On Discrimination and Responsible Consumption: Essays in Behavioral Economics

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In memory of my father

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Chapter 1

Introduction

This thesis contributes to answering two questions that are puzzling from the standard economic theory view and have long been discussed controversially by economists. First, why does discrimination in markets exist, i.e., why are individuals treated differently based on their group belonging? Second, do consumers possess preferences for responsible consumption, i.e., are they willing to compensate firms for their Corporate Social Responsibility (CSR) efforts?

Both discrimination and responsible consumption have in common that the standard economic theory of competitive markets predicts their absence. Responsible for this lack of explanatory power is, among other things, the assumption that all economic actors narrowly pursue their self-interest without regard for others. In competitive markets, consumers are assumed to buy from firms that offer the lowest price. Firms anticipate this and refrain from any efforts resulting in a higher price. On the one hand, this logic prevents firms from undertaking virtuous CSR efforts such as costly measures to protect the environment. On the other hand, it also prevents firms from costly vices like discriminatory behavior in the labor market. The latter is because productivity is the only criteria firms consider in treating workers. Employers are assumed to have complete information about workers' productivity. Hence, irrespective of group belonging, firms hire the most productive workers for any given market wage. Any deviation from such non-discriminatory behavior means firms forgo labor cost reductions that competitors can achieve by being non-discriminatory. Consequently, discriminatory firms do not survive in the competition against non-discriminatory firms.

To justify the non-other-regarding, narrow self-interest assumption, sometimes, a quote from the foundational book of economics of Adam Smith (1776), *The Wealth of Nations*, is used: *"It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest."* The same Adam Smith (1759), often regarded as the "father of economics," writes in the opening sentence of his deeply insightful book about human nature, *The Theory of Moral Sentiments*: *"How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortune of others, and render their happiness necessary to him, though he derives nothing from it except the pleasure of seeing it."* The remaining of the book is devoted to the study of various aspects of other-regarding behavior. It is remarkable how profoundly *The Wealth of Nations* has influenced our current-day economic thinking and, at the same time, how seemingly unaffected it is by *The Theory of Moral Sentiments*.

The Nobel laureate Vernon Smith (1998) reconciles what he calls "the two faces of Adam Smith" by making the distinction that the former quote might apply to impersonal markets with a large number of participants and the latter to personal exchange in small groups. This interpretation, of course, opens the question of why Adam Smith himself did not make such an utmost important distinction explicit. Did he miss doing so, or did he not feel the need to do so because he considered the insights in *The Theory of Moral Sentiments* so fundamental about human nature that he saw them applying to all human interactions, including trading in markets?

From the latter perspective, the above quote from *The Wealth of Nations* does not need to be interpreted as support for the unconditional pursuit of self-interest in markets but rather as a pursuit of self-interest conditional on human nature, which Adam Smith clearly did not assume to be without regard for others. Also, regard for the own interest does not necessarily exclude regard for others. Sometimes, i.e., when incentives are aligned, the pursuit of own interest helps others. For example, in competitive markets without externalities, attempting to maximize own profits simultaneously maximizes total social benefits, as described by Adam Smith's "invisible hand." However, when incentives are not aligned, e.g., when own behavior causes externalities for others, there are trade-offs between benefits for oneself and others. However, it is not clear that maximizing own benefits will always win over regard for others. For example, sometimes we might be willing to give up our own monetary benefits to help others, e.g., by buying responsible products. In other cases, we might be willing to give up monetary benefits to avoid interacting with people from certain groups, e.g., when we discriminate.

Empirical evidence for both virtuous and immoral economically-relevant other-regarding behavior is mounting. Such evidence is not new and, in some cases, such as for charitable giving and discrimination, apparent throughout history without the need for sophisticated scientific methods. Especially with the rise of behavioral economics, however, there is clear proof, from the economic laboratory and the field, that other-regarding preferences and beliefs, e.g., about fairness and equality, but also discrimination, substantially influence human economic decision-making (see, e.g., Levitt and List, 2007; Dellavigna, 2009; Irlenbusch and Villeval, 2015; Thaler, 2016; Bertrand and Duflo, 2017, for excellent reviews on some topics in behavioral economics related to other-regarding behavior).

This thesis presents empirical evidence that contributes toward enhancing our understanding of economically-relevant other-regarding behavior. Chapter 2 contains the thesis's main contribution toward answering the question of why discrimination exists. The research here identifies (biased) beliefs about others' discriminatory behavior as a (root) cause of discrimination. Chapter 3 shows that when confounding factors such as brand reputations are controlled, consumers in competitive markets have a preference for responsible consumption and value a product with CSR characteristics more than an identical product without CSR characteristics. Chapter 4 investigates responsible consumption behavior in the field and, unlike Chapter 3, does not find evidence of a positive effect on consumer behavior. Thus, the evidence for whether consumers are willing to compensate firms for their CSR effort is mixed. The review of related literature in Chapters 3 and 4, however, reveals that results about responsible consumption are mixed in general. Hence, there seem to be unidentified variables causing variation in results that future research needs to investigate.

The above insights are generated by applying different types of incentive-compatible randomized controlled experiments. Chapter 2 develops a novel experimental paradigm based on a discrete choice decision to study discrimination. Data analyzed in Chapter 2 is collected online. Chapter 3 develops an experimental design for two framed field experiments, i.e., experiments that employ laboratory methods to control the decision situation as tightly as possible but simultaneously increase realism in some dimensions.[1] Data for the first experiment reported in Chapter 3 is collected outside the laboratory by recruiting participants at different locations on different university campuses. Data for the second experiment reported in Chapter 3 is collected via video calls, an innovation that the COVID19 pandemic made necessary. Chapter 4 reports results from a natural field experiment in e-commerce where subjects are unaware that their behavior is being studied as part of a scientific investigation.

In the following, results from each chapter are discussed in the context of the respective broader debate between economists. The attempt is to start at the very beginning of economic thought and then work the way through a selected number of main contributions from economists to the respective topic until we arrive at the contributions of this thesis. Details about the different studies, especially of methodological nature, are left to the respective chapters.

[1] See Harrison and List (2004) for classification of different types of experiments used in economics.

Discrimination. As described above, standard economic theory is unable to predict the existence of discrimination. Fortunately, economists have proposed refinements to enable the theory to explain and analyze discriminatory behavior in markets. As a result, two major streams of influential theories of discrimination have emerged.

On the one hand, theories of "statistical discrimination" relax the complete information assumption by introducing the unobservability of individual workers' productivity. Firms aim to narrowly maximize expected profit by hiring the most productive workers without having any appetite for discrimination. However, since they do not know who the most productive workers are, they use workers' group affiliation as a proxy for it. Of course, this critically relies on another assumption: firms can unbiasedly predict differences in the average productivity of different groups (Arrow, 1971; Phelps, 1972) or the variance of productivity of these groups (Aigner and Cain, 1977). Note also that this refinement does not only enable economic theory to predict the existence of discrimination, but it also implies that discriminatory behavior of this type is efficiency-enhancing.

On the other hand, the theory of "taste-based discrimination" alters the profit-maximization assumption (Becker, 1957). Here, decision-makers in firms either discriminate because they themselves have utility-generating preferences over groups or because they unbiasedly believe that their employees or customers have such preferences. In the former case, the theory is unable to predict the existence of discrimination because, as in the standard model described above, discriminatory firms do not survive in the competition against non-discriminatory firms. However, when employees or customers have preferences for discrimination, firms cannot reduce costs by being non-discriminatory because employees demand higher wages to work with coworkers from certain groups, or customers are willing to pay less for products and services produced by these workers.

Note that when employers discriminate based on beliefs about their employees' or customers' preferences for discrimination, they practically take the role of intermediaries in decision-making. Further, the theory excludes any discrimination on behalf of others that might be based on the expectation of statistical discrimination by others. Lastly, since decision-makers' beliefs about others are assumed to be unbiased, intermediation does not create discrimination that would not exist if employees and customers would decide for themselves.

Chapter 2, consisting of the single-authored paper *"In Your Name! The Effect of Intermediaries on Discrimination,"* asks how intermediation in decision-making affects discrimination when there are incentives in place to match the behavior of the person on behalf of whom one decides. Specifically for the study of this question, a novel experimental paradigm is developed. Decision-makers in the experiment choose either for themselves or on behalf of another person from a list with multiple candidates that differ only in either having a typical Western or Middle-Eastern first name. When in the role of intermediaries, decision-makers get to know only the first name (and gender) of the person on behalf of whom they act, who either is their in-group or out-group member. Further, intermediaries are monetary incentives to match as well as possible the choice that they believe the person on behalf of whom they act would take for himself.

The results of the study are striking. When decision-makers decide for themselves, on average, they do not discriminate. However, when intermediaries decide for another participant, they start discriminating strongly in favor of the in-group of the person on behalf of whom they act. This result even holds when the other is an out-group member of the decision-maker. Further, additional diagnostic treatments show that neither taste-based nor statistical discrimination can fully explain the detected effects. An analysis of beliefs data reveals that decision-makers expect strong discrimination in favor of the in-group from those on behalf of whom they act. However, since decision-makers do not discriminate when deciding for themselves, these beliefs are inaccurate and biased towards expecting others to favor their in-groups. Discrimination emerges because intermediaries attempt to match the discriminatory behavior they falsely expect from the person on behalf of whom they act. The belief that others discriminate sometimes rests on expecting others to be statistical discriminators, i.e., when they are in-group members, and sometimes on expecting others to have a preference for their in-group, i.e., when they are out-group members.

The study's main contribution is identifying biased beliefs about others' discriminatory behavior as a root cause of discrimination. Discrimination based on this mechanism could be referred to as *"higher-order discrimination."* In this study, the higher-order discrimination that emerges when decision-makers decide for others is of second order.[2] However, it is not difficult to imagine even stronger discrimination of third or even higher order when, e.g., decision-makers find themselves in more hierarchical situations. For example, employees are sometimes tasked with pre-selecting job applicants for a middle manager, who then proposes some of them to a top manager, who then selects the final candidate.

Further, decision-making for others is likely not the only situation in which higher-order discrimination might exist. For example, it might also be present in collective decision-making when a group of individuals needs to coordinate a decision, e.g., in hiring committees. Likewise, it might also play a role in individual decision-making when the consequences of the decision depend on others' discriminatory behavior. For example, job seekers might avoid applying to firms they expect to have discriminatory hiring commit-

[2]The definition of orders is as follows. The zeroth order refers to a random choice; the first order refers to a choice based on own preferences or beliefs; the second order refers to a choice based on beliefs about somebody else's choice; the third order refers to a choice based on the belief about somebody else's belief about somebody else's choice, and so on.

tees. In the social realm, individuals belonging to minorities might avoid calling the police when they expect the police to be biased against them.

Lastly, while identifying higher-order thinking as a cause of discrimination is new to the economic literature on discrimination, it is not new to the economic literature as a whole. It appears in John Maynard Keynes's analogy of stock market behavior with a newspaper beauty contest (cf., Keynes, 1936) where decision-makers do not choose what they themselves like but rather what they expect others to do, or what they expect others to expect what others do, and so on. In behavioral economics, the "guessing game" also known as "the beauty contest game," first studied by Rosemarie Nagel (1995), has been frequently used to determine the order of thinking. In the game, participants must guess a number from 0 to 100, with those closest to two-thirds of the average guess winning a price. Higher-order thinking, sometimes referred to as "level-k thinking", is also the core of several game-theoretic concepts (see, e.g., Stahl, 1993; Stahl and Wilson, 1995; Ho et al., 1998).

Responsible Consumption and CSR. The debate about CSR is at the heart of economics. The economist Howard Bowen (1953) defines it in his seminal book *Social responsibilities of the businessmen* as *"[...] the obligations of businessmen to pursue those policies, to make those decisions, or to follow those lines of action which are desirable in terms of the objectives and values of our society."* Bowen further specifies that CSR is based on the voluntary assumption of social responsibility. Therefore, CSR goes beyond legal obligations.

In his book, Bowen envisions an economic system with firms that tackle social problems through CSR. He explicitly contrasts it with the standard economic theory view, in which social welfare is maximized through the pursuit of narrow self-interest. Bowen's criticism of the standard economic theory, which he refers to as the "laissez faire economic system," is primarily based on two pillars. First, he points out that firms' self-interested behavior does not always align with social interest, as it is postulated by the idea of Adam Smith's invisible hand. For example, deceptive behavior such as lying about product quality or faking accounting records benefits firms but harms society. Similarly, firms profit from exploiting workers, e.g., through savings on workplace safety measures or disregard for contractual obligations such as paying agreed wages. Second, Bowen also criticizes that the other conditions required by the standard economic theory to achieve social welfare maximization "have never been and could not be fully realized in practice." For example, large-scale corporations concentrate economic power, hindering perfect competition. Similarly, firms' economic activities produce externalities for the society that are disregarded in the standard economic theory.

Some influential economists have responded fiercely to calls for an economic system in which firms are to assume responsibility for societal goals. Theodore Levitt (1958) warns in an essay called *The Dangers of Social Responsibility* of empowering firms with social tasks they neither have appropriate incentives to fulfill nor any expertise or political legitimacy. He fears that if firms assume social responsibilities when tasked with solving social problems, they most likely will misuse their power for their own benefit. Therefore, he proposes, the only goal firms should pursue is long-run profit maximization within the boundaries of the "elementary canons of everyday face-to-face civility (honesty, good faith, and so on)." Any social goals such as welfare are to be left to governments. The Nobel laureate Milton Friedman (1970), too, argues in a famous newspaper article, *The Social Responsibility of Business is to Increase its Profit*, that only governments should take care of social issues. Firms are solely to focus on maximizing their profits within the boundaries of the law and ethical custom. Friedman builds his argument on the fact that managers in large firms act on behalf of the firm's owners and therefore have a duty to act in the owners' best interest, which he assumes to consist primarily of making as much money as possible. Nevertheless, Friedman does recognize that sometimes owners of firms themselves might want to pursue social goals, either because they themselves have a preference for doing so or because they expect it to be beneficial for the firm's profit-maximization. In the latter case, Friedman regards CSR merely as cloaking profit-maximization as pursuing social goals. While he refrains from entirely condemning what he calls "hypocritical window dressing," he does consider such behavior by firms harmful to "the foundation of a free society" on the ground that "[...] it helps to strengthen the already too prevalent view that the pursuit of profits is wicked and immoral and must be curbed and controlled by external forces."

The above view of dichotomic labor division with firms being responsible solely for generating profits and governments taking care of all social issues rests on two assumptions. First, governments are assumed to be able to effectively tackle all social issues, including those generated by firms' economic activities, such as rapid exploitation of common pool resources and negative social and environmental externalities. Of course, governments can fail to do so for various reasons. Benabou and Tirole (2010) identify three such reasons why governments' ability to implement optimal policies can be limited or inferior to firms taking socially responsible actions themselves: (i) lobbyism, i.e., influential groups of people asserting influence to change policies in a way beneficial to them, (ii) the territoriality of jurisdictions, i.e., the inability of governments to implement policies outside their territory, and (iii) governments' competitive disadvantage, e.g., in policing employees behavior. Second, socially responsible behavior by firms is generally assumed to be harmful to profit maximization. Empirical research in recent years, however, has identified several reasons why CSR efforts can help make profits. For example, it has been shown that CSR can motivate workers to do a better job. It is used as a tool of political influence taking in the realm of private politics, e.g., avoiding calls for boycotts by activists, but also in public politics, e.g., for avoiding governmental regulation (see Kitzmueller and Shimshack, 2012, for an extensive review of the theoretical and empirical economic perspective on CSR). Chapters 3 and 4 of this thesis investigate if CSR might enhance profitability through consumers' willingness to reward firms for it, either through paying higher prices or increased demand.

Chapter 3 consists of the paper *"How Much is a Socially and/or Ecologically Responsible Production Worth to Consumers? Evidence from two Framed Field Experiments,"* which is joint work with Julian Conrads, Alexandra Eyberg, and Bernd Irlenbusch.[3] We experimentally investigate whether consumers possess a preference for products of firms that invest in CSR and how much they are willing to pay extra for certain types of CSR efforts. In a first incentivized framed field experiment with two treatments, we test if the increase in participants' valuation for a backpack is stronger when, in addition to some enhanced-functionality-related information, participants are informed that the production of the backpack was socially and ecologically responsible. Results show that adding CSR information leads to an increase in valuation of 53%, compared to a 21% increase when functionality-related information is provided without CSR information. Hence, we find clear evidence that, in our setting, consumers possess a preference for CSR and are willing to compensate firms for such efforts.

In a second incentivized framed field experiment, we investigate if consumers' preference for CSR depends on the specific dimension of CSR efforts being social or ecological. In two treatments, we either add only social CSR information or only ecological CSR information to the functionality-related information. We do not find a significant treatment difference and, therefore, any evidence that consumers reward the specific dimension of CSR efforts differently. In both cases, participants' valuation for the backpack is effectively increased. However, with 38% for ecological and 34% for social CSR, the increases are somewhat smaller than when both dimensions were combined in the first experiment.

Lastly, in both experiments, we find that participants who value the backpack relatively more before receiving any information about it react significantly stronger to CSR information. We do not detect a similar effect in the treatment that does not include CSR information. Therefore, it is likely that a positive interaction effect between CSR information and the strength of preference for non-CSR characteristics of products exists.

Chapter 4 contains the paper *"Consumer Demand for Responsibly Produced Products: Evidence from a Natural Field Experiment in E-Commerce,"* which is joint work with Julian Conrads, Bernd Irlenbusch, and Dirk Sliwka.[4] We conduct a natural field experiment in the e-commerce webshop of a German kindergarten backpack producer to test if sales increase when we add the information that the production of the backpacks is socially and ecologically responsible. Despite having 227,655 visits to the two versions of the webshop that we

[3] See Appendix D.1 for details on authors' contributions.
[4] See Appendix D.1 for details on authors' contributions.

activated simultaneously, one with CSR and one without CSR information, we do not detect any significant effects of CSR on sales. However, we find that visits to the CSR version webshop last significantly longer than visits to the control version without CSR information. Thus, while visitors seem to take their time to read the CSR information we provide, on average, they disregard it in their purchasing behavior.

Taken together, the evidence presented in Chapters 3 and 4 is mixed. In a stylized experimental setting with subjects aware of their participation in a scientific experiment, we find strong evidence that consumers are willing to compensate firms for their CSR efforts. This is consistent with a growing body of evidence from the economic laboratory. Taken to the field, we do not find such a preference in a setting where parents shop online for trendy backpacks for their kindergarten-aged kids. This is consistent with null results from other field experiments in the literature conducted in other settings. There might be multiple explanations for such a divergence in results about consumer preferences for CSR. One explanation could be that methodological reasons such as social desirability bias cause results from experiments employing laboratory methods to detect consumer preferences for CSR where non exist. But then, there is also a large number of field-experimental findings detecting CSR preferences outside the laboratory. Another explanation for our null results could be that CSR plays less of a role when parents shop for their kids. Or alternatively, it is also possible that, in our setting, consumers did not trust the CSR information we provided. Future research is needed to disentangle these competing explanations and shed more light on when consumer preferences for CSR show and when not.

Chapter 2

In Your Name! The Effect of Intermediaries on Discrimination

Abstract. Research within economics has shown a change in behavior when people decide for others compared to when they decide for themselves. In this study, the effect of intermediaries, defined as decision-makers who act on behalf of others, on discrimination is investigated. A novel incentivized discrete choice experiment is employed. Decision-makers choose from a list of candidates with either typical Western or Middle-Eastern first names. The study finds that while, on average, there is no discrimination when decision-makers decide for themselves, significant discrimination emerges when they act on behalf of another person and have an incentive to match the other's choice. The study further shows that this finding can neither be fully explained by a preference for discrimination nor by the classical notion of statistical discrimination based on beliefs over candidates' performance. Instead, the study finds strong evidence that biased beliefs about others' discriminatory behavior drive the effect. Thus, from a general perspective, the study contributes to long-standing literature that is in the search for reasons why people discriminate.[*]

Keywords: Discrimination, Intermediaries, Biased beliefs

JEL Classification Numbers: C91, D01, D83, J15, J71

[*]The results of this project were presented at research seminars at the University of Cologne, the University of Chicago, the University of Erfurt, the University of Innsbruck, the New York University Abu Dhabi and at the 6th M-BEPS, the 96th WEAI, the 2021 ESA Global, the 2021 VfS, the 2021 ESA North America, the 92nd SEA, the 2022 JILAEE, and the 2022 ESA Asia conferences. I thank Theo Simon for his excellent assistance with data collection. The project received IRB approval at the University of Cologne (200013MS). The experiment was pre-registered with an analysis plan in the AEA RCT Registry (AEARCTR-0006093). Financial support from the Cologne Center for Social and Economic Behavior (C-SEB) grant Rd10-2020-JSUG-Sarin is gratefully acknowledged. I declare that I have no relevant financial interests related to the research described in this paper. Dedicated to all who are marginalized.

2.1 Introduction

Intermediaries, defined as decision-makers who act on behalf of others, are omnipresent in (economic) life.[1] Intermediaries are supposed to make decisions that please the person on behalf of whom they act and, usually, have an incentive to do so. At the same time, they often lack complete information about the principal's preferences and behavior and thus need to form beliefs. However, predicting others' preferences or behavior is a tremendously difficult task that could be prone to systematic mistakes. For example, consider a situation where a Western rental agent must propose one among multiple Western and Eastern candidates as a rentee to a Western landlord. The rental agent has no information about the landlord's preferred choice. Still, he wants to match it because he only earns a commission fee if the landlord accepts the proposed candidate. Will he be more likely to believe that the landlord prefers an in-group over an out-group candidate? Whom will he propose? How will his choice differ from the one the landlord would take himself?

This study experimentally investigates if, how, and why intermediation in decision-making affects discrimination. These questions are challenging to address in naturally occurring contexts because observed discriminatory behavior can have many causes, and in the real world, it is challenging to isolate them from one another cleanly.

Discrimination is defined as the unequal treatment of individuals based on their group belonging. Economists typically distinguish between two types: "statistical" and "taste-based." Models of statistical discrimination postulate that people discriminate against particular groups when they believe that the group's average performance is lower (Phelps, 1972; Arrow, 1971) or its variance is higher (Aigner and Cain, 1977) compared to other groups. Critically, for discrimination to be defined as statistical, it needs to be based on beliefs about the discriminated group's performance. The classical models of statistical discrimination rely on the assumption of unbiased beliefs, meaning that they explain discrimination as being based on actual differences in group performances. More recently, Bohren et al. (2019) and Bohren et al. (2020) show that people also discriminate based on biased beliefs about performance differences. The literature on statistical discrimination is mute on the potential effects of intermediation in decision-making. On the other hand, literature on taste-based discrimination, at least in its origin, includes a somewhat subtle discussion of mechanisms that might differ when decision-makers are in the role of

[1]The Cambridge Dictionary (Walter, 2008) defines an intermediary as *"Someone who acts to arrange an agreement between people who are unwilling or unable to communicate directly."* In a narrow sense, one might define intermediaries as agents commissioned to act on behalf of a specific and known principal, e.g., rental agents acting on behalf of a landlord, headhunters acting on behalf of a manager, employees acting on behalf of an employer. In a broader sense, one might also include people who act on behalf of unspecified others, e.g., politicians acting on behalf of citizens, media outlets acting on behalf of content recipients, and businesses acting on behalf of customers.

intermediaries. Gary Becker (1957), in his pioneering analysis of *The Economics of Discrimination*, proposes utility-generating preferences (taste) for groups as the reason why people discriminate. More precisely, he models three types of labor market discrimination: "employer," "coworker," and "customer discrimination." In all three types, the employer is the ultimate decision-maker discriminating in his hiring decision. Only in the first type, however, does he discriminate based on his own preferences for groups. In the latter two types, the employer discriminates based on his beliefs about coworkers' or customers' preferences. Here, the employer essentially takes the role of an intermediary who discriminates on behalf of others. Three aspects of Becker's analysis are important to note regarding discrimination by intermediaries. First, unlike employer discrimination, coworker and customer discrimination prevail in competitive markets.[2] Second, Becker's theory allows only for discrimination on behalf of others that is based on the expectation of preference-based discrimination by others. However, preferences do not need to be the only reason why others' choice can be discriminatory. For example, and as suggested by the results of this study, intermediaries might discriminate on behalf of others without expecting them to have discriminatory preferences but based on their second-order beliefs that the others expect some groups to overperform other groups, and thus, based on the expectation of statistical discrimination. Third, since Becker's theory also relies on the unbiased beliefs assumption, intermediation does not create discrimination that would otherwise not exist if coworkers and customers decide themselves.

