	(0.058)	(0.052)	(0.071)	(0.068)	(0.068)
Mean (w/o info)	0.356	0.380	0.382	0.413	0.439	0.444
Baseline level	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes
Observations	173	173	173	88	88	88

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.11: Information Effect in the Field by Location – Avoider and Seeker

	Mannheim			Bonn		
	IPW	WLS		IPW	WLS	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Share of meals with meat						
Avoider						
Info effect	-0.120	-0.003	-0.001	-0.245**	-0.034	-0.015
	(0.090)	(0.076)	(0.074)	(0.117)	(0.117)	(0.114)
Mean (w/o info)	0.377	0.436	0.424	0.377	0.436	0.424
Seeker						
Info effect	-0.097	-0.088	-0.085	-0.159*	-0.104*	-0.105*
	(0.060)	(0.062)	(0.061)	(0.084)	(0.057)	(0.058)
Mean (w/o info)	0.423	0.417	0.406	0.423	0.417	0.406
Comparison of effects (p-value)	0.83	0.38	0.38	0.55	0.59	0.46
B. Share of meals without meat						
Avoider						
Info effect	0.197**	0.046	0.041	0.363***	0.161	0.138
	(0.084)	(0.070)	(0.068)	(0.105)	(0.145)	(0.140)
Mean (w/o info)	0.377	0.436	0.424	0.377	0.436	0.424
Seeker						
Info effect	0.094	0.088	0.084	0.121	0.107	0.104
	(0.060)	(0.075)	(0.068)	(0.088)	(0.074)	(0.074)
Mean (w/o info)	0.423	0.417	0.406	0.423	0.417	0.406
Comparison of effects (p-value)	0.32	0.68	0.65	0.08	0.73	0.82
Baseline level	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes
Observations	173	173	173	88	88	88

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

A.6 Instructions for WTP Elicitation

In the following, we present the translated instructions for the WTP elicitation task. The only difference across locations was that in Mannheim, the electronic payment card is called "ecUM" while in Bonn, it is called "MensaCard". Whether the video about the living conditions of pigs in intensive farming was presented as Option A or Option B was randomized, and the comprehension question was adjusted accordingly. Figure A.4 shows a screenshot of the decision screen.

Start of the instructions

In a few minutes you will watch another 360° video. There are two different options available to you.

On the next few pages, we present the two options and explain how to choose one of the options. Only then will you make your decision(s).

Page break

You have the following options to choose from:

Option A: You watch a 360° video about intensive farming of pigs.

If you choose this option, you'll see excerpts from a video by the organization Animal Equality. This video shows the life of pigs in intensive farming, from birth until slaughter. The scenes were recorded between December 2014 and January 2016 in Germany, Italy, Mexico and Spain. According to Animal Equality, all the scenes correspond to standard practice in Europe.

Please note: *This video contains scenes in which blood can be seen and which may be shocking.*

Option B: You watch the 360° video about the Deutsche Bundesbank again.

If you choose this option, you will again see the excerpts from the video of the Deutsche Bundesbank. This video shows the virtual tour through the Bundesbank building and corresponds exactly to the video that you saw earlier. The scenes were recorded in 2019. Most of the rooms shown are part of the official tour of the Bundesbank building.

The two videos are of the same length (approx. 5 minutes).

Page break

You can choose between the two options as follows:

You make a total of 11 decisions, at each of which you choose one of the two options. The only difference between the 11 decisions is for which option you receive a bonus payment and how high this bonus payment is.

One of your 11 decisions is then randomly selected and implemented. This means that you watch the video selected in this decision and receive the corresponding bonus payment **in addition to your guaranteed participation fee** of €5.00. You will also receive the bonus payment in the form of a credit to your ecUM.

Since each of your decisions can be selected, you should carefully consider each decision.

We will now show you an example to illustrate this procedure. **You only make your decisions after the example.**

Page break

For a better understanding, we will now show you an example. The table in which you will enter your decisions will look like the one below.

You only make your decisions on the next page. You cannot select any options in this table.

[multiple price list without active radio buttons]

Each line of the table contains a decision to be made, which is identified by a decision number. In every decision you make, you either choose Option A or Option B by ticking the corresponding option.

Afterwards, one of the decision numbers is randomly selected by the computer and the corresponding decision is implemented. This means that you watch the video selected in this decision and receive the corresponding bonus payment **in addition to your guaranteed participation fee** of €5.00.

Since any decision number can be selected, **you should carefully consider all decisions.** (The decision numbers E1 and E11 are each selected with a probability of 27.5%, each of other numbers is selected with a probability of 5%).

We would now like you to answer a comprehension question. You cannot proceed until you have correctly answered this question. If something is still unclear to you, please raise your hand and wait for someone to come to your seat.

1. What happens if you choose Option B in decision E8 and decision E8 is randomly selected as the decision to be implemented?

   ◯ In addition to your guaranteed participation fee of **€5.00**, you will receive a bonus payment of **€1.00** and watch the **360° video about the Deutsche Bundesbank**.

   ◯ In addition to your guaranteed participation fee of **€0.00**, you will receive a bonus payment of **€0.00** and watch the **360° video about the Deutsche Bundesbank**.

   ◯ In addition to your guaranteed participation fee of **€5.00**, you will receive a bonus payment of **€0.00** and watch the **360° video about the Deutsche Bundesbank**.

   ◯ In addition to your guaranteed participation fee of **€5.00**, you will receive a bonus payment of **€1.00** and watch the **360° video about intensive farming**.

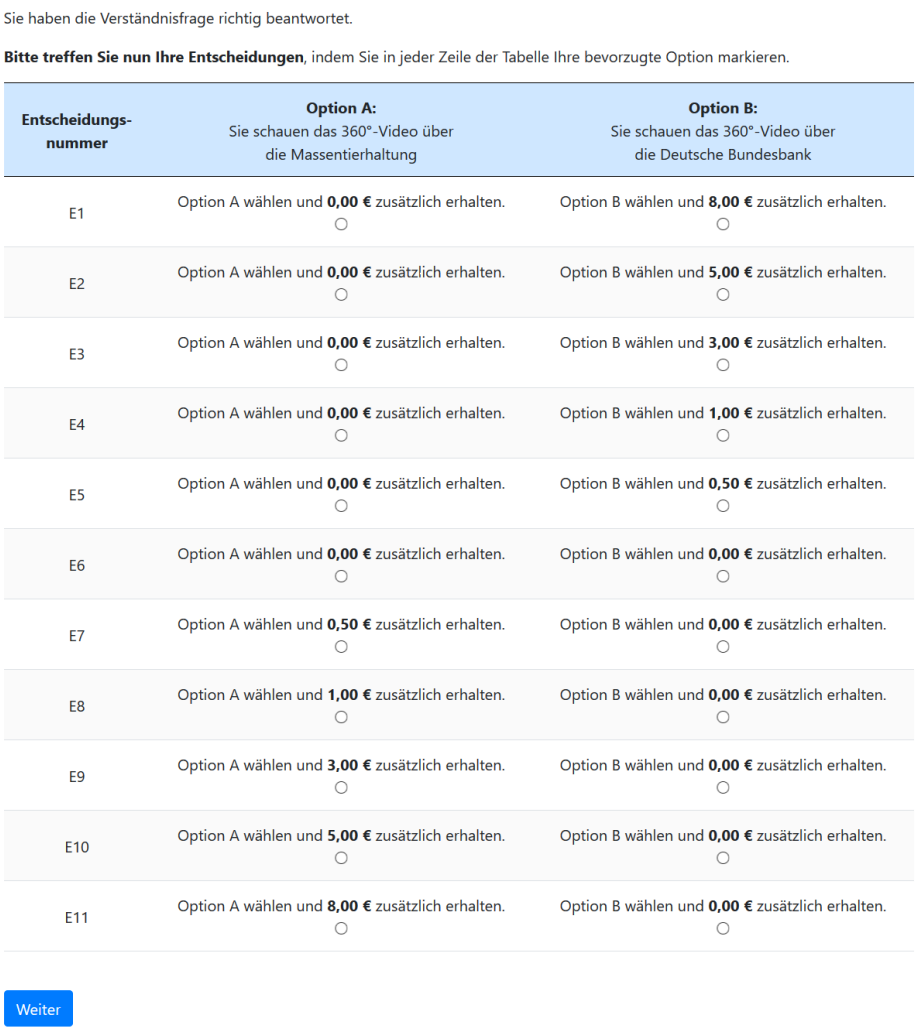
Page break

You answered the comprehension question correctly.

Please make your decisions now by selecting your preferred option in each row of the table.

Decision number	**Option A:** You watch the 360° video about intensive farming	**Option B:** You watch the 360° video about the Deutsche Bundesbank
E1	Choose **Option A** and obtain **€0.00** additionally. ◯	Choose **Option B** and obtain **€8.00** additionally. ◯
E2	Choose **Option A** and obtain **€0.00** additionally. ◯	Choose **Option B** and obtain **€5.00** additionally. ◯
E3	Choose **Option A** and obtain **€0.00** additionally. ◯	Choose **Option B** and obtain **€3.00** additionally. ◯
E4	Choose **Option A** and obtain **€0.00** additionally. ◯	Choose **Option B** and obtain **€1.00** additionally. ◯
E5	Choose **Option A** and obtain **€0.00** additionally. ◯	Choose **Option B** and obtain **€0.50** additionally. ◯
E6	Choose **Option A** and obtain **€0.00** additionally. ◯	Choose **Option B** and obtain **€0.00** additionally. ◯
E7	Choose **Option A** and obtain **€0.50** additionally. ◯	Choose **Option B** and obtain **€0.00** additionally. ◯
E8	Choose **Option A** and obtain **€1.00** additionally. ◯	Choose **Option B** and obtain **€0.00** additionally. ◯
E9	Choose **Option A** and obtain **€3.00** additionally. ◯	Choose **Option B** and obtain **€0.00** additionally. ◯
E10	Choose **Option A** and obtain **€5.00** additionally. ◯	Choose **Option B** and obtain **€0.00** additionally. ◯
E11	Choose **Option A** and obtain **€8.00** additionally. ◯	Choose **Option B** and obtain **€0.00** additionally. ◯

End of instructions



Sie haben die Verständnisfrage richtig beantwortet.

Bitte treffen Sie nun Ihre Entscheidungen, indem Sie in jeder Zeile der Tabelle Ihre bevorzugte Option markieren.

Entscheidungs-nummer	**Option A:** Sie schauen das 360°-Video über die Massentierhaltung	**Option B:** Sie schauen das 360°-Video über die Deutsche Bundesbank
E1	Option A wählen und **0,00 €** zusätzlich erhalten. ○	Option B wählen und **8,00 €** zusätzlich erhalten. ○
E2	Option A wählen und **0,00 €** zusätzlich erhalten. ○	Option B wählen und **5,00 €** zusätzlich erhalten. ○
E3	Option A wählen und **0,00 €** zusätzlich erhalten. ○	Option B wählen und **3,00 €** zusätzlich erhalten. ○
E4	Option A wählen und **0,00 €** zusätzlich erhalten. ○	Option B wählen und **1,00 €** zusätzlich erhalten. ○
E5	Option A wählen und **0,00 €** zusätzlich erhalten. ○	Option B wählen und **0,50 €** zusätzlich erhalten. ○
E6	Option A wählen und **0,00 €** zusätzlich erhalten. ○	Option B wählen und **0,00 €** zusätzlich erhalten. ○
E7	Option A wählen und **0,50 €** zusätzlich erhalten. ○	Option B wählen und **0,00 €** zusätzlich erhalten. ○
E8	Option A wählen und **1,00 €** zusätzlich erhalten. ○	Option B wählen und **0,00 €** zusätzlich erhalten. ○
E9	Option A wählen und **3,00 €** zusätzlich erhalten. ○	Option B wählen und **0,00 €** zusätzlich erhalten. ○
E10	Option A wählen und **5,00 €** zusätzlich erhalten. ○	Option B wählen und **0,00 €** zusätzlich erhalten. ○
E11	Option A wählen und **8,00 €** zusätzlich erhalten. ○	Option B wählen und **0,00 €** zusätzlich erhalten. ○

Weiter

Figure A.4: Screenshot Multiple Price List

Notes: The figure shows a screenshot of the decision screen that was used to elicit subjects' willingness-to-pay for information on the living conditions of pigs in intensive farming.

A.7 Video Content

Table A.12: Video Content

Minute	Original Video	Description	Narrator (translated)
0:00–0:29	0:32–1:01	Fully-grown pigs kept individually; Housing facilities are separated by bars; Slatted floor (approx. 70%) and a connected area in front of the feeding trough	You don't know what you're in for but you're behind bars. Looking around, you see countless others like you. Your neighbor on the left tries to seek comfort from you. It has been this way as long as you can remember.
0:29–0:48	1:30–1:49	Sow with piglets in farrowing pen; Slatted floor; Stillbirths/dead piglets on the ground; Small lying area without litter available	Many of your siblings won't even survive a week in that breeding ground for disease.
0:49–1:08	1:50–2:09	Castration of piglet that is conscious	If you do survive that first week, you're enforced to endure a tooth clipping, tail docking, and castration. All without any pain relief.
1:09–1:29	2:10–2:30	Piglets are returned to the cage; Crate stall; Slatted floor;	
1:30–2:10	2:31–3:11	Crate stall; Slatted floor; Lying area; Area with litter	Without proper medical care, you return to the cage where you were born. A cell that's anything but sanitary. Here, you'll never once take a single breath of fresh air, never taste fresh water, never even see the sun. This punishment is worse than that faced by even the most heinous offenders in prison. Yet, you're guilty of being born and even that was never a choice.
2:11–2:35	3:12–3:36	Sows in individual housing; Bar biting	If you happen to be born a girl, you'll likely be cycled back through the gauntlet of cages. Artificially inseminated and then robbed of your children over and over and over again for as long as you remain fertile. The same fate awaits most daughters you will ever have.
2:36–3:06	3:37–4:07	Pigs that are unsuitable for breeding or male kept in groups (fattening); More than 20 animals; Slatted floor; Car tire to stay busy; Injured animals	And if you're deemed unfit for breeding or if you happen to have been born a boy, you're taken to a fattening farm. Here, you live in cramped quarters for the next five months, again, without any stimulation, no fresh air, or water. The boredom and frustration may drive you mad, and if not you, your siblings. Inevitably, some act out biting and hurting each other.
3:07–3:58	4:33–5:24	Slaughterhouse: Electrical stunning of the pigs with stunning forceps; Pigs are hung up by one leg after being anesthetized; Bleeding the pigs by piercing the center of the neck once; Paddling movement of pigs after anesthesia (typical behavior after anesthesia)	Even if you survive the fattening farm, you'll soon learn that those first six months of your life will be your last. Your next and final stop is the slaughterhouse. Here, huddled together with all the other pigs, you see the bodies of those that came here moments before, now strung up and bleeding. You watch helplessly as the others are picked off one by one, stunned with an electrical current just enough to be chained and hoisted up by one leg, until finally, it's your turn.
3:59–4:49	5:40–6:30	Bleeding the pigs by piercing the center of the neck once; Pig with severe cramps falls from the holding device	

Notes: In total, the video is 5 minutes long as it additionally shows a black screen for about 11 seconds at the beginning. The black screen was shown for the same duration in the video about the German central bank.