This study shows that, through biased beliefs about others' behavior, intermediation in decision-making causes discrimination that does not exist in decision-makers' own behavior. A novel experimental paradigm is developed based on a discrete choice decision situation in which decision-makers either choose a candidate for themselves or on behalf of somebody else from a list with multiple candidates. The experiment is implemented in three subsequent phases. Phase one includes four male participants in the role of candidates. The purpose of phase one is to build up a list of candidates. Two of the four candidates have typical Western first names, i.e., Alexander and Christian (henceforth "Western candidates"), and the other two typical Middle-Eastern first names, i.e., Hussein and Mohamed (henceforth "Eastern candidates"). Each candidate is endowed with 100 euros that he can divide between himself and a recipient. Importantly, candidates neither get any information about the recipient nor the subsequent phases of the experiment. Thus, candidates' decisions represent unconditional generosity towards an entirely unknown other

[2]More precisely, Becker investigates competitive markets where all firms provide the same homogeneous good, and workers' productivity is independent of their group belonging. Employers who discriminate based on their own taste forgo potential reductions in labor costs and thus profits. Consequently, they do not prevail in the competition against non-discriminatory employers. However, when coworkers or customers have a taste for discrimination, employers cannot necessarily increase profits by being non-discriminatory because either coworkers demand higher wages to work with individuals from particular groups or customers are willing to pay less for goods and services produced by individuals from these groups.

participant. Phase two includes other male participants who take the role of recipients. Recipients are informed about the decision situation in phase one but not about candidates' specific choices. Then, they are given a list of candidates from which they can choose. The list contains the first names of the four candidates from phase one and the information that all four are males, between 20 and 25 years old, and have a monthly income between 800 to 1100 euros. Each recipient is matched with one candidate based on the recipient's choice. The matched candidate's choice, i.e., the amount shared, becomes the recipient's payoff. Recipients are informed that only the matched candidate can earn a payoff from their decision, i.e., the amount kept, and that the three unmatched candidates go empty-handed. Thus, the decision situation also has a resource allocation character. Following the choice, recipients report non-incentivized beliefs about candidates' generosity. They are not informed about the subsequent third phase of the experiment. Participants in the role of recipients either have typical Western first names (henceforth "Western recipients") or typical Middle-Eastern first names (henceforth "Eastern recipients"). Phase three includes other male participants with typical Western first names in the role of intermediaries (henceforth "Western intermediaries"). They are informed about the decision situations in phases one and two but not about candidates' and recipients' specific choices. Intermediaries are explicitly instructed to act on behalf of a randomly assigned recipient from phase two. They are informed only about the gender and the first name of the recipient. Intermediaries either act on behalf of an in-group member, i.e., a Western recipient, or an out-group member, i.e., an Eastern recipient. They make the same decision as recipients in phase two, for which they receive the same list with the same information about the four candidates. Based on their decision, one candidate is matched with the recipient. The amount shared by the matched candidate becomes the payoff of the recipient. Intermediaries are monetarily incentivized to match the recipient's choice as well as possible. Following their choice, intermediaries report non-incentivized beliefs about candidates' generosity, the recipient's choice, and the recipient's beliefs about candidates' generosity.

Do participants believe Western or Eastern candidates to be more generous? Overall, neither Western nor Eastern participants report candidates from one group to be more generous than candidates from the other group. What do Western intermediaries think about Western recipients' choice and beliefs? An overwhelming majority of 81% report believing that Western recipients discriminate in favor of Western candidates. Similarly, 78% of them report believing that Western recipients expect their in-group to be more generous than the out-group. What do Western intermediaries think about Eastern recipients' choice and beliefs? A majority of around half report believing that Eastern recipients discriminate in favor of Eastern candidates. The other half divides equally on reporting to believe that Eastern recipients discriminate in favor of Western candidates or do not discriminate. When asked about Eastern recipients' beliefs, on average, Western intermediaries' do not report believing Eastern recipients to expect one group to be more generous than the other. Thus, Western intermediaries expect discrimination in favor of the in-group from both Western and Eastern recipients. However, while the beliefs they report about Western recipients are aligned with the expectation of statistical discrimination, the beliefs they report about Eastern recipients point to the expectation of taste-based discrimination.

Are Western intermediaries' beliefs that, on average, both Western and Eastern recipients discriminate in favor of their in-group unbiased? The answer is clear for beliefs about Western recipients' behavior; they are astoundingly biased. While Western intermediaries expect Western recipients to discriminate massively in favor of Western candidates, Western recipients, on average, actually do not discriminate at all. Average beliefs about Eastern recipients look remarkably unbiased. Eastern recipients' choice shows an almost same-sized discriminatory tendency in favor of Eastern candidates as Western intermediaries report expecting. However, decomposing the averages reveals that while Eastern recipients' choice is highly polarized, Western intermediaries' do not anticipate the polarization. Hence, in this case, too, beliefs are biased, even if the bias does not show on the surface.

How does discrimination change when Western intermediaries decide for Western recipients compared to when Western recipients decide for themselves? Here, intermediation in decision-making leads to the emergence of strong discrimination in favor of Western candidates that does not exist when Western recipients decide for themselves. Nevertheless, it falls somewhat short of the payoff-maximizing level required by the reported beliefs about the choice of Western recipients. Thus, a substantial fraction of Western intermediaries seems to forgo expected payoffs to not discriminate against Eastern candidates, hinting at either a distaste for discrimination or a taste for Eastern candidates. How does discrimination change when Western intermediaries decide for Eastern recipients? Now, Western intermediaries' discrimination flips the direction, and, on average, they start discriminating in favor of Eastern candidates. Compared to Eastern recipients' average choice, intermediation seems not to affect discrimination much. In both cases, discrimination in favor of Eastern candidates is almost same-sized, even if only that by intermediaries reaches statistical significance. But, again, looking at averages alone hides the fact that while Eastern recipients' discriminatory behavior is polarized, Western intermediaries' discrimination on their behalf is not.

What is the precise reason intermediation leads to discrimination? The above results suggest that it might be intermediaries' beliefs about recipients' behavior. Alternatively, the discrimination could also be caused by the pure change in role. Therefore, to test for this alternative explanation, two additional treatments are conducted in which intermediaries' monetary incentives are perfectly aligned to those of recipients by paying them whatever amount the chosen candidate has decided to share. Intermediaries again either act on behalf of a Western or Eastern recipient. Results of these two treatments with aligned incentives show that the pure change in the role explains a maximum of 48% of intermediaries' discrimination with incentivized to match Western recipients' choice and 44% of their discrimination when incentivized to match Eastern recipients' choice. The difference between the two treatments with aligned incentives is statistically significant. Thus, despite keeping monetary incentives constant, Western intermediaries' discrimination changes the direction in favor of the group of the person on behalf of whom they act. Next, to further identify whether the effect of a pure role-change works through taste-based or statistical discrimination, another two treatments are conducted in which intermediaries' monetary incentives are fixed to 50 euros. Since incentives are fixed, any discrimination observed needs to result from group preferences, i.e., a taste for discrimination. With these fixed incentives, Western intermediaries who act on behalf of Western recipients do not discriminate. Hence, taste-based discrimination has no explanatory power for Western intermediaries' discrimination when incentivized to match Western recipients' choice. This further implies that a change in statistical discrimination causes the role-change effect when intermediaries act on behalf of in-group members. On the contrary, when Western intermediaries with fixed monetary incentives act on behalf of Eastern recipients, they discriminate in favor of Eastern recipients. Their discrimination matches precisely the discrimination when incentivized to match Eastern recipients' choice. Thus, taste-based discrimination can explain 100% of the effect in this case. A comparison with the respective treatment with aligned incentives shows that in the latter case, discrimination is lower. Hence, adding monetary incentives based on candidates' generosity, again, moves behavior directionally in favor of Western candidates. Therefore, in this case, the change in role seems to alter both taste-based and statistical discrimination.

The results from the four additional treatments clarify that neither statistical nor taste-based discrimination can fully explain the emergence of discrimination in Western intermediaries' choice and provide indirect evidence for the idea that biased beliefs about others' behavior are its root cause. Two further results provide more direct support. First, no significant treatment differences exist in participants' propensity to maximize their payoffs. More specifically, neither changing decision-makers' monetary incentives nor changing the recipient's group belonging affects the share of individual choices that align with own-payoff-maximization. Second, monetary incentives to match others' behavior, compared to monetary incentives based on candidates' generosity and fixed monetary incentives, cause a significant increase in the share of intermediaries' choices consistent with the motive of matching the recipient's choice but not maximizing his payoff.

This paper contributes to a broad literature that studies how intermediation changes outcomes in economically relevant decision situations such as bargaining (e.g., Schelling, 1956; Fershtman et al., 1991; Choy et al., 2016), wage-setting (e.g., Charness et al., 2016), investment (e.g., Eriksen and Kvaløy, 2010), risk-taking (see review Polman and Wu, 2020), and intertemporal decisions related to patience (Albrecht et al., 2011). Further, a growing number of studies investigate the effects of intermediation related to moral behavior, such as an increase in self-interested choices (Hamman et al., 2010), shifting blame and punishment from the principal to the intermediary (Bartling and Fischbacher, 2012), reducing punishment for unethical and rewards for ethical behavior (Coffman, 2011), facilitating lying (Erat, 2013) and increasing corruption (Drugov et al., 2014).[3] Cochard et al. (2019) study the effect of intermediation on discrimination in an investment game when intermediaries are paid a fixed payoff. The authors document discrimination in favor of the in-group when participants decide as intermediaries for an in-group member but not when they decide as investors for themselves. Therefore, their findings differ from the respective treatments with fixed incentives in this study. One potential reason why results differ could be that they use artificially induced group identities, whereas here, real group identities are used. It is unlikely that discrimination against artificially induced groups comes with similar (or any?) moral costs. This view is in line with the finding from the meta-analysis of Lane (2016) that discrimination measured in laboratory experiments is strongest when artificially induced group identities are used compared to all types of real identities.

By identifying biased beliefs about others' discriminatory behavior as a root cause of discrimination, this study contributes to long-standing literature that is in search of reasons why people discriminate. Beyond taste-based and statistical discrimination, several other reasons have been proposed as causes of discrimination. Bertrand et al. (2005) argue that people sometimes "implicitly" discriminate without consciously realizing that they do so. Peski and Szentes (2013) propose a theory in which discriminatory behavior prevails solely as a coordination device without decision-makers having a preference for groups or differential beliefs about group performances. Akerlof and Kranton (2000) introduce identity as a mediator between preferences and discriminatory behavior. They argue that people have preferences to identify with particular groups, not necessarily restricted to inherent in-groups such as skin color or ethnicity. To achieve identification and thus gain utility, people adhere to the respective group's social norms, which can prescribe discriminatory behavior. The idea that social identity is why people discriminate is based on pioneering work from social psychology, theorized in Tajfel and Turner (1979) and known as "social identity theory." It argues that the mere possibility of categorization in groups, even if minimal in the sense of being founded on arbitrary criteria such as preferences for art or colors,

[3]The exact mechanisms through which intermediation in decision-making changes outcomes might differ from situation to situation and, in each case, need thorough investigation. For example, while in a bargaining situation, intermediation might change outcomes because it provides a commitment device (Fershtman et al., 1991), in a wage-setting situation, it might work through an increase in employee motivation (Charness et al., 2016). Concerning moral behavior, Paharia et al. (2009) show that indirect agency affects the moral judgment of actions through differences in the activation of the affective/intuitive and cognitive/reflective mental judgment systems. For intertemporal decisions, Albrecht et al. (2011) provide evidence from an fMRI study showing that acting on behalf of others activates other brain regions than deciding for oneself.

triggers discrimination. A similar thought is found in the landmark book, *The nature of prejudice,* of Gordon Allport (1954): *"Psychologically, the crux of the matter is that the familiar provides the indispensable basis for our existence. Since existence is good, its accompanying groundwork seems good and desirable. A child's parents, neighborhood, region, nation are given to him–so too his religion, race, and social traditions. To him all these affiliations are taken for granted. Since he is part of them, and they are part of him, they are good."* Another line of research, starting with Yamagishi et al. (1999), strongly objects to the idea that the existence of social categories per se causes discrimination and proposes "bounded generalized reciprocity" as an alternative theory. According to it, people discriminate in favor of their in-group because they expect in-group members to do the same, and hence, that, at some point, they themselves will benefit from reciprocal discrimination. Groups are viewed as containers for generalized reciprocity and cooperation among current and future in-group members.

The findings of this study have clear, practical implications for research and policymaking. First, researchers have shown discrimination to exist in numerous situations and markets (see reviews Charles and Guryan, 2011; Bertrand and Duflo, 2017). In many cases, we are not only interested in detecting discrimination but also want to understand its precise cause.[4] Existing research, however, has not yet sufficiently distinguished between discrimination by intermediaries acting on behalf of others and discrimination by decision-makers who decide for themselves. Especially in markets such as for housing and labor, where intermediaries take a large portion of the decisions, this is problematic. Not considering the additional motivations intermediaries can have to discriminate can make researchers falsy identify the cause of discrimination. Second, policymakers design anti-discrimination policies based on their understanding of the reason behind the discrimination in a particular situation. When their understanding is flawed anti-discrimination policies will be ineffective and waste valuable resources. Further, the results of this study urge caution with delegating decisions to intermediaries. Sometimes, the most effective way to reduce discrimination might be by reducing the delegation of decisions. If that is not possible or too costly, policymakers and organizations should think of methods to, if necessary, correct intermediaries' beliefs about others' preferences, beliefs, and behavior. For example, regulators could measure the beliefs of intermediaries such as rental agents about the people on behalf of whom they act. If a bias is detected, as a first step, one could provide this information to the intermediaries and then study whether this helps reduce the bias. Lastly, the finding that revealed preferences for groups are context-dependent urges for wariness with "one-fit-all" policies. Even for the same target group of people, what works as an anti-discrimination policy in one situation, might not work in another context.

[4]See, for example, Fershtman and Gneezy (2001) and List (2004) for early innovative applications of the experimental method in economics for the identification of the cause of discrimination.

The paper proceeds as follows: Section 2.2 describes the experimental design in more detail. Section 2.3 develops hypotheses based on a simple choice rule. Section 2.4 presents the results, where 2.4.1 shows average discrimination across the four main treatments, 2.4.2 decomposes the average effects in order to study heterogeneity, and 2.4.3 identifies the exact reasons why intermediaries discriminate. Section 2.5 reflects on the main findings. Section 2.6 concludes.

2.2 Experimental design

Decision-makers choose from a list of candidates belonging to one of two groups. They either decide for themselves or on behalf of another participant. Data is collected online in three subsequent phases. In phase one, participants in the role of candidates decide to divide 100 euros with an unknown recipient. In phase two, participants in the role of recipients choose from a list of candidates for themselves. In the third phase, participants in the role of intermediaries choose from the same list of candidates but on behalf of a recipient. See Appendix A.4 for translated screen by screen instructions for each phase.

Phase 1 (Candidates). Four male participants – Alexander, Christian, Hussein, and Mohamed – form the list of candidates from which decision-makers in the subsequent phases can choose.[5] Each candidate is endowed with 100 euros, which he can share in integer amounts from 0 to 100 with another unknown participant – the recipient. Before participants learn their role, the basic decision situation is described neutrally. Candidates do not receive any information about the recipient. They are also not informed about the subsequent phases of the experiment. Candidates are guaranteed that their decision will be paid out at least once but also informed that it will be used multiple times in the study and therefore might lead to multiple payouts. Their decisions present unconditional generosity towards fully unknown other participants.

Phase 2 (Recipients). The role of recipients is filled by male participants with either typical Western first names or typical Middle-Eastern first names (see Appendix A.2 for a complete list of participants' first names per treatment condition). Before they learn their role,

[5]This study is conducted in Germany. Therefore, the "Western" group consists of participants with typical native-German first names. The "Eastern" group consists of participants with typical Middle-Eastern first names. For the purpose of this study, the term "Middle-Eastern" is broadly defined to include people that are perceived to be from North Africa or Western and Central Asia, e.g., Arabs and Turks. People with a perceived Middle-Eastern heritage, especially men, are among the most discriminated groups in Germany. For example, the newsmagazine "Der Spiegel" and "Der Bayerischer Rundfunk," a public-service radio and television broadcaster, conducted a large-scale correspondence study with fictitious applications to study discrimination in the German housing market. Results show the strongest discrimination against Turkish and Arabic male names. The chance of success, i.e., getting a viewing appointment, is approximately a third smaller for Turkish and Arabic names than for native-German names (Bayerischer Rundfunk, 2017).

neutrally written instructions inform recipients about the basic decision situation. After they learn their role, instructions emphasize that candidates' have already taken their decisions in a previous phase of the experiment. It is further made explicit that candidates had not received any information about either the recipient or his decision situation. Next, they receive a list with each of the four candidates' first names, followed by the same information for each candidate, stating that he is male, between 20 and 25 years old, and has a monthly income of between 800 and 1100 euros. Then, recipients choose their most (second-most, third-most, fourth-most) preferred candidate by ranking them from one to four, where rank 1 (2, 3, 4) comes with a 40% (30%, 20%, 10%) matching probability. Based on the allocated probabilities, one candidate is assigned to the recipient, and the assigned candidate's decision becomes payout-relevant. Importantly, before recipients choose, instructions explicitly explain that their decision affects not only their own payoff but also candidates' payoffs. It is further clarified that the three candidates who are not assigned to them go empty-handed from their decision. The choice ranking (henceforth "C-Rank") is followed by an unincentivized belief ranking (henceforth "B-Rank"), where recipients report their beliefs about candidates' generosity, i.e., whom they expect to have shared the highest (second-highest, third-highest, fourth-highest) amount. Candidates appear in a randomized order on the list for each decision-maker and each ranking.

Phase 3 (Intermediaries). The role of intermediaries is filled by male participants with typical Western first names only. Intermediaries learn about the first two phases of the experiment. Then, they are informed that they will act on behalf of a randomly assigned recipient from phase two. The only information they see about the recipient is his first name and gender. They then receive the same list of candidates as recipients in phase two, rank candidates from one to four, and thereby assign matching probabilities as described above. Intermediaries are monetarily incentivized to match the choice of the recipient on behalf of whom they act. More precisely, they earned 25 euros for each match of their choice ranking with the recipient's choice ranking. Thus, an intermediary can earn up to 100 euros when his choice ranking fully overlaps with the recipient's choice ranking. Alike, he earns 50 euros for two matches, 25 euros for one match, and 0 euros when there is no overlap. Based on the probabilities allocated by the intermediary, one candidate is assigned to the recipient, and the assigned candidate's decision becomes payout-relevant. The choice ranking (C-Rank) is followed by three unincentivized belief rankings, where one asks for intermediaries' beliefs about candidates' generosity (B-Rank), one asks about beliefs over the recipient's choice (henceforth "BoC-Rank"), and one asks about beliefs over the recipient's beliefs over candidates' generosity (henceforth "BoB-rank"). Belief rankings appear in a randomized order on three separate screens.

Main treatments. The study compares behavior along two treatment dimensions: the decision-maker's role, i.e., recipient or intermediary, and the recipient's group belonging, i.e., Western or Eastern. Combining both dimensions results in four main treatments: Western recipients deciding for themselves (Own-WR), Eastern recipients deciding for themselves (Own-ER), Western intermediaries deciding for a Western recipient and incentivized to match his choice (WI-Match-WR), and Western intermediaries deciding for an Eastern recipient and incentivized to match his choice (WI-Match-ER).

Diagnostic treatments. Four additional diagnostic treatments are conducted to test if a pure change in the role by itself might affect discriminatory behavior and whether such an effect is caused by taste-based or statistical discrimination. Roles are defined as decision-makers deciding for (i) themselves, (ii) an in-group member, or (iii) an out-group member. First, to test if a pure change in role affects discrimination, intermediaries' monetary incentives are perfectly aligned to those of recipients by paying them whatever amount the matched candidate has decided to share. Western intermediaries either act on behalf of Western (WI-Same-WR) or Eastern recipients (WI-Same-ER). Second, intermediaries are paid a fixed payoff of 50 euros in another two diagnostic treatments. Since fixedly paid intermediaries have no economic reason to discriminate, any discriminatory behavior must result from preferences, i.e., taste for discrimination. Again, Western intermediaries either act on behalf of Western (WI-Fix-WR) or Eastern recipients (WI-Fix-ER). Lastly, keeping recipients' group belonging constant and subtracting intermediaries' discrimination when paid a fixed payoff from their discrimination when incentives are aligned with those of recipients then gives the portion of statistical discrimination. Table 2.1 presents the number of participants per treatment condition.[6]

Table 2.1: Number of participants per treatment condition

	Own Decision	Western Intermediary (WI) Match	Fixed	Same	Total
Western recipient (WR)	54	41	50	45	190
Eastern recipient (ER)	48	54	48	46	196
Total	102	95	98	91	386

Procedures. Data was collected online in three subsequent phases between June 30^{th} and July 16^{th}, 2020. Participants were recruited via ORSEE (Greiner, 2004, 2015) from the Cologne Laboratory for Economic Research (CLER) subject pool at the University of Cologne. Potential participants were pre-selected based on their gender to be recorded as male, and, for all treatments with Western decision-makers, their first name to be typical Western. Based on the pre-selection criteria, the allocation of potential participants to the treatments was random. Similarly, subjects were pre-selected for the treatment with Eastern decision-makers based on their first names being typical Middle-Eastern. Nobody participated in more than one experimental condition. Only male participants were invited to

[6]The number of participants presented in Table 2.1 are after data cleaning. See Appendix A.3 for details on data cleaning.

avoid confounding gender discrimination effects. On average, phase 1 (2, 3) lasted 4 (8, 11) minutes, including a short questionnaire at the end of each phase, where participants reported their age, current occupational status, highest educational degree, field of study, and were allowed to comment on their decisions and the study in general. Checking for treatment differences in demographic variables shows no unexpected variation, thus confirming that randomization has worked (see Table A.1 in the Appendix). Subjects were paid 2.50 euros for participation and could earn extra money as described in the following. Candidates earned at least once the amount they kept from the 100 euros endowment but could earn a multiple of it if lucky. Recipients and intermediaries earned additional money only when they were lucky to be drawn in a lottery with up to 49 others. Participants were not given any feedback about their total earnings at the end of their phase but were informed that payouts could take several weeks.

2.3 Hypotheses

Let i be a decision-maker belonging to one of two groups, $g \in \{West, East\}$, who chooses his preferred candidate from a list of candidates that also belong to one of the two groups. Denote the group that is not g with $-g$. The only observable difference between candidates is their group belonging. Assume the decision-maker's choice to be guided by the following utility function:

$$u_{i,g} = \pi_g + \rho_{i,g} \tag{2.1}$$

where π_g is a monetary payoff and $\rho_{i,g} \in \mathbb{R}$ represents preference for group g. Since actual monetary payoffs are unknown to i, he forms expectations, $\hat{\mathbb{E}}_i[\pi_g]$, based on his subjective beliefs. Let $c_{i,g}$ be an indicator variable being 1 if i chooses a candidate from group g, and 0 otherwise. Utility maximization demands the following choice rule:[7]

$$c_{i,g} = \begin{cases} 1 & \text{if } \hat{\mathbb{E}}_i[\pi_g] + \rho_{i,g} \geq \hat{\mathbb{E}}_i[\pi_{-g}] + \rho_{i,-g} \\ 0 & \text{if } \hat{\mathbb{E}}_i[\pi_g] + \rho_{i,g} \leq \hat{\mathbb{E}}_i[\pi_{-g}] + \rho_{i,-g} \end{cases} \tag{2.2}$$

Recipients' monetary payoff is whatever amount the matched candidate has decided to share. Thus, own-payoff-maximization requires that recipients choose according to their beliefs over candidates' generosity. The experimental design includes several measures that reduce recipients' likelihood of believing one group to be more generous than the

[7] See Thurstone (1927); Luce (1959); Marschak (1960); McFadden (1973); Manski (1977); Beggs et al. (1981) for original work on the analysis of discrete choice behavior as maximization of underlying utility.

other. First, instructions are written such that in each phase, participants expect that they can be allocated the role of a candidate. Therefore, participants in phases two and three can expect candidates to have been recruited in the same ways, e.g., from the same subject pool mainly consisting of current and past students and, therefore, to be alike.[8] Second, this impression should be strengthened because decision-makers are informed that all candidates are males, between 20 and 25 years old, and have a monthly income between 800 and 1100 euros. Third, to ensure participants' understanding that candidates' could not condition their decision on the recipient's group belonging or any other characteristics, instructions remind them multiple times that candidates have already taken their decision without having any information about the recipient. These design choices make it less likely that payoff-maximization considerations cause discrimination in recipients' choice. On the contrary, recipients' potential preferences for the in-group are considered likely to cause discrimination. That, in general, people possess a preference for their in-group is a well-established result. Tajfel et al. (1971) and subsequent literature show that artificially induced group identities based on arbitrary criteria trigger in-group bias. Hedegaard and Tyran (2018) provide evidence from a discrete choice experiment, showing that participants were willing to forgo a substantial portion of their payoffs for the choice of an in-group candidate. Their result is especially relevant for predicting decision-makers' behavior here because they investigated real group identities indicated by typical Western and Middle-Eastern first names.

Hypothesis 2.1 (Recipients)**.** *Recipients will discriminate in favor of their in-group.*

Intermediaries, like recipients, are also assumed to have a preference for the in-group, i.e., for Western candidates. However, unlike recipients, intermediaries' payoffs do not directly depend on candidates' generosity. To earn any payoff, intermediaries need to match the choice of the recipient on behalf of whom they act. Own-payoff-maximization demands that they rank candidates according to their beliefs about the recipient's choice ranking. Looking into the discrimination literature, it seems that people's tendency to believe that others discriminate in favor of their in-group has never been seriously challenged. On the contrary, some have considered it axiomatic and the very basis of discrimination. For example, in his early work, Tajfel (1970) describes a *"generic norm of discrimination."* He defines a norm as *"[...] an individual's expectation of how others expect him to behave and his expectation of how others will behave in any given situation."* Yamagishi and Kiyonari (2000) take this idea a step further and provide empirical evidence that *"expectations of generalized reciprocity from in-group members"* are a root cause of discrimination. Sub-

[8]In each phase, instructions start with a screen containing a short description of the basic decision situation, i.e., the dictator game. At the bottom of this screen, participants are informed that their roles will be assigned on the next screen. Thus, independent of the differences in instructions that follow, participants of all three phases will have expected that they themselves could have been assigned the role of a candidate.

sequent research extends their finding to a well-developed theory about discrimination.[9] Bernhard et al. (2006) provide further evidence from an experiment with real identities and show that participants expect to be punished less for unfair behavior by in-group members than by out-group members. Thus, it is likely that intermediaries will conclude that own-payoff-maximization requires choosing the recipient's in-group. When they act on behalf of a Western recipient, the choice prescribed by their own preference for groups and the choice prescribed by own-payoff-maximization are likely to be identical and in favor of Western candidates. However, when they act on behalf of an Eastern recipient, the two effects are likely to point in opposite directions. Nevertheless, the "all or nothing" nature of their incentives makes it reasonable to expect that the desire for a positive monetary payoff will win over their group preference and cause discrimination in favor of Eastern candidates.