Table A.13: Legal Situation

Minute	Legal situation (translated and condensed)
0:00–0:29	*§ 22 Allgemeine Anforderungen an Haltungseinrichtungen für Schweine (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* Housing facilities need to guarantee that individually held pigs can see the other pigs and all pigs are able to lay down, stand up, and adopt a natural position. *§ 24 Besondere Anforderungen an Haltungseinrichtungen für Jungsauen und Sauen (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* The lying area of individually hold gilts and sows should at most be partially perforated. Crate stalls need to prevent that pigs hurt themselves and guarantee that all pigs can stand up, lay down and, stretch out.
0:29–0:48	*§ 23 Besondere Anforderungen an Haltungseinrichtungen für Saugferkel (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* The living area of the piglets need to have safety devices so no piglet gets squashed. Additionally it need to be guaranteed that all can suck and relax at the same time. The lying area either needs to be thermally insulated and heatable or covered by litter. Perforated floor in the lying area must be covered. *§ 24 Besondere Anforderungen an Haltungseinrichtungen für Jungsauen und Sauen (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* Crate stalls need to prevent that pigs hurt themselves and guarantee that all pigs can stand up, lay down and, stretch out.
0:49–1:08	*Richtlinie 2008/120/EG des Rates vom 18. Dezember 2008 über Mindestanforderungen für den Schutz von Schweinen (kodifizierte Fassung) (ABl. L 47 vom 18.2.2009, S. 5-13)* Veterinarians are allowed to shrink the canines of piglets, crop the tail, castrate male pigs, and attach nose rings. Cropping the tail or shrinking the canines is only allowed if it is shown that the sow's teats or the ears of other pigs have been injured. However, before doing such painful interventions other measures need to be taken to prevent behavioral disorders, such as adapting unsuitable accommodations.
1:09–1:29	See above.
1:30–2:10	See above.
2:11–2:35	*§ 30 Besondere Anforderungen an das Halten von Jungsauen und Sauen (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* Gilts and sows are supposed to be hold in a group until one week before giving birth. The living area here should offer at least 1.85sqm per gilt and 2.50sqm per sow. Gilts or sows that are hold individually need to be able to turn around unimpededly during that time. Gilts and sows are only allowed to be hold in crate stalls when it is not obvious that this husbandry condition leads to negative arousal.
2:36–3:06	*§ 26 Allgemeine Anforderungen an das Halten von Schweinen (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* Every pig should have access to enough toys that are harmless for health to maintain their exploratory behavior. *§ 29 Besondere Anforderungen an das Halten von Zuchtläufern und Mastschweinen (TierSchNutztV, Abschnitt 5 Anforderungen an das Halten von Schweinen)* Breeding runners and fattening pigs should be hold in groups without frequent changes in the composition. *Richtlinie 2008/120/EG des Rates vom 18. Dezember 2008 über Mindestanforderungen für den Schutz von Schweinen (kodifizierte Fassung) (ABl. L 47 vom 18.2.2009, S. 5-13)* Aggressive as well as injured animals need to be separated from the rest of the group. Sick or injured pigs are held individually.
3:07–3:58	*§ 12 Betäuben, Schlachten und Töten (Tierschutz-Schlachtverordnung – TierSchlV, Abschnitt 4 Vorschriften über das Ruhigstellen, Betäuben, Schlachten und Töten von Tieren)* When slaughtering an animal, the bleeding needs to be started within 20 seconds after the anaesthesia, while the animal still cannot feel anything. Additionally, the owner of the slaughterhouse needs to guarantee that if it is necessary the animal is bled by hand. *Verordnung (EG) Nr. 1099/2009 des Rates vom 24. September 2009 über den Schutz von Tieren zum Zeitpunkt der Tötung* Animals are only getting killed after being stunned. This state of numbness and unconsciousness needs to hold on until the animal is dead. It is not allowed to immobilize an animal by hanging it up before it got stunned. Using head-only electronic stunning for pigs demands a minimum amperage from 1.3A. The whole procedure of stunning, hanging and bleeding an animal needs to be finished before starting with the next one. Further steps are allowed after checking whether the animal really is dead.
3:59–4:49	See above.

A.8 Additional Tables

Table A.14: Comparison by Whether Subjects Purchased Meal after Experiment

	Purchased meal after experiment		Comparison
Variable	Yes	No	p-value
WTP for information on intensive farming (euro)[a]	0.39	0.01	0.21
	(0.11)	(0.28)	
Age (years)	21.95	22.22	0.62
	(0.19)	(0.51)	
Female (1:yes, 0:no)	0.43	0.61	0.01
	(0.03)	(0.07)	
Bachelor's degree or higher (1:yes, 0:no)	0.28	0.35	0.31
	(0.03)	(0.07)	
Location: Mannheim (1:yes, 0:no)	0.64	0.48	0.03
	(0.03)	(0.07)	
Meat-eating habit (stated frequency)			
Never	0.14	0.07	0.13
	(0.02)	(0.04)	
Several times per year	0.10	0.17	0.21
	(0.02)	(0.05)	
Several times per month	0.25	0.30	0.47
	(0.03)	(0.06)	
Several times per week	0.38	0.37	0.85
	(0.03)	(0.07)	
Daily	0.13	0.09	0.36
	(0.02)	(0.04)	
Received information (1:yes, 0:no)	0.53	0.50	0.70
	(0.03)	(0.07)	
Number of purchases before experiment	21.79	5.69	0.00
	(1.05)	(2.09)	
Observation period before experiment (days)	126.40	143.65	0.02
	(2.37)	(6.66)	
Observation period after experiment (days)	80.65	53.76	0.00
	(2.37)	(4.35)	
Share of meals with meat before experiment[b]	0.42	0.36	0.37
	(0.02)	(0.07)	
Share of meals without meat before experiment[b]	0.43	0.48	0.53
	(0.02)	(0.07)	
Share of ambiguous meals before experiment[b]	0.15	0.17	0.79
	(0.01)	(0.06)	
Observations	276	54	

Notes: Standard errors are in parentheses. p-values are obtained from regressing the corresponding variable on a dummy for purchasing at least one food item in the post-experimental period, using robust standard errors.
[a] The WTP for information is based on the midpoint of the corresponding WTP interval and only subjects with a WTP that is bounded by the prices in the multiple price list are considered (261 in the first and 51 in the second group)
[b] These values are only available for subjects with a pre-experimental purchase (263 in the first and 29 in the second group).

Table A.15: Comparison of Treatment Groups

	Before weighting		After weighting		Comparison
Variable	No info	Info	No info	Info	p-value
WTP for information on int. farm. (euro)	-0.07	0.69	0.32	0.32	1.00
	(0.15)	(0.14)	(0.10)	(0.10)	
Age (years)	22.06	21.92	22.18	21.82	0.33
	(0.27)	(0.26)	(0.29)	(0.25)	
Female (1:yes, 0:no)	0.45	0.45	0.42	0.47	0.34
	(0.04)	(0.04)	(0.04)	(0.04)	
Bachelor's degree or higher (1:yes, 0:no)	0.33	0.25	0.35	0.24	0.02
	(0.04)	(0.03)	(0.04)	(0.03)	
Location: Mannheim (1:yes, 0:no)	0.64	0.63	0.63	0.62	0.91
	(0.04)	(0.04)	(0.04)	(0.04)	
Meat-eating habit (stated frequency)					
Never	0.11	0.15	0.09	0.16	0.05
	(0.03)	(0.03)	(0.02)	(0.03)	
Several times per year	0.07	0.14	0.07	0.13	0.07
	(0.02)	(0.03)	(0.02)	(0.03)	
Several times per month	0.30	0.22	0.29	0.24	0.32
	(0.04)	(0.03)	(0.04)	(0.04)	
Several times per week	0.41	0.35	0.45	0.33	0.03
	(0.04)	(0.04)	(0.04)	(0.04)	
Daily	0.11	0.14	0.10	0.14	0.34
	(0.03)	(0.03)	(0.02)	(0.03)	
Number of purchases before exp.	20.03	18.95	19.97	18.95	0.63
	(1.53)	(1.38)	(1.58)	(1.37)	
Observation period before exp. (days)	130.24	128.20	129.62	127.58	0.66
	(3.42)	(3.23)	(3.40)	(3.12)	
Observation period after exp. (days)	76.27	77.81	76.46	78.11	0.72
	(3.25)	(3.13)	(3.38)	(3.12)	
Share of meals with meat before exp.[a]	0.43	0.40	0.44	0.38	0.08
	(0.03)	(0.03)	(0.03)	(0.02)	
Share of meals without meat before exp.[a]	0.40	0.46	0.40	0.47	0.05
	(0.03)	(0.03)	(0.02)	(0.02)	
Share of ambiguous meals before exp.[a]	0.17	0.14	0.16	0.15	0.75
	(0.02)	(0.02)	(0.02)	(0.02)	
Observations	151	161	151	161	

Notes: Standard errors are in parentheses. Comparisons between the treatment and control group are based on the IPW estimator applied to the corresponding variable of interest.
[a]These values are only available for subjects who purchased a meal before the experiment (135 and 141, respectively).

Table A.16: Correlation of WTP and Observable Characteristics

	(1)	(2)	(3)
Demographics			
Age (years)	0.02	0.02	0.03
	(0.03)	(0.03)	(0.03)
Female (1:yes, 0:no)	-1.11***	-0.75***	-0.75***
	(0.24)	(0.24)	(0.23)
Bachelor's degree or higher (1:yes, 0:no)	-0.04	0.00	0.10
	(0.25)	(0.25)	(0.23)
Location: Mannheim (1:yes, 0:no)	0.04	0.05	0.08
	(0.22)	(0.21)	(0.20)
Meat-eating habit (0:never, 4:daily)	-0.20*	-0.23**	0.01
	(0.11)	(0.11)	(0.13)
Videos and virtual reality			
Own VR glasses (1:yes, 0:no)		-0.46**	-0.24
		(0.22)	(0.30)
Used VR glasses to watch a 360° video (1:yes, 0:no)		0.32	0.25
		(0.20)	(0.19)
Tolerance of violence in videos (1:very high, 5:very low)		-0.37***	-0.40***
		(0.10)	(0.10)
Related to intensive farming			
Evaluation of pigs' living conditions (1:very good, 5:very bad)			-0.11
			(0.23)
Evaluation of chickens' living conditions (1:very good, 5:very bad)			-0.07
			(0.22)
Interest in pigs' living conditions (1:very much, 5:not at all)			0.16
			(0.10)
Relevance of pigs' living conditions (1:very relevant, 5:not at all)			-0.44***
			(0.12)
Informed about pigs' living conditions (1:very well, 5:not at all)			0.10
			(0.14)
Seen pigs' living conditions somewhere			0.61**
			(0.28)
Pigs' capacity to feel pain (0:no capacity, 10:like humans)			-0.01
			(0.04)
Related to meat consumption			
Dishes with meat taste good (1:totally agree, 5:totally disagree)			-0.04
			(0.11)
Dishes w/o meat taste good (1:totally agree, 5:totally disagree)			-0.14
			(0.12)
Meat is important for health (1:totally agree, 5:totally disagree)			0.14
			(0.10)
Eating meat is normal (1:totally agree, 5:totally disagree)			0.16
			(0.10)
Friends would show understanding for decision to not eat meat			0.08
(1:totally agree, 5:totally disagree)			(0.13)
R^2	0.08	0.13	0.20
Observations	312	312	312

Notes: Robust standard errors are in parentheses. Coefficient estimates are based on OLS regressions with the midpoint of the WTP interval as dependent variable. Only subjects whose WTP is bounded in the multiple price list are included. For variables which are elicited on a likert scale, we only provide the start and end of the scale in parentheses after the variable name.

Table A.17: Likelihood to Purchase Meal in the Field

	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
A. Any meal purchase					
Info effect	0.006	0.008	0.007	0.010	0.009
	(0.041)	(0.044)	(0.041)	(0.036)	(0.036)
Mean (w/o info)	0.837	0.840	0.841	0.840	0.841
B. Meal purchases per day					
Info effect	-0.004	-0.003	-0.002	-0.003	-0.002
	(0.019)	(0.019)	(0.016)	(0.018)	(0.015)
Mean (w/o info)	0.158	0.159	0.158	0.159	0.159
Baseline level	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	312	312	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP. In Panel A, the dependent variable is whether an individual purchased a meal after the experiment. Logit regressions are used to predict outcome levels for the AIPW estimator (columns 4 and 5). In Panel B, the dependent variable is the number of meal purchases per day after the experiment (not counting Sundays). OLS regressions are used to predict outcome levels for the AIPW estimator (columns 4 and 5).
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.18: Information Effect in the Field Controlling for Meat Eating Habit

	WLS		AIPW	
	(1)	(2)	(3)	(4)
A. Share of meals with meat				
Info effect	-0.060*	-0.067**	-0.062*	-0.069**
	(0.034)	(0.033)	(0.033)	(0.033)
Mean (w/o info)	0.447	0.451	0.447	0.449
B. Share of meals without meat				
Info effect	0.091**	0.098**	0.094**	0.100***
	(0.044)	(0.042)	(0.037)	(0.037)
Mean (w/o info)	0.403	0.399	0.403	0.402
Meat-eating habit	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Observations	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit, we use indicator variables. Additional controls are age, gender, degree, location, and WTP.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.19: Information Effect in the Laboratory with Restricted Sample

	Dependent variable: Choosing voucher for meal with meat				
	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
Info effect	-0.193***	-0.124**	-0.114**	-0.127**	-0.113**
	(0.059)	(0.057)	(0.051)	(0.050)	(0.048)
Mean (w/o info)	0.477	0.447	0.441	0.447	0.445
Baseline level from field	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	261	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). The share of purchased meals containing meat before the experiment is used as baseline level. Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.20: Information Effect in the Field – Avoider and Seeker

	IPW	WLS	
	(1)	(2)	(3)
A. Share of meals with meat			
Avoider			
Info effect	-0.153**	-0.006	-0.010
	(0.076)	(0.061)	(0.062)
Mean (w/o info)	0.482	0.423	0.425
Seeker			
Info effect	-0.112**	-0.087**	-0.089**
	(0.050)	(0.038)	(0.037)
Mean (w/o info)	0.479	0.466	0.467
Comparison of effects (p-value)	0.65	0.26	0.28
B. Share of meals without meat			
Avoider			
Info effect	0.249***	0.073	0.073
	(0.071)	(0.066)	(0.065)
Mean (w/o info)	0.331	0.400	0.400
Seeker			
Info effect	0.103**	0.095*	0.095*
	(0.051)	(0.055)	(0.049)
Mean (w/o info)	0.393	0.399	0.399
Comparison of effects (p-value)	0.09	0.79	0.79
Baseline level	No	Yes	Yes
Additional controls	No	No	Yes
Observations	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.21: Information Effect in the Laboratory with Restricted Sample – Avoider and Seeker

	Dependent variable: Choosing voucher for meal with meat		
	IPW	WLS	
	(1)	(2)	(3)
Avoider			
Info effect	-0.340***	-0.174*	-0.157*
	(0.109)	(0.100)	(0.093)
Mean (w/o info)	0.597	0.530	0.517
Seeker			
Info effect	-0.131*	-0.103	-0.097
	(0.070)	(0.065)	(0.060)
Mean (w/o info)	0.426	0.412	0.409
Comparison of effects (p-value)	0.11	0.55	0.58
Baseline level from field	No	Yes	Yes
Additional controls	No	No	Yes
Observations	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). The share of purchased meals containing meat before the experiment is used as baseline level. Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.22: Information Effect in the Field – Meat from Terrestrial and Aquatic Animals