Hypothesis 2.2 (Intermediaries)**.** *Intermediaries will discriminate in favor of recipients' in-group.*

The above hypotheses predict discrimination in favor of recipients' in-group, regardless if recipients decide for themselves or intermediaries decide on their behalf. Who will discriminate more? According to the choice rule above, behavior can deviate for two reasons: a difference in monetary incentives or a difference in group preferences. As argued above, recipients' monetary incentives are unlikely to cause discrimination because they are based on candidates' generosity, and the experimental design reduces room for rationalizing beliefs of group-based differences in generosity. On the contrary, intermediaries' monetary incentives are based on recipients' choice, and in combination with beliefs that others have an in-group preference, they likely cause discrimination. Beyond monetary incentives, a difference in recipients' and intermediaries' discrimination can also be caused by a role-dependent change in preferences. Presuming preferences for groups to be malleable, as suggested by the description of a situation-dependent *"generic norm of discrimination"* in Tajfel (1970), changes in preference could stem from context-dependent differences in the moral costs of discrimination.[10] In general, several reasons, including diffuse responsibility, role-dependent differences in anti-discrimination norms, less harm to the self or social image, might cause a role-dependent change in decision-makers' moral costs

[9]See Balliet et al. (2014) for a meta-analysis on in-group bias in cooperation.

[10]Moral costs come from social preferences. There is well-established literature within economics investigating social preferences (Rabin, 1995; Levitt and List, 2007). For instance, people are averse to inequality (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), to violations of social norm (Elster, 1989; Krupka and Weber, 2013), to harming their self-image and identity (Bénabou and Tirole, 2011), or view some behavior such as paying unfair wages (Pigors and Rockenbach, 2016) or lying (Abeler et al., 2019; Dufwenberg and Dufwenberg, 2018; Gneezy et al., 2018; Khalmetski and Sliwka, 2019) as unethical per se. Further, a growing literature suggests that moral costs depend on and can change with the context (Dana et al., 2007; Shalvi et al., 2011). For example, they decrease when responsibility can be diffuse or denied (Dana et al., 2007; Conrads et al., 2013; Falk and Szech, 2013a,b; Behnk et al., 2017).

of discrimination. Besides, as agents of recipients, intermediaries' moral costs of discrimination might also be affected by a potential desire to act in recipients' "best interest." If they anticipate that recipients prefer the in-group, the psychological unease from discrimination could decrease and reinforce discrimination.[11]

Hypothesis 2.3 (Intermediaries vs. Recipients)**.** *Intermediaries will discriminate more in favor of recipients' in-group than recipients will do themselves.*

2.4 Results

The results section is organized as follows. Section 2.4.1 contains the study's main results on the average effects of group belonging. Section 2.4.2 decomposes the average effects to shed light on heterogeneity. Section 2.4.3 identifies the precise source of discrimination: first, by utilizing results from the four diagnostic treatments in Section 2.4.3.1, and second, by directly analyzing individual motives of choice in Section 2.4.3.2.

2.4.1 The average effects of group belonging

Table 2.2 presents the study's main results on treatment differences in the average effects of candidates' group belonging. It shows regression results from rank-ordered logit regressions (Beggs et al., 1981) with each of the four rankings as dependent variables. The following relationship is tested:

$$y_{i,j} = \beta_1 East_{i,j} + \beta_{1k} Treat_i^k East_{i,j} + \epsilon \tag{2.3}$$

where $y_{i,j}$ is the rank a candidate j receives from a decision-maker i, $East_{i,j}$ is a dummy variable being 1 when the candidate belongs to the Eastern group and 0 when he belongs to the Western group, $Treat_i^k$ is a categorical variable that indicates decision-maker i's treatment, and ϵ is an independent and identically distributed (iid) error term.[12] Table 2.2 subdivides into the four parts (a), (b), (c), and (d), where each part shows within-treatment effects as the coefficients on the dummy variable *East* and differences to the other treatments

[11] It has been shown that sometimes people act unethically to help others (Danilov et al., 2013; Gino et al., 2013) or to satisfy a preference for collaboration (Weisel and Shalvi, 2015).

[12] Note that the iid assumption on the error term is unproblematic because randomization controls for correlations between decision-makers and between alternatives correlations are controlled by including the only difference between candidates, i.e., their group belonging, as an explanatory variable into the regression model. Further, the usual difficulties with interpreting coefficients from non-linear regression models, as outlined in Ai and Norton (2003), do not apply here because only dummy variables and their interactions are included in the model.

as coefficients on the interaction effects of *East* with the respective treatment dummy.[13] Coefficients represent the difference between the average rank that the two Eastern candidates received and the average rank that the two Western candidates received. Since the maximum possible difference in average ranks is 2, coefficients on the main effects of *East* range from -2 to 2, including 0 when, on average, there is no discrimination. Coefficients on the interaction effects of *East* with a treatment dummy range from -4 to 4.

Table 2.2: Average effects of candidates' Eastern names (main treatments)

	Dependent variable: Baseline treatment			C-Rank (1)	B-Rank (2)	BoC-Rank (3)	BoB-Rank (4)
(a)	Own-WR:	East		-0.078	0.061	n/a	n/a
				(0.176)	(0.174)		
		East	× Own-ER	0.380	0.022	n/a	n/a
				(0.260)	(0.257)		
		East	× WI-Match-WR	-0.582**	-0.401	n/a	n/a
				(0.273)	(0.265)		
		East	× WI-Match-ER	0.438*	-0.239	n/a	n/a
				(0.250)	(0.247)		
(b)	Own-ER:	East		0.302	0.083	n/a	n/a
				(0.191)	(0.189)		
		East	× WI-Match-WR	-0.962***	-0.423	n/a	n/a
				(0.283)	(0.274)		
		East	× WI-Match-ER	0.059	-0.261	n/a	n/a
				(0.261)	(0.257)		
(c)	WI-Match-WR:	East		-0.660***	-0.340*	-1.467***	-1.453***
				(0.209)	(0.199)	(0.246)	(0.246)
		East	× WI-Match-ER	1.020***	0.162	1.856***	1.497***
				(0.274)	(0.265)	(0.306)	(0.307)
(d)	WI-Match-ER:	East		0.361**	-0.178	0.389**	0.044
				(0.177)	(0.175)	(0.182)	(0.183)
	Observations			788	788	380	380
	Individuals			197	197	95	95
	Log likelihood			-617.457	-623.931	-278.261	-280.916

Notes: The table reports regression results from rank-ordered logit regressions. The dependent variable in columns (1), (2), (3), and (4) is, respectively, the C-Rank, the B-Rank, the BoC-Rank, and the BoB-Rank. The independent variables are the dummy variable *East*, being one if a candidate belongs to the Eastern group and 0 otherwise, and its interaction with a categorical variable indicating the four main treatments. The coefficient on *East* presents the average treatment effect of candidates having an Middle-Eastern name in the treatment defined as the baseline treatment. Part (a), (b), (c), and (d) report regression results when the baseline treatment is defined to be Own-WR, Own-ER, WI-Match-WR, or WI-Match-ER. For a full table including the four diagnostic treatments, see Table A.2 in the Appendix. For a graphical illustration, see Figure A.1 in the Appendix. Interaction effects are reported only once to avoid doubling. Standard errors are reported in parentheses. Statistical significance is indicated as follows. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The first column of Table 2.2 shows results from the choice ranking. The -0.078 coefficient on *East* in part (a) means that when Western recipients decide, on average, they rank East-

[13]Interaction effects are reported only once to avoid doubling. Depending on which of the two treatments in a comparison is defined as the baseline, the sign of the interaction effect reverses.

ern candidates 0.078 ranks lower than Western candidates. Statistically, this is not significantly different from zero. In other words, on average, Western recipients do not discriminate. The 0.302 coefficient on *East* in part (b) shows a statistically insignificant tendency in Eastern recipients' choice to favor Eastern candidates. The coefficient on the interaction of *East* with the treatment dummy for *Own-ER* in part (a) means that, on average, Eastern recipients rank Eastern candidates 0.380 ranks higher than Western recipients rank Eastern candidates. However, also this difference is statistically insignificant.

Result 2.1. *On average, neither Western nor Eastern recipients discriminate.*

The coefficient on *East* in column (1) of part (c) in Table 2.2 shows that, in contrast to recipients, Western intermediaries incentivized to match Western recipients' choice strongly discriminate against Eastern candidates. On average, they rank Eastern candidates 0.660 ranks lower than Western candidates. On the contrary, the coefficient on *East* in part (d) shows that when Western intermediaries are incentivized to match Eastern recipients' behavior, their discrimination flips the direction in favor of Eastern candidates. Here, they rank Eastern candidates, on average, 0.361 ranks higher than Western candidates. The difference between the two treatments, represented by the coefficient on the interaction term in part (c), shows that changing the recipient's group from Western to Eastern makes Western intermediaries rank Eastern candidates 1.020 ranks higher than Western candidates. All three of the above differences are statistically significant. Further, comparing Western intermediaries' choice when acting on behalf of Western recipients with Western recipients' own choice reveals that the change in the role leads to the emergence of significant discrimination that does not show in recipients' choice (see -0.582 coefficient on the interaction term *East* × *WI-Match-WR* in part (a) of Table 2.2). On the contrary, Western intermediaries' average discrimination when acting on behalf of Eastern recipients is statistically indistinguishable from the discriminatory tendency shown by Eastern recipients themselves (see 0.059 coefficient on *East* × *WI-Match-ER* in part (b) of Table 2.2).

Result 2.2. *On average, Western intermediaries discriminate significantly in favor of recipients' in-group.*

The second column of Table 2.2 shows results from the belief ranking about candidates' generosity. Overall, there is no substantial evidence that either Western or Eastern participants believe candidates from one group to be more generous than candidates from the other group. This conclusion is confirmed by re-running the regression without treatment dummies but with pooled data over all Western participants in the four main treatments, resulting in an insignificant coefficient of -0.137 (standard error = 0.105, $p = 0.191$) for *East*.[14] Nevertheless, the -0.340 coefficient on *East* in part (c) of Table 2.2 is marginally

[14]The regression is based on 596 observation from 149 individuals in Own-WR, WI-Match-WR, and WI-Match-ER.

significant. Hence, in the treatment in which Western intermediaries strongly discriminate against Eastern candidates, they start reporting to believe Western candidates to be more generous than Eastern candidates. However, as shown by the coefficients on all interaction terms in column (2) of Table 2.2, there is no statistically significant difference in generosity beliefs among the four main treatments.

Result 2.3. *On average, neither Western nor Eastern participants report believing candidates from one group to be more generous than candidates from the other group.*

The third and fourth columns of Table 2.2 report results from intermediaries' belief ranking about recipients' choice and intermediaries' belief ranking over recipients' beliefs of candidates' generosity. Western intermediaries' beliefs about Western recipients' choice and generosity beliefs are vastly biased against Eastern candidates. The coefficients of -1.467 and -1.453 on *East* in part (c) of Table 2.2 are highly significant and reach in magnitude 73% of the maximal possible negative effect of -2. Yet, compared to Western recipients' actual choice and beliefs, they are astonishingly biased. On the contrary, Western intermediaries' predictions about Eastern recipients' choice and beliefs are, on average, remarkably accurate. The 0.389 and 0.044 in columns (3) and (4) of part (d) match the respective coefficients for Eastern recipients' actual choice and beliefs from columns (1) and (2) of part (b) in Table 2.2. Western intermediaries report believing that Eastern recipients discriminate in favor of Eastern candidates in their choice but without believing that Eastern candidates are more generous than Western candidates.

Result 2.4. *On average, Western intermediaries report believing that Western and Eastern recipients discriminate in favor of the in-group. They report that Western recipients believe their in-group to be more generous but that Eastern recipients believe both groups to be similarly generous.*

2.4.2 Heterogeneity

The purpose of this section is twofold. First, it sheds light on the composition of the above-presented average treatment effects. Second, it provides a way to quantify the impact of differences in average ranks from the point of view of a discrete choice decision with the three choice alternatives of favoring Western candidates, not favoring one group over the other, or favoring Eastern candidates.

Table 2.3 shows how a decision-maker's complete ranking is categorized into being in favor of Western candidates ("Fav West"), not favoring one group over the other ("No Fav"), or being in favor of Eastern candidates ("Fav East"). Note that the experimental design provides decision-makers with two ways of not discriminating: first, by arranging the complete ranking such that matching probabilities are equally divided between the two groups, and second, by randomly ranking candidates. Further, randomly ranking is set as the default by randomizing the order in which candidates appear on the list in each decision screen across decision-makers and within any decision-maker's different rankings. Hence, any systematic effect found must result from active cognitive engagement by decision-makers.

Table 2.3: Categorization of complete ranking orderings

Complete ranking ordering	Rank 1	West	West	West	East	East	East
	Rank 2	West	East	East	West	West	East
	Rank 3	East	West	East	West	East	West
	Rank 4	East	East	West	East	West	West
Average group rank	West	1.5	2.0	2.5	2.5	3.0	3.5
	East	3.5	3.0	2.5	2.5	2.0	1.5
Matching probability	West	70%	60%	50%	50%	40%	30%
	East	30%	40%	50%	50%	60%	70%
Categorization		Fav West		No Fav		Fav East	

Notes: The table shows how a decision-maker's complete ranking is categorized into being in favor of Western candidates ("Fav West"), not favoring one group over the other ("No Fav"), or being in favor of Eastern candidates ("Fav East"). The upper part lists the six orders in which one can arrange two groups on four ranks. The two middle parts show the average group ranks and matching probabilities that result from each of the six possible complete ranking orderings. The lower part presents the categorization.

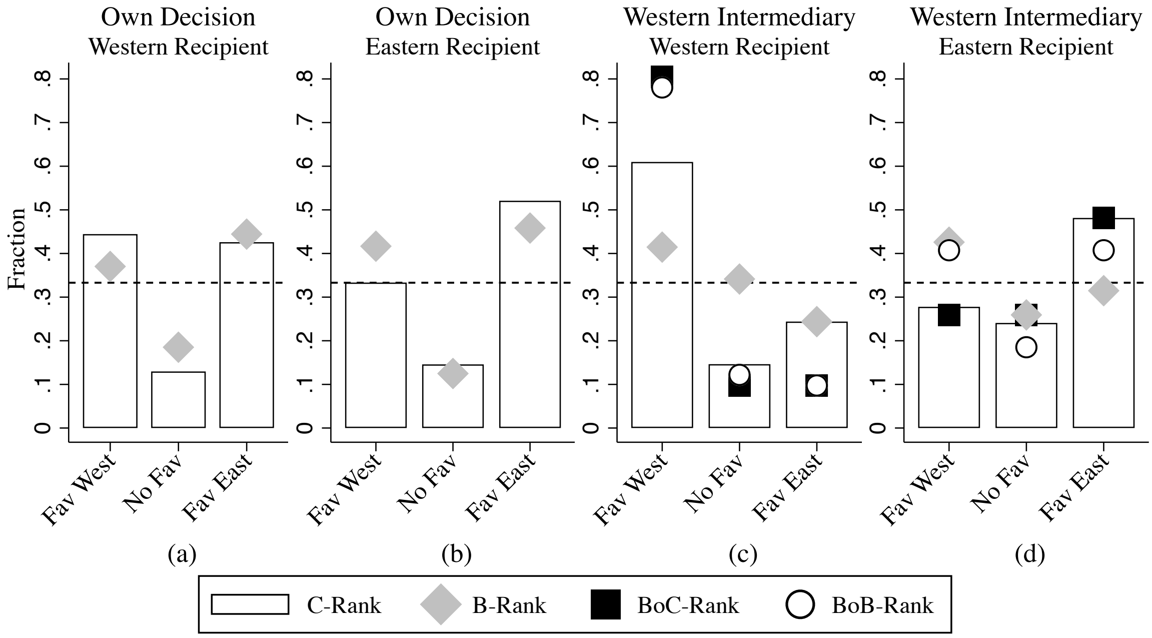
Figure 2.1 illustrates each treatment's shares of complete rankings categorized according to Table 2.3.[15] Bars, diamonds, squares, and circles symbolize the choice, belief, belief over the recipient's choice, and belief over the recipient's belief ranking, respectively. The dashed line at 0.333 represents the shares that would result from an entirely random ranking of candidates. Decision-makers do not randomly rank candidates in any ranking of any treatment ($p < 0.001$ for all rankings in all treatments, Kolmogorov-Smirnov tests against uniform distribution). Table 2.4 complements Figure 2.1 by providing statistical tests and odds ratios from multinomial logistic regressions for within-treatment comparisons of the shares of the three categories of ranking behavior.

The bars in graphs (a) and (b) of Figure 2.1 reveal that the surprising result of no discrimination in recipients' choice is caused by stark polarization and not because recipients choose not to discriminate. With both Western and Eastern recipients, two large fractions that each discriminate in favor of another group balance each other out in the average. In both cases, the fraction of choices that can undoubtfully be identified as non-discriminatory is actually below 15%.[16] Part (a) of Table 2.4 shows that the share of Western recipients favoring West-

[15] See Table A.3 in the Appendix for the data underlying Figure 2.1.

[16] Note that since participants who choose to rank randomly have an equal probability of ending up in either one of the three ranking categories, the actual share of non-discriminatory choices might be larger than the 15%.

Figure 2.1: Heterogeneity



Notes: The figure illustrates the each of the four main treatment's shares of complete rankings categorized according to Table 2.3. The dashed line at 0.333 represents the shares that would result from an entirely random ranking of candidates. For a illustration of all eight treatments, see Figure A.2 in the Appendix.

ern (Eastern) candidates is 3.429 (3.286) times larger than the share of non-discriminatory choice rankings. Part (b) of Table 2.4 shows a similar polarization for Eastern recipients, however, with the pole in favor of Eastern candidates being 1.563 (1/0.640) times larger than the pole in favor of Western candidates, although without reaching statistical significance.

Result 2.5. *Decomposing recipients' average choice reveals a stark polarization with two large fractions that balance each other out in the average by discriminating in favor of different groups.*

The bars in graphs (c) and (d) of Figure 2.1 illustrate that when Western participants change their role from recipients to intermediaries, the polarization in their choice disappears. When incentivized to match the choice of Western recipients, 61% of intermediaries favor Western candidates. Part (c) of Table 2.4 shows that this is 4.167 and 2.500 times larger than the shares of non-discriminatory choice rankings and those that favor Eastern candidates, respectively. When they are incentivized to match Eastern recipients' choice, 48% discriminate in favor of Eastern candidates, which is 2.000 times the size of the non-discriminatory choices and 1.733 (1/0.577) times the share of those who favor Western candidates (see part (d) of Table 2.4). Further, while the average discrimination by Western intermediaries acting on behalf of Eastern recipients and Eastern recipients' own discriminatory tenden-

Table 2.4: Heterogeneity

Dependent variable:			Categorization of ...			
			C-Rank	B-Rank	BoC-Rank	BoB-Rank
	Treatment		(1)	(2)	(3)	(4)
(a)	Own-WR	Fav West / Fav East	1.043	0.833	n/a	n/a
			(0.304)	(0.252)		
		Fav West / No Fav	3.429***	2.000*	n/a	n/a
			(1.473)	(0.775)		
		Fav East / No Fav	3.286***	2.400**	n/a	n/a
			(1.418)	(0.903)		
		Observations	54	54	n/a	n/a
		Log likelihood	-53.394	-56.191	n/a	n/a
(b)	Own-ER	Fav West. / Fav East	0.640	0.909	n/a	n/a
			(0.205)	(0.281)		
		Fav West / No Fav	2.286*	3.333**	n/a	n/a
			(1.036)	(1.552)		
		Fav East / No Fav	3.571***	3.667***	n/a	n/a
			(1.527)	(1.689)		
		Observations	48	48	n/a	n/a
		Log likelihood	-47.363	-47.150	n/a	n/a
(c)	WI-Match-WR	Fav West / Fav East	2.500**	1.700	8.250***	8.000***
			(0.935)	(0.677)	(4.368)	(4.243)
		Fav West / No Fav	4.167***	1.214	8.250***	6.400***
			(1.894)	(0.438)	(4.368)	(3.078)
		Fav East / No Fav	1.667	0.714	1.000	0.800
			(0.861)	(0.296)	(0.707)	(0.537)
		Observations	41	41	41	41
		Log likelihood	-38.008	-44.119	-25.781	-27.761
(d)	WI-Match-ER	Fav West / Fav East	0.577*	1.353	0.538*	1.000
			(0.187)	(0.433)	(0.178)	(0.302)
		Fav West / No Fav	1.154	1.643	1.000	2.200**
			(0.437)	(0.557)	(0.378)	(0.839)
		Fav East / No Fav	2.000**	1.214	1.857*	2.200**
			(0.679)	(0.438)	(0.616)	(0.839)
		Observations	54	54	54	54
		Log likelihood	-56.730	-58.177	-56.801	-56.373

Notes: The table reports exponentiated coefficients (odds ratios) from multinomial logistic regressions. The dependent variable in columns (1), (2), (3), and (4) is the categorization according to Table 2.3 of the C-Rank, the B-Rank, the BoC-Rank, and the BoB-Rank, respectively. No independent variables are included. Parts (a), (b), (c), and (d) report within-treatment results for the indicated treatment. For the respective results from the four diagnostic treatments, see Table A.4 in the Appendix. Note that multinomial regressions require defining one outcome as the baseline for the odds ratios. The baseline outcome is altered to retrieve all three within-treatment ratios. Standard errors are reported in parentheses. Statistical significance is indicated as follows. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

cies looked remarkably similar, the analysis of heterogeneity reveals that they do differ in respect to polarization and thus, after all, are not as similar as it seemed.

Result 2.6. *Decomposing Western intermediaries' average choice reveals that intermediation creates discrimination by eliminating polarization.*

The diamonds in Figure 2.1 show that recipients' generosity beliefs are similarly polarized as their choice. The polarization is stronger in Eastern recipients' beliefs, where the fraction reporting believing Western (Eastern) candidates to be more generous is 3.333 (3.667) times larger than those who report not believing in a group-based difference (see column (2) in part (a) of Table 2.4). The respective number for Western recipients is 2.000 (2.400) (see column (2) in part (b) of Table 2.4). As with their choice, changing Western participants' role from recipients to intermediaries eliminates polarization in reported beliefs. However, in all three main treatments with Western participants, the fraction of those who report believing Western candidates to be more generous is left unchanged at approximately 42%. Hence, the disappearance of polarization is driven by a shift from the fraction reporting to believe Eastern candidates to be more generous towards the fraction that does not report to expect a group-based difference in generosity.

Result 2.7. *Decomposing recipients' generosity beliefs reveals a stark polarization that disappears when decision-makers are in the role of intermediaries.*

The squares and circles in graphs (c) and (d) of Figure 2.1 decompose the average effects of the BoC-Rank and BoB-Rank. An overwhelming majority of 81% of Western intermediaries report believing that Western recipients discriminate in favor of Western candidates. This fraction is 8.250 times larger than the fraction reporting that Western recipients discriminate in favor of Eastern candidates and also 8.250 times the fraction reporting that Western recipients do not discriminate (see column (3) in part (c) of Table 2.4). A similar large fraction of 78% reports that Western recipients expect Western candidates to be more generous than Eastern candidates. This fraction is 8.000 larger than the fraction reporting that Western recipients expect Eastern candidates to be more generous and 6.400 times the fraction reporting that Western recipients do not believe in a group-based difference in generosity (see column (4) in part (c) of Table 2.4). When asked about Eastern recipients, 48% of Western intermediaries report that Eastern recipients discriminate in favor of Eastern candidates. This fraction is 1.859 (1/0.538) times larger than the fractions reporting that Eastern recipients discriminate in favor of Western candidates and 1.857 times the fraction reporting that Eastern recipients do not discriminate (see column (3) in part (d) of Table 2.4). Here, again, the analysis of heterogeneity reveals that the comparison of the average effect from Western intermediaries' beliefs about Eastern recipients' choice with the average effect of Eastern recipients' actual choice was misleadingly suggesting that beliefs are unbiased. While Western intermediaries accurately predict the fraction of Eastern recipients discriminating in favor of Eastern candidates, they underestimate the fraction discriminating in favor of Western candidates and overestimate the fraction of non-discriminatory choices. Western intermediaries' second-order beliefs about Eastern recipients' generosity beliefs are, as Eastern recipients' actual beliefs, highly polarized. Both poles are 41% in size and 2.200 times the size of Western intermediaries reporting to believe that Eastern recipients do not expect a group-based difference in generosity (see column (4) in part (d) of Table 2.4).

Result 2.8. *The vast majority of 81% of Western intermediaries report believing that Western recipients discriminate in favor of Western candidates, and 78% report believing that Western recipients expect that Western candidates are more generous than Eastern candidates. When asked about Eastern recipients' choice, a majority of around half report believing that Eastern recipients discriminate in favor of Eastern candidates but without reporting to believe that Eastern recipients expect one group to be more generous than the other.*

2.4.3 Reasons to discriminate

The above results show that changing Western participants' role transforms their highly polarized but, on average, non-discriminatory choice as recipients into significant discrimination in favor of recipients' in-group when being in the role of intermediaries. The change in discrimination seems to be caused by biased beliefs about recipients' behavior. This section tests alternative explanations.

2.4.3.1 Testing taste-based and statistical discrimination

Economists explain discrimination as either being driven by decision-makers' beliefs about differences in groups' performances (statistical discrimination) or decision-makers' preferences (taste/preference-based discrimination) for groups. Result 2.1 shows that in combination, on average, they do not cause discrimination in Western recipients' choice. However, concluding from Western recipients' behavior that both explanations are not causing discrimination in Western intermediaries' choice might be problematic because, on the one hand, the decision situations differ in multiple aspects and, on the other hand, the two types of discrimination might have canceled each other out. This section utilizes results from the four diagnostic treatments to test (i) whether changing Western participants' role by itself creates discrimination and (ii) what portion of the role-dependent effects are caused by taste-based and statistical discrimination.