	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
A. Share of meals with meat from terrestrial animals					
Info effect	-0.076**	-0.022	-0.022	-0.022	-0.024
	(0.038)	(0.030)	(0.030)	(0.030)	(0.030)
Mean (w/o info)	0.372	0.348	0.348	0.351	0.350
B. Share of meals without meat from terrestrial animals					
Info effect	0.099**	0.048	0.045	0.046	0.043
	(0.040)	(0.042)	(0.039)	(0.034)	(0.034)
Mean (w/o info)	0.479	0.500	0.502	0.498	0.503
C. Share of meals with meat from aquatic animals					
Info effect	-0.047**	-0.045**	-0.047**	-0.046**	-0.053***
	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)
Mean (w/o info)	0.105	0.104	0.105	0.105	0.109
D. Share of meals without meat from aquatic animals					
Info effect	0.033	0.043	0.043	0.045*	0.044*
	(0.030)	(0.033)	(0.031)	(0.026)	(0.025)
Mean (w/o info)	0.822	0.814	0.814	0.814	0.816
Baseline level	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	261	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.23: Information Effect in the Field – Pork and Meat from Other Terrestrial Animals

	IPW	WLS		AIPW	
	(1)	(2)	(3)	(4)	(5)
A. Share of meals with pork					
Info effect	-0.035	-0.021	-0.020	-0.026	-0.027
	(0.026)	(0.025)	(0.025)	(0.025)	(0.025)
Mean (w/o info)	0.186	0.179	0.179	0.182	0.182
B. Share of meals without pork					
Info effect	0.030	0.012	0.009	0.017	0.015
	(0.037)	(0.036)	(0.035)	(0.033)	(0.032)
Mean (w/o info)	0.710	0.716	0.717	0.713	0.717
C. Share of meals with meat from other terrestrial animals					
Info effect	-0.060**	-0.032	-0.030	-0.031	-0.025
	(0.029)	(0.031)	(0.030)	(0.027)	(0.027)
Mean (w/o info)	0.229	0.217	0.216	0.219	0.216
D. Share of meals without meat from other terrestrial animals					
Info effect	0.063*	0.032	0.027	0.030	0.025
	(0.034)	(0.036)	(0.033)	(0.033)	(0.032)
Mean (w/o info)	0.674	0.688	0.691	0.683	0.687
Baseline level	No	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	No	Yes
Observations	261	261	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.24: Information Effect in the Field over Time

	Considered days after the experiment:			
	0-3	0-7	> 7	all
A. Share of meals with meat				
Info effect	-0.173**	-0.118**	-0.033	-0.066**
	(0.072)	(0.049)	(0.034)	(0.032)
Mean (w/o info)	0.483	0.451	0.434	0.455
B. Share of meals without meat				
Info effect	0.178***	0.113**	0.068	0.089**
	(0.067)	(0.052)	(0.043)	(0.041)
Mean (w/o info)	0.366	0.400	0.415	0.399
Baseline level	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	133	199	248	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP. All estimates are based on the WLS estimator. To estimate propensity scores for the period of days 0–3, we merge WTP groups only consisting of treated or untreated subjects with neighboring WTP groups.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.25: Likelihood to Purchase Food over Time

	Considered days after the experiment:			
	0-3	0-7	> 7	all
A. Any purchase				
Info effect	-0.041	-0.053	-0.006	0.007
	(0.064)	(0.057)	(0.045)	(0.041)
Mean (w/o info)	0.457	0.674	0.807	0.841
B. Purchases per day				
Info effect	-0.012	-0.019	0.002	-0.002
	(0.031)	(0.025)	(0.016)	(0.016)
Mean (w/o info)	0.209	0.264	0.142	0.158
Baseline level	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	312	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP. All estimates are based on the WLS estimator. In Panel A, the dependent variable is whether an individual purchased a meal after the experiment. In Panel B, the dependent variable is the number of meal purchases per day after the experiment (not counting Sundays).
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.26: Information Effect in the Field over Time for Constant Sample

	Considered days after the experiment:			
	0-3	0-7	> 7	all
A. Share of meals with meat				
Info effect	-0.153**	-0.132**	0.005	-0.027
	(0.078)	(0.051)	(0.039)	(0.034)
Mean (w/o info)	0.471	0.449	0.415	0.428
B. Share of meals without meat				
Info effect	0.163**	0.119**	0.025	0.040
	(0.073)	(0.054)	(0.041)	(0.037)
Mean (w/o info)	0.381	0.401	0.452	0.438
Baseline level	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	127	186	186	186

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results).Additional controls are age, gender, degree, location, and WTP. All estimates are based on the WLS estimator. Only subjects who purchase a meal in the first week after the experiment as well as in the subsequent period (week 2 and following) are considered.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.27: Information Effect in the Laboratory – Low and High WTP

	Dependent variable: Choosing voucher for meal with meat		
	IPW	WLS	
	(1)	(2)	(3)
WTP < 0.5			
Info effect	-0.206***	-0.164***	-0.168***
	(0.067)	(0.056)	(0.055)
Mean (w/o info)	0.481	0.463	0.462
WTP ≥ 0.5			
Info effect	-0.055	-0.009	-0.011
	(0.096)	(0.107)	(0.102)
Mean (w/o info)	0.387	0.359	0.361
Comparison of effects (p-value)	0.19	0.20	0.18
Meat-eating habit	No	Yes	Yes
Additional controls	No	No	Yes
Observations	312	312	312

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). To control for the meat-eating habit in columns 2 and 3, we use indicator variables. Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.28: Information Effect in the Field – Low and High WTP

	IPW	WLS	
	(1)	(2)	(3)
A. Share of meals with meat			
WTP < 0.5			
Info effect	-0.125**	-0.048	-0.052
	(0.050)	(0.040)	(0.040)
Mean (w/o info)	0.451	0.419	0.420
WTP ≥ 0.5			
Info effect	-0.122	-0.094*	-0.093
	(0.075)	(0.056)	(0.056)
Mean (w/o info)	0.538	0.523	0.523
Comparison of effects (p-value)	0.97	0.50	0.56
B. Share of meals without meat			
WTP < 0.5			
Info effect	0.167***	0.091**	0.093**
	(0.049)	(0.042)	(0.041)
Mean (w/o info)	0.394	0.423	0.422
WTP ≥ 0.5			
Info effect	0.107	0.086	0.080
	(0.077)	(0.097)	(0.090)
Mean (w/o info)	0.336	0.350	0.354
Comparison of effects (p-value)	0.51	0.97	0.89
Baseline level	No	Yes	Yes
Additional controls	No	No	Yes
Observations	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.29: Information Effect in the Field for Meat from Terrestrial Animals – Avoider and Seeker

	IPW	WLS	
	(1)	(2)	(3)
A. Share of meals with meat from terrestrial animals			
Avoider			
Info effect	-0.088	0.032	0.034
	(0.065)	(0.054)	(0.054)
Mean (w/o info)	0.336	0.289	0.287
Seeker			
Info effect	-0.070	-0.044	-0.045
	(0.046)	(0.036)	(0.036)
Mean (w/o info)	0.387	0.374	0.374
Comparison of effects (p-value)	0.82	0.24	0.23
B. Share of meals without meat from terrestrial animals			
Avoider			
Info effect	0.187***	0.037	0.031
	(0.068)	(0.065)	(0.062)
Mean (w/o info)	0.474	0.531	0.535
Seeker			
Info effect	0.061	0.053	0.051
	(0.050)	(0.052)	(0.047)
Mean (w/o info)	0.481	0.487	0.488
Comparison of effects (p-value)	0.14	0.85	0.79
Baseline level	No	Yes	Yes
Additional controls	No	No	Yes
Observations	261	261	261

Notes: Standard errors are in parentheses. *Mean (w/o info)* corresponds to the counterfactual mean outcome in the absence of information (based on the estimation results). Additional controls are age, gender, degree, location, and WTP.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

A.9 Additional Figures

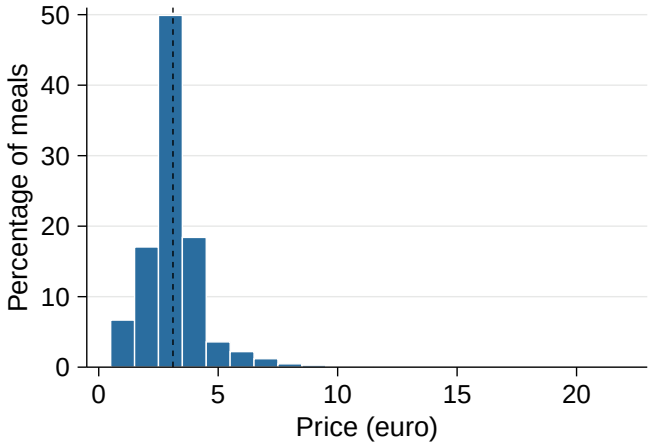


Figure A.5: Distribution of Prices

Notes: The figure shows the distribution of prices for meals. The dashed line represents the average price. The minimum and maximum prices are 0.35 and 22.42 euro, respectively.

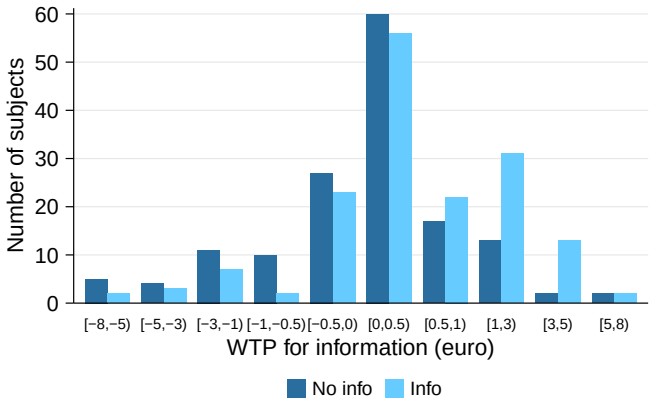


Figure A.6: Treatment Status by WTP for Information

Notes: The figure shows the allocation of subjects with a given WTP into the different treatment groups.

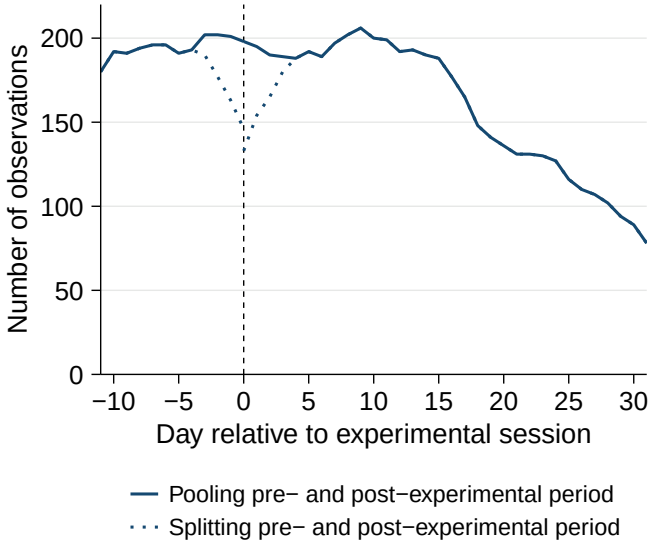


Figure A.7: Observations in Moving Time Window

Notes: The figure shows the number of subjects who purchase at least one meal in the time window of seven days, with the day on the x-axis as the midpoint (±3 days). Day 0 represents the day of the experimental session. For the dotted line, the time windows become shorter around zero since they only capture observations before or after the experiment.

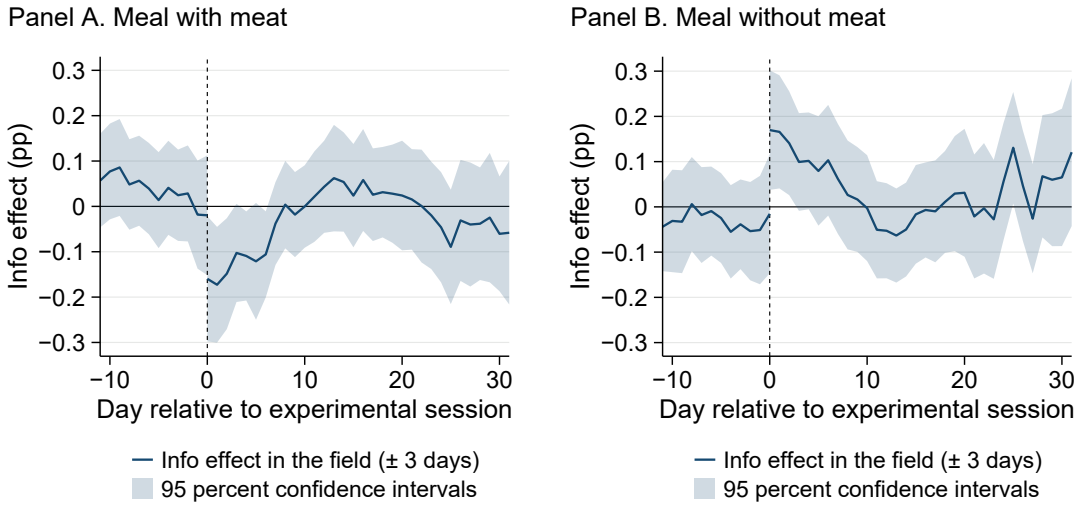


Figure A.8: Effect of Information over Time

Notes: The figure shows the average effect of receiving information on the propensity to eat a meal with meat (Panel A) and to eat a meal without meat (Panel B) over time. Day 0 represents the day of the experimental session. The effect is estimated based on a moving time window of seven days with the day on the x-axis as the midpoint (±3 days). Around day 0, the time window captures only those days before or after the experiment. We use the WLS specification and control for covariates and the meat consumption before day −14 (missing values are imputed as described in Section 1.4.3). To estimate propensity scores, we merge WTP groups only consisting of treated or untreated subjects with neighboring WTP groups.

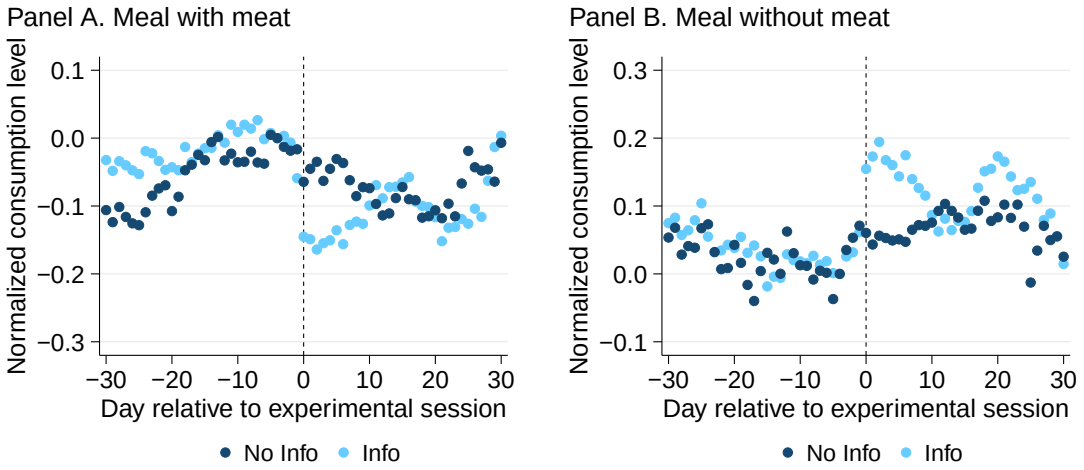


Figure A.9: Moving Average of Meat Consumption

Notes: The figure shows the 7-day moving average level of meat consumption for subjects that do and those that do not receive information, with the day on the x-axis as midpoint (±3 days). Day 0 represents the day of the experimental session. Around day 0, the time window captures only those days before or after the experiment. At day 0, we only show the values for the time window that focuses on the post-experimental period. All values are normalized based on the week before the experiment.