First, for a clean test of the potential effects of a pure change in role, in the treatments WI-Same-WR and WI-Same-ER, intermediaries' monetary incentives are perfectly aligned with recipients' incentives by paying them whatever amount the match candidate has decided to share with the recipient. In other words, while monetary incentives are kept constant, Western participants' role changes from a recipient deciding for himself to an intermediary deciding either for an in- or out-group member. Second, in the treatments WI-Fix-WR and WI-Fix-ER, intermediaries' monetary incentives are altered to a fixed payment of 50 euros. Thus, since with fixed payment, intermediaries have no economic incentive to discriminate, any discrimination detected must result from their preferences for groups. Lastly, subtracting the discrimination in WI-Fix-WR from that in WI-Same-WR and the discrimination in WI-Fix-ER from that in WI-Same-ER reveals the respective fractions of statistical discrimination.

Table 2.5: Average effect of candidates' Eastern names (diagnostic treatments)

Dependent variable:		C-Rank	B-Rank	BoC-Rank	BoB-Rank
Baseline treatment		(1)	(2)	(3)	(4)
WI-Same-WR:	East	-0.281	0.175	-0.895***	-0.883***
		(0.194)	(0.198)	(0.207)	(0.207)
WI-Same-ER:	East	0.191	-0.063	0.271	0.616***
		(0.184)	(0.186)	(0.195)	(0.201)
WI-Fix-WR:	East	0.090	0.331*	-0.918***	-0.907***
		(0.179)	(0.185)	(0.201)	(0.201)
WI-Fix-ER:	East	0.362*	0.390**	0.833***	0.480**
		(0.188)	(0.187)	(0.203)	(0.190)

Notes: The table summarizes the within-treatment effects from Table A.2 in the Appendix, which itself expands Table 2.2 by including all treatments into the rank-ordered regressions. See notes of Table 2.2 for further explanations. See Table A.2 in the Appendix for full table of results, including interaction terms between all eight treatment. Standard errors are in parentheses. Statistical significance is indicated as follows. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.5 presents the within-treatment effects of candidates' Eastern names on each of the four rankings and from each of the four diagnostic treatments.[17] Results from the treatments WI-Same-WR and WI-Same-ER in column (1) show that a pure change in role does not cause the same level of discrimination as detected in the two main intermediation treatments. The -0.281 and 0.191 coefficients from WI-Same-WR and WI-Same-ER account for 48% and 44% of the discrimination in WI-Match-WR and WI-Match-ER, respectively. While both coefficients are neither statistically different from zero nor from the coefficient on *East* from the treatments Own-WR, WI-Match-WR, and WI-Match-ER in Table 2.2, they do significantly differ from one another (see Table A.2 in the Appendix). Thus, under the same monetary incentives, Western intermediaries' discrimination significantly shifts from a tendency to favor Western candidates when acting on behalf of Western recip-

[17]See Table A.2 in the Appendix for a full table including interaction effects with all other treatments.

ients to a tendency to favor Eastern candidates when acting on behalf of Eastern recipients. This result is the first of two pieces of evidence showing Western intermediaries' revealed preferences for social groups to be malleable and context-dependent.

Result 2.9. *Keeping monetary incentives constant and changing Western participants' role from a recipient deciding for himself to an intermediary deciding on behalf of an in-group member in WI-Same-WR or out-group member in WI-Same-ER causes 48% and 44% of the discrimination detected in WI-Match-WR and WI-Match-ER, respectively.*

Results from the treatments WI-Fix-WR and WI-Fix-ER in column (1) of Table 2.5 provide the second piece of evidence showing that Western intermediaries' revealed preferences for groups are malleable and context-dependent. Western intermediaries without monetary incentives to discriminate show no preference for a group when they act on behalf of Western recipients. Further, the insignificant and close-to-zero coefficient from WI-Fix-WR in column (1) of Table 2.5 has no explanatory value for the discrimination detected in WI-Match-WR. On the contrary, when acting on behalf of Eastern recipients, Western intermediaries with fixed payoffs reveal a preference for Eastern candidates. Moreover, the significant coefficient of 0.362 in WI-Fix-ER almost precisely matches the respective effect in WI-Match-ER. Thus, preference-based discrimination can explain 100% of Western intermediaries' discrimination when incentivized to match Eastern recipients' choice. Of course, this result does not mean that biased beliefs about others' behavior would not have caused an effect – as they do in WI-Match-WR.

Further, to calculate the strength of statistical discrimination, one needs to subtract the 0.090 coefficient in WI-Fix-WR from the -0.281 coefficient in WI-Same-WR and gets -0,371. Thus, when Western intermediaries decide on behalf of Western recipients, there seems to be some statistical discrimination against Eastern candidates. Nevertheless, the -0.371 accounts for 56% of the -0.660 coefficient in WI-Match-WR. In other words, statistical discrimination reaches, at maximum, 56% of the total discrimination in WI-Match-WR. Similarly, when one subtracts the 0.362 coefficient in WI-Fix-ER from the 0.191 coefficient in WI-Same-ER, one gets -0,171 for the strength of statistical discrimination. Thus, when Western intermediaries decide on behalf of Eastern recipients, statistical reasoning, again, causes discrimination against Eastern candidates. Further, since discrimination runs directionally opposite the discrimination in WI-Match-ER, statistical discrimination has no explanatory power in this case.[18]

[18] See in Table A.2 of the Appendix that the here-described treatment differences are not statistically significant. Therefore, while this analysis is informative, it is also rather suggestive than definitive evidence regarding the strength of taste-based and statistical discrimination. Nevertheless, it fulfills its primary objective of identifying whether taste-based or statistical discrimination can explain the discrimination in WI-Match-WR and WI-Match-ER.

Result 2.10. *Taste-based discrimination cannot explain any of the discrimination in WI-Match-WR but can explain 100% of the discrimination in WI-Match-ER. Statistical discrimination reaches 56% of the discrimination in WI-Match-WR but cannot explain any of the discrimination in WI-Match-ER.*

The second column of Table 2.5 shows results from the belief rankings of the four additional treatments. The insignificant coefficients of 0.175 from WI-Same-WR and -0.063 from WI-Same-WR show that Western intermediaries with monetary incentives based on candidates' generosity do not report believing in a group-based difference in generosity. However, Western intermediaries start reporting that Eastern candidates are more generous than Western candidates when their payoff is fixed. Interestingly, this effect appears independent of the recipient's group (see 0.331 and 0.390 coefficients in column (2) of Table 2.5).

Since there are no significant treatment differences in reported generosity within each of the three incentive schemes (see Table A.2 in the Appendix), the regression is re-run with pooled data over recipients' group belonging. Results show that with incentives based on candidates' generosity, Western intermediaries do not report believing in a group-based difference in generosity (coefficient *East* = 0.049, standard error = 0.135, $p = 0.719$). On the other hand, the treatments with fixed payments cause Western intermediaries to report believing that Eastern candidates are more generous than Western candidates (coefficient on *East* = 0.360, standard error = 0.131, $p = 0.006$). In contrast, treatments with monetary incentives to match recipients' choice make them report to believe that Western candidates are more generous than Eastern candidates (coefficient *East* = -0.249, standard error = 0.131, $p = 0.058$). Thus, monetary incentives affect what Western intermediaries report to believe about candidates' generosity.[19]

Finally, to check whether there is an overall tendency in Western intermediaries' beliefs, data are pooled over all six treatments with Western intermediaries, and the regression is again re-run. Overall, Western intermediaries do not report believing in a group-based difference in candidates' generosity (coefficient on *East* = 0.054, standard error = 0.076, $p = 0.477$).[20]

[19]This regression is based on 1,136 observations from 284 individuals in the role of Western intermediaries. Differences between incentives schemes "Fix" and "Same" is marginally significant ($p = 0.098$), between "Fix" and "Match" is highly significant ($p = 0.001$), and between "Same" and "Match" is not significant ($p = 0.114$). However, when Western recipients are included in the "Same" inventive scheme to increase power, the difference between "Same" and "Match" becomes marginally significant ($p = 0.058$). In the latter case, the regression is based on 1,352 observations from 338 individuals.

[20]This regression, too, is based on 1,136 observations from 284 individuals in the role of Western intermediaries. Differences between incentive schemes are significant. Results neither change when Western recipients are included nor when both Western and Eastern recipients are included.

Result 2.11. *Treatments affect the generosity beliefs Western intermediaries report as follows. Western intermediaries with incentives based on candidates' generosity do not report believing in a group-based difference. With fixed incentives, they report believing Eastern candidates to be more generous than Western candidates, and with incentives based on matching others' behavior, vice versa. Overall, they do not report believing in a group-based difference.*

The third and fourth columns of Table 2.5 show results from the BoC-Rank and the BoB-Rank. When Western intermediaries are asked about the choice and beliefs of Western recipients, their answer is clear and consistent with their answer in the main intermediation treatments. They report believing that Western recipients strongly discriminate in favor of Western candidates (see -0.895 and -0.918 coefficients in column (3) of Table 2.5) and that they expect Western candidates to be more generous than Eastern candidates (see -0.883 and -0.907 coefficients in column (4) of Table 2.5). Nevertheless, compared to the respective results from WI-Match-WR, both effects are significantly weaker in WI-Same-WR and WI-Fix-WR (see Table A.2 in the Appendix).

Western intermediaries' answers are less clear when asked about Eastern recipients' choice and beliefs. As before, they report believing that Eastern recipients will discriminate in favor of Eastern candidates, even if statistical significance is only reached in WI-Fix-ER but not in WI-Same-ER (see 0.271 and 0.833 coefficients in column (3) of Table 2.5). When asked about Eastern recipients' generosity beliefs, their answer now differs from the highly polarized result WI-Match-ER. In both WI-Same-ER and WI-Fix-ER, polarization disappears, and Western intermediaries, on average, report believing that Eastern recipients expect Eastern candidates to be more generous than Western candidates (see 0.616 and 0.480 coefficients in column (4) of Table 2.5).

To check for overall effects, data is pooled over incentive schemes, and the regression is rerun. Unsurprisingly, the results show that Western intermediaries believe Western recipients to discriminate strongly in favor of Western candidates (coefficient on *East* = -1.063, standard error = 0.124, $p < 0.001$) and to expect Western candidates to be more generous than Eastern candidates (coefficient on *East* = -1.051, standard error = 0.124, $p < 0.001$). The pooled data also provide a clearer picture of Western intermediaries' beliefs about Eastern recipients. Overall, they report believing Eastern recipients to discriminate in favor of Eastern candidates (coefficient on *East* = 0.490, standard error = 0.111, $p < 0.001$) and to believe Eastern candidates to be more generous than Western candidates (coefficient on *East* = 0.366, standard error = 0.110, $p = 0.001$). Both effects are weaker in the reported beliefs about Eastern recipients compared to beliefs about Western recipients.[21]

[21] Both regressions here are based 1,136 observations from on 284 participants in the role of intermediaries. The log likelihood of the regression with the BoC-Rank as dependent variable is -852.142 and that of the regression with the BoB-Rank as dependent variable is -857.342.

Further, while reported beliefs about Western recipients seem to be in line with expecting them to be statistical discriminators, beliefs about Eastern recipients suggest that they are expected to have a taste for the in-group. To check whether the data backs this impression, the share of Western intermediaries whose reported beliefs are in line with expecting the recipient to be a payoff-maximizer is calculated, i.e., when BoC-Rank and BoB-Rank overlap. Results show that 64% of Western intermediaries report believing Western recipients to be payoff-maximizers, whereas only 53% report believing Eastern recipients to do so (p = 0.055, Pearson χ^2 test).[22]

Result 2.12. *Overall, Western intermediaries report believing recipients to discriminate in favor of their in-group and to believe that the in-group is more generous than the out-group. While Western intermediaries seem not to expect Western recipients to have a taste for discrimination, they expect Eastern recipients to do so.*

2.4.3.2 Beliefs about others' behavior as a source of discrimination

This section utilizes beliefs to shed light on changes in specific motives of choice. The first part tests whether treatments affect participants' propensity to maximize their payoff. The second part further analyzes intermediaries' motives regarding being in line with maximizing the recipient's payoff or matching the recipient's choice.

Decision-makers in all treatments except those with fixed payments can maximize their own monetary payoff. When monetary incentives are based on candidates' generosity, i.e., in Own-WR, Own-ER, WI-Same-WR, and WI-Same-ER, decision-makers maximize their payoff by choosing according to their beliefs about candidates' generosity. Here, a decision-maker is categorized as an own-payoff-maximizer when his C-Rank overlaps with his B-Rank. In WI-Match-WR and WI-Match-ER, decision-makers maximize their monetary payoff by choosing according to their beliefs about recipients' choice. Here, a decision-maker is categorized as an own-payoff-maximizer when his C-Rank overlaps with his BoC-Rank.[23]

Treatments do not cause a statistically significant change in the share of participants whose choice is consistent with own-payoff-maximization. In Own-WR and Own-ER, the respective shares are 54% and 46% and do not differ from one another ($p = 0.427$, Pearson χ^2 test). Holding monetary incentives constant but changing decision-makers' role to an intermediary who is acting on behalf of a Western recipient does increase the share of own-payoff-maximizers to 67% in WI-Same-WR; however, it neither differs significantly from the 50%

[22]The calculation of these shares is based on all 284 participants in the role of intermediaries. Note that the remaining 36% and 47% of Western intermediaries who do not report Western and Eastern recipients to be payoff-maximizers include the share of Western intermediaries that might have ranked candidates randomly.

[23]Note that individual motives are identified through checking for overlaps of the choice ranking with the respective beliefs ranking based on the categorization of complete rankings provided in Table 2.3.

in WI-Same-ER ($p = 0.107$, Pearson χ^2 test) nor from the respective number in Own-WR ($p = 0.191$, Pearson χ^2 test). In the main intermediation treatments, WI-Match-WR and WI-Match-ER, the respective shares are 56% and 50%, and here too, they do not differ from one another ($p = 0.555$, Pearson χ^2 test). Importantly, both also do not differ significantly from the 54% share in Own-WR ($p \geq 0.700$ from pairwise comparisons with Own-WR, Pearson χ^2 tests). Therefore, the data does not provide evidence that the emergence of discrimination in WI-Match-WR and WI-Match-ER can be explained by a change in participants' propensity to maximize their own payoff.

Result 2.13. *Treatments have no significant effect on the share of participants who report their choice to be consistent with own-payoff-maximization.*

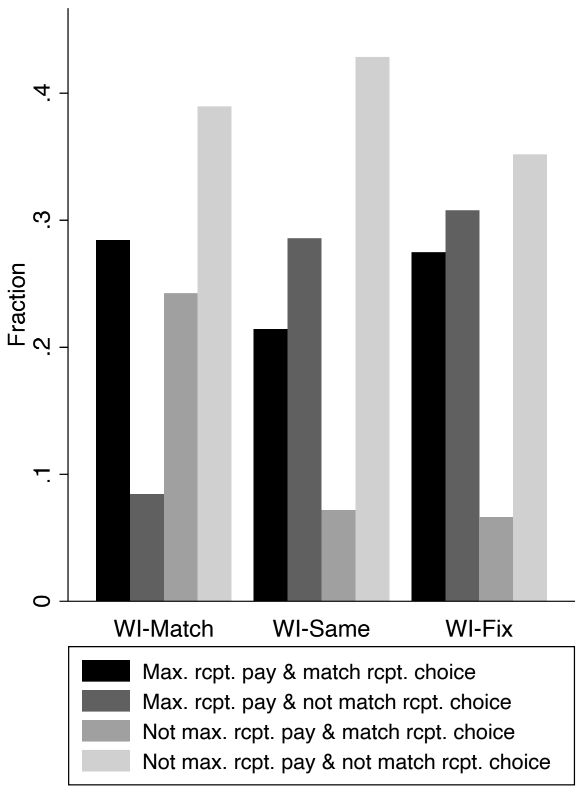
Intermediaries' motives can be further characterized as being in line with maximizing the recipient's payoff or matching the recipient's choice. An intermediary is categorized as a recipient-payoff-maximizer when his choice ranking overlaps with his belief ranking about candidates' generosity and as a recipient-choice-matcher when his choice ranking overlaps with his belief over the recipient's choice ranking. Hence, the experimental design allows to identify the following four distinct motives that can drive intermediaries' choice: (i) maximizing the recipient's payoff and matching his choice, (ii) maximizing the recipient's payoff but not matching his choice, (iii) not maximizing the recipient's payoff but matching his choice, (iv) neither maximizing the recipient's payoff nor matching his choice.

Although changing the recipient's group causes treatment differences in choice within each of the three incentive schemes, it does not result in differences in the distribution of the above four motives ($p \geq 0.160$, Pearson χ^2 tests with pairwise comparisons).[24] Therefore, Figure 2.2 illustrates the distribution of intermediaries' motives for each of the three incentive schemes, pooled over recipients' group belonging. The distributions of motives produced by fixed monetary incentives and monetary incentives based on beliefs over candidates' generosity are indistinguishable ($p = 0.678$, Pearson χ^2 test). However, the distribution of motives that results from monetary incentives to match recipients' choice does significantly differ from both of the aforementioned ($p < 0.001$ for both pairwise comparisons, Pearson χ^2 tests). Taking a closer look at the shares of the four motives reveals that the difference stems from a shift from the share of intermediaries reporting to maximize the recipient's payoff and not matching his choice to the share of intermediaries reporting not to maximize the recipient's payoff but to match his choice.

Result 2.14. *Monetary incentives to match others' behavior shift Western intermediaries' self-reported motives of choice away from maximizing the recipient's payoff but not matching his choice towards matching the recipient's choice but not maximizing his payoff.*

[24]See Table A.5 in the Appendix for exact shares.

Figure 2.2: Motives of discrimination



Notes: The figure illustrates the distribution of the four distinct motives behind Western intermediaries' behavior, separately for each of the three incentive schemes that intermediaries faced and pooled over the recipients' group belonging.

2.5 Discussion

The main finding is clear; intermediation leads to the emergence of discrimination that does not exist in participants' choice when they decide for themselves. Intermediaries' choice is unambiguously directed in favor of recipients' in-group. This happens despite the experimental design providing two ways not to discriminate: either through the equal or random allocation of probabilities. The latter of the two is even set to be the default. But, this is only part of the story. Two further aspects of the study are crucial in understanding what happens. First, the analysis of heterogeneity reveals that the reason why there is no discrimination when recipients decide for themselves is not that they choose not to discriminate but because their behavior is starkly polarized. With both Western and Eastern recipients deciding, a large fraction favors in-group candidates, and a counterbalancing fraction favors out-group candidates. Intermediation eliminates polarization and leads to discrimination in favor of recipients' in-group. Second, the detected discrimination can neither be fully explained by preferences for groups nor by beliefs about differences in groups' performances. Instead, the study finds strong evidence that biased beliefs about others' discriminatory behavior are its root cause. Intermediaries discriminate because they want to maximize their payoff by matching the recipient's choice; however, they utterly fail to predict it accurately. Their failure stems from an inability to foresee that large fractions of recipients discriminate in favor of the out-group.

Identifying why intermediaries' beliefs are biased is beyond the scope of this paper.[25] Nevertheless, a few possibilities are worth discussing. First, some version of projection bias might be at work. Projection bias describes humans' tendency to assume that others' preferences, beliefs, and behavior are similar to their own.[26] Adam Smith (1759) formulated this idea in *The theory of moral sentiments* as follows: *"As we have no immediate experience of what other men feel, we can form no idea of the manner in which they are affected, but by conceiving what we ourselves should feel in the like situation."* However, projection bias alone cannot explain why Western intermediaries fail to predict the behavior of Western recipients, i.e., their in-group's behavior. Here, projection bias might be coupled with the tendency to see oneself as morally superior to others. Western intermediaries could be correctly projecting their own innate preferences into Western recipients, but, at the same time, they might assume that while they themselves are moral enough to resist it, others might not be. Second, the bias in intermediaries' beliefs could also be driven by stereotypical thinking, as described in Bordalo et al. (2016). The authors present a formal model of stereotypes, based on the representativeness heuristic of Kahneman and Tversky (1972), stating that a group is assessed by overweighting its representative type, i.e., its relatively most frequent type. If most intermediaries believe discrimination in favor of the in-group to be recipients' most frequent choice, the bias in their beliefs could accumulate mechanically despite them having unbiased beliefs about the full distribution of types of recipients' behavior. Third, intermediaries may also underestimate social norms that prescribe non-discriminatory behavior or even affirmative action. A similar result from another decision domain is presented in Bursztyn et al. (2020), showing that Saudi Arabian men privately support women working outside the home but at the same time underestimate support by other similar men. The authors relate their result to the idea of "pluralistic ignorance," which describes a situation in that people privately hold an opinion but incorrectly assume that others either do not hold a similar opinion or even hold the contrary opinion (Katz et al., 1931).

[25] Biased beliefs have been documented in several instances, such as in predicting wealth inequality (Norton and Ariely, 2011), the returns on schooling (Jensen, 2010), the prevalence of affirmative actions (Kravitz and Platania, 1993), others' mathematics skills (Bohren et al., 2019) and others' support for women working outside the home (Bursztyn et al., 2020).

[26] Projection bias in predicting one's own future preferences is the subject of a growing literature in economics, starting with Loewenstein et al. (2003). More recently, Bushong and Gagnon-Bartsch (2020) provide evidence for interpersonal projection bias. Ambuehl et al. (2019) show that it extends to situations of paternalistic behavior towards others.

This study provides another essential insight; revealed preferences for groups are malleable and context-dependent. When Western participants decide for themselves or as fixedly paid intermediaries on behalf of in-group members, their choice reveals no specific group preference. However, when they act as fixedly paid intermediaries on behalf of Eastern recipients, they clearly show a preference for Eastern candidates. The change in preference is likely to be caused by context-dependent differences in the moral costs of discrimination. When Western intermediaries act as agents of Eastern recipients, moral costs of discrimination in favor of the in-group might increase because intermediaries account for the in-group preference they believe Eastern recipients have. Interestingly, they seem not to expect Western recipients to have a preference for the in-group. Comparing Western participants' choice when deciding for themselves with their choice as intermediaries acting on behalf of in-group members and with the same monetary incentives as recipients shows that the pure change in role does create a tendency to discriminate in favor of Western candidates. In this case, intermediation seems to decrease the costs of discrimination to some extent. However, since they do not expect Western recipients to have a preference for the in-group, a reduction in moral costs cannot be caused by intermediaries accounting for recipients' taste but might instead be due to a more general mechanism such as diffusion of responsibility.

Interpretation of the study's results requires a discussion of potential limitations. Two general and related concerns with social science studies that include subjects aware of their participation are the experimenter demand effect and social desirability bias.[27] Both state that, under certain conditions, subjects' behavior in experiments might deviate from their actual behavior. The experimenter demand effect occurs when subjects form beliefs about the study's objectives and adjust their behavior to help the experimenter achieve these objectives. The social desirability bias occurs when subjects adjust their behavior in a way they deem socially appropriate, mostly leading to an underdetection of socially undesirable and overdetection of socially desirable behavior. For the study at hand, it seems unlikely that subjects would discriminate only to help the experimenter. Social desirability bias, on the other hand, cannot be entirely ruled out. In general, two sources might cause it. First, subjects could fear that the experimenter or others think badly of them if they show discriminatory behavior. Second, they could also want to avoid taking actions that threaten their self-image as non-discriminatory persons. The experimental design includes several measures to prevent a social desirability bias induced solely due to observability by others. These measures include providing monetary incentives, keeping any explicit clues about the purpose of the study out from the neutrally held instruction, ensuring complete anonymity, and conducting the study online, self-administered by subjects and without the presence of an experimenter. Thus, observability by others is probably lower than in most

[27] For a discussion of the experimenter demand effect and the social desirability bias, see Zizzo (2010) and Fisher (1993), respectively.

intermediation situations in the real world. Social desirability effects due to self-image concerns, of course, cannot be ruled out. However, they are also inseparable from the research question. In most real-world situations where one chooses from a list of candidates belonging to different groups, especially when some of the groups are undoubtfully identified as being discriminated against, reflecting one's decision, at least implicitly, regarding self-image concerns might be unavoidable.

As with all experimental studies, one should be cautious with extrapolating from the results. The purpose of this study is to cleanly isolate biased beliefs about others' behavior as a driving force of discrimination. Therefore, the focus of the study is clearly on internal validity. Nevertheless, several aspects of the experimental design speak for the external validity of the main insights. The decision situation is unlikely to be perceived as artificial or abnormal by participants because: (i) choosing between candidates based on (ii) real identities that are politically highly vocal in the debate of discrimination, and (iii) acting on behalf of others (iv) incentivized by either one of the three incentive schemes are frequently occurring decision situations outside the laboratory. On the other hand, of course, the choice of the sample limits the generalization of the results. However, it is unclear if, compared to a representative sample, the choice of the particular all-male sample leads to an over-or underdetection of discrimination. On the one hand, it might lead to overdetection because the literature on discrimination has documented that women sometimes discriminate less than men (Lane, 2016). On the other hand, it might lead to underdetection because, compared to a representative sample, participants are younger and more educated. Compared to real-life intermediation situations, discrimination might also be under-detected because of the relatively low monetary incentives.

While one can argue about the exact size of the effect, the idea that beliefs about others' behavior can cause discrimination is supported by field evidence. For example, Yinger (1986) investigates discrimination in the US housing market and concludes that *"Housing agents cater to the racial prejudice of current or potential white customers."* Over the years, Yinger and coauthors have repeated and extended the analysis numerous times and consistently found support for what they call the *"white-customer-prejudice hypothesis"* (see, e.g., Ondrich et al., 2003). Another example of field evidence in support of the idea that beliefs about others behavior cause discrimination is found in Bar and Zussman (2017). The authors study discrimination against Arab workers in the Israeli market for labor-intensive services and document that employers avoid hiring Arabs because they believe their Israeli customers do not want Arabs to work inside their homes.