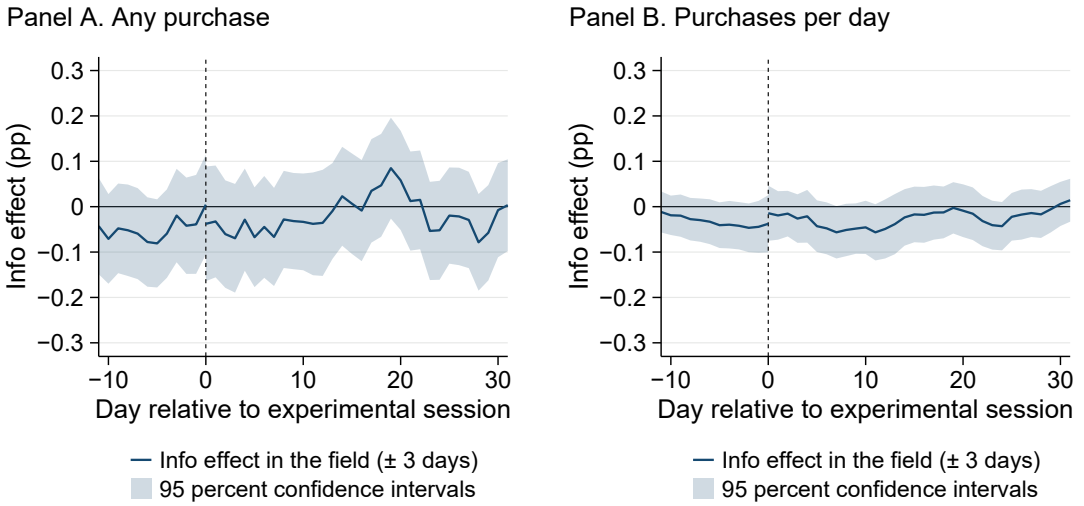


Figure A.10: Likelihood to Purchase Food over Time

Notes: The figure shows the average effect of receiving information on the propensity to purchase a meal (Panel A) and the number of purchases per day (Panel B) over time. Day 0 represents the day of the experimental session. The effect is estimated based on a moving time window of seven days with the day on the x-axis as the midpoint (±3 days). Around day 0, the time window captures only those days before or after the experiment. We use the WLS specification and control for covariates and the meat consumption before day −14 (missing values are imputed as described in Section 1.4.3). To estimate propensity scores, we merge WTP groups only consisting of treated or untreated subjects with neighboring WTP groups.

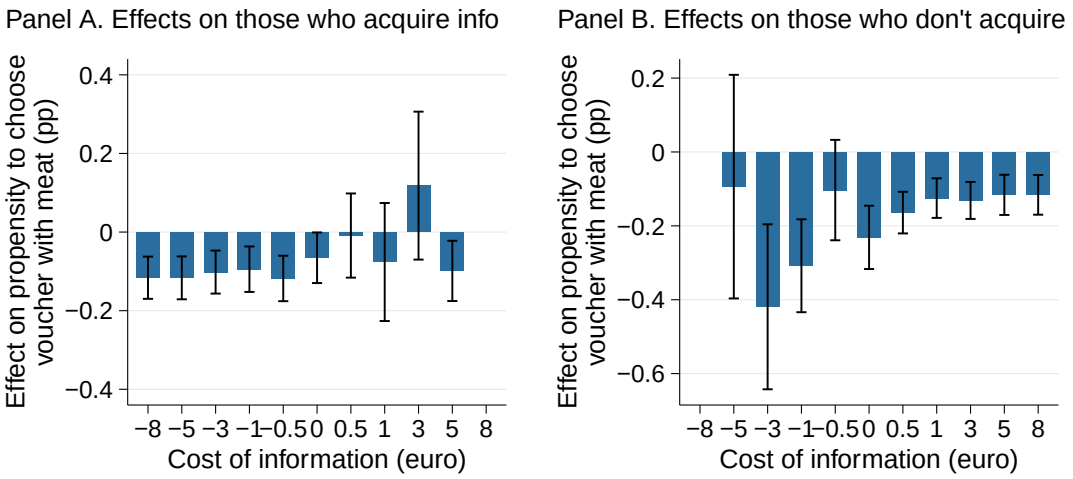


Figure A.11: Information Effects by Selection in the Laboratory

Notes: The figure shows the information effects for individuals who do and those that do not select into information at a given cost (Panel A and Panel B, respectively). The information effects are based on the WLS estimator with choosing the meat voucher as dependent variable and controlling for the stated meat-eating habit. Only individuals whose WTP for information is bounded by −8 and 8 euro are considered. Whiskers show standard errors. When the cost is high (low), the estimated information effect for individuals who (do not) select into information is only based on few observation but is shown nevertheless for illustrative purposes.

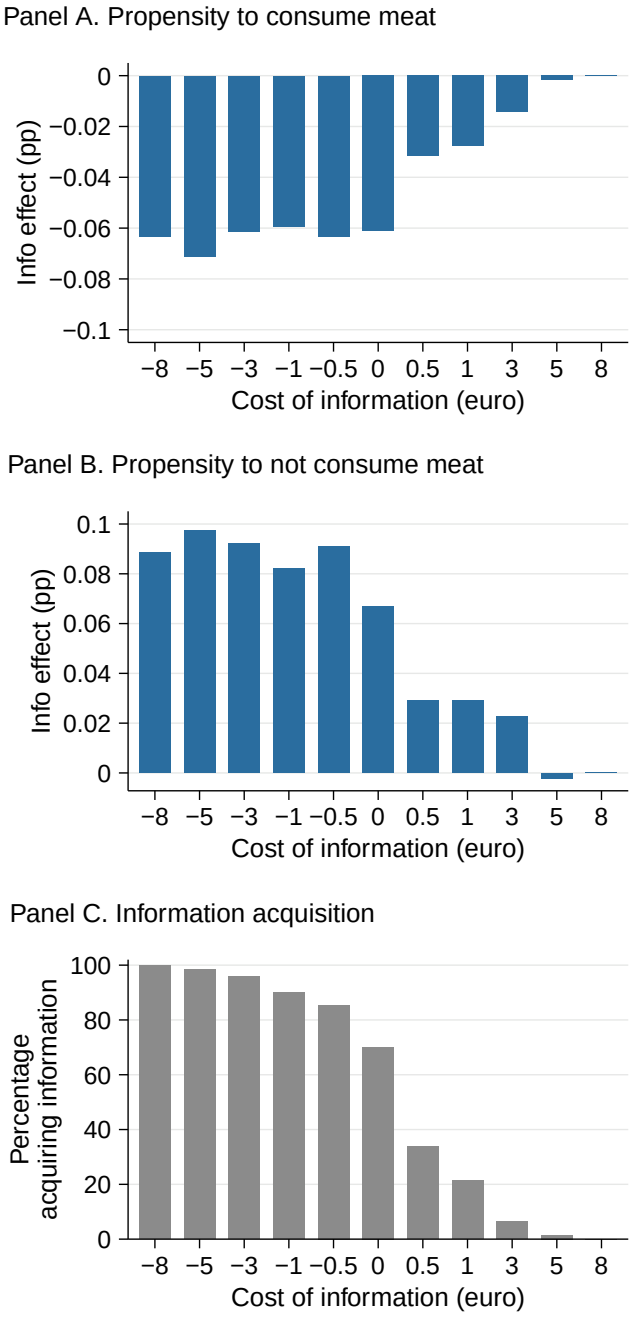


Figure A.12: Effect of Information Provision on Meat Consumption in the Field

Notes: Panel A and Panel B show the average effect of making information available at a given cost. The outcome variable is the share of purchased meals with meat and the share of meals without meat, respectively. The effect at a given cost c is calculated as $ATE(WTP \geq c)\frac{1}{N}\sum_{i=1}^{N}\mathbb{1}\{WTP_i \geq c\}$, where WTP_i denotes the willingness-to-pay of subject i, $ATE(WTP \geq c)$ denotes the average treatment effect of receiving information for subjects with a willingness-to-pay for information at least as large as the cost, $\mathbb{1}\{\cdot\}$ denotes the indicator function, and N denotes the number of subjects. Estimates of $ATE(WTP \geq c)$ are based on the WLS estimator, allowing for heterogeneous information effects based on the selection into information at a given cost and controlling for baseline levels. Panel C shows the fraction of subjects acquiring information at a given cost, $\frac{1}{N}\sum_{i=1}^{N}\mathbb{1}\{WTP_i \geq c\}$. Only individuals whose WTP for information is bounded by -8 and 8 euro are considered.

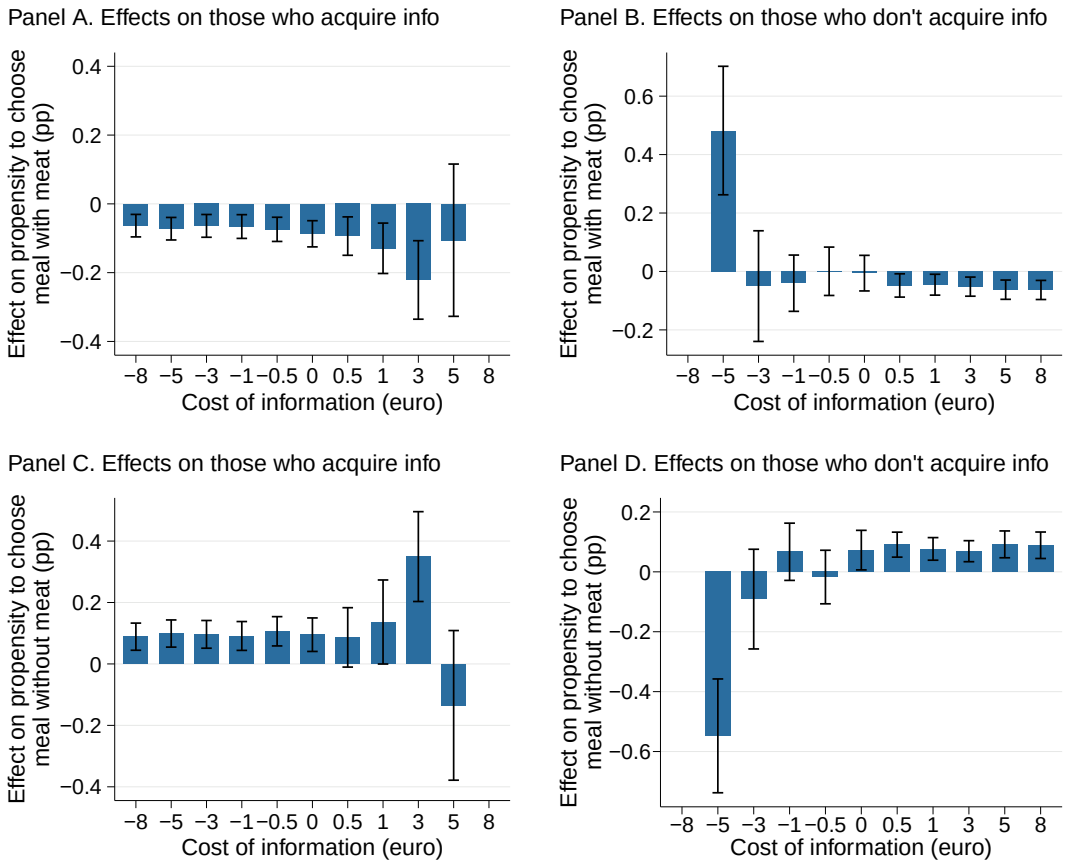


Figure A.13: Information Effects by Selection in the Field

Notes: The figure shows the information effects for individuals who do and those that do not select into information at a given cost (left and right panels, respectively). All estimated information effects are based on the WLS estimator controlling for the baseline level before the experiment. In the top panels, the outcome variable is the share of meals with meat. In the bottom panels, the outcome variable is the share of meals without meat. In all cases, only individuals whose WTP for information is bounded by −8 and 8 euro are considered. Whiskers show standard errors. When the cost is high (low), the estimated information effect for individuals who (do not) select into information is only based on few observation but is shown nevertheless for illustrative purposes.

B Appendix to Chapter 2

B.1 Wording of Donation Appeal

Part I:

As part of this survey, each participant will have the **opportunity to support the provision of nutritious food for malnourished children** in the African country of South Sudan.

[Picture of malnourished child]

Decades of civil war have **devastated South Sudan** and **many children are severely malnourished**. The rations of the nutritious food are supplied by Sign of Hope, an accredited relief organization from Germany cooperating with a hospital in South Sudan.

Sign of Hope holds the **certificate for responsible and transparent use of collected donations** ("Spendenzertifikat") **awarded by the "Deutscher Spendenrat"**, a German umbrella association for charitable organizations. 86 cents of every dollar they receive go directly into the relief efforts, while the remaining fourteen cents cover their overheads. Learn more about the organization at http://www.sign-ofhope.org.

[Picture of malnourished child]

Part II – Money Donation:

The nutritious food consists of a **specially developed paste** and **energy-rich biscuits** that help children gain weight.

In this survey, you may **donate all, part, or none of your reward of $7.00** for this MTurk HIT **to Sign of Hope for providing the nutritious food**. Thus, you may **choose any amount from $0 to $7.00. The amount you choose will be subtracted from your reward.**

Please indicate how much money you wish to donate below:

[Numeric field to indicate dollar amount]

Part II – Unit Donation – Large:

One nutritional ration, which feeds one malnourished child for one week, can be provided by the charity for a donation of $3.50. The nutritious food consists of a **specially developed paste** and **energy-rich biscuits** that help children gain weight.

In this survey, you may **use all, part, or none of your reward of $7.00** for this MTurk HIT **to provide these nutritional rations**. Thus, you may **choose a number from 0 to 2 rations. $3.50 per ration will be subtracted from your reward.**

Please indicate how many rations you wish to provide below:

[Numeric field to indicate number of rations, restricted to 0, 1, and 2]

Part II – Unit Donation – Small:

One nutritional ration, which feeds one malnourished child for one day, can be provided by the charity for a donation of $0.50. The nutritious food consists of a **specially developed paste** and **energy-rich biscuits** that help children gain weight.

In this survey, you may **use all, part, or none of your reward of $7.00** for this MTurk HIT **to provide these nutritional rations**. Thus, you may **choose a number from 0 to 14 rations. $0.50 per ration will be subtracted from your reward.**

Please indicate how many rations you wish to provide below:

[Numeric field to indicate number of rations, restricted to integers between 0 and 14]

Part II – Info:

One nutritional ration, which feeds one malnourished child for one week, can be provided by the charity for a donation of $3.50. The

nutritious food consists of a **specially developed paste** and **energy-rich biscuits** that help children gain weight.