2.6 Conclusion

By identifying biased beliefs about others' discriminatory behavior as a root cause of discrimination, this study adds to long-standing literature that is in the search for reasons why people discriminate. Further, it shows the implications of this additional reason of discrimination for decision situations where intermediaries decide on behalf of others and documents the emergence of discrimination that does not exist without intermediation. However, intermediation in decision-making is likely not the only situation where people discriminate because they incorrectly believe that others want to discriminate. For example, Daskalova (2018) finds discrimination to occur in groups of two in-group members but not in their individual decisions. She reflects that "... *higher-order beliefs about co-decision maker behavior may be a factor behind discrimination in collective settings* ...." If one would want to coin a term for this type of discrimination, "*higher-order discrimination*" appears to be a good candidate, where the type investigated in this paper would be of second-order, but future research could certainly aim to study situations of third or even higher order. In that sense, the underlying behavioral mechanism seems closely related to the mechanism described by John Maynard Keynes in his analogy of stock market behavior with a newspaper beauty contest (cf., Keynes, 1936). As in Keynes' example, decision-makers do not necessarily choose what they prefer themselves; instead, the given incentive structure makes them choose what they think others choose (or even what they think others think about what others choose and so on). It is up to future research to identify more settings in which this mechanism might cause discrimination, deepen our understanding of why beliefs about others are sometimes so biased, and, of course, search for effective ways of preventing discrimination.

Chapter 3

How Much is a Socially and/or Ecologically Responsible Production Worth to Consumers? Evidence from two Framed Field Experiments

This chapter is based on joint work with Julian Conrads, Alexandra Eyberg, and Bernd Irlenbusch.[*]

Abstract. We conduct two framed field experiments to test (i) if and how much consumers are willing to compensate firms' Corporate Social Responsibility (CSR) efforts to produce responsibly and (ii) if and how much consumers' willingness for compensating efforts in the ecological vs. social dimension of CSR differ. We elicit valuations for an actual product, i.e., a backpack, from samples of participants that match the product's target group of consumers. We elicit two valuations from each participant through the incentive-compatible BDM mechanism: once pre- and once post-intervention. In the first experiment, our interventions consist of either informing solely about functionality-related features or, in addition, about social and ecological CSR. In the second experiment, we either add only ecological or only social CSR information. In the first experiment, we find that while the functionality-related intervention increases the average valuation by 21%, adding CSR information increases it by 53%. The 32 percentage points difference is highly significant. In

[*]Alexandra Eyberg joined the research project as a master's student during the project's design phase. Her master's thesis (Eyberg, 2019) includes an initial analysis of the data from the first experiment. We thank Meike Felten and Theo Simon for their excellent assistance with data collection. The project received IRB approval at the University of Cologne (20001MS). The experiments were pre-registered in the AEA RCT Registry (AEARCTR-0004576 & AEARCTR-0005066). Funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy - EXC 2126/1-390838866.

the second experiment, we do not find a significant treatment difference. The intervention with ecological CSR information increases the average valuation by 38% and the one with social CSR information by 34%. Further, in both studies, we find that the increase in valuation caused by CSR interventions is significantly larger for participants with a higher pre-intervention valuation.

Keywords: CSR, Responsible Consumption, Experiment

JEL Classification Numbers: C9, D12, M14

3.1 Introduction

Milton Friedman (1970), in his influential article *"The Social Responsibility of Business is to Increase its Profits,"* argues along the lines of the standard economic theory view that, in general, firms that invest in corporate social responsibility (CSR) would not survive in competitive markets against firms that save CSR costs and therefore offer lower prices to consumers. Gary Becker (2008) respectfully disagrees with this view by noting that this does not need to be true when consumers are willing to compensate for CSR investments. The behavior of firms today seems to support Becker's view. For example, recent research about CSR reporting finds that out of 5200 companies, including the top 100 firms by revenue from each of the 52 countries surveyed, 80% report annually about their CSR measures. Out of the world's top 250 firms, even 96% report about CSR efforts (KPMG, 2020). Also, more and more survey studies show that the proportion of consumers to whom CSR is important is steadily increasing (see, e.g., BCG, 2019). Surprisingly, field-experimental evidence is mixed, and therefore, whether consumers are actually willing to compensate for firms' CSR efforts is still an open question.[1]

In two studies, we conduct two framed field experiments to answer (i) if and how much consumers are willing to compensate for a socially and ecologically responsible production and (ii) if consumers' willingness to do so depends on the CSR domain, i.e., social vs. ecological. Our framed field experiments allow us to increase realism but, at the same time, also to abstract from potentially confounding factors that are present in naturally occurring settings, e.g., brand reputation. Our study provides an important bridge between findings

[1] CSR investments can have a positive effect on profits for reasons beyond consumers' behavior. For example, in a field experiment, Hedblom et al. (2019) show that firms can attract more productive workers that produce higher quality outputs when they advertise jobs with CSR. Bertrand et al. (2020) provide empirical evidence for CSR being used by corporations as a tool of political influence. However, on the other hand, too much emphasis on CSR can also backfire. Beyond the typical "greenwashing" impression it can have on consumers, List and Momeni (2021) show that, through moral licensing, CSR can increase employee misconduct and shirking.

from abstract, conventional laboratory experiments with induced-valuation "goods" and such from natural field experiments.[2]

We elicit valuations for an actual product, i.e., a backpack, from samples of participants that match the product's target group of consumers. We elicit two valuations from each participant through the incentive-compatible Becker-DeGroot-Marschak (BDM) mechanism (Becker et al., 1964): once pre- and once post-intervention. Treatments vary the intervention participants receive between the two valuations. In the first experiment, our two interventions either inform participants solely about the backpack's functionality-related features, much like in conventional marketing campaigns, or, in addition, include the CSR information that the production was socially and ecologically responsible. In the second experiment, we either add only information about the social or only the ecological dimension to the functionality-related information.

In the first experiment, we find a highly significant treatment difference. While the average valuation is increased by 21% with the functionality-related intervention, adding CSR information increases it by 53%. Further, while the increase caused by functionality-related information stems from 58% of participants, the increase with CSR information stems from 84% of participants. Hence, adding CSR information seems not only to affect consumers who are receptive to conventional marketing but also creates new demand from additional consumer segments.

In addition, we extend the economic literature on responsible consumption with the novel insight that the effect of CSR information is increasing in consumers' pre-valuation of the product. In other words, CSR information has a stronger effect on participants who, after inspecting the backpack but before receiving any information, already value it more. We find no such effect for the functionality-related intervention.

In the second experiment, we do not find a significant treatment difference. The combination of functionality-related with ecological CSR information increases the average valuation by 38% and the combination with social CSR information by 34%. We refrain from a joint analysis of the four treatments from the two studies because our samples of participants and experimental procedures differ significantly between studies. Nevertheless, a cautious comparison of our two CSR interventions in Study 2 with the CSR intervention in

[2]Potentially confounding factors make it challenging to interpret results from natural field experiments as fully sterile evidence in favor or against the existence of preferences for responsible consumption. This is relatively obvious for null results because they can be caused by other reasons, e.g., distrust in a brand's CSR communication, instead of an absence of other-regarding preferences. But also results about positive effects of CSR information can be confounded, even if at first glance less obvious. For example, consumers might interpret the presence of CSR as a signal for enhanced trustworthiness of the firm, as suggested by Elfenbein et al. (2012). Consequently, positive effects on demand can be caused by selfish instead of other-regarding preferences. Our experimental design attempts to minimize both types of confounds.

Study 1 reveals no inconsistencies. On the contrary, the effects of the three CSR interventions look very similar in the sense that in both studies, 84-89% of participants react with an increase in valuation, and participants with a higher pre-valuation react significantly stronger than those with lower pre-valuation.

Our study adds to a young but growing economic literature that employs the experimental method in controlled laboratory or naturally occurring settings to make causal inferences about responsible consumption behavior.[3] Evidence from conventional laboratory experiments overwhelmingly points towards humans possessing social preferences and being willing to forgo monetary gains to avoid negative externalities under different institutional arrangements such as biliteral group decision making (Irlenbusch and Saxler, 2019), double auction markets (Falk and Szech, 2013a; Kirchler et al., 2016), multilateral double auction markets (Falk and Szech, 2013a; Sutter et al., 2020), and competitive laboratory markets with consumers as price-takers (Rode et al., 2008; Bartling et al., 2015; Feicht et al., 2016; Pigors and Rockenbach, 2016; Bartling et al., 2018; Engelmann et al., 2018; Bartling et al., 2019; Danz et al., 2022).[4] On the other hand, evidence from natural field experiments is mixed. Some studies report a positive effect of CSR information on consumer demand. For example, Hainmueller et al. (2015) conduct a field experiment in US grocery stores and report that a Fair Trade label has a significant positive effect on sales of coffee. Dubé et al. (2017) find that donation-based promotions significantly increase the purchase likelihood for cinema tickets when donations are sufficiently large, albeit the effect is less strong compared to same-sized discount-based promotions. Buell and Kalkanci (2021) report results from two natural field experiments. In the first, point-of-sale videos containing CSR information in the social domain increase apparel sales in a university book store. In the second, point-of-sale CSR information in the ecological dimension increases coffee sales in a grocery store. Other studies report null results. For example, Gneezy et al. (2010) find that combining the sale of souvenir photos with a charitable donation has no effect when the photos are sold for a fixed price. Singh et al. (2019) report that sending out charity-linked promotions to customers of an online taxi-booking platform does not affect demand. Conrads et al. (2022b) run a field experiment in the e-commerce shop of a German kindergarten backpack producer to test whether informing consumers about a socially and ecologically

[3]In addition to results from randomized experiments, the economic literature on responsible consumption also offers evidence from analyzing naturally occurring data (see, e.g., Elfenbein and McManus, 2010; Elfenbein et al., 2012; Lee and Bateman, 2021).

[4]Like results from incentivized laboratory experiments, extensive business literature finds consumers willing to purchase responsible products (see reviews Newholm and Shaw, 2007; Andorfer and Liebe, 2012; Tully and Winer, 2014). Most of those investigations apply either the survey methodology (see, e.g., Kassarjian, 1971; Mohr and Webb, 2005; Du et al., 2011) or non-incentivized choice experiments (e.g., De Pelsmacker et al., 2005), and therefore, elicit intentions and stated preferences. However, after observing a discrepancy between high levels of stated responsible consumption but tiny market shares of responsible products, scholars have questioned (e.g., Carrigan and Attalla, 2001; Carrington et al., 2010) whether these results are predictive of actual market behavior or if rather measurement errors related to social desirability bias (Fisher, 1993) and experimenter demand effect (Zizzo, 2010) have inflated them.

responsible production affects demand and report that sales were not affected.

Our findings have several important managerial implications. First, they suggest that, in principle, consumers are willing to compensate for firms' CSR efforts. Nevertheless, whether consumers can fulfill their preference for responsible consumption in actual markets might also depend on additional factors such as trusting CSR communication. Hence, for each firm, managers should carefully evaluate whether their specific consumer segment reacts positively to CSR information. Randomized natural field experiments provide a good tool for such tests. Second, the result that there is a positive interaction between the strength of consumers' preference for a product and the effect of CSR information provides a new rationale for strong brands to invest in CSR and for CSR brands to invest in improving conventional product attributes. Third, in our setting, we find no evidence that the specific dimension of the CSR engagement matters much. Therefore, firms should focus on investments in the dimension that fits them best.

The paper proceeds as follows. Section 3.2 describes the experimental design in detail. Section 3.3 develops hypotheses about causal effects that the experiment allows to test. Section 3.4 presents the results. Section 3.5 reflects on the main findings and Section 3.6 concludes.

3.2 Experimental design

We conduct framed field experiments to simultaneously achieve a high degree of control over the setting and realism (see Harrison and List, 2004). We use the incentive-compatible BDM mechanism to elicit two valuations from each participant for an actual product, i.e., a backpack. The first valuation is elicited after participants inspect the backpack but before receiving any treatment. The second valuation is elicited after we intervene by providing information on the backpack's characteristics that either does or does not include CSR information. We are interested in the differential effects of our interventions on the change between participants' pre- and post-intervention valuations (difference-in-difference).

The BDM elicitation method. The valuations we elicit through the BDM method represent the minimum monetary amount participants are willing to accept (WTA) to give up the backpack.[5] Participants are given the backpack and told that, if drawn in a lottery, they

[5]In theory, participants' WTA and willingness to pay (WTP) for a product should be the same. However, it is a well-known result that the WTA is often higher than the WTP (see, e.g., Kahneman et al., 1990). While we cannot rule out that this might also be the case in our data, we consider it less problematic because we are mainly interested in changes in valuation and not their absolute levels. We do note that it has been documented that sometimes the WTA-WTP disparity can be larger for products with a moral dimension (Anderson et al., 2000).

either keep the backpack or can sell it back to us through a specific auction-like mechanism. Then, they are informed that they need to bid the minimum amount of money for which they would sell back the backpack to us, but also that the bid does not determine the actual price for which they would sell back the backpack and that the actual price will be determined randomly. If the randomly drawn price is higher or equal to their bid, they receive a monetary amount equal to the randomly drawn price and need to give up the backpack. If the randomly drawn price is below their bid, they do not receive any money and keep the backpack.[6]

The product. We cooperate with a local start-up that invests in CSR by aiming to make the production process of their products more socially and ecologically responsible. At the time of our experiments, the firm's CSR efforts in the social dimension included long-term and transparent partnerships with suppliers and regular audits of the working conditions in the Asian factories by the independent third-party certifier Fair Wear Foundation (FWF). The FWF is an internationally active non-profit organization seeking to improve working conditions in the textile industry (Fair Wear Foundation, 2022). The firm's CSR efforts in the ecological dimension included using fabrics made out of recycled PET bottles, producing PFC-free, and using bluesign® system certified materials. Bluesign® is a sustainability standard for textile producers that bans toxic chemicals in production (Bluesign, 2022). The specific product we use is a black, mid-sized backpack that, at the time of the experiments, was priced € 119,90 and targeted students and young professionals as the primary consumer segment. We carefully debranded the backpack to control for potentially con-

[6]While, in theory, the BDM mechanism is incentive-compatible, i.e., predicted to elicit respondents' true valuations, it has been criticized for being difficult to comprehend by subjects in its practical applications. Most prominently, Cason and Plott (2014) show that when the BDM mechanism is employed with minimal instructions and without training subjects, "game form misconceptions," i.e., failures to understand the mechanism's incentive structure fully, lead to systematic overreporting of valuations, even when participants clearly know the value of a commodity. Therefore, Cason and Plott (2014) rightfully warrant interpreting BDM choice data as evidence for or against any particular preference theory without ensuring complete comprehension of the method. There are three reasons why this and similar criticisms are less of a concern for our difference-in-difference experimental design. First, we take great care to ensure participants understand the BDM mechanism. Meticulously crafted instructions based on Plott and Zeiler (2005), Lusk and Shogren (2007), and Isoni et al. (2011), read out one-on-one by the experimenter to the participant, explain the mechanism (see Appendix B.3). Further, instructions include an illustrative application of the method for a chocolate bar with a hypothetical induced value. Also, participants are explicitly made aware that, out of self-interest, the dominant strategy is to state the true valuation since both over-and understating it will lead to a loss in utility. Then, a training round with an apple is played, followed by comprehension questions about the consequences, i.e., whether the apple would need to be sold and for what price. In addition, participants are asked what would have happened if the randomly drawn price had been 5 cents above and 5 cents below their bid. If a participant indicates or reveals comprehension problems at any point in the instruction process, the experimenter returns to the respective passage and explains it again. Second, it is less likely that misunderstanding about the method, such as mistaking the second-price auction nature of the BDM method with a first-price auction against the randomly drawn price (cf., Cason and Plott, 2014), affect the change from a participant's pre-intervention to the post-intervention valuation. Third, any fraction of participants with comprehension problems that affect the change in their valuation, e.g., when comprehension problems lead to random behavior, will divide equally on treatment conditions due to randomization. Of course, if the fraction is large, it might make it harder for us to detect a treatment difference, but it is unlikely that it would make it easier.

founding factors such as pre-knowledge or distrust in CSR communication.[7]

Treatments. Both studies include two treatments each. Within each study, treatments are randomized between subjects. In Study 1, treatment interventions either solely inform about functionality-related characteristics of the backpack or, in addition, include the CSR information that production was socially and ecologically responsible. In the treatment "FN," solely functionality-related information is provided.

> FN: *"The backpack is characterized by*
> *• high functionality (e.g., expandable from 26 to 33 liters, padded laptop compartment, 2 accesses to main compartment: rolltop system and circulating zipper),*
> *• outstanding wearing comfort (e.g., ergonomic shoulder straps, padded back section, removable and adjustable chest strap),*
> *• water-repellence."*

In treatment "ECO&SCL&FN," the intervention includes, in addition to the above functionality-related information, CSR information in the ecological and the social dimension.

> ECO&SCL&FN: *"The backpack is characterized by*
> *• an ecologically compatible production (e.g., fabrics made from 100% recycled PET bottles, backpack free of PFCs harmful to the environment and health and continuous reduction of negative environmental impacts in production as certified by the independent third party bluesign®),*
> *• a socially compatible production (e.g., regular auditing of working conditions of the backpack production by the independent third party Fair Wear Foundation (including occupational safety reviews, employee training, and exclusion of child labor), and long-term and transparent partnerships with suppliers),*
> *•high functionality (e.g., expandable from 26 to 33 liters, padded laptop compartment, 2 accesses to main compartment: rolltop system and circulating zipper),*
> *• outstanding wearing comfort (e.g., ergonomic shoulder straps, padded back section, removable and adjustable chest strap),*
> *• water-repellence."*

In Study 2, the treatment "ECO&FN" includes an intervention that combines the above bullet point with CSR information in the ecological dimension with the above functionality-related information. Similarly, the treatment "SCL&FN" combines the above bullet point with CSR information in the social dimension with the functionality-related information.

[7] See Appendix B.1 for a picture of the debranded backpack.

Questionnaire. At the end of both studies, participants answer a questionnaire. It includes demographic questions, i.e., about gender, age, occupational status, and educational background. Further, participants are asked to assess on a Likert scale from one to five how much they like the backpack's design, how much they currently need a backpack and their general knowledge about the backpack market. Participants also report whether they recognize the brand and, if yes, how much they know about the brand's CSR efforts. Four additional questions ask about past responsible consumption behavior; two ask whether they usually consume socially or ecologically responsible products, and two whether they have paid price premiums to buy responsible products. Lastly, depending on the treatment, participants rank-order the specific characteristics that their information intervention included and report whether they know the FWF or bluesign®.

The following seven steps summarize the experimental design: (i) recruitment, (ii) instructions, training round, and comprehension questions, (iii) inspection of the backpack, (iv) pre-intervention valuation, (v) intervention, (vi) post-intervention valuation, and (vii) questionnaire. We apply this basic experimental design in both studies reported in this paper. However, the specifics of the implementations differ across studies as described below.

Procedures Study 1. In Study 1, we recruited 368 participants between August 19^{th} and September 24^{th}, 2019.[8] To generate a sample of subjects more diverse in their background than a standard sample, we chose to leave the laboratory and recruit at four different locations on different university campuses in Cologne. Three locations were at the University of Cologne: the canteen, the library, and the main university square. To further diversify our sample, we added a location at the main campus of the University of Applied Sciences in Cologne, which, unlike the Univesity of Cologne, specializes in teaching technical subjects. The recruitment procedure was simple. The experimenter situated a small stand at the location and asked young people who were passing by alone if they would like to participate in a consumer survey conducted by the University of Cologne in which they could win a backpack or money.[9] A single experimenter collected the data in this experiment. We neither paid for participation nor provided information on the lotteries' winning probabilities. We assured complete anonymity.

Procedures Study 2. Study 2 took place between April 20^{st} and May 4^{th}, 2020. While we aimed to keep as many factors similar to Study 1, the COVID19 pandemic forced us to deviate in the following aspects. First, instead of recruiting participants outside the laboratory,

[8]We conducted four pilot studies with the control treatment. In the first pilot, including 21 participants, no pre-valuations were elicited. The second pilot, including 17 participants, was like the first, but, in addition, participants were informed about the price segment of the backpack to reduce variance. The third pilot with 20 participants included both a pre-valuation and information about the price segment of the backpack. The final design was found in a fourth control treatment pilot, including 20 participants. Data from these pilots is not included in the final data set.

[9]See Appendix B.1 for a picture the stand used in the first experiment to collect data.

we recruited 127 participants through ORSEE (Greiner, 2004, 2015) from the subject pool of the Cologne Laboratory for Economic Research (CLER) at the University of Cologne. Second, we conducted the study via video call instead of face-to-face.[10] Consequently, the inspection needed to be accomplished remotely. To compensate as well as possible for the physical experience with the backpack, participants could ask questions and instruct the experimenter to show certain parts of the backpacks through the camera in more detail. However, we were cautious not to reveal more information than a physical inspection would have allowed observing. Third, the laboratory protocol at the time at CLER required reimbursing participation with € 4. Further, two experimenters collected the data in this experiment.

3.3 Hypotheses

We are interested in whether price-taking consumers in competitive product markets are willing to compensate firms for their CSR efforts. Results from a growing number of incentive-compatible experimental studies suggest that they might be. For example, Bartling et al. (2015) and Pigors and Rockenbach (2016) both provide convincing evidence from conventional laboratory market experiments with abstract induced-valuation goods that consumers are willing to pay higher prices for responsibly produced products. Hainmueller et al. (2015) report evidence from a large-scale natural field experiment in a U.S. grocery store chain, where attaching a Fair Trade label significantly increased the sales of the two most popular coffee brands.

Hypothesis 3.1 (ECO&SCL&FN vs. FN)**.** *Providing participants with CSR information in addition to functionality-related information will have a significantly stronger effect on their valuation than providing only functionality-related information.*

Our second hypothesis is somewhat exploratory because the economic literature on responsible consumption does not provide much evidence on differences in the effectiveness of CSR information in the ecological vs. social dimension. Buell and Kalkanci (2021) report a significantly stronger effect of CSR information in the ecological dimension; however, their results stem from two separately conducted field experiments where each dimension was tested in a different setting and for a different product. Therefore, their result has only limited predictive power for our study. On the other hand, Tully and Winer (2014) report significantly stronger effects for the social dimension. However, their meta-analysis on responsible consumption includes a large number of non-incentivized experiments.

[10] We did not require that participants turn on their cameras.

Hypothesis 3.2 (ECO&FN vs. SCL&FN). *Combining functionality-related information with CSR information in the ecological dimension will have a stronger/equal/weaker effect on participants' valuation than combining it with CSR information in the social dimension.*

Our third and last hypothesis is about a differential effect of CSR information based on participants' pre-valuation of the backpack. The results of Bartling et al. (2018) suggest that responsible consumption is a "normal good," i.e., its demand increases with income. Suppose that higher pre-valuations are also caused by having more income available. We should then expect participants with higher pre-valuations to bid higher prices after receiving an intervention that includes CSR information than participants with lower pre-valuations.

Hypothesis 3.3 (Pre-valuation-based heterogeneity). *The effect of CSR information will be significantly stronger on the valuation of participants who already value the backpack more before receiving the intervention.*

3.4 Results

The results section is organized as follows. Section 3.4.1 begins with a description of our two samples of participants. Section 3.4.2 presents Study 1 results and Section 3.4.3 the results of Study 2.

3.4.1 Samples

Table 3.1 summarizes the characteristics of our two samples of participants. In Study 1, after removing two outliers, we analyze data from 366 participants, 182 in FN and 184 in ECO&SCL&FN.[11] In Study 2, we have 127 participants, with 64 in ECO&FN and 63 in SCL&FN. Demographics show, with one exception, no statistically significant treatment differences within either study and, therefore, that randomization has worked well.[12] Comparing demographics across studies reveal that our two samples differ from one another. In Study 1, participants are slightly younger, more balanced in gender, more likely to be still studying, and more diverse in their fields of study than in Study 2. Nevertheless, both samples match the target consumer segment of the backpack.

[11]We remove two outliers. The first is a participant from treatment FN with a pre-intervention valuation of € 999 and a post-intervention valuation of € 30. This participant stated post-experimentally to have believed that only the second valuation would be relevant. The second outlier is a participant from treatment ECO&SCL&FN with a pre-intervention valuation of € 100 but a post-intervention valuation of € 1000. The main result of a positive effect from adding CSR information is robust to leaving either one or both in the data set.

[12]In Study 2, we have a few more participants with a first university degree in ECO&FN than in SCL&FN.

Table 3.1: Characteristics of samples of participants

	Within Study 1			Within Study 2			Between Studies		
	FN	ECO&SCL &FN	p-value	ECO &FN	SCL &FN	p-value	Study 1	Study 2	p-value
Number of participants	182	184		64	63		366	127	
Demographics									
Age	23.4	23.8	.454	26.2	25.3	.523	23.6	25.8	.001
Female	48%	50%	.752	67%	65%	.802	49%	66%	.001
Highest educational degree			.989			.059			.009
High school diploma	57%	57%		31%	48%		57%	39%	
University diploma	43%	43%		69%	52%		43%	61%	
Occupational status			.242			.257			.011
Student	92%	87%		77%	83%		90%	80%	
Full-/part-time job	7%	11%		22%	13%		9%	17%	
Other	1%	2%		1%	4%		1%	3%	
Field of study			.131			.167			.000
Business/Economics	27%	20%		39%	47%		24%	43%	
Humanities	21%	20%		25%	16%		21%	20%	
Education	14%	20%		9%	10%		17%	10%	
Technical sciences	18%	13%		9%	2%		15%	6%	
Natural sciences	7%	6%		3%	13%		6%	8%	
Law	9%	17%		6%	6%		13%	6%	
Other	4%	4%		9%	6%		4%	7%	
Self-assess. i.r.t. backpack									
Liking design (Yes/No)	69%/17%	64%/20%	.901	77%/14%	81%/14%	.863	66%/18%	79%/14%	.001
Needing backpack (Yes/No)	25%/65%	35%/55%	.035	38%/52%	35%/57%	.415	30%/60%	36%/54%	.027
Mrkt. knwldg. (Good/Bad)	5%/61%	4%/66%	.405	6%/52%	10%/48%	.439	5%/64%	8%/50%	.001
Past resp. consumption									
Buy ECO (Yes/No)	21%/20%	26%/26%	.944	22%/34%	29%/25%	.245	24%/23%	25%/30%	.196
Buy SCL (Yes/No)	21%/25%	20%/27%	.648	23%/31%	37%/22%	.100	20%/26%	30%/27%	.049
Premium ECO (Yes/No)	46%/33%	50%/33%	.644	43%/28%	48%/29%	.465	48%/33%	46%/28%	.425
Premium SCL (Yes/No)	49%/31%	52%/34%	.917	49%/26%	56%/31%	.761	51%/32%	53%/28%	.727

Notes: The table summarizes data from the post-experimental questionnaire to check if and how samples of participants differ within and between studies. The upper part shows demographics, the middle part shows participants' self-assessment related to the backpack, and the bottom summarizes self-reporting about past responsible consumption. Answers in the middle and bottom parts are given on a five-point Likert scale. The two affirmative points are taken together to "Yes" or "Good," the two negating points to "No" or "Bad," and the neutral point is left out. The following statistical tests are used: double-sided t-Test on variable "age" and Pearson's χ-squared on remaining variables.