In this survey, you may **donate all, part, or none of your reward of $7.00** for this MTurk HIT **to Sign of Hope for providing the nutritious food**. Thus, you may **choose any amount from $0 to $7.00. The amount you choose will be subtracted from your reward.**

Please indicate how much money you wish to donate below:

[Numeric field to indicate dollar amount]

Part II – Info + Frame:

One nutritional ration, which feeds one malnourished child for one week, can be provided by the charity for a donation of $3.50. The nutritious food consists of a **specially developed paste** and **energy-rich biscuits** that help children gain weight.

In this survey, you may **use all, part, or none of your reward of $7.00 for this MTurk HIT to provide these nutritional rations**. Thus, you may **choose any number from 0 to 2 rations (including fractions). $3.50 per ration (or the appropriate fraction) will be subtracted from your reward.**

Please indicate how many rations you wish to provide below:

[Numeric field to indicate number of rations]

Part II – Info + Restricted:

One nutritional ration, which feeds one malnourished child for one week, can be provided by the charity for a donation of $3.50. The nutritious food consists of a **specially developed paste** and **energy-rich biscuits** that help children gain weight.

In this survey, you may **donate all, part, or none of your reward of $7.00** for this MTurk HIT **to Sign of Hope for providing the nutritious food**. In

particular, you may **choose an amount of \$0, \$3.50, or \$7.00. The amount you choose will be subtracted from your reward.**

Please indicate how much money you wish to donate below:

[Numeric field to indicate dollar amount, restricted to \$0, \$3.50, and \$7.00]

B.2 Additional Tables

Table B.1: Summary Statistics by Treatment

Treatment	Female	Age (years)	Has children	College graduate	Crowd-worker	N
A. Pure schemes						
Money Donation	0.52	37.20	0.47	0.49	0.25	152
	(0.50)	(10.64)	(0.50)	(0.50)	(0.43)	
Unit Donation – Large ($p = \$3.50$)	0.52	37.34	0.50	0.53	0.23	121
	(0.50)	(10.30)	(0.50)	(0.50)	(0.42)	
Unit Donation – Small ($p = \$0.50$)	0.60	37.42	0.48	0.48	0.23	146
	(0.49)	(10.20)	(0.50)	(0.50)	(0.42)	
B. Intermediate schemes						
Info ($p = \$3.50$)	0.52	36.71	0.46	0.53	0.28	150
	(0.50)	(10.68)	(0.50)	(0.50)	(0.45)	
Info + Unit frame ($p = \$3.50$)	0.54	36.33	0.43	0.48	0.19	132
	(0.50)	(10.40)	(0.50)	(0.50)	(0.39)	
Info + Restricted ($p = \$3.50$)	0.55	37.65	0.45	0.44	0.31	146
	(0.50)	(11.34)	(0.50)	(0.50)	(0.47)	
F-test (p-value)	0.75	0.91	0.93	0.71	0.25	

Notes: Standard deviations in parentheses. The number of observations per variable does not always correspond to the number of subjects in the treatment group since we did not force subjects to answer the survey questions. The last row shows the p-value of the F-test for the joint significance of the treatment dummies when regressing the respective variable on the treatment dummies and a constant.

C Appendix to Chapter 3

C.1 Ordered Probit Model

Consider an individual, i, who decides how many units, g_i, of the charitable good to provide. Since the individual has a limited endowment and only complete units of the good can be provided, the individual faces a discrete and ordered choice set. For example, subjects who are assigned to the control condition can give 0, 1, 2, 3 or 4 packages. We estimate an Ordered Probit Model with the individual choice as dependent variable, which is subsequently employed to analyze the effect of the different subsidies on the level of charity receipts. This is possible, since, given a subsidy scheme, the individual choice directly translates into a level of charity receipts. The advantage of this procedure will become clear after explaining the model in more detail.

The model is based on a latent variable

$$g_i^* = x_i'\beta + s_i'\gamma + \epsilon_i \tag{C.1}$$

where x_i is a vector of covariates, including a constant, s_i is a vector consisting of a dummy for each subsidy type as well as subsidy type specific dummies for whether the offered subsidy rate is high and therefore the effective price is low (\$0.25), β and γ are vectors of parameters to be estimated and ϵ_i is an i.i.d. standard normally distributed error term. In general, each of the possible choices an individual can make, $g_i \in \{g^1, ..., g^J\}$, is associated with a certain interval of the latent variable:

$$g_i = g^j \qquad \text{if } \alpha_{j-1} < g_i^* \leq \alpha_j \qquad \text{for } j = 1, ..., J \tag{C.2}$$

where α_0 and α_J are set to $-\infty$ and ∞, respectively, $\alpha_1 = 0$ and $\alpha_2, ..., \alpha_{J-1}$ are threshold parameters to be estimated.

A specific feature of the experimental design that we need to account for in the estimation is that the choice sets differ across treatments. For subjects facing a match, rebate, or no subsidy, each selected unit requires an expenditure of \$0.5. Although in case of the rebate, part of this expenditure is refunded, this refund cannot be donated (similar to most money donation experiments). Since the endowment is \$2, the maximum number of packages that can be selected in

those treatment conditions is four. In contrast, each unit selected in the discount treatments requires an expenditure of only \$0.33 or \$0.25 since the nominal price per unit is discounted upfront. Therefore, subjects can select up to six or eight packages, depending on whether the discount rate is low or high (see Table C.3 in Appendix C.2).

We account for this by adding censoring to the model. Since we do not observe a choice of seven packages in our data, we cannot include this category in the model. Furthermore, only a single subject provided five packages. In our main analysis, we treat this observation as if the subject had donated six packages. Results are similar if we explicitly include the choice category of five packages or omit the observation.[18] Consequently, the choice sets in the following analysis are $g_i \in \{0, 1, 2, 3, 4, 6, 8\}$ for the 50% discount treatment, $g_i \in \{0, 1, 2, 3, 4, 6\}$ for the 33% discount treatment and $g_i \in \{0, 1, 2, 3, 4\}$ for all other treatments. Table C.1 in Appendix C.2 illustrates how the latent variable translates into a certain choice conditional on the treatment.

Let g_i^{max} be the maximum number of packages an individual i can give, which depends on the treatment the individual is assigned to. The probability to observe a choice g_i from the set $\{g^1, ..., g^7\} = \{0, 1, 2, 3, 4, 6, 8\}$ is then given by

$$\begin{aligned} Pr(g_i = g^j | x_i, s_i) = \mathbb{1}\{g^j < g_i^{max}\}\{&\Phi(\alpha_j - x_i'\beta - s_i'\gamma) \\ &- \Phi(\alpha_{j-1} - x_i'\beta - s_i'\gamma)\} \\ &+ \mathbb{1}\{g^j = g_i^{max}\}\{1 - \Phi(\alpha_{j-1} - x_i'\beta - s_i'\gamma)\} \\ &\text{for } j = 1, ..., 7 \end{aligned} \tag{C.3}$$

The parameters $\theta = (\beta, \gamma, \alpha_2, \alpha_3, \alpha_4, \alpha_5, \alpha_6)$ are estimated by maximum likelihood, without and with covariates. The covariates include indicator variables for gender, marital status, whether the individual holds a college degree, whether children under the age of 16 live in the household, whether the individual is a registered voter, whether the individual frequently attends religious services, whether the individual works for a not-for-profit organization, and for task order. We also include categorical variables for age, income, residential environment, and religion as well as scores for the Big Five personality dimensions and risk preferences.

To check for misspecification of the model we use the Lagrange Multiplier test

[18] Results are available from the authors upon request

derived by Glewwe (1997). In both model specifications, the Null of normally distributed error terms cannot be rejected ($p > 0.35$ and $p > 0.90$, without and with covariates, respectively). We also find little evidence that the results are substantively affected by allowing for heteroscedasticity.[19]

We use the estimated coefficients of the model to calculate the average marginal effect of each subsidy on charity receipts. The formulas used to calculate these effects are based on the deterministic relationship between the individual choice and charity receipts. Using the individual choice as dependent variable simplifies the estimation procedure: If the choice sets of any two treatments differ, one is a subset of the other, and the smaller set is simply censored from above. Furthermore, the selected number of packages represents subjects' immediate choice and therefore is probably the most intuitive concept for modeling the decision process. The average marginal effects are calculated as follows. We can explicitly write s_i in equation (C.1) as

$$s_i = \begin{pmatrix} rebate_i \\ match_i \\ discount_i \\ rebate_i \times low\ price_i \\ match_i \times low\ price_i \\ discount_i \times lowprice_i \end{pmatrix},$$

where $rebate_i$, $match_i$, and $discount_i$ are dummies for whether individual i faces a particular subsidy type, $rebate_i \times low\ price_i$, $match_i \times low\ price_i$, and $discount_i \times lowprice_i$ are subsidy type specific dummies indicating whether the subsidy rate is high and the effective price is low (\$0.25).

We use the estimated parameters $\hat{\theta}$ and the deterministic relationship between the selected number of packages g_i (individual's choice) and charity receipts cr_i to

[19] We expand the model in column 2 of Table 3.4 by modeling the variance as $exp(z_i'\rho)$. We estimate this model with different sets of covariates included in z_i. Set 1 includes age, income, gender, whether the individual frequently attends religious services, and task order. Set 2 additionally contains the Big Five personality dimensions and risk preferences. Set 3 includes all covariates. Only if we use the whole set of covariates to explicitly model heteroscedasticity, the model with homoscedasticity is rejected ($p < 0.01$). Still, rebates and matches do not significantly differ in the level of charity receipts at the low subsidy rate, but the difference approaches marginal significance at the high subsidy rate (where charity receipts are higher under matches). One should be careful with relying on this expanded model specification as it is sensitive to small changes in the set of covariates included. Results are available from the authors upon request.

calculate the expected level of charity receipts $\hat{E}_i$ that is predicted by the model for each individual under each treatment condition. For example, to receive individual i's expected level of charity receipts under the 50% rebate we set $s_{i,r25} = (1, 0, 0, 1, 0, 0)'$ – the subscript $r25$ indicates the subsidy type and the implied effective price in cents – and calculate the predicted value of the latent variable according to equation (C.1):

$$\hat{g}^*_{i,r25} = x_i'\hat{\beta} + s_{i,r25}'\hat{\gamma}$$

Afterwards, we estimate the expected level of charity receipts by

$$\begin{aligned}\hat{E}_{i,r25} &= \sum_{k=0}^{8} k\hat{P}(cr_i = k|x_i, s_i = s_{i,r25}) = \sum_{k=0}^{4} k\hat{P}(g_i = k|x_i, s_i = s_{i,r25}) \\ &= 4 - \Phi(\hat{a}_4 - \hat{g}^*_{i,r25}) - \Phi(\hat{a}_3 - \hat{g}^*_{i,r25}) - \Phi(\hat{a}_2 - \hat{g}^*_{i,r25}) - \Phi(-\hat{g}^*_{i,r25})\end{aligned}$$

where $\hat{P}(cr_i = k|x_i, s_i = s_{i,r25}) = 0$ for $k > 4$ since the maximum level of charity receipts under the rebate is four. The second equality then follows from the fact that for all treatment conditions except matches, the individual choice (in physical units) is equal to the level of charity receipts (in physical units). The last equality follows from using equation (C.3) to calculate $\hat{P}(y_i = k|x_i, s_i = s_{i,r25})$. The expected levels of charity receipts for the other six conditions (no subsidy, 33% rebate, 1:2 match, 1:1 match, 33% discount, and 50% discount) are accordingly calculated as

$$\begin{aligned}\hat{E}_{i,n50} &= 4 - \Phi(\hat{a}_4 - \hat{g}^*_{i,n50}) - \Phi(\hat{a}_3 - \hat{g}^*_{i,n50}) - \Phi(\hat{a}_2 - \hat{g}^*_{i,n50}) - \Phi(-\hat{g}^*_{i,n50}) \\ \hat{E}_{i,r33} &= 4 - \Phi(\hat{a}_4 - \hat{g}^*_{i,r33}) - \Phi(\hat{a}_3 - \hat{g}^*_{i,r33}) - \Phi(\hat{a}_2 - \hat{g}^*_{i,r33}) - \Phi(-\hat{g}^*_{i,r33}) \\ \hat{E}_{i,m33} &= 6 - 2\Phi(\hat{a}_4 - \hat{g}^*_{i,m33}) - \Phi(\hat{a}_3 - \hat{g}^*_{i,m33}) - 2\Phi(\hat{a}_2 - \hat{g}^*_{i,m33}) - \Phi(-\hat{g}^*_{i,m33}) \\ \hat{E}_{i,m25} &= 8 - 2\Phi(\hat{a}_4 - \hat{g}^*_{i,m25}) - 2\Phi(\hat{a}_3 - \hat{g}^*_{i,m25}) - 2\Phi(\hat{a}_2 - \hat{g}^*_{i,m25}) - 2\Phi(-\hat{g}^*_{i,m25}) \\ \hat{E}_{i,d33} &= 6 - 2\Phi(\hat{a}_5 - \hat{g}^*_{i,d33}) - \Phi(\hat{a}_4 - \hat{g}^*_{i,d33}) - \Phi(\hat{a}_3 - \hat{g}^*_{i,d33}) - \Phi(\hat{a}_2 - \hat{g}^*_{i,d33}) \\ &\quad - \Phi(-\hat{g}^*_{i,d33}) \\ \hat{E}_{i,d25} &= 8 - 2\Phi(\hat{a}_6 - \hat{g}^*_{i,d25}) - 2\Phi(\hat{a}_5 - \hat{g}^*_{i,d25}) - \Phi(\hat{a}_4 - \hat{g}^*_{i,d25}) - \Phi(\hat{a}_3 - \hat{g}^*_{i,d25}) \\ &\quad - \Phi(\hat{a}_2 - \hat{g}^*_{i,d25}) - \Phi(-\hat{g}^*_{i,d25})\end{aligned}$$

We use the expected level of charity receipts to calculate average marginal effects (AMEs) for introducing a subsidy type at the low rate (rebate, match, discount)

and for changing the subsidy rate for a specific subsidy type from low to high (rebate × low price, match × low price, discount × low price):

$$AME_{rebate} = \frac{1}{N}\sum_{i=1}^{N} \hat{E}_{i,r33} - \hat{E}_{i,n50}$$

$$AME_{match} = \frac{1}{N}\sum_{i=1}^{N} \hat{E}_{i,m33} - \hat{E}_{i,n50}$$

$$AME_{discount} = \frac{1}{N}\sum_{i=1}^{N} \hat{E}_{i,d33} - \hat{E}_{i,n50}$$

$$AME_{rebate\ \times\ low\ price} = \frac{1}{N}\sum_{i=1}^{N} \hat{E}_{i,r25} - \hat{E}_{i,r33}$$

$$AME_{match\ \times\ low\ price} = \frac{1}{N}\sum_{i=1}^{N} \hat{E}_{i,m25} - \hat{E}_{i,m33}$$

$$AME_{discount\ \times\ low\ price} = \frac{1}{N}\sum_{i=1}^{N} \hat{E}_{i,d25} - \hat{E}_{i,d33}$$

These average marginal effects are presented in column 1 and 2 of Table 3.4. Standard errors are calculated based on the delta method.