The middle part of Table 3.1 summarizes participants' answers to the questions of how much they like the backpack's design, how much they currently need a backpack, and how well they assess their knowledge about the market for backpacks. Concerning the design, the vast majority of participants in both studies report (totally or rather) liking it. Again, no treatment differences appear within studies, but more participants in Study 2 (79%) report (totally or rather) liking the design than in Study 1 (66%). Regarding the need for a backpack, an interesting treatment difference appears in Study 1. When CSR information is added, a significantly larger fraction of participants report that they currently (totally or rather) need a backpack. This larger fraction is similar to the respective fractions (36%) in Study 2, where no treatment difference appears. Lastly, in both studies, a small fraction below 10% of participants report having good or above-average knowledge of the backpack market, and most report bad or below-average knowledge. Once again, no treatment differences appear within studies, but Study 2 participants are significantly less likely to report below-average knowledge than Study 1 participants.[13]

The lower part of Table 3.1 describes participants' self-reported past responsible consumption behavior. When asked if they usually buy ecologically or socially responsible products, around a fifth to a third of participants answer yes (always or often), similar numbers answer no (seldom or never), and the rest answer that they do so sometimes. When asked if previously they have paid a price premium for responsibly produced products, around half of participants answer yes (always or sometimes), approximately a third answer no (never or seldom), and the rest answer sometimes. Within Study 1 and Study 2, there are no significant treatment differences. In Study 2, the percentage of those reporting to usually consume socially responsible products is somewhat larger than in Study 1.

3.4.2 Study 1

Table 3.2: Study 1 valuations

Treatment	Pre-intervention valuation (in €)	Post-intervention valuation (in €)	Change in valuation (in €)
FN	35.30	42.78	7.48
ECO&SCL&FN	32.79	50.25	17.46
Treatment difference	-2.51	7.47	9.98

Tables 3.2 and 3.3 present the results of Study 1. We find no significant treatment difference between the €35.30 pre-intervention valuation in FN and the €32.79 in ECO&SCL&FN (p = 0.369, double-sided t-Test). On the other hand, the €42.48 post-intervention valuation in FN is significantly smaller than the €50.25 in ECO&SCL&FN ($p = 0.029$, double-sided t-Test). Nevertheless, both interventions lead to a significant increase in valuations (see column (1) in Table 3.3). Informing participants solely about functionality-related features leads to a 21% increase in the average valuation and adding CSR information to a 53% increase. The 32 percentage points difference represents the highly significant treatment effect caused by CSR information. Further, while in FN, the increase in valuation stems from 58% of participants, in ECO&SCL&FN, it is caused by 84%. The 26 percentage points difference is highly significant ($p < 0.001$, double-sided t-Test) and shows that CSR information works on both the intensive and extensive margin.

[13] In the first study, 9 out of 368 report to have recognized the brand and 5 out of 127 do so in the second study. No treatment difference appears in either study. Further, Out of the 184 participants in ECO&SCL&FN, 37 and 10 report knowing the FWF and bluesign®, respectively. In SCL&FN, 12 out of 63 participants report knowing the FWF, and in ECO&FN, 4 out of 64 participants report knowing bluesign®.

Result 3.1. *Providing participants with CSR information in addition to functionality-related information has a significantly stronger effect on the increase in their valuation than just providing functionality-related information. While the former increases the average valuation by 53%, the latter increases it by 21%.*

Table 3.3: Study 1, The effect of CSR on valuation

	Difference post-pre-valuation	
	(1)	(2)
Constant	7.482***	7.487***
	(0.774)	(1.282)
ECO&SCL&FN	9.982***	5.193***
	(1.480)	(1.799)
Higher pre-valuation		-0.009
		(1.589)
Higher pre-valuation × ECO&SCL&FN		10.614***
		(2.977)
Observations	366	366
R^2	0.111	0.173

Notes: The table presents results from ordinary least squares (OLS) regressions with robust standard errors in parentheses. Column (1) tests the effect of CSR information on the valuation. Column (2) tests whether the effect depends on participants having a higher or lower pre-valuation for the backpack. Statistical significance is indicated as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Next, we analyze the treatment effect regarding pre-valuation-based heterogeneity. For this purpose, we categorize participants as "higher pre-valuation participants" when their pre-valuation is larger than the median of all Study 1 participants and as "lower pre-valuation participants" otherwise.[14] Column (2) of Table 3.3 shows that the FN intervention causes an €7.49 increase in lower pre-valuation participants' valuation. Adding CSR information adds another €5.19 to the increase in valuation for lower pre-valuation participants. Their total increase in ECO&SCL&FN equals €12.68. The FN intervention has almost the same effect on higher pre-valuation participants and increases their valuation by €7.48. On the other hand, the effect of the ECO&SCL&FN intervention is by € 10.61 stronger on the higher pre-valuation category, which leads to a total increase of €23.29.[15]

[14] In FN, we end up with 83 lower pre-valuation and 99 higher pre-valuation participants. In ECO&SCL&FN, the respective numbers are 101 and 83. Pre-valuations of three participants equal the median of €29, all in ECO&SCL&FN. Changing the categorization such that the higher pre-valuation category includes these three observations does not qualitatively affect results. Further, results also remain qualitatively unaffected when we enter the pre-valuation as an explanatory variable into the regression and refrain from the categorization.

[15] See Table B.1 in the Appendix for average valuations from Study 1 by type of participant.

Result 3.2. *The effect of additional CSR information is significantly stronger for participants who already valued the backpack relatively more before receiving the intervention. We do not detect such an effect when participants receive only functionality-related information.*

3.4.3 Study 2

Table 3.4: Study 2 valuations

Treatment	Pre-intervention valuation (in €)	Post-intervention valuation (in €)	Change in valuation (in €)
ECO&FN	38.67	53.37	14.70
SCL&FN	43.41	58.28	14.87
Treatment difference	4.74	4.91	0.17

Table 3.4 and Table 3.5 show the results of Study 2. The pre-intervention valuations of €38.67 in ECO&FN and €43.41 in SCL&FN do not differ from another significantly ($p = 0.306$, double-sided t-Test). Similar, the post-intervention valuations of € 53.37 in ECO&FN and €58.28 in SCL&FN do not differ significantly ($p = 0.400$, double-sided t-Test). The within-treatment change in valuation of € 14.70 in ECO&FN and € 14.87 in SCL&FN amount to a significant increase of 38% and 34%, respectively. Unsurprisingly, the treatment difference is statistically insignificant (see column (1) of Table 3.5). Thus, the specific CSR dimension does not affect the positive effect of CSR information. Further, the share of participants who increase their valuation is 86% in ECO&FN and 89% in SCL&FN. The difference, again, is not statistically significant ($p = 0.620$, double-sided t-Test).

Result 3.3. *The combination of functionality-related information with either CSR information in the ecological dimension or CSR information in the social dimension leads to a 38% and 34% increase in the average valuations, respectively. The difference is statistically insignificant.*

Next, as in Study 1, we again check for pre-valuation-based heterogeneous effects.[16] Column (2) of Table 3.5 shows that lower pre-valuation participants react with an €11.20 increase to the ECO&FN intervention. The insignificant 0.348 coefficient shows that, as with the overall treatment difference, lower pre-valuation participants do not react differently

[16]Following the same categorization procedure as in Study 1, in ECO&FN, we end up with 35 lower and 29 higher pre-valuation participants. In SCL&FN, the respective numbers are 31 and 32. Pre-valuations of seven participants equal the median of €35, three of whom are in ECO&FN and the other four in SCL&FN. Changing the categorization such that the higher pre-valuation category includes these seven observations does not qualitatively affect results. Further, results also remain qualitatively unaffected when we enter the pre-valuation as an explanatory variable into the regression and refrain from the categorization.

Table 3.5: Study 2, The effect of ecological vs. social CSR on valuation

	Difference post-pre-valuation	
	(1)	(2)
Constant	14.703***	11.200***
	(1.837)	(2.694)
SCL&FN	0.170	0.348
	(2.921)	(3.476)
Higher pre-valuation		7.731**
		(3.496)
Higher pre-valuation × SCL&FN		-1.186
		(5.671)
Observations	127	127
R^2	0.001	0.048

Notes: The table presents results from ordinary least squares (OLS) regressions with robust standard errors in parentheses. Column (1) tests whether the effect of CSR information depends on the dimension being ecological vs. social. Column (2) tests whether the positive interaction between the pre-valuation type and CSR information depends on the dimension. Statistical significance is indicated as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

to the specific dimension of the CSR information. Higher pre-valuation participants react with an €18.93 increase to the ECO&FN intervention. Hence, the increase in their valuation is, by €7.73, significantly larger than the respective reaction of lower pre-valuation participants and does not differ significantly from the increase of higher pre-valuation participants in SCL&FN (see column (2) of Table 3.5). As in Study 1, this result suggests that the effect of CSR information is significantly stronger for participants who already value the backpack relatively more before receiving the intervention.

Result 3.4. *The positive interaction effect between participants' pre-valuations and CSR information does not depend on the specific dimension of the CSR information being ecological or social.*

3.5 Discussion

We conduct two framed field experiments with an actual product, i.e., a backpack, and samples of participants that match the product's target group to test whether consumers are willing to compensate for firms' CSR efforts. Our results suggest that, indeed, they are willing to do so. Participants react to a combination of functionality-related with CSR information with a 53% increase in the valuation. Functionality-related information alone also effectively increases participants' valuation; however, without CSR information, the increase is 21%. The 32 percentage points treatment difference is highly significant.

In addition to this finding, we extend the economic literature on responsible consumption with the novel insight that a positive interaction exists between the pre-valuation of a product and the effect of CSR information. We do not find such a pre-valuation-based heterogeneous effect for the intervention containing solely functionality-related information. Therefore, it is unlikely that differences in participants' income cause this differential effect. But, if not income differences, then, according to economic theory (e.g., Lancaster, 1966; Rosen, 1974), the variation in pre-valuations needs to be caused by differences in the strength of preference for the backpack. Consequently, the above result indicates a positive interaction effect between the strength of preference for the non-CSR characteristics of a product and the effect of CSR information.[17] This result might provide a missing link in the literature to explain why CSR information can significantly decrease the price elasticity of a higher-priced product while leaving the price-elasticity of a very close but lower-priced substitute unchanged. As detected in Hainmueller et al. (2015), differences in income level do not always explain this observation. Singh et al. (2019) provide further evidence for the idea that consumers who already appreciate a product more might also react more positively to information on the product's CSR characteristics. The authors run a natural field experiment on an online taxi-booking platform to test the effectiveness of charity-linked promotions. Their results show that charity-linked promotions were taken up disproportionately more often by customers who used the platform more frequently and, therefore, by those who probably also appreciated the service more.

Having established a positive effect of CSR information on consumer valuation and, therefore, on demand, a natural question is whether the effect stems from an increase in the intensive margin or if it also works on the extensive margin. Our above-described result sug-

[17]Our data do not allow us to pinpoint what precisely causes differences in pre-valuations. Nevertheless, Table B.3 and Table B.4 in the Appendix show that while participants' self-assessment of market knowledge is not a significant predictor of pre-valuations, liking the design and currently needing a backpack are so. Further, in Study 1, we find a negative association between reporting to be female and the pre-valuation. In Study 2, we find a positive association between pre-valuation and being a business or economics major and between pre-valuation and reporting to have paid premiums for ecologically responsible products. On the contrary, we find a negative association between pre-valuation and reporting to have paid premiums for socially responsible products.

gests that firms with customers who appreciate their products can effectively utilize CSR investments and benefit from the intensive margin. However, the following results suggest that CSR information also works on the extensive margin. First, while functionality-related information causes 58% of our participants to increase their valuation, adding CSR information increases this number by 26 percentage points to 84%. Second, adding CSR information increases the share of participants who self-report currently needing a backpack significantly.

The results from our second experiment replicate those of the first experiment, as described in the following. First, the combination of functionality-related with CSR information, as in Study 1, effectively increases consumer valuation, albeit with 38% for ecological CSR and 34% for social CSR the increase is somewhat smaller than the 53% increase when both dimensions are included in Study 1.[18] Second, the increase stems from 86% and 89% of participants when ecological or social CSR information, respectively, is included in the intervention. The corresponding number for the Study 1 CSR intervention was 84%. Third, the CSR interventions cause the valuation of participants with a higher pre-valuation to increase significantly stronger than that of lower pre-valuation participants.

We find no significant treatment difference in Study 2 and therefore document that, in our setting, whether the CSR information is ecological or social does not matter. Determining why precisely we find no difference here is beyond the purpose of our experimental design. In general, there might be two explanations that relate to the nature of the motivation behind responsible consumption. First, it could be motivated by genuine concerns (other-regarding preferences) for the well-being of, e.g., the environment or animals in the ecological or people in the social dimension. If so, then, probably by chance, these concerns do not differ in our setting. Second, responsible consumption could instead be motivated by self-centered preferences, e.g., for "warm glow" utility from the act of doing something good (Andreoni, 1989, 1990) or for utility from an improved self- or social image (Bénabou and Tirole, 2006). If this is true, then the actual dimension of CSR might not matter much as long as it still allows to see responsible consumption as a moral act. Results from laboratory and natural field experiments suggest that the second reason does play an essential role in the motivation of responsible consumption. For example, Engel and Szech (2020) show that experimental participants significantly reduce their WTP for CSR in the social dimension when they know that the product already has some CSR features in the ecological dimension. Such an effect should not appear if responsible consumption would be motivated by genuine concerns over well-being outcomes. Dubé et al. (2017) provide evidence from a field experiment that responsible consumption is motivated by the ability to

[18] Note that Study 2 does not include a control treatment without CSR information. Therefore, we can not entirely exclude the observed increase in valuation to come from functionality-related information alone. Further, the comparison between Study 1 and Study 2 should be seen as suggestive rather than as clear causal evidence because we did not randomize between studies, and samples of participants and procedures differ.

self-signal that, by it, one is acting morally.

Like all research, our study, too, has limitations. First, our results might suffer from the experimenter demand effect, i.e., participants might anticipate the purpose of the study and adjust their behavior to help the experimenter achieve it. However, if such an effect is present, it should also be there in the control treatment and, therefore, would not affect treatment differences. Second, our results might also be influenced by social desirability bias, i.e., participants might want to appear as moral persons to the experimenter or themselves by adjusting their behavior in the study such that it appears to be more socially acceptable. Despite taking precautions such as providing monetary incentives, ensuring complete anonymity, and thoroughly reminding participants that, from a self-interest perspective, their dominant strategy is to state actual valuations, we cannot strictly exclude social desirability effects. However, observability by others is also a common feature of many real-world consumption situations. For example, people often shop together with others who can observe their choices. Further, others frequently observe product choices after purchase, especially for apparel such as backpacks. Third, while we are interested in consumer behavior, the practical implementation of our design forced us to elicit WTA instead of WTP because we neither wanted to endow participants with a fixed monetary amount for purchasing that could have had an anchoring effect nor could we force them to purchase the backpack with their own money. Two reasons make this limitation less of a concern for us. First, we are not interested in absolute valuations but rather in changes in valuations. In addition, changes in valuations are also only in comparisons across treatments of interest. Fourth, of course, the choice of our specific samples limits the generalizability of the results. The purpose of this study is to test laboratory findings of responsible consumption in a more realistic but still highly controlled setting that allows us to abstract from confounding factors present in naturally occurring settings. We achieve higher realism through eliciting valuations for an actual product from participants that match the product's target group of consumers. Consequently, our participants are young, highly educated, live in an urban center, and therefore, not representative of the general population. It is an open question for future research whether our results also hold for a more representative sample or in other markets.

3.6 Conclusion

Our results and a growing body of research from the economic laboratory suggest that consumers possess preferences for responsible consumption and are willing to compensate firms for their CSR efforts. On the other hand, results from natural field experiments are mixed. Since both types of experiments are designed in an incentive-compatible way, this new inconclusiveness in the literature cannot be explained by the classical "attitude-behavior-gap," described, e.g., in Carrigan and Attalla (2001) as a divergence between what people answer in surveys and how they behave in actual markets. It seems more likely that constraints in markets, e.g., distrust in firms' CSR communications or unavailability of a sufficient assortment of responsible products, hinder consumers from acting according to their preferences. This market failure, i.e., the inability of current product markets to sufficiently fulfill consumers' preferences for responsible consumption, might decrease with an increased supply of more appealing responsible products and through government regulations that, for example, improve the credibility of CSR information used to promote products. More responsible consumption is one of the key factors in achieving sustainable development goals such as those of the United Nations (United Nations, 2015).

Chapter 4

Consumer Demand for Responsibly Produced Products: Evidence from a Natural Field Experiment in E-Commerce

This chapter is based on joint work with Julian Conrads, Bernd Irlenbusch, and Dirk Sliwka.[*]

Abstract. We study if consumers reward a responsible production of products with higher demand. More precisely, we run a natural field experiment in the online shop of a German kindergarten backpack producer by simultaneously activating two versions of the shop and randomly directing visitors to one of them (A/B-testing). We include and emphasize information about the backpacks' socially and ecologically responsible production in the CSR treatment but leave any such information out in the control treatment. We do not find a significant treatment effect on sales.

Keywords: CSR, Responsible Consumption, Field Experiment

JEL Classification Numbers: C9, D12, M14

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4.1 Introduction

Engaging in Corporate Social Responsibility (CSR) has become ubiquitous in the business world. A comprehensive analysis of 5200 companies, including the top 100 firms by revenue from each of the 52 countries researched, shows that 80% issue annual reports about their CSR measures (KPMG, 2020). The respective number from the same report for the world's top 250 companies is even at 96%. On the consumer side, results from a recent survey with 3000 participants from the USA, UK, France, China, and Brazil show that 97% report considering social and ecological issues at least somewhat important for their consumption decisions, and 75% even state that they consider them very or extremely important (BCG, 2019). Yet, when it comes to purchasing behavior in actual markets, the review of scientific evidence from randomized field experiments reveals that it is still unclear whether consumers reward firms' CSR efforts through increased demand for responsibly produced products.[1]

We conduct a natural field experiment in the e-commerce webshop of a German kindergarten backpack producer to test whether informing consumers about socially and ecologically responsible production affects demand. Our methodological choice is motivated by two reasons. First, our field experiment is free of social desirability effects that can bias results from surveys and laboratory experiments towards the socially desirable outcome. Second, the choice of the e-commerce setting is important because e-commerce has steeply risen in the past years, and the economic literature on responsible consumption barely provides any scientific evidence on the causal effect of CSR information when consumers shop online.[2]

We implement our experiment through the web analytics service offered by a well-known search engine provider. More specifically, we simultaneously activate two versions of the e-commerce webshop and, based on browser cookies, randomly direct visitors to one of the two versions (A/B-testing). The control version does not include any CSR-related information. In the CSR version, we include and emphasize information on the complete set of the firm's CSR measures. More precisely, (i) we alter the brand's slogan on the landing page of the webshop from "the kids' backpack" in control to "the sustainable kids' backpack" in the CSR treatment.[3] On each product details subpage, we add the information that (ii) the

[1]There are other reasons beyond consumer behavior for firms to engage in CSR. For example,Hedblom et al. (2019) show in a field experiment that advertising jobs with CSR attracts more productive employees. Bertrand, Bombardini, Fisman, and Trebbi (2020) provide evidence that firms use CSR investments to influence political decisions. On the other hand, sometimes, CSR might also have unintended adverse effects. For example, List and Momeni (2021) show that it can increase employee misconduct through a moral licensing effect.

[2]The worldwide retail e-commerce sales have increased from 1,336 billion US dollars in 2014 to 4,938 billion in 2021 and are forecasted to reach 7,391 billion in 2025 (EMarketer, 2022).

[3]In German, the translation for "sustainable" is "nachhaltig," which is often used synonymically with "responsible."

backpacks' are certified to be produced under fair labor standards, (iii) certified to be produced without the use of harmful chemicals and (iv) made out of recycled materials. Lastly, in the CSR treatment, (v) we add a subpage to the webshop that exclusively informs about the firm's CSR measures.

During the three-month-long period of our experiment, we record 227,655 visits, where 112,449 appear in control and 115,206 in the CSR version of the webshop. The difference is not statistically significant. The data from the web analytics tool shows that the vast majority of visits stem from Germany. Further, the largest fraction of visits is recorded from female visitors aged between 25 and 44. Concerning the surfing behavior in the webshop, based on browser cookies, the web analytics data shows that 51% of the total number of visits are from visitors who visit the webshop multiple times. In 57% of the total visits, visitors access multiple subpages of the webshop, with 6.8 subpages being opened on average. Surprisingly, the CSR subpage, which exists solely in the CSR treatment, is visited only in 1,878 or by 1.6% of the 115,206 visits. We detect no treatment differences in the above measures. However, and as manipulation check importantly, we find that visits with multiple pageviews last 13 seconds longer in the CSR treatment, namely 3:56 minutes, than in the control treatment, 3:43 minutes. The 13 additional seconds seem like a reasonable time to read the CSR information that we provided on each product subpage.

Turning to the sales data, we observe a total number of 4,638 transactions in which 5,488 products are sold, generating 205,936 euros in revenue. Simple treatment comparisons show neither the number of transactions (2,311 in control vs. 2,327 in treatment) nor quantities sold (2,726 in control vs. 2,762 in treatment) nor revenues (102,495 vs. 103,441) to differ significantly between treatments. Next to absolute sales data, in e-commerce settings, it is common to consider sales relative to the number of visits to the webshop as key measures. Optimizing the conversion of a visit into a transaction is important because webshop providers "pay per click" to online marketing platforms such as search engines or social media networks to generate "traffic" (visits). Thus, evaluating any changes to the webshop, such as our intervention, not only requires analyzing the effects on absolute sales numbers but also how it affects the number of visits. In our experiment, we randomly allocate any visitor who, based on browser cookies, is registered as "new" to one of the two versions of the webshop. Hence, the number of "new visits" is equalized between treatments per design. However, the treatment can affect the number of visits by any individual visitor. For example, providing CSR information could induce some visitors to revisit more frequently to enhance understanding, attempt to verify the information, or for any other reason. Therefore, in our main variables of interest, we look at sales data relative to the number of visits in the respective treatment. This approach results in a conversion rate (transactions/visits) of 2.06% in control and an insignificantly lower rate of 2,02% in the CSR treatment. Similarly, every 100 visits in control result in 2.42 products sold, which is insignificantly higher than the respective number of 2.39 in the CSR treatment. Likewise, a single visit in control generates 91.15 cents of revenue, which is statistically indistinguishable from the 89.79 cents in the CSR treatment. Based on the 95% confidence intervals, the data narrows down the average treatment effect of our intervention to be somewhere between -8% and +5% for all three outcome variables.

The paper proceeds as follows. Section 4.2 reviews related field experimental results. Section 4.3 describes the experimental design. Section 4.4 presents the results. Section 4.5 concludes.

4.2 Related literature

Several studies employ field experiments to investigate whether consumers prefer responsible products to conventional substitutes when both are offered for the same price. Results are mixed such that some report null effects, some find evidence in favor of a preference for responsible products, and a few find a preference for the responsible alternative for some products but not others. Prasad et al. (2004) place conventional socks next to identical responsibly produced socks, i.e., certified to be "sweatshop-free," to test which type of socks shoppers of a US department store prefer. The authors document that both types were sold equally often when there was no price differential. Gneezy et al. (2010) tie the sale of a souvenir phone in an amusement park to a donation and report that, when the photos were sold for a fixed price, sales were not affected by adding the donation. Vanclay et al. (2011) mark a variety of products in an Australian grocery store with eco-labels stating that the product either had below-average, average, or above-average carbon footprints and report to find no effect on sales for products with similar prices. On the other hand, several paper document a positive effect of CSR information on sales. Anderson and Hansen (2004) offer a conventional plywood product next to one carrying an eco-label and report that two-thirds of customers in two US Home Depot stores preferred the eco-labeled product when both were sold for the same price. Hiscox and Smyth (2011) report that displaying a fair labor standard label increased the sales of candles and towels by around 10% when prices did not differ. Becchetti et al. (2019) placed a poster in Italian grocery stores with a ranking of brands according to their CSR efforts and found that it increased the market share of higher-ranked brands and decreased that of lower-ranked and unranked brands. Some paper report that CSR information affected demand for some but not all tested products. Hainmueller and Hiscox (2015a) investigate eco-labels about reduced wastewater for women's and men's jeans in retail and outlet stores. Compared to when the products carried no label, the eco-label increased the sales of women's jeans in retail stores but did not affect sales of men's jeans in retail stores and women's and men's jeans in outlet stores.

Hainmueller and Hiscox (2015b) test a fair labor standard label for expensive women's suits, cheap women's yoga pants, and cheap men's t-shirts. Compared to caring no label, the eco-label increased sales of only the expensive women's suits but not of the two lower-priced products.