C.2 Additional Figures and Tables

Table C.1: Latent Variable and Individual Choice

	Individual choice (g_i) in ...		
Latent variabel g_i^*	no subsidy control, 33% rebate, 1:2 match, 50% rebate, 1:1 match	33% discount	50% discount
$(-\infty, 0]$	0	0	0
$(0, \alpha_2]$	1	1	1
$(\alpha_2, \alpha_3]$	2	2	2
$(\alpha_3, \alpha_4]$	3	3	3
$(\alpha_4, \alpha_5]$	4	4	4
$(\alpha_5, \alpha_6]$	4	6	6
(α_6, ∞)	4	6	8

Table C.2: Summary Statistics

Variable	Full sample			Control			33% rebate			1:2 match			33% discount			50% rebate			1:1 match			50% discount			F-test
	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ	N	p-value
Female	0.48	0.50	558	0.45	0.50	83	0.49	0.50	71	0.48	0.50	85	0.44	0.50	90	0.55	0.50	58	0.44	0.50	80	0.53	0.50	91	0.75
Age (years):																									
18–25	0.25	0.44	558	0.22	0.41	83	0.28	0.45	71	0.26	0.44	85	0.27	0.44	90	0.36	0.48	58	0.28	0.45	80	0.16	0.37	91	0.17
26–34	0.37	0.48	558	0.39	0.49	83	0.38	0.49	71	0.34	0.48	85	0.37	0.48	90	0.31	0.47	58	0.40	0.49	80	0.41	0.49	91	0.90
35–54	0.29	0.46	558	0.29	0.46	83	0.30	0.46	71	0.34	0.48	85	0.29	0.46	90	0.24	0.43	58	0.23	0.42	80	0.35	0.48	91	0.53
$\geq$ 55	0.08	0.27	558	0.11	0.31	83	0.04	0.20	71	0.06	0.24	85	0.08	0.27	90	0.09	0.28	58	0.10	0.30	80	0.08	0.27	91	0.70
Married	0.34	0.47	554	0.35	0.48	82	0.30	0.46	71	0.30	0.46	84	0.38	0.49	90	0.31	0.47	58	0.28	0.45	79	0.41	0.49	90	0.49
Children[a]	0.30	0.46	558	0.34	0.48	83	0.25	0.44	71	0.29	0.46	85	0.24	0.43	90	0.31	0.47	58	0.28	0.45	80	0.40	0.49	91	0.37
College degree	0.47	0.50	558	0.49	0.50	83	0.46	0.50	71	0.49	0.50	85	0.43	0.50	90	0.47	0.50	58	0.44	0.50	80	0.48	0.50	91	0.97
Income[b] (US$):																									
<10,000	0.09	0.28	540	0.07	0.26	81	0.10	0.31	68	0.07	0.26	84	0.10	0.31	86	0.07	0.26	56	0.14	0.35	77	0.06	0.23	88	0.62
<10,000–19,999	0.11	0.31	540	0.09	0.28	81	0.09	0.29	68	0.13	0.34	84	0.10	0.31	86	0.11	0.31	56	0.13	0.34	77	0.11	0.32	88	0.96
<20,000–29,999	0.12	0.33	540	0.14	0.34	81	0.16	0.37	68	0.08	0.28	84	0.08	0.28	86	0.07	0.26	56	0.10	0.31	77	0.19	0.40	88	0.20
<30,000–39,999	0.11	0.32	540	0.10	0.30	81	0.12	0.32	68	0.19	0.40	84	0.09	0.29	86	0.11	0.31	56	0.09	0.29	77	0.10	0.30	88	0.63
<40,000–49,999	0.15	0.35	540	0.05	0.22	81	0.12	0.32	68	0.18	0.39	84	0.19	0.39	86	0.18	0.39	56	0.21	0.41	77	0.11	0.32	88	0.01
<50,000–74,999	0.21	0.41	540	0.33	0.47	81	0.13	0.34	68	0.15	0.36	84	0.22	0.42	86	0.21	0.41	56	0.18	0.39	77	0.24	0.43	88	0.08
<75,000–99,999	0.09	0.28	540	0.07	0.26	81	0.09	0.29	68	0.08	0.28	84	0.12	0.32	86	0.14	0.35	56	0.06	0.25	77	0.07	0.25	88	0.76
$\geq$ 100,000	0.12	0.32	540	0.15	0.36	81	0.19	0.40	68	0.11	0.31	84	0.09	0.29	86	0.11	0.31	56	0.08	0.27	77	0.11	0.32	88	0.52
Residential environment:																									
Rural	0.20	0.40	558	0.16	0.37	83	0.20	0.40	71	0.22	0.42	85	0.21	0.41	90	0.12	0.33	58	0.24	0.43	80	0.22	0.42	91	0.48
Suburban	0.51	0.50	558	0.60	0.49	83	0.56	0.50	71	0.41	0.50	85	0.50	0.50	90	0.53	0.50	58	0.45	0.50	80	0.53	0.50	91	0.21
Urban	0.29	0.45	558	0.24	0.43	83	0.24	0.43	71	0.36	0.48	85	0.29	0.46	90	0.34	0.48	58	0.31	0.47	80	0.25	0.44	91	0.46
Registered voter	0.86	0.34	552	0.83	0.38	82	0.91	0.28	70	0.85	0.36	84	0.82	0.39	89	0.86	0.35	58	0.89	0.32	79	0.89	0.32	90	0.53
Not-for-profit[c]	0.06	0.23	558	0.02	0.15	83	0.03	0.17	71	0.08	0.28	85	0.04	0.21	90	0.07	0.26	58	0.10	0.30	80	0.05	0.23	91	0.31
Religious[d]	0.13	0.34	548	0.07	0.26	82	0.10	0.30	71	0.17	0.38	82	0.15	0.36	86	0.10	0.31	58	0.14	0.35	80	0.16	0.37	89	0.38
Religion:																									
Atheist	0.38	0.49	533	0.37	0.49	81	0.40	0.49	65	0.42	0.50	83	0.46	0.50	85	0.32	0.47	57	0.33	0.47	75	0.32	0.47	87	0.44
Agostic	0.08	0.28	533	0.10	0.30	81	0.14	0.35	65	0.07	0.26	83	0.07	0.26	85	0.05	0.23	57	0.07	0.25	75	0.08	0.27	87	0.76
Roman-Catholic	0.12	0.32	533	0.10	0.30	81	0.12	0.33	65	0.06	0.24	83	0.07	0.26	85	0.14	0.35	57	0.16	0.37	75	0.17	0.38	87	0.15
Protestant	0.18	0.38	533	0.12	0.33	81	0.12	0.33	65	0.23	0.42	83	0.20	0.40	85	0.25	0.43	57	0.16	0.37	75	0.16	0.37	87	0.34
Other Christian	0.13	0.33	533	0.15	0.36	81	0.11	0.31	65	0.13	0.34	83	0.07	0.26	85	0.09	0.29	57	0.17	0.38	75	0.15	0.36	87	0.36
Other	0.12	0.33	533	0.16	0.37	81	0.11	0.31	65	0.08	0.28	83	0.13	0.34	85	0.16	0.37	57	0.11	0.31	75	0.11	0.32	87	0.78
Big Five (scale 1–7):																									
Extraversion	3.18	1.60	520	3.14	1.55	77	3.00	1.41	67	3.42	1.69	77	3.07	1.66	85	3.41	1.69	54	3.14	1.57	74	3.16	1.60	86	0.67
Agreeableness	5.04	1.24	523	4.92	1.25	77	4.94	1.16	68	5.14	1.35	81	4.77	1.32	86	5.28	1.04	53	5.23	1.05	74	5.08	1.33	84	0.11
Conscientioussn.	5.13	1.30	525	5.04	1.38	78	5.01	1.40	68	5.05	1.33	81	5.15	1.24	85	5.46	1.04	56	5.13	1.32	75	5.16	1.33	82	0.35
Emotional stab.	4.62	1.53	531	4.57	1.38	79	4.62	1.44	68	4.63	1.65	80	4.42	1.64	86	4.73	1.58	56	4.78	1.55	76	4.66	1.46	86	0.86
Openness	4.67	1.29	532	4.71	1.15	80	4.67	1.22	69	4.77	1.21	77	4.86	1.21	87	4.73	1.20	56	4.76	1.50	76	4.25	1.40	87	0.10
Risk pref. (scl. 1–6)	4.06	1.78	554	3.96	1.82	83	4.25	1.80	71	4.05	1.77	84	4.18	1.73	89	3.83	1.74	58	4.20	1.75	80	3.94	1.87	89	0.76
Task order[e]	0.52	0.50	558	0.43	0.50	83	0.52	0.50	71	0.45	0.50	85	0.58	0.50	90	0.59	0.50	58	0.59	0.50	80	0.53	0.50	91	0.24
Manipulationon check questions(scale 1-5):																									
Clarity[f]	4.58	0.67	551	4.56	0.69	82	4.54	0.70	70	4.60	0.60	84	4.57	0.75	89	4.60	0.60	55	4.64	0.53	80	4.55	0.79	91	0.97
Anonymity[g]	4.49	0.72	551	4.48	0.63	82	4.56	0.71	70	4.46	0.72	84	4.51	0.76	89	4.64	0.56	55	4.47	0.67	80	4.41	0.89	91	0.52
Trust exp.[h]	4.04	0.93	549	4.08	0.87	80	4.07	1.02	71	4.13	0.92	84	4.02	0.97	88	4.02	0.82	56	4.05	0.95	79	3.93	0.96	91	0.90
Trust charity[i]	4.13	0.90	550	4.17	0.78	80	4.17	0.93	71	4.24	0.89	84	4.01	1.02	89	4.09	0.86	56	4.18	0.87	79	4.08	0.93	91	0.75
Deserving recip.[j]	4.48	0.82	549	4.47	0.74	81	4.43	0.94	70	4.52	0.75	84	4.40	0.91	89	4.71	0.53	55	4.58	0.63	79	4.32	0.99	91	0.03

Notes: The last column reports p-values from regressing each variable on treatment dummies and conducting an F-test for the joint significance of the regressors. [a] Has children under age 16 living in household. [b] Household income. [c] Works for a not-for-profit organization. [d] Frequently attends religious services. [e] 1 if the subject encountered the donation task after the questionnaire, 0 if before. [f] "The instructions, questions, and tasks in this survey were clear and easy to understand." [g] "The procedures followed in this experiment preserved your anonymity." [h] "The money you donated to the charity will be given to the charity." [i] "The charity will use the money to provide the chosen number of nutrition packages." [j] "The recipients of the donations are deserving of support."

Table C.3: Choice Set by Treatment

Treatment	Individual choice [units]	Corresponding net donations [$]	Corresponding charity receipts [units]
No subsidy	0	0	0
	1	0.5	1
	2	1	2
	3	1.5	3
	4	2	4
33% rebate	0	0	0
	1	0.33	1
	2	0.66	2
	3	0.99	3
	4	1.32	4
1:2 match	0	0	0
	1	0.5	1
	2	1	3
	3	1.5	4
	4	2	6
33% discount	0	0	0
	1	0.33	1
	2	0.66	2
	3	0.99	3
	4	1.32	4
	5	1.65	5
	6	1.98	6
50% rebate	0	0	0
	1	0.25	1
	2	0.5	2
	3	0.75	3
	4	1	4
1:1 match	0	0	0
	1	0.5	2
	2	1	4
	3	1.5	6
	4	2	8
50% discount	0	0	0
	1	0.25	1
	2	0.5	2
	3	0.75	3
	4	1	4
	5	1.25	5
	6	1.5	6
	7	1.75	7
	8	2	8

Table C.4: Robustness Check for Charity Receipts Censored at 4 Packages

Treatment	Charity receipts	
	unconditional (units)	conditional (units)
	(1)	(2)
A. Mean values (S.D.)		
No subsidy	1.169	2.256
	(1.413)	(1.177)
33% rebate	1.690	2.400
	(1.545)	(1.294)
1:2 match	1.271	2.571
	(1.538)	(1.192)
33% discount	1.233	2.313
	(1.446)	(1.188)
50% rebate	1.931	2.732
	(1.705)	(1.379)
1:1 match	1.800	3.064
	(1.702)	(1.009)
50% discount	1.495	2.473
	(1.615)	(1.372)
B. Tests of subsidy types: p-values		
B1. At effective price of $0.33		
33% rebate vs. 1:2 match	0.09	0.51
33% rebate vs. 33% discount	0.06	0.73
1:2 match vs. 33% discount	0.87	0.31
B2. At effective price of $0.25		
50% rebate vs. 1:1 match	0.65	0.21
50% rebate vs. 50% discount	0.12	0.36
1:1 match vs. 50% discount	0.23	0.01
C. Tests of subsidized prices: p-values		
50% vs. 33% rebate	0.41	0.24
1:1 vs. 1:2 match	0.04	0.04
50% vs. 33% discount	0.25	0.53
D. Tests of subsidized vs. unsubsidized prices: p-values		
D1. Low subsidy rate		
33% rebate vs. no subsidy	0.03	0.58
1:2 match vs. no subsidy	0.66	0.22
33% discount vs. no subsidy	0.77	0.82
D2. High subsidy rate		
50% rebate vs. no subsidy	0.01	0.09
1:1 match vs. no subsidy	0.01	0.00
50% discount vs. no subsidy	0.16	0.40

Notes: Panel A shows mean values of the donation variables for each treatment (standard deviations in parentheses). Column 1 shows unconditional charity receipts with each number of packages above four recoded to four. Column 2 shows the corresponding numbers for charity receipts conditional on being a donor. Shown in panels B and C are p-values of two-tailed t-tests with unequal variances.

C.3 Instructions

In this survey, each participant will have the opportunity to provide nutritional packages for malnourished children in the African country of South Sudan.

Decades of civil war have ravaged South Sudan, and many children are severely malnourished. The nutritional packages will be delivered by Sign of Hope, an accredited relief organization that operates two hospitals in South Sudan.

In 2010, Sign of Hope won the Transparency Award for German non-profit organizations. Eighty cents out of every dollar they receive go directly to relief efforts, while the remaining twenty cents cover their overhead costs. You can read more about the organization on http://www.hoffnungszeichen.de/sign-of-hope-africa.html.

One nutritional package, which feeds one malnourished child for one day, can be provided by the charity for a donation of $0.50. The package consists of a specially designed paste and high-energy cookies to help the children gain weight.

In this survey, you will be able to provide these nutritional packages for **$0.25** apiece (a third party will fund the remaining $0.25). You may use all, part, or none of your reward of $2.00 for this HIT to provide packages. Thus, you may choose any number between 0 and 8 packages. $0.25 per package will be subtracted from your reward.

Please indicate your choice below:

Figure C.1: Example Donation Appeal, 50% Discount Treatment. The Final Paragraph Differed between Treatments.

C.4 Within-Subjects Design

If the results of the within-subjects (WS) design mirrored the results of the between-subjects (BS) design, the WS variation could be used to learn more about how those results come about. However, as we will show, the results of the WS design substantially differ. Although under these circumstances we rank the external validity of the BS design higher, the WS data can provide insights into subjects' decision process when they are forced to compare different conditions.