Other studies compare promoting products with CSR information to more traditional promotions such as informing about product quality. Results are, again, mixed. Hainmueller and Hiscox (2015a) not only compare an eco-label to a condition without a label but also to a fashion label emphasizing product quality. The authors report the fashion label to have been more effective in increasing sales of men's jeans in retail stores than the eco-label. Dubé et al. (2017) promote sales of cinema tickets with donations and report that while sufficiently large donations significantly increased the purchase likelihood, similar-sized discount-based promotions were more effective. Singh et al. (2019) compare donation-based with discount-based promotions in a field experiment on an online taxi-booking platform in Singapore. They, too, report that the latter was much more effective than the former in increasing purchasing probabilities. On the other hand, Hainmueller et al. (2015) compare the effect of a Fair Trade label on the sales of bulk coffee in a US grocery store chain with a label highlighting the brand and report the Fair Trade label to have increased sales by 10% stronger than the brand label. Hainmueller and Hiscox (2015b) report that a fair labor standards label but not a fashion label positively affected sales of expensive women's suits. Buell and Kalkanci (2021) show that point-of-sale CSR videos were more effective than standard brand image videos in increasing apparel sales in a US university book store and coffee sales in a US grocery store.[4]

4.3 Experimental design

We run a natural field experiment (see Harrison and List, 2004) in the e-commerce webshop of a German kindergarten backpack producer to test whether consumers award firms' CSR investments with increased demand. More precisely, for 91 days from July 5^{th} until October 3^{rd}, 2018, we simultaneously activate two versions of the webshop, where only one includes CSR information, and randomly allocate visitors based on browser cookies to one of the two (A/B-Testing). We implement the experiment through the web analytics tool of a well-known search engine provider.

The firm. We cooperate with a young German firm founded in 2010 in Cologne with the aim to produce innovative backpacks that combine the ergonomic features of trekking back-

[4]Some studies find demand for responsible products to be less price elastic than that of conventional substitutes (see, e.g., Prasad et al., 2004; Arnot et al., 2006; Hainmueller et al., 2015) and, related, that consumers are sometimes willing to pay a price premium for responsible products (see, e.g., Hiscox et al., 2011a,b).

packs with the requirements of elementary school backpacks. At the time of the experiment, the start-up had grown from its initial elementary school backpacks brand to eight brands offering anything from kindergarten backpacks to such for students and young professionals. The firm invests in CSR by aiming to produce ecologically and socially responsible. The firm's CSR efforts in the ecological dimension, on the one hand, consisted of increasingly using fabrics made of recycled PET bottles and, on the other hand, being certified bluesign® systems partner.[5] The firm's CSR efforts in the social dimension included having long and transparent partnerships with suppliers and ensuring minimum working standards in the Asian factories through audits in cooperation with the Fair Wear Foundation (FWF).[6]

The brand. We conduct our experiment in cooperation with the firm's brand for kindergarten backpacks. The brand's webshop provided a promising setting for our test because, prior to the experiment, the brand had not emphasized the firm's CSR efforts in promotional campaigns and, importantly, also not in its webshop, where it appeared at the bottom together with a range of other "small-printed" information, e.g., about payment, delivery, and legal details. The brand's unique selling point is the combination of ergonomic features with cute animal designs that kids seem to love (see Appendix C.1 for pictures). At the time of the experiment, the brand offered backpacks in two sizes: a smaller one for 34.90 euros and a larger one for 49.90 euros. Beyond backpacks, a small number of additional products such as lunchboxes and drinking bottles were offered.

The intervention. Our treatment alters the brand's webshop in three ways: by changing the slogan on the front page, adding CSR information on each product subpage of backpacks, and including a CSR subpage (see Appendix C.1 for screenshots). The front page of the control version included the slogan "The kids' backpack." The slogan in the CSR version was altered to "The sustainable kids' backpack."[7]

The product subpages divided information vertically into multiple parts that visitors could scroll down. At the top appeared a picture of the backpack, its name, price, and size. The CSR treatment contained the additional information that, depending on the size of the

[5]bluesign® is a sustainability standard for textile producers that mainly focuses on banning toxic chemicals from being used in textiles and textile production. bluesign® conducts on-site company assessments and provides consulting services. Certified partners can carry the bluesign® label to signal the use of chemically unharmful and sustainable materials (Bluesign, 2022).

[6]The FWF is a non-profit organization that works towards the improvement of working conditions in the production countries of the textile industry. Member brands commit to continuously improving conditions in production by implementing the following eight Fair Wear labor standards in their supply chain: free choice of employment, no discrimination in employment, no exploitation of child labor, freedom of association and the right to collective bargaining, payment of a living wage, no excessive working hours, safe and healthy working conditions, and legally binding employment relationships (Fair Wear Foundation, 2022).

[7]While "the kids' backpack" might appear as an unusual slogan to non-German readers, such slogans are not uncommon in marketing in Germany. For example, Volkswagen commercials include the slogan "Das Auto." The German national soccer team is promoted with "Die Mannschaft," or in English, "The Team."

backpack, either three or six PET bottles were recycled for its materials. The next part displayed the headings “Product details”, “Functions”, and “Material” and explained them with a few bullet points. The bullet points did not contain CSR-related information in the control treatment, but in the CSR treatment, “Material” again pointed to the recycled PET bottles. The CSR treatment also showed a fourth heading called “Responsibility,” and explained it with the brand being bluesign® and FWF certified, therefore ensuring that no chemically harmful materials are used and that working conditions are audited to be fair. The labels of bluesign® and the FWF were also depicted graphically. In the CSR treatment only, a third part appeared. On the top, additional information about the positive environmental impact of recycled fabrics was summarized, i.e., reducing sewage by 20%, lowering energy consumption by 50%, and reducing pollutant emissions by 60%. Below, it was stated that the brand cared about fair working conditions in the Asian production facilities, and a picture of a happy-appearing Asian factory worker holding a sign with the text “I made your [brand name]” was displayed. This part included two links, where one led to the CSR subpage, and the other was called “Ask the expert” and opened a pop-up window with the pictures and information of the firm’s two CSR experts.[8]

Lastly, the CSR treatment included an additional CSR subpage in the webshop that separately informed about the brand’s CSR efforts and also contained a graphical animation of six steps through which bottles are converted into backpacks.

4.4 Results

4.4.1 Traffic

During the 91 days long period of the experiment, the total number of visits to the online webshop was 227,655, of which 112.449 appeared in control and 115.206 in the CSR treatment condition, with the difference being statistically insignificant[9] ($p = 0.529$, double-sided t-Test).[10] In the following, we utilize data from the web analytics tool for the following purposes. First, we decompose the total traffic based on location, gender, and age category to get an idea of who our consumers are. Next, we look at the type of device and the source of the traffic to understand how and from where visitors arrive at the webshop. Lastly, we analyze available data to understand visitors’ behavior while visiting the webshop.

[8]The product details subpages also contain a fourth part that was identical in both treatments, containing a basic description of the backpack’s functionality.

[9]All statistical tests reported in this paper except regression results are conducted on daily averages.

[10]A visit is an interaction between the website and a visitor’s browser. A visit ends when the visitor closes the browser tab or is inactive for 30 minutes or more. Visitors who return after 30 minutes to the open browser tab are counted as new visits.

Consumer profile. Breaking down the traffic by location results in 85% of visits stemming from Germany, 10% from Austria, 2% from Switzerland, and the remaining from the rest of the world. Since we implemented our experiment only in the German version of the webshop, it is no surprise that visitors are almost exclusively located in German-speaking countries. We find no treatment differences for the three countries ($p \geq 0.540$, double-sided t-Tests). The treatment group includes slightly more visits from the rest of the world (3,675) than the control group (3,352), with the difference being marginally significant ($p = 0.087$, double-sided t-Test).[11] Further, for 53% of the total traffic, the gender of the visitor is unknown, 40% are identified as female, and the remaining 7% as male visitors. Similarly, for 54% of the total traffic, the age category of the visitor is unknown, 5% are between 18 and 24 years old, 24% between 25 and 34 years, another 13% are between 35 and 44 years, and the remaining 4% are 45 years and older.[12] We neither find any significant treatment differences in gender nor age categories ($p \geq 0.124$, double-sided t-Tests). The available data on location, gender, and age indicates that most of our visitors are young to middle-aged female Germans, which fits the brand's target consumer profile.[13]

Source and device. Traffic arrives at the webshop from six different sources. With 44%, the largest fraction of visitors reaches the webshop after clicking on paid search engine advertisement, followed by 19% who click on paid social media advertisement, 14% who click on search engine results that are not paid for, 10% use a referral link from another web pages, 10% use a direct link, and the remaining 3% get the link in an email.[14] Further, a majority of 82% of visitors use smartphones, 12% computers, and the remaining 6% tablet devices. We neither detect significant treatment differences in the source nor the device of visitors ($p \geq 0.229$, double-sided t-Tests).

Surfing behavior. Approximately half (49%) of the total of 227,655 visits are recorded as visits from visitors who visited just a single time, and the other half (51%) from visitors who visited the webshop multiple times. The 110,486 single visits divide, per design, equally between treatment conditions (55,221 in control vs. 55,265 in treatment, $p = 0.985$, double-sided t-Test). From the 117,169 multiple visits, slightly more are recorded in the CSR treatment than in control, albeit without statistical significance (57,228 in control vs. 59,941 in

[11]Taking a deeper look at the sessions from the rest of the world reveals that while such from visitors with a single visit are approximately identical (1,871 in treatment vs. 1,786 in control, $p = 0.431$, double-sided t-Test), those from visitors who visit multiple times differ (1,804 in treatment vs. 1,566 in control, $p = 0.032$, double-sided t-Test). One explanation could be that visitors from non-German speaking countries have more difficulties understanding the additional information in the CSR treatment and, therefore, visit the webshop more frequently.

[12]See Figure C.5 in the Appendix for graphical a illustration of the traffic breakdown by category.

[13]The data on gender and age is collected by the web analytics tool by recording the gender and age of online users who stay logged in to the provider's other services, e.g., in the widely used email service. Therefore, the gender and age of a little over half of the visitors are unknown. There might be systematic gender-based differences in the composition of gender and age categories.

[14]Referrals from other web pages include such from the brand's social media pages. The category of direct link users also includes all arrivals that cannot be assigned to either of the other categories.

treatment, $p = 0.219$, double-sided t-Test). It is important to note that the recorded number of multiple visits represents a lower bound because some visitors might have switched browsers or devices between visits, which would lead to each of their different visits being recorded as a new single visit. Further, in 43% of all visits, visitors view just a single subpage of the webshop and leave, and in the remaining 57%, they, on average, view 6.8 subpages. The average pageviews per visit do not differ between treatments (6.8 in both treatments, $p = 0.889$, double-sided t-Test). However, the average time per visit visitors with multiple pageviews spent in the webshop significantly differs between treatments. While in the control group, they spent 3:43 minutes, in the CSR treatment, they spent 3:56 minutes. The highly significant difference of 13 seconds ($p < 0.001$, double-sided t-Tests) seems a reasonable average duration to read the additional CSR information. Further, the CSR subpage included only in the CSR treatment was visited merely 1,878 times or by 1.6% of the treatment's 115,206 visits.

4.4.2 Sales

Table 4.1 summarizes the sales generated in the two versions of the webshop during 91 days of the experiment. We document 4,638 transactions, in which 5,488 products were sold that generated total revenue of 205,936 euros. Only minor and statistically insignificant treatment differences appear, where the CSR treatment exceeds the control by 16 transactions, 36 products sold, and 916 euros ($p \geq 0.809$, double-sided t-Tests with pairwise treatment comparisons). While these absolute sales numbers suggest a null effect, our dependent variables of interest put them in relation to the number of visits, as is common in e-commerce settings.

Table 4.1: Sales data in absolute terms

	Visits	Transactions	Quantity	Revenue in euros
Control	112,449	2,311	2,726	102,495
Treatment	115,206	2,327	2,762	103,441
Total	227,655	4,638	5,488	205,936

Table 4.2 shows results from regressing the occurrence of a transaction and its quantity and revenues on the treatment dummy, based on the total number of visits. In the control treatment, the coefficient of 0.0206 in column (1) represents a conversion rate, i.e., the percentage of visits resulting in a transaction, of 2.06%. Adding CSR information in the treatment version of the webshop reduces the conversion rate by 0.04 percentage points to 2.02%, however, without being statistically significant. The lower and upper bounds of the 95% confidence interval for the treatment coefficient lie at -0.0015 and +0.0008, corresponding

Table 4.2: The effect of CSR on sales

	Transactions		Quantity		Revenue in euros	
	(1)	(2)	(3)	(4)	(5)	(6)
Control	0.0206	0.0206	0.0242	0.0242	0.9115	0.9110
(constant)	(0.0004)	(0.0005)	(0.0006)	(0.0007)	(0.0206)	(0.0254)
Treatment	-0.0004	-0.0003	-0.0003	-0.0003	-0.0136	-0.0133
	(0.0006)	(0.0005)	(0.0008)	(.0007)	(0.0289)	(0.0273)
95% Confidence Interval						
Lower bound constant	0.0197	0.0195	0.0231	0.0229	0.8712	0.8611
Upper bound constant	0.0214	0.0216	0.0253	0.0256	0.9518	0.9608
Lower bound treat. coef.	-0.0015	-0.0014	-0.0018	-0.0017	-0.0702	-0.0669
Upper bound treat. coef.	0.0008	0.0007	0.0013	0.0012	0.0430	0.0402
Day-random-effects	No	Yes	No	Yes	No	Yes
Observations	227,655	227,655	227,655	227,655	227,655	227,655
R^2	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Columns (1), (3), and (5) present results from ordinary least squares (OLS) and columns (2), (4), and (6) from generalized least squares (GLS) regressions with robust standard errors in parentheses. Columns (1), (2), and (3) test whether CSR information affects the conversion of a visit into a transaction, quantity sold, and revenues, respectively. Columns (2), (4), and (6) repeat the analysis but control for day-random-effects. Statistical significance is indicated as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

to an average treatment effect between -7.3% and +3.9% compared to the value in the control treatment. The coefficient of 0.0242 in columns (3) shows that each one hundred visits in the control treatment, on average, resulted in 2.42 products being sold. Informing about CSR reduces this number insignificantly to 2.39. The lower and upper bounds of the confidence interval correspond to an average treatment effect between -7.4% and +4.9%. Lastly, on average, a visit in the control treatment generated 91.15 cents in revenues (see 0.9115 coefficient in column (5)). Again, informing about CSR reduced this number insignificantly by 1.36 cents to 89,79 cents. The boundaries of the confidence interval point to an average treatment effect between -7.7% and 4.4%. Columns (2), (4), and (6) show that these results are unaffected by allowing for day-random-effects.

In sum, we neither detect any significant treatment difference on the extensive margin (transactions/visits) nor the intensive margin (quantity/visits and revenues/visits).[15] Based on the confidence intervals in Table 4.2, the average treatment effect of our intervention lies somewhere between -8% and +5%.[16]

[15] See Figure C.6 in the Appendix for a graphical illustration of the sales data over time.

[16] Prio to conducting the experiment, we ran a power analysis based on a conversion rate (transactions/visits) of 1.5%, an average treatment effect of 10%, and an alpha of 5% that resulted in needed 216,000 observations to achieve a power of 80%. We refrain from conducting any post hoc power calculation because of its fundamental flaws and inappropriateness as a post-experimental data analysis tool, as explained in detail in the well-cited work of Schuirmann (1987) and Hoenig and Heisey (2001).

4.5 Discussion

We conduct a natural field experiment in the webshop of a kindergarten backpack producer by randomly directing visitors to either a version of the shop with CSR-related information or another identical version without such information. In our three-month-long experiment with 227,655 visits, we do not detect any significant effects on sales.

Our results and other null results from field-experimental investigations reviewed in Section 4.2 seem to be at odds with findings from a growing literature from the economic laboratory that consistently shows that significant fractions of laboratory participants prefer responsible goods and are willing to sacrifice monetary earnings to prevent negative externalities on others (see, e.g., Rode et al., 2008; Falk and Szech, 2013a; Bartling et al., 2015; Feicht et al., 2016; Kirchler et al., 2016; Pigors and Rockenbach, 2016; Bartling et al., 2018; Engelmann et al., 2018; Bartling et al., 2019; Irlenbusch and Saxler, 2019; Danz et al., 2022; Sutter et al., 2020). One potential explanation for this inconclusiveness could be that findings from abstract laboratory experiments, predominantly using induced valuation goods to investigate responsible consumption behavior, do not readily generalize to actual market behavior, as voiced by some economists (see, e.g., Levitt and List, 2007). Conrads et al. (2022a) test whether responsible consumption preferences detected in the laboratory are solely an artifact of the abstract laboratory setting or whether they also show in settings with a higher degree of realism. The authors use a laboratory method but investigate behavior towards an actual product, i.e., a backpack, of participants that match the product's target consumer segment. They report that informing about responsible production significantly increases participants' valuation of the product. Therefore, it is unlikely that the above-cited laboratory findings are solely an artifact of the laboratory setting. However, if we accept that, in general, humans possess preferences for responsible consumption, then there need to be other reasons why these preferences do not always show in field settings. In the following, we discuss several such reasons that might apply to our study.

The first potential reason for our null result is a technical limitation: the randomization of individuals is based on browser cookies. Therefore, if a large fraction of visitors do not allow cookies or switch browsers or devices between visits, this might have led to a measurement error in the data. Table 4.3 presents the distributions of visits and transactions by the device used and whether the data is registered to be from single- or multiple-time visitors.[17] When we look at the table's top part, we see that while 82% of visitors use a phone to visit the webshop, the fraction of transactions made by phone lies at 63%. On the other hand, transactions are conducted disproportionally more often through computers and

[17]The table does not split the data by treatments and does not provide distributions on order quantity and revenues because they are very similar to the data presented. See Table C.2 in the Appendix for complete data.

tablets. There can be two explanations for this difference in distributions. Either some visitors visit via phone but then switch to a potentially more convenient computer or tablet for purchasing, or the different types of devices have different probabilities of leading to a transaction. When we look at the middle part of the table, we see that the difference in distributions is especially large for visits and transactions by visitors who are registered to have visited just a single time. This suggests that, indeed, a fraction of users might visit through a phone but then switch to a computer for the purchase, where he/she is then documented as a new visitor. However, when we look at the lower part, we see a difference in the distributions for visitors documented to have visited the webshop multiple times and, therefore, to have allowed for cookies. This suggests that the different devices might also have different probabilities of leading to a purchase. When we condition our analysis on the subset of transactions and visits from visitors with multiple visits, we still do not find significant treatment differences, which is reassuring because any measurement error through switching browsers or devices is much less likely for those visitors from whom we know to have allowed cookies.[18]

Table 4.3: Distributions of visits and transactions by device and type of visitor

		Phone	Computer	Tablet	Total
Overall	Visits	82%	12%	6%	227,655
	Transactions	63%	27%	10%	4,638
Single-time visitors	Visits	80%	14%	6%	110,486
	Transactions	45%	44%	11%	1,493
Multiple-time visitors	Visits	83%	10%	7%	117,169
	Transactions	71%	20%	9%	3,145

Second, our null result could be due to visitors not noticing the intervention. However, since we find that visits in the CSR treatment last significantly longer than visits in the control treatment, we are confident that our intervention was noticed. Also, the way we presented and emphasized CSR information (see Appendix C.1 for screenshots) makes it highly unlikely that visitors might have overlooked it.

Third, one might also argue that while our intervention was noticed, it might not have been informative because most visitors who arrived at the webshop already could have known the brand and its CSR efforts. Indeed, when we look at the search terms used in a well-known search engine prior to 111,412 visits to find the webshop, roughly 60% of them include the brand name. However, while in the majority, again roughly 60%, of searches that

[18]For regression results conditional on data for single-time vs. multiple-time visitors, see Table C.1 in the Appendix. Note that, of course, it is also possible that visitors with multiple visits have visited before from a different browser or device. Therefore, the analysis in this part should be interpreted as suggestive rather than clear evidence.

include the brand name, it is combined with other terms such as "kindergarten backpack" or "animal design," in solely 12 searches, the brand name is combined with a CSR-related term such as "sustainability." Overall, CSR-related terms are used in only 288 of the 111,412 searches. Hence, while most visitors seem to have known the brand before arriving at the webshop, we have no evidence suggesting that visitors also had pre-knowledge of the CSR information we provided. This is consistent with the fact that, prior to our experiment, the brand neither strongly emphasized CSR in their marketing efforts nor on the webshop.

Table 4.4: Examples of some more and some less frequently used search terms

Rank	Search term	Times used prior a visit
1	"[brand name]"	26,736
2	"[brand name] backpack"	16,357
3	"kindergarten backpack"	2,606
4	"backpack [brand name]"	1,408
5	"kid's backpack"	1,099
...	...	...
870	"kindergarten backpack sustainable"	9
...	...	...
17,042	"two backpack light blue"	1
...	...	...
Total		111,412

Lastly, and from our perspective, the most likely explanation for our null result, visitors might have noticed the CSR information, and it might also have been informative, but, at the same time, they might have just disregarded it in their purchasing behavior. Support for this view provides the fact that in 115,206 visits to the CSR treatment version of the webshop, the CSR subpage was accessed merely 1,878 times, despite a link to it being provided on each product subpage. Further, the firm's CSR experts were contacted just twice during the experiment's three-month duration, despite their contact information being available on each product details page.

One reason the CSR information might have been disregarded could be that the brand's backpacks had become a strong trend product that kids wanted. Sales data shows strong growth prior to our experiment. From 2017 to 2018, total sales increased by 115%, and sales from the webshop even by 184%. Thus, it seems that in our setting, where young parents search online for a trendy backpack for their kindergarten-age children, other factors, e.g., the funny animal designs that kids want to have, could have played a more critical role in the purchasing decision than the firm's CSR efforts.

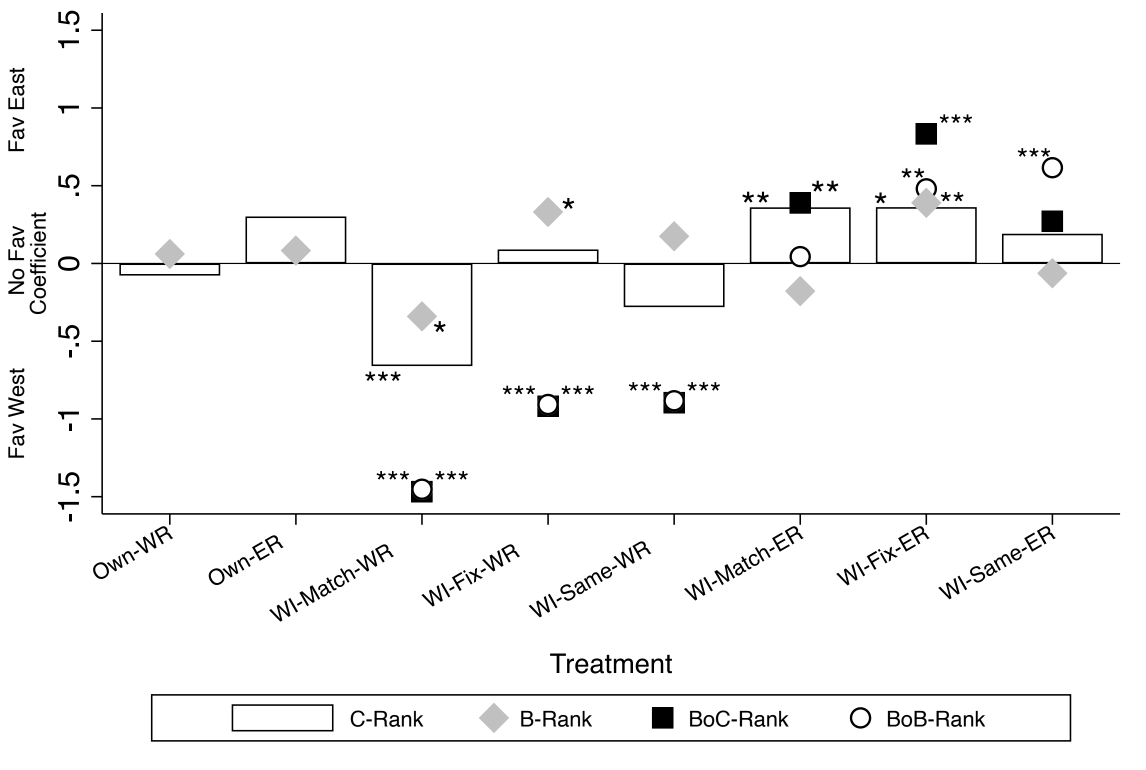
Our natural field experiment is free of social desirability effects caused by experimental subjects being aware of their participation in a scientific study and adjusting their behavior such that it appears more socially acceptable. Further, the e-commerce setting removes any social pressure present in offline retail stores where one could feel observed by sales staff or other customers. The increased social distance and anonymity of our online setting could be another important reason behind the low interest in CSR information and its ineffectiveness. Results from the literature show that, indeed, an increase in social distance and anonymity adversely affects moral behavior (see, e.g., Conrads and Lotz, 2015). In this sense, our investigation provides a tough but, due to the ever-increasing prevalence of e-commerce, important test for responsible consumption.