In the experiment, 146 subjects were randomly assigned to the WS treatment in which all seven treatment conditions were jointly displayed on the donation call page in random order (Figure C.2). Instructions informed subjects that one of the conditions would be randomly selected through a lottery and implemented. Subjects then entered, for each condition, an integer number indicating their desired number of units. 119 subjects completed the survey of which 6 were subsequently removed because of duplicate IP addresses. Table C.5 shows summary statistics of the sample, including p-values from pairwise comparisons with the sample that was assigned to the BS design.

Table C.6 presents the main results analogously to Table 3.3. Beginning with the unconditional level of charity receipts in column 3, we observe that under a WS design, matches and discounts are more effective in providing the charitable good than rebates. This finding is most pronounced for the low price of \$0.25. Potentially, the discontinuities in the match – the first and third unit funded not resulting in an additional matched unit – discourage giving at the effective price of \$0.33. For net donations, we observe in column 2 that introducing matches and discounts significantly crowds in net donations while an increase in the rebate rate induces crowding-out.

At first glance the discrepancy in results compared to the BS sample might come as a surprise, but a closer look at the extensive and intensive margins in columns 4 and 5 offers a simple explanation for most differences. Unlike in the BS design, we find that for a given effective price, subsidies are equally successful in attracting donors at the extensive margin. We speculate that under a WS design, subjects may not decide whether to donate for each subsidy separately, but rather make a single participation choice across all subsidies with a similar rate and then respond to the subsidy type mostly at the intensive margin. This explanation is also supported by looking at the decision of whether to give at the individual

In this survey, each participant will have the opportunity to provide nutritional packages for malnourished children in the African country of South Sudan.

Decades of civil war have ravaged South Sudan, and many children are severely malnourished. The nutritional packages will be delivered by Sign of Hope, an accredited relief organization that operates two hospitals in South Sudan.

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One nutritional package, which feeds one malnourished child for one day, can be provided by the charity for a donation of $0.50. The package consists of a specially designed paste and high-energy cookies to help the children gain weight.

In this survey, you may use all, part, or none of your reward of $2.00 for this HIT to provide these nutrition packages.

Below, you find seven different price conditions. For each condition, please indicate the number of packages you choose to provide under this condition.

One of the seven price conditions will be implemented in real. This condition will be randomly selected through a lottery after this survey. So, since each case may become real with equal probability, you will want to answer each condition as if it is the condition that will be implemented .

Your choice:

Condition C:
In this condition, you will be able to provide these nutritional packages for **$0.50 apiece**. Thus, you may choose any number between 0 and 4 packages. $0.50 per package will be subtracted from your reward.

Condition E:
In this condition, you will be able to provide these nutritional packages for **$0.33** apiece (a third party will fund the remaining $0.17). Thus, you may choose any number between 0 and 6 packages. $0.33 per package will be subtracted from your reward.

Condition D:
In this condition, you will be able to provide these nutritional packages for **$0.25** apiece (a third party will fund the remaining $0.25). Thus, you may choose any number between 0 and 8 packages. $0.25 per package will be subtracted from your reward.

Condition R:
In this condition, you will be able to provide these nutritional packages for $0.50 apiece. Thus, you may choose any number between 0 and 4 packages. $0.50 per package will be subtracted from your reward. A third party has agreed to fund a **33% rebate** for each package you provide. Upon completion of the survey, the rebate ($0.17 per package provided) will be added to your reward.

Condition S:
In this condition, you will be able to provide these nutritional packages for $0.50 apiece. Thus, you may choose any number between 0 and 4 packages. $0.50 per package will be subtracted from your reward. A third party has agreed to fund a **50% rebate** for each package you provide. Upon completion of the survey, the rebate ($0.25 per package provided) will be added to your reward.

Condition M:
In this condition, you will be able to provide these nutritional packages for $0.50 apiece. Thus, you may choose any number between 0 and 4 packages. $0.50 per package will be subtracted from your reward. A third party has agreed to **match every two packages you provide**, at no additional cost to you. So, for example, if you choose to provide 2 packages, Sign of Hope will receive 3.

Condition N:
In this condition, you will be able to provide these nutritional packages for $0.50 apiece. Thus, you may choose any number between 0 and 4 packages. $0.50 per package will be subtracted from your reward. A third party has agreed to **match each package you provide**, at no additional cost to you. So, for example, if you choose to provide 2 packages, Sign of Hope will receive 4.

Figure C.2: Donation Appeal in the WS Design

level: 91 and 82 percent of individuals make the same decision of whether to give across the different subsidy types at the high and low subsidy rate, respectively. In contrast, the introduction of a subsidy and the height of its rate seem to be highly relevant for the participation decision, regardless of its type. This behavior is very different from the one observed in the BS design and likely to be affected by demand effects from "nudging" subjects to compare options in the WS design (Charness et al., 2012). We therefore follow the literature and ascribe higher external validity to the results in the between-subjects design.

Table C.5: Summary Statistics for BS Sample, WS Sample, and Combined Sample

Variable	Combined sample			BS sample			WS sample			Comparison
	Mean	SD	*N*	Mean	SD	*N*	Mean	SD	*N*	*p*-value
Female	0.48	0.50	671	0.48	0.50	558	0.46	0.50	113	0.70
Age (years):										
18–25	0.25	0.43	671	0.25	0.44	558	0.21	0.41	113	0.34
26–34	0.38	0.49	671	0.37	0.48	558	0.43	0.50	113	0.23
35–54	0.29	0.45	671	0.29	0.46	558	0.27	0.45	113	0.68
≥ 55	0.08	0.27	671	0.08	0.27	558	0.08	0.27	113	0.98
Married	0.33	0.47	667	0.34	0.47	554	0.27	0.45	113	0.20
Children[a]	0.30	0.46	671	0.30	0.46	558	0.26	0.44	113	0.33
College degree	0.48	0.50	670	0.47	0.50	558	0.54	0.50	112	0.19
Income[b] (US$):										
<10,000	0.09	0.28	649	0.09	0.28	540	0.07	0.26	109	0.60
10,000–19,999	0.11	0.31	649	0.11	0.31	540	0.11	0.31	109	0.98
20,000–29,999	0.12	0.33	649	0.12	0.33	540	0.14	0.35	109	0.62
30,000–39,999	0.13	0.34	649	0.11	0.32	540	0.20	0.40	109	0.01
40,000–49,999	0.15	0.35	649	0.15	0.35	540	0.15	0.36	109	0.99
50,000–74,999	0.20	0.40	649	0.21	0.41	540	0.12	0.33	109	0.03
75,000–99,999	0.09	0.29	649	0.09	0.28	540	0.09	0.29	109	0.92
$\geq 100,000$	0.12	0.32	649	0.12	0.32	540	0.12	0.33	109	0.98
Residential environment:										
Rural	0.20	0.40	671	0.20	0.40	558	0.19	0.39	113	0.75
Suburban	0.52	0.50	671	0.51	0.50	558	0.54	0.50	113	0.57
Urban	0.29	0.45	671	0.29	0.45	558	0.27	0.45	113	0.73
Registered voter	0.87	0.34	663	0.86	0.34	552	0.88	0.32	111	0.56
Not-for-profit[c]	0.05	0.22	671	0.06	0.23	558	0.03	0.16	113	0.18
Religious[d]	0.13	0.34	659	0.13	0.34	548	0.14	0.34	111	0.87
Religion:										
Atheist	0.37	0.48	643	0.38	0.49	533	0.36	0.48	110	0.79
Agostic	0.09	0.28	643	0.08	0.28	533	0.10	0.30	110	0.55
Roman-Catholic	0.12	0.32	643	0.12	0.32	533	0.14	0.34	110	0.56
Protestant	0.18	0.38	643	0.18	0.38	533	0.17	0.38	110	0.93
Other Christian	0.12	0.33	643	0.13	0.33	533	0.11	0.31	110	0.63
Other	0.12	0.33	643	0.12	0.33	533	0.12	0.32	110	0.91
Big Five (scale 1–7):										
Extraversion	3.21	1.62	626	3.18	1.60	520	3.32	1.73	106	0.45
Agreeableness	5.02	1.23	628	5.04	1.24	523	4.91	1.20	105	0.34
Conscientiousness	5.11	1.29	630	5.13	1.30	525	4.97	1.24	105	0.23
Emotional stability	4.65	1.52	638	4.62	1.53	531	4.79	1.49	107	0.30
Openness	4.70	1.29	640	4.67	1.29	532	4.83	1.32	108	0.25
Risk pref. (scale 1–6)	4.00	1.79	667	4.06	1.78	554	3.65	1.79	113	0.03
Task order[e]	0.51	0.50	671	0.52	0.50	558	0.46	0.50	113	0.22
Manipulation check questions (scale 1-5):										
Clarity[f]	4.56	0.68	663	4.58	0.67	551	4.46	0.70	112	0.11
Anonymity[g]	4.48	0.73	663	4.49	0.72	551	4.43	0.78	112	0.42
Trust experiment[h]	4.04	0.93	660	4.04	0.93	549	4.01	0.93	111	0.73
Trust charity[i]	4.13	0.90	662	4.13	0.90	550	4.13	0.92	112	0.94
Deserving recipients[j]	4.47	0.81	661	4.48	0.82	549	4.42	0.79	112	0.49

Notes: The last column reports *p*-values from comparing each variable across the two samples based on a χ^2-test if the variable is binary and a *t*-test with unequal variances if the variable is not binary. [a]Has children under age 16 living in household. [b]Household income. [c]Works for a not-for-profit organization. [d]Frequently attends religious services. [e]1 if the subject encountered the donation task after the questionnaire, 0 if before. [f]"The instructions, questions, and tasks in this survey were clear and easy to understand." [g]"The procedures followed in this experiment preserved your anonymity." [h]"The money you donated to the charity will be given to the charity." [i]"The charity will use the money to provide the chosen number of nutrition packages." [j]"The recipients of the donations are deserving of support."

Table C.6: Descriptive Results, Within-Subjects Design

Treatments			Donation variable				
Condition	Nominal unit price ($)	Effective unit price ($)	Individual choice (units)	Net donation ($)	Charity receipt, uncond. (units)	Charity receipt, cond. (units)	Prob. of donation
			(1)	(2)	(3)	(4)	(5)
A. Mean values (S.D.)							
No subsidy	0.50	0.50	0.558 (1.026)	0.279 (0.513)	0.558 (1.026)	1.750 (1.105)	0.319 (0.468)
33% rebate	0.50	0.33	0.867 (1.278)	0.286 (0.422)	0.867 (1.278)	2.130 (1.147)	0.407 (0.493)
1:2 match	0.50	0.33	0.699 (1.085)	0.350 (0.542)	0.965 (1.614)	2.535 (1.695)	0.381 (0.488)
33% discount	0.33	0.33	0.982 (1.547)	0.324 (0.510)	0.982 (1.547)	2.362 (1.580)	0.416 (0.495)
50% rebate	0.50	0.25	0.991 (1.373)	0.248 (0.343)	0.991 (1.373)	2.196 (1.233)	0.451 (0.500)
1:1 match	0.50	0.25	0.805 (1.109)	0.403 (0.554)	1.611 (2.218)	3.434 (2.052)	0.469 (0.501)
50% discount	0.25	0.25	1.363 (1.996)	0.341 (0.499)	1.363 (1.996)	2.906 (2.003)	0.469 (0.501)
B. Tests of subsidy types: p-values							
B1. At effective price of $0.33							
33% rebate vs. 1:2 match			0.01	0.03	0.27	0.19	0.32
33% rebate vs. 33% discount			0.08	0.08	0.08	0.42	0.56
1:2 match vs. 33% discount			0.00	0.25	0.80	0.62	0.16
B2. At effective price of $0.25							
50% rebate vs. 1:1 match			0.04	0.00	0.00	0.00	0.56
50% rebate vs. 50% discount			0.00	0.00	0.00	0.03	0.59
1:1 match vs. 50% discount			0.00	0.05	0.05	0.18	1.00
C. Tests of subsidized prices: p-values							
50% vs. 33% rebate			0.06	0.05	0.06	0.79	0.10
1:1 vs. 1:2 match			0.13	0.13	0.00	0.02	0.01
50% vs. 33% discount			0.00	0.24	0.00	0.13	0.06
D. Tests of subsidized vs. unsubsidized prices: p-values							
D1. Low subsidy rate							
33% rebate vs. no subsidy			0.00	0.78	0.00	0.13	0.00
1:2 match vs. no subsidy			0.01	0.01	0.00	0.02	0.02
33% discount vs. no subsidy			0.00	0.02	0.00	0.04	0.00
D2. High subsidy rate							
50% rebate vs. no subsidy			0.00	0.32	0.00	0.08	0.00
1:1 match vs. no subsidy			0.00	0.00	0.00	0.00	0.00
50% discount vs. no subsidy			0.00	0.01	0.00	0.00	0.00

Notes: Panel A shows mean values of the donation variables for each treatment (standard deviations in parentheses). Column 1 reports the number of packages that subjects selected to give at the nominal price. Column 2 shows the net dollar contribution implied by subjects' choices, i.e., column 1 evaluated at the nominal price minus the rebate (if any). Column 3 reports the overall number of packages received by the charity, i.e., column 1 plus matched units (if any). Column 4 reports the same measure as column 3 but conditional on giving (intensive margin). Column 5 reports the share of subjects who donated at least one package (extensive margin). Panels B to D show pairwise tests between treatment conditions. Panel B compares subsidy types conditional on the effective price. Panel C compares the two subsidized prices, $0.25 and $0.33, conditional on subsidy type. Panel D compares the unsubsidized price with the subsidized price arising from the low subsidy rate for each subsidy type. In panels B to D, columns 1 to 3 report p-values of two-tailed paired t-tests, column 4 reports p-values of two-tailed unpaired t-tests with unequal variances, and column 5 reports p-values of McNemar's χ^2 tests for paired binary data.

D Appendix to Chapter 4

D.1 Additional Tables

Table D.1: Statements by Treatment Group (Translated)

Statement	Group Positive	Group Negative
(1)	Politicians do not always have the required expertise to make reasonable decisions. Companies and organizations share their know-how with politicians and thereby facilitate better decision making. This makes it possible, for example, to implement more realistic environmental regulations.	Lobbyists only represent their own interests and do not consider the well-being of the general public. For example, many companies try to use lobbying to prevent stricter environmental regulations.
(2)	Not only companies lobby. Nonprofit organizations also use lobbying to influence political decisions and to implement their own and their members' interests. For example, an environmental organization can try to implement a stricter climate policy by using lobbying.	Big companies have the required resources available to lobby. In contrast, nonprofit organizations and citizens often lack such resources. This results in a climate policy on behalf of companies.
(3)	Lobbying is getting more transparent. For example, on January 31, 2019, the EU parliament decided to implement stricter lobbying rules to improve transparency. Therefore, citizens will have sufficient information to understand lobbying activities and the impact of different actors.	Lobbying is intransparent. Citizens do not have enough information to understand the lobbying activities and the influence of different actors on political decision making.

Table D.2: Contribution Decision (Translated)

Screen	Content
(1)	For answering this survey you will receive 8 Euro, instead of the usual 4 Euro. This means you get **additional 4 Euro**. On the next screen, you have the opportunity to donate part or all of this additional 4 Euro to the nonprofit climate protection organization atmosfair. atmosfair supports projects for climate protection – e.g., the installation of renewable energies in developing countries – to reduce CO_2 emissions and fight climate change. In 2018, atmosfair received the quality valuation "excellent" from Stiftung Warentest – Finanztest.
(2)	Please enter the amount you would like to donate to the climate protection organization atmosfair below: [answer field] Euro [Help text:] The part of the 4 Euro that you do not donate to atmosfair will be transferred onto your study account and paid out according to your selected mode of payment in April 2020. The part of the 4 Euro that you donate to atmosfair will be transferred to atmosfair in April 2020. atmosfair will issue a certificate for the total amount of donations received.

Table D.3: Variables

Name	Description	Based on
A. General		
Female	Whether individual is female.	gender_19 (wave 44)
Age	Calculated based on the midpoint of the available year of birth categories.	year_of_birth_cat_19 (wave 44)
Married	Whether individual is married.	marital_status_19 (wave 44)
Employment	Employment status of individual. Categories: (1) full-time employed, (2) part-time employed, (3) retired, (4) other.	occupation_19 (wave 44)
Internet usage	Whether individual uses internet more than once a week.	Internet_usage_19 (wave 44)
Recruited 2018	Whether individual was recruited for GIP in 2018.	sample (wave 44)
Payment mode	The mode in which cumulative earnings were paid out in October 2019. Categories: (1) donation, (2) Amazon voucher, (3) bank transfer.	art201910 (on-site access)
Pro-environmental	Whether the environment and climate protection are important or very important to the individual.	CE41048 (wave 41)
Income	Personal income of individual. Categories: (1) $<$ €1000, (2) [€1000, €2000), (3) [€2000, €3000), (4) $\geq$ €3000.	AA43054 (wave 43)
B. Beliefs		
Belief 2018	Answer to the question "How does lobbying affect the level of climate protection in the European Union?" in November 2018. For more details see Belief 2019.	BG38009 (wave 38)

continued on the next page ...

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Belief 2019	Answer to the question "How does lobbying affect the level of climate protection in the European Union? Lobbying leads to ..." in the survey experiment (November 2019). Categories: (2) much more climate protection, (1) rather more, (0) neither more nor less, (−1) rather less, (−2) much less climate protection, (-) I don't know. "I don't know" is coded as (0), except when the full distribution of responses is shown.	BG44009 (wave 44)
Belief 2020	Answer to the question "How does lobbying affect the level of climate protection in the European Union? Lobbying leads to ..." in the follow-up survey (July 2020). For more details see Belief 2019.	BG48009 (wave 48)
C. Pro-environmental behavior		
Contribution to CO_2 reduction	The amount of money (in euro) donated to atmosfair. Possible values: 0, 1, 2, 3, 4.	BG44017 (wave 44)
Total environmental donation	The sum of money (in euro) donated to atmosfair (choice in the survey experiment) and WWF (as part of the payment of the cumulative earnings in April 2020)	BG44017 (wave 44); art201910, betr202004, betr202004_mcs, and win202004 (on-site access)
Car usage	Number of days per week individual usually uses a car (self-reported).	BG48023 (wave 48)
Taking the plane	Whether individual took a plane at least once within the last six months (self-reported).	BG48024_h (wave 48)
Purchasing local, organic products	Whether the individual purchased local, organic products at least once within the last six months (self-reported).	BG48024_g (wave 48)

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Bringing own shopping bag	Whether individual brought his/her own bag for shopping at least once within the last six months (self-reported).	BG48024_d (wave 48)
Considering product sustainability	Whether the individual considered a product's sustainability in a pruchasing decision at least once within the last six months (self-reported).	BG48024_a (wave 48)
Donating	Whether individual donated to an environmental organization at least once within the last six months (self-reported).	BG48024_f (wave 48)
Volunteering	Whether individual volunteered for an environmental project at least once within the last six months (self-reported).	BG48024_b (wave 48)
Protesting	Whether individual protested for more environmental or climate protection at least once within the last six months (self-reported).	BG48024_c (wave 48)
Signing petition	Whether individual signed a petition for more environmental or climate protection at least once within the last six months (self-reported).	BG48024_e (wave 48)

Table D.4: Estimation Results – Beliefs

	Belief 2019			Belief 2020		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Group Positive[a]	0.44***	0.44***	0.42***	0.04	0.02	0.01
	(0.05)	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)
Group Negative[b]	0.06	0.07*	0.08*	0.05	0.05	0.04
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
p-value ($a = b$)	0.00	0.00	0.00	0.63	0.56	0.61
Belief 2018	Yes	Yes	Yes	Yes	Yes	Yes
Female		Yes	Yes		Yes	Yes
Age		Yes	Yes		Yes	Yes
Married		Yes	Yes		Yes	Yes
Employment status		Yes	Yes		Yes	Yes
Online status		Yes	Yes		Yes	Yes
Recruited 2018		Yes	Yes		Yes	Yes
Income			Yes			Yes
Pro-environmental			Yes			Yes
Observations	3071	3041	2514	2819	2802	2359

Notes: The table reports the average marginal effects from ordered logit regressions. In columns 1 to 3, the dependent variable is the response to the belief elicitation question in November 2019. In columns 4 to 6, the dependent variable is the response to the belief elicitation question in July 2020. In both cases, the answer categories are (2) "much more climate protection," (1) "rather more," (0) "neither more nor less," (−1) "rather less," and (−2) "much less climate protection." The response "I don't know" is treated as neither expecting lobbying to increase nor to decrease the level of climate protection. The average marginal effects are calulcated based on the predicted beliefs if everyone in the sample would be assigned to the indicated treatment instead of the control group. Standard errors are in parentheses.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table D.5: Summary Statistics – Outcome Variables

Variable	Treatment: Group Positive	Treatment: Group Negative	Comparison p-value
A. Observed behavior			
Contribution to CO_2 reduction	2.40	2.30	0.22
	(1.84)	(1.85)	
Total environmental donation	3.05	3.06	0.94
	(2.84)	(2.87)	
Observations	1031	1026	
B. Stated behavior			
Car usage (days per week)[a]	3.65	3.78	0.22
	(2.26)	(2.26)	
Taking a plane	0.04	0.07	0.00
	(0.18)	(0.26)	
Purchasing local, organic products	0.80	0.78	0.25
	(0.40)	(0.42)	
Bringing own shopping bag	0.95	0.94	0.16
	(0.21)	(0.24)	
Considering product sustainability	0.68	0.66	0.50
	(0.47)	(0.47)	
Donating	0.11	0.10	0.47
	(0.31)	(0.30)	
Volunteering	0.04	0.04	0.89
	(0.21)	(0.20)	
Protesting	0.03	0.03	0.34
	(0.18)	(0.16)	
Signing petition	0.14	0.13	0.44
	(0.35)	(0.34)	
Stated behavior index[a]	0.00	-0.06	0.00
	(0.43)	(0.45)	
Observations	940	944	

Notes: The table reports the averages by treatment group with standard deviations in parentheses. The last column presents p-values from comparing each variable across the two treatment groups based on a χ^2-test if the variable is binary and a t-test with unequal variances otherwise.
[a]Based on 935 and 942 observations in Group Positive and Group Negative, respectively.

D.2 Additional Figures

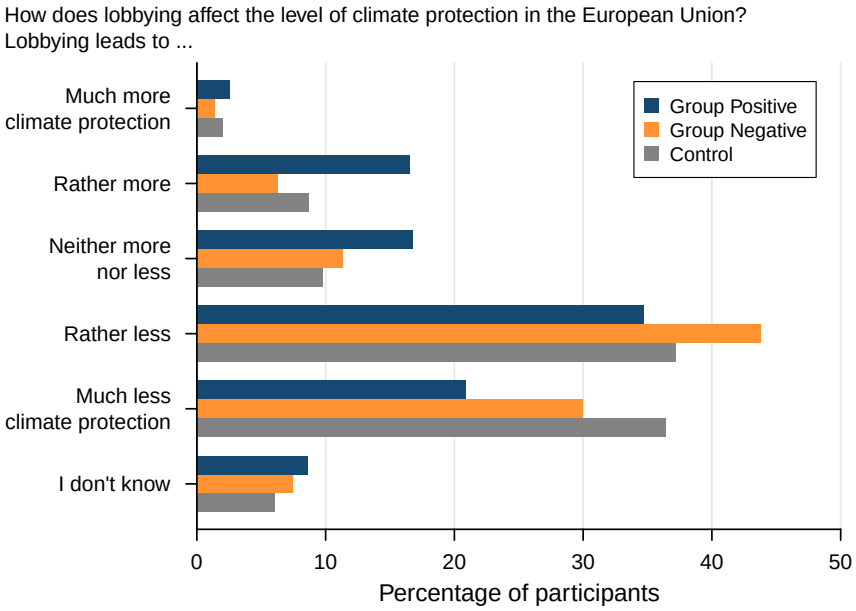

Figure D.1: Distribution of Beliefs in 2019

Notes: The figure shows the distribution of responses to the belief elicitation question in the survey experiment (November 2019) by experimental group. The (translated) wording of the belief elicitation question is presented at the top of the figure.

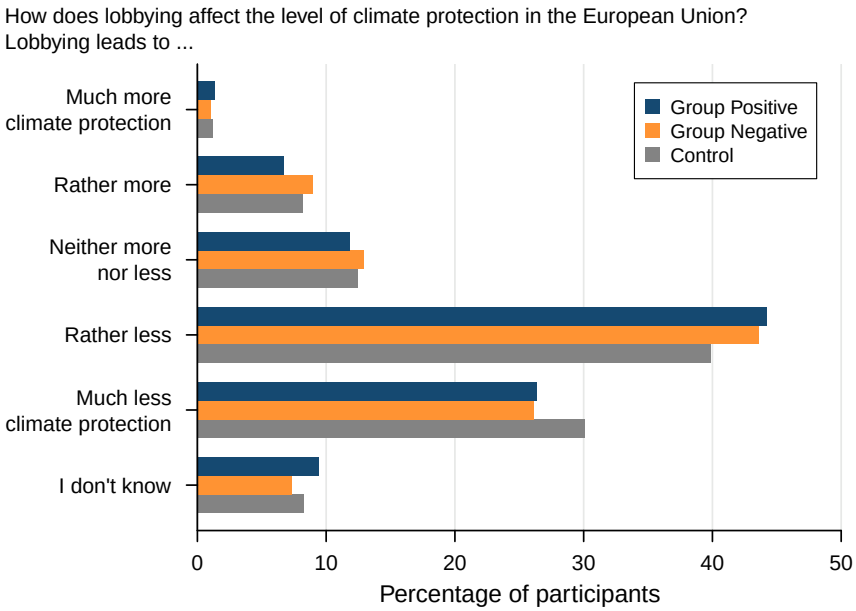

Figure D.2: Distribution of Beliefs in 2020

Notes: The figure shows the distribution of responses to the belief elicitation question in the follow-up survey (July 2020) by experimental group. The (translated) wording of the belief elicitation question is presented at the top of the figure.

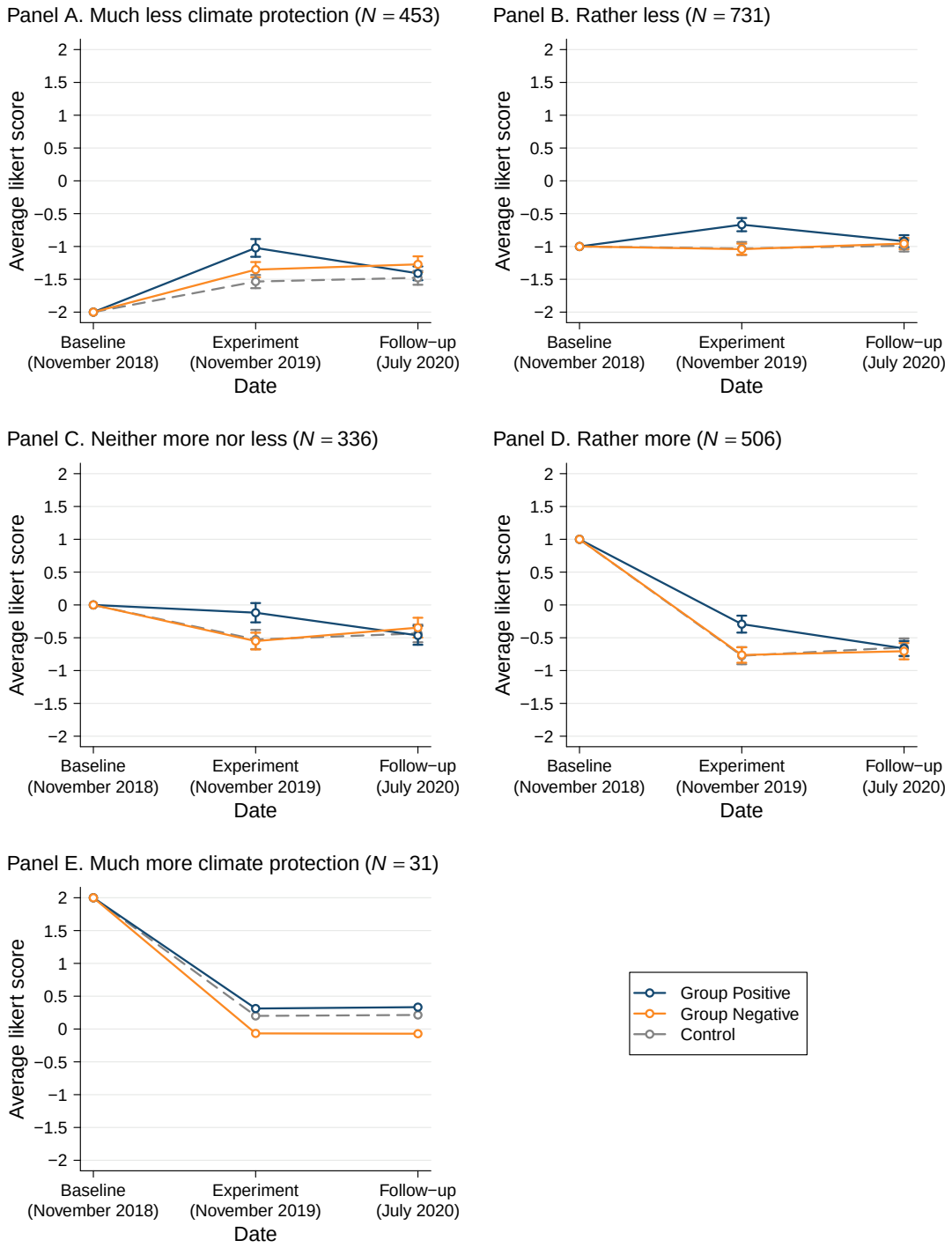

Figure D.3: Beliefs over Time by Baseline Belief

Notes: Panels A to E show the average likert score for the response to the belief elicitation question of the survey experiment (November 2019) by experimental group for individuals with a given baseline belief. The likert scale ranges from −2 (lobbying leads to much less climate protection) to 2 (lobbying leads to much more climate protection). The response "I don't know" is treated as neither expecting a positive nor a negative impact. The error bars represent 95 percent confidence intervals. Panel E does not contain error bars since only 46 individuals hold such a baseline belief.

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Eidesstattliche Erklärung

Hiermit erkläre ich, dass ich die vorliegende Dissertation selbstständig angefertigt und die benutzten Hilfsmittel vollständig und deutlich angegeben habe.

Mannheim, 28.04.2021

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