Appendix

A Appendix to Chapter 2

A.1 Supplementary figures and tables

Figure A.1: Average effects of candidates' Eastern names on rankings



Notes: Illustration of the main effects from rank-ordered logistic regressions from Table A.2.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.1: Demographics

Treatment	Own-WR	Own-ER	WI-Match-WR	WI-Match-ER	WI-Same-WR	WI-Same-ER	WI-Fix-WR	WI-Fix-ER
Participants	54	48	41	54	45	46	50	48
Age (years)	28.5	27.1	29.5	25.5	27.2	27.3	27.1	26.5
Male (%)	100	100	100	100	100	100	100	100
Income (€)	1,398	877	1,255	1,189	1,079	1,130	1,269	1,056
Occupational status								
Student (%)	69	77	73	72	82	80	80	83
Full-time job (%)	19	23	17	24	11	13	18	8
Part-time job (%)	9	0	7	2	2	4	2	4
Unemployed (%)	2	0	0	2	2	0	0	4
Retired (%)	2	0	1	0	2	2	0	0
Highest educational degree								
High school diploma (%)	41	54	46	33	42	41	44	42
Bachelor (%)	39	29	29	48	42	46	36	42
Master (%)	20	17	24	19	16	13	20	17
Field of study								
Business/Econ. (%)	46	35	39	50	53	46	46	44
Natural sciences (%)	9	4	12	13	7	11	8	13
Law (%)	7	8	7	7	11	7	12	13
Technical sciences (%)	7	6	0	4	4	9	6	6
Education (%)	7	6	7	2	0	2	12	2
Humanities (%)	0	2	15	6	4	7	4	4
Other (%)	19	29	17	13	16	17	8	19
No information (%)	4	8	2	6	4	2	4	0

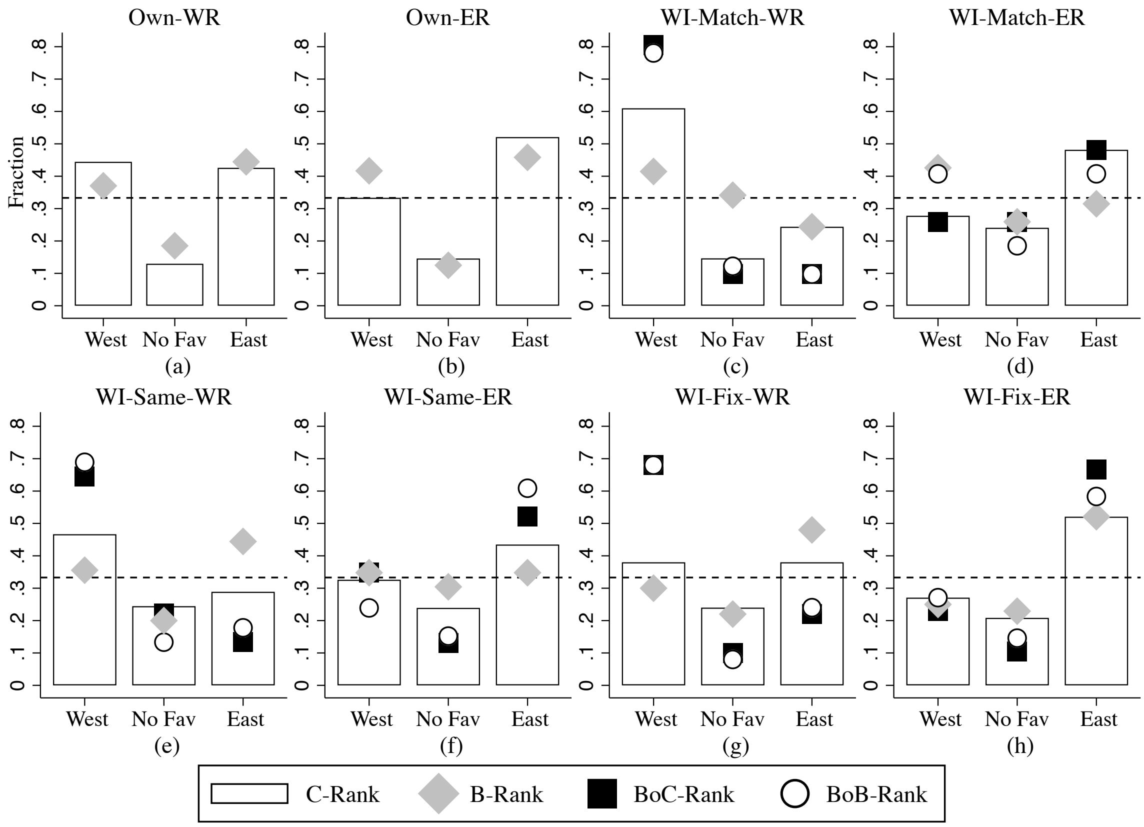
Notes: The table breaks down each treatment's sample of participants according to the demographic variables available. Following statistical tests are conducted. *Age*: No significant differences in overall comparison of distribution across treatments (p=0.298, ANOVA test). Pairwise treatment comparisons with double-sided t-Tests show insignificance in all expect between Own-WR and WI-Match-ER (p=0.045), WI-Match-WR, and WI-Match-ER (p=0.005), and WI-Match-WR and WI-Fix-ER (p=0.076). The differences can be explained by older participants that are age-wise outliers. *Male*: Only participants who had registered as males in ORSEE were invited to the experiment. *Income*: No significant differences in the overall comparison of distribution across treatments (p=0.237, ANOVA test). Pairwise treatment comparisons with double-sided t-Tests show insignificance in all expect between Own-ER with Own-WR (p=0.0108), WI-Match-WR (p=0.046), WI-Match-ER (p=0.033), and WI-Fix-WR (p=0.038). Participants with Middle-Eastern first names report having lower income. *Occupational status*: No significant differences in overall comparison of distribution across treatments (p=0.552, Pearson χ^2 test). Pairwise treatment comparisons with Pearson χ^2 tests show insignificance in all expect between Own-ER and WI-Fix-ER (p=0.061). *Highest educational degree*: No significant differences in the overall comparison of distribution across treatments (p=0.827, Pearson χ^2 test). Pairwise treatment comparisons with Pearson χ^2 tests show insignificance in all expect between Own-ER and WI-Same-WR (p=0.085). *Field of study*: No significant differences in overall comparison of distribution across treatments (p= 0.537, Pearson χ^2 test). Pairwise treatment comparisons with Pearson χ^2 tests show insignificance in all cases.

Table A.2: Average effect of candidates' Eastern names on ranking (all treatment)

	Dep.variable: Base treatment			C-Rank (1)	B-Rank (2)	BoC-Rank (3)	BoB-Rank (4)
(a)	Own-WR:	East		-0.078	0.061	n/a	n/a
				(0.176)	(0.174)		
		East	× Own-ER	0.380	0.022	n/a	n/a
				(0.260)	(0.257)		
		East	× WI-Match-WR	-0.582**	-0.401	n/a	n/a
				(0.273)	(0.265)		
		East	× WI-Match-ER	0.438*	-0.239	n/a	n/a
				(0.250)	(0.247)		
		East	× WI-Same-WR	-0.203	0.114	n/a	n/a
				(0.262)	(0.263)		
		East	× WI-Same-ER	0.269	-0.124	n/a	n/a
				(0.255)	(0.255)		
		East	× WI-Fix-WR	0.168	0.270	n/a	n/a
				(0.251)	(0.254)		
		East	× WI-Fix-ER	0.439*	0.330	n/a	n/a
				(0.258)	(0.255)		
(b)	Own-ER:	East		0.302	0.083	n/a	n/a
				(0.191)	(0.189)		
		East	× WI-Match-WR	-0.962***	-0.423	n/a	n/a
				(0.283)	(0.274)		
		East	× WI-Match-ER	0.059	-0.261	n/a	n/a
				(0.261)	(0.257)		
		East	× WI-Same-WR	-0.583**	0.092	n/a	n/a
				(0.273)	(0.273)		
		East	× WI-Same-ER	-0.111	-0.146	n/a	n/a
				(0.265)	(0.265)		
		East	× WI-Fix-WR	-0.212	0.248	n/a	n/a
				(0.262)	(0.264)		
		East	× WI-Fix-ER	0.059	0.307	n/a	n/a
				(0.268)	(0.266)		
(c)	WI-Match-WR:	East		-0.660***	-0.340*	-1.467***	-1.453***
				(0.209)	(0.199)	(0.246)	(0.246)
		East	× WI-Match-ER	1.020***	0.162	1.856***	1.497***
				(0.274)	(0.265)	(0.306)	(0.307)
		East	× WI-Same-WR	0.379	0.515*	0.572*	0.570*
				(0.285)	(0.281)	(0.322)	(0.322)
		East	× WI-Same-ER	0.851***	0.277	1.738***	2.068***
				(0.278)	(0.273)	0.314	(0.318)
		East	× WI-Fix-WR	0.750***	0.671**	0.549*	0.546*
				(0.275)	(0.272)	(0.318)	(0.318)
		East	× WI-Fix-ER	1.021***	0.730***	2.300***	1.933***
				(0.281)	(0.273)	(0.319)	(0.311)
(d)	WI-Match-ER:	East		0.361**	-0.178	0.389**	0.044
				(0.177)	(0.175)	(0.182)	(0.183)
		East	× WI-Same-WR	-0.642**	0.353	-1.284***	-0.927***
				(0.263)	(0.264)	(0.276)	(0.276)
		East	× WI-Same-ER	-0.170	0.115	-0.118	0.571**
				(0.255)	(0.255)	(0.267)	(0.272)
		East	× WI-Fix-WR	-0.270	0.509**	-1.307***	-0.951***
				(0.252)	(0.254)	(0.271)	(0.271)
		East	× WI-Fix-ER	0.001	0.568**	0.444	0.436*
				(0.259)	(0.256)	(0.272)	(0.263)
(e)	WI-Same-WR:	East		-0.281	0.175	-0.895***	-0.883***
				(0.194)	(0.198)	(0.207)	(0.207)
		East	× WI-Same-ER	0.472*	-0.238	1.166***	1.499***
				(0.268)	(0.271)	(0.285)	(0.289)
		East	× WI-Fix-WR	0.371	0.156	-0.023	-0.024
				(0.264)	(0.270)	(0.289)	(0.289)
		East	× WI-Fix-ER	0.643**	0.215	1.728***	1.363***
				(0.271)	(0.272)	(0.290)	(0.281)
(f)	WI-Same-ER:	East		0.191	-0.063	0.271	0.616***
				(0.184)	(0.186)	(0.195)	(0.201)
		East	× WI-Fix-WR	-0.101	0.394	-1.189***	-1.522***
				(0.257)	(0.262)	(0.280)	(0.284)
		East	× WI-Fix-ER	0.171	0.454*	0.562**	-0.136
				(0.263)	(0.264)	(0.282)	(0.276)
(g)	WI-Fix-WR:	East		0.090	0.331*	-0.918***	0.907***
				(0.179)	(0.185)	(0.201)	(0.201)
		East	× WI-Fix-ER	0.271	0.059	1.751***	1.387***
				(0.260)	(0.263)	(0.286)	(0.276)
(h)	WI-Fix-ER:	East		0.362*	0.390**	0.833***	0.480**
				(0.188)	(0.187)	(0.203)	(0.190)
	Observations			1544	1544	1136	1136
	Individuals			386	386	284	284
	Log likelihood			-1214.525	-1220.305	-847.880	-852.843

Notes: The table reports results from rank-ordered logistic regressions and is an extension of Table 2.2, including all eight treatments. See the note of Table 2.2 for more details. Standard errors are in parentheses. Statistical significance is indicates as follows. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.2: Heterogeneity - all treatments



Notes: The figure is an extension of Figure 2.1. See note of Figure 2.1 for more details.

Table A.3: Heterogeneity - Data

		C-Rank			B-Rank			BoC-Rank			BoB-Rank			
		West	No Fav	East	West	No Fav	East	West	No Fav	East	West	No Fav	East	N
(a) Own-WR	44.4	13.0	42.6	37.0	18.5	44.4	n/a	n/a	n/a	n/a	n/a	n/a	54	
(b) Own-ER	33.3	14.6	52.1	41.7	12.5	45.8	n/a	n/a	n/a	n/a	n/a	n/a	48	
(c) WI-Match-WR	61.0	14.6	24.4	41.5	34.2	24.4	80.5	9.8	9.8	78.1	12.2	9.8	41	
(d) WI-Match-ER	27.8	24.1	48.2	42.6	25.9	31.5	25.9	25.9	48.2	40.7	18.5	40.7	54	
(e) WI-Same-WR	32.6	23.9	43.5	34.8	30.4	34.8	34.8	13.0	52.1	23.9	15.2	60.9	46	
(f) WI-Same-ER	46.7	24.4	28.9	35.6	20.0	44.4	64.4	22.2	13.3	68.9	13.3.	17.8	45	
(g) WI-Fix-WR	38.0	24.0	38.0	30.0	22.0	48.0	68.0	10.0	22.0	68.0	8.0	24.0	50	
(h) WI-Fix-ER	27.1	20.8	52.1	25.0	22.9	52.1	22.9	10.4	66.7	27.1	14.6	58.3	48	

Notes: The table presents the data (in %) behind Figure A.2.

Table A.4: Heterogeneity (diagnostic treatments)

	Dependent variable:		Categorization of ...			
			C-Rank	B-Rank	BoC-Rank	BoB-Rank
	Treatment		(1)	(2)	(3)	(4)
(e)	WI-Same-WR	Fav West / Fav East	1.615	0.450**	4.833***	3.875***
			(0.570)	(0.181)	(2.168)	(1.537)
		Fav West / No Fav	1.909*	1.778	2.900***	5.167***
			(0.711)	(0.741)	(1.063)	(2.304)
		Fav East / No Fav	1.182	2.222**	0.600	1.333
			(0.484)	(0.892)	(0.310)	(0.720)
		Observations	45	45	45	45
		Log likelihood	-47.644	-47.249	-39.872	-37.460
(f)	WI-Same-ER	Fav West. / Fav East	0.750	1.000	0.667	0.393**0
			(0.256)	(0.281)	(0.215)	(0.140)
		Fav West / No Fav	1.364	1.143	2.667**	1.571
			(0.541)	(0.418)	(1.277)	(0.760)
		Fav East / No Fav	1.818	1.143	4.000***	4.000***
			(0.683)	(0.418)	(1.826)	(1.690)
		Observations	46	46	46	46
		Log likelihood	-49.205	-50.448	-44.732	-42.818
(g)	WI-Fix-WR	Fav West / Fav East	1 .000	0.625	3.091***	2.833***
			(0.324)	(0.206)	(1.072)	(0.951)
		Fav West / No Fav	1.583	1.364	6.800***	8.500***
			(0.584)	(0.541)	(3.257)	(4.493)
		Fav East / No Fav	1.583	2.182**	2.200	3.000**
			(0.584)	(0.794)	(1.187)	(1.732)
		Observations	50	50	50	50
		Log likelihood	-53.894	-52.330	-41.281	-40.340
(h)	WI-Fix-ER	Fav West / Fav East	0.520*	0.480**	0.343***	0.464**
			(0.178)	(0.169)	(0.120)	(0.156)
		Fav West / No Fav	1.300	1.091	2.200	1.857
			(0.547)	(0.455)	(1.187)	(0.871)
		Fav East / No Fav	2.500**	2.273**	6.400***	4.000***
			(0.935)	(0.822)	(3.078)	(1.690)
		Observations	48	48	48	48
		Log likelihood	-48.976	-49.150	-40.490	-45.550

Notes: This table is an expansion of Table 2.4. For details, see notes of Table 2.4. Standard errors are reported in parentheses. Statistical significance is indicated as follows. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Motives - Data

	Max. own pay.	Not max. own pay	Max. rcpt. pay & match rcpt. choice	Max. rcpt. pay & not match rcpt. choice	Not max. rcpt. pay & match rcpt. choice	Not max. rcpt. pay & not match rcpt. choice
Own-WR	54%	46%	n/a	n/a	n/a	n/a
Own-ER	46%	54%	n/a	n/a	n/a	n/a
WI-Match-WR	56%	44%	32%	12%	24%	32%
WI-Match-ER	50%	50%	26%	6%	24%	44%
WI-Fix-WR	n/a	n/a	16%	30%	12%	42%
WI-Fix-ER	n/a	n/a	27%	27%	2%	44%
WI-Same-WR	67%	33%	38%	29%	4%	29%
WI-Same-ER	50%	50%	17%	33%	9%	41%

Notes: The table presents for each treatment the shares (in %) of the motives to (not) maximize their own payoff and, in addition, the four distinct motives intermediaries can have of (i) maximizing the recipient's payoff and matching his choice, (ii) maximizing the recipient's payoff but not matching his choice, (iii) not maximizing the recipient's payoff but matching his choice, (vi) neither maximizing the recipient's payoff nor matching his choice.

A.2 First names of participants

See below the first names of participants in the respective treatment condition.

Own-WR. Eugen, Samuel, Max, Holger, Leonard, Fynn, Oliver, Moritz, Robert, Julius, Benny, Adam, Reinhold, Luca, Johannes, Eike, Niclas, David, Jörgen, Lukas, Janis, Tim, Christian-Lennart, Martin, Thorsten, Jan, Marlon, Marcel, Marius, Ulrich, David, Jan, Darius, Matthias, David, Sven, Hanno, Robin, Jonas, Jakob, Christopher, Bernhard, Martin, Nicolas, Julian, Yannick, Peter, Lars, Michael, Konstantin, Erik, Raphael, Tristan, Kai-Steffen.

Own-ER. Ferhat, Mert, Berkan, Ali, Yassin, Sercan, Ahmed, Tanyeem, Fouad, Hamza, Adar, Cihan, Reza, Lutfi, Bilal, Serhat, Raschid, Sorush, Mehmet, Fatih, Emre, Khalid, Jamal, Ömer, Kubilay, Berkan, Selim, Seyid-Berdan, Najib, Kamiran, Rashad, Kubi, Najim, Bayram, Osama, Sinan, Fatih, Abdurrahman, Nihat, Selim, Semih, Omer, Mustafa, Süleyman, Kazim, Ümit, Azat, Abdul-Dawi.

For being an Eastern recipient on behalf of whom Western intermediaries decide, participants are further pre-selected to have unambiguously Middle-Eastern first names.

Eastern recipients on behalf of whom Western intermediaries act (subset of Own-ER). Ferhat, Mert, Berkan, Ali, Yassin, Sercan, Ahmed, Fouad, Hamza, Adar, Cihan, Reza, Bilal, Serhat, Raschid, Sorush, Mehmet, Fatih, Emre, Khalid, Jamal, Ömer, Selim, Seyid-Berdan, Najib, Kamiran, Rashad, Najim, Bayram, Osama, Sinan, Fatih, Abdurrahman, Nihat, Selim, Semih, Mustafa, Süleyman, Kazim, Ümit, Azat, Abdul-Dawi.

WI-Match-WR. Tim, Konstantin, Max, Philipp, David, Philipp, Michael, Philipp, Lukas, Marius, Adam, Frieder, Lars, Peter, Tim, Nick, Steffen, Jens, Fabio, Simon, Daniel, Manfred, Robert, Marvin, Benedikt, Linus, Tim, Sven, Sebastian, Max, Arthur, Reinhold, Leonard, Pascal, Lennart, Alexander, Christian, Valentin, Jan, David, Paul.

WI-Match-ER. Christian, Timo, Jonas, Vincent, Fabian, Maximilian, Reinhard, Joe, Lukas, Nils, David, Pascal, Maurice, Leo, Jerome, Malte, Lukas, Moritz, Laux, Max, Lukas, Valentin, Luca, Moritz, Till, Tobias, Martin, Christian, Florian, Marc, Gerrit, Dennis, Mario, Vincent, Tim, Luca, Jakob, Wolfgang, Sebastian, Fridolin, Daniel, Joachim, Johannes, Philip, Eric, Christian, Keno, Florian, Robin, Michael, Nils, Thomas, Philipp, Tim.

WI-Same-WR. Robin, Julian, Dennis, Niklas, Daniel, Simon, Felix, Severin, Pascal, Yannic, Kevin, Matthias, Jacob, Maik, Thomas, Alex, Jonathan, Michael, Sebastian, Eric, Rainer, Fabian, Marco, Tobias, Marco, Andreas, Maxi, Andreas, Lars, Till, Adrian, Lukas, Sebastian, Florian, Marco, Martin, Benjamin, Daniel, Michael, Tobias, Leon-Moritz, Marvin, Ralf, Marco, Marc.

WI-Same-ER. Jan, Nikolas, Johannes, Lukas, Jonathan, Miro, Daniel, Tim, Robert, Michael, Dominik, Leon, Tobias, Rene, David, Hendrik, Leon, Tobias, Andreas, Sebastian, Johannes, Timo, Kilian, Valentin, Jurek, Christoph, Johannes, Moritz, Marcel, Steffen, Wolfgang, Fabian, Thomas, Mark, Jake, Thomas, John, Conny, Wilhelm, Nikolai, Michael, Mirko, Max, Mario, Mathias, Joachim.

WI-Fix-WR. Michel, Benedikt, Jonas, Sascha, Mario, Robin, Christof, Dominic, Moritz, Ole, Simon, Julian, Moritz, Laurens, Florian, Marcel, Malte, Julian, Daniel, Marius, Severin, Florian, Jannik, Kai, Samuel, Lennart, Raphael, Max, Leon, Alberto, Jonas, Joachim, Sebastian, Tim, Mirko, Michael, Jonny, Timm, Maximilian, Alexander, Stefan, Jonas, Alexander, Frederik, Patrick, Chris, Ulrich, Dominik, Ralf, Sebastian.

WI-Fix-ER. Kai, Fabian, Arved, Daniel, Hendrik, Lukas, Peer, Pascal, Joachim, Max, Tom, Niklas, Rafael, Julian-Paul, Christopher, Lukas, Jonas, Johannes, Dominik, Kevin, Philipp, Dennis, Jakob, Sascha, Mathias, Thomas, Christian, Michael, Marius, Marc, Leon, Maximilian, Tim, Bruno, Philipp, Arne, Nico, Paul, Andre, Paul, Tobias, Niklas, Philipp, Marcel, Andre, Kevin, Thorsten, Alex.

A.3 Details on data cleaning

Before conducting any analysis, data is cleaned according to the following three criteria. First, seven participants with names that do not appear on the ORSEE list of registered participants are removed. It seems that some registered participants shared the link to the study with friends, or some of them participated with fake names.

Second, ten entries of participants who have participated twice are removed. Data from their initial participation is kept. Likely reasons for double participation could be to increase winning probabilities or regretting the decision taken in the initial participation.

Third, another sixteen participants are removed because their first *and* last names are judged as non-native-German in treatments with Western decision-makers or non-Middle-Eastern in the treatment with Eastern decision-makers. Despite thoroughly pre-selecting thousands of participants based on their names being typical native German or Middle-Eastern, a few seem to have slipped this procedure and were invited despite having names that did not fit. For each treatment condition, the removed first names are listed below. For the last names, the region from where they might originate is indicated without making the name public.

Non-Middle-Eastern names removed from Own-ER. Gbesimi [last name Sub-Saharan-African], Bartosz [last name Eastern-European], Lachezar [last name Eastern-European].

Non-native-German names removed WI-Match-WR. Ali [last name Middle-Easter].

Non-native-German names removed WI-Match-ER. Stanislav [last name Eastern-European], Nikolay [last name Eastern-European], Börge [last name Scandinavian].

Non-native-German names removed WI-Same-WR. Guan Yu [last name Far-East-Asian], Tidjane [last name French], Aleksandar [last name Eastern-European].

Non-native-German names removed WI-Same-ER. Metin [last name Middle-Eastern], Adil [last name Middle-Eastern], Filip [last name Eastern-European], Patrice [last name Sub-Saharan-African].

Non-native-German names removed WI-Fix-WR. Bryan [last name Eastern-European], Andrey [last name Eastern-European].

Table A.6 shows the number of removed participants for all treatment conditions, except for candidates in phase one. One participant in the role of a candidate participated twice, and his second participation was removed from the data. In his comments, he stated that he regretted his first decision and wanted to make a different one in the second participation.

Table A.6: Number of participants removed per treatment condition

	Own Decision	Western Intermediary (WI) Match	Fixed	Same	Total
Western recipient (WR)	3	2	2	4	11
Eastern recipient (ER)	8	4	2	7	21
Total	11	6	4	11	32

There was also a candidate who unfortunately turned out to be too old for the intended age category. Therefore, he needed to be replaced by another younger participant.

In sum, the raw data has 424 entries for participants, of whom 34 are removed as described above, 4 are participants in the role of candidates in phase one, 102 are participants in the role of recipients in phase two, and 284 are participants in the role of intermediaries phase three.

Results are qualitatively robust to (i) not removing any observation, (ii) removing solely double participation, and (iii) removing double participations plus unregistered participants. In some cases, significance levels change. In a few cases, results that were on the brink of being marginally significant become significant, i.e., the discriminatory tendency in Own-WR or the difference between WI-Match-WR and WI-Same-WR. On the other hand, the marginally significant difference between WI-Same-WR and WI-Same-ER becomes insignificant. Yet, the value of these types of robustness tests is unclear because, undoubtedly, data cleaning, as described above, is necessary.

A.4 Instructions

This section contains a English translation of the original German instructions. The confirmation email to the study included two link and instructions how to proceed. The first link, directed participants to a short payment information survey, including some general information about data privacy. The second link directed participant to the actual experiment. Depending on each participants role, he either was directed to the candidates' survey, the recipients' survey, or the intermediaries' survey.

A.4.1 Payment information survey - all participants

Screen 1

We welcome you to this scientific experiment. It is very important that you read the following explanations carefully.

Today's survey will take about 10-15 minutes. For participating in this experiment, you will receive 2.50 euros. In addition, there is a possibility to win extra money. The exact amount depends on your decision, the decisions of the other participants, and luck.

You will not receive your earnings (participation fee + potential extra earnings) immediately. You will receive your earnings once the data collection of the research project is completed. This can be several weeks after participation. You can receive your earnings either via PayPal, as a bank transfer, or as an Amazon voucher. Please provide the required information for your preferred payment method.

Please indicate your preferred payment method (no duplicates, please).

- Pay-Pal account (email):
- or Amazon voucher (email):
- or bank transfer (name & IBAN):

In this study, you will interact with other participants in a time-shifted manner. To enable this time-shifted interaction, we need your first name. Your first name will be displayed to other participants in their decision-making situation during the experiment.

During data collection, we treat your payment data in a strictly pseudo-anonymized manner. This means that the payment dataset is kept separate from the decision dataset. Two different experimenters will each have access to only one of the two datasets. For this purpose, a randomly generated seven-digit number will be displayed to you on the next screen representing your participant ID.

Please note the participant ID. You will have to enter it at the beginning of the experiment.

The payout of your earnings depends on the participant ID. As soon as the earnings of all participants have been determined, the responsible experimenter transmits the participant ID and the respective earnings to the second experimenter who is responsible for the transfer. The latter then transfers the earnings without having access to the decision data.

After transfers of all participants' earnings are completed, we will delete the dataset with the payment information, and thus converting the pseudo-anonymization into anonymization. Therefore, except for the first names, the analysis and presentation of the data will be anonymized.

If you agree that the payment information you provide and your first name are used in the manner described here, please click "Continue."

Screen 2

Your participant ID is: 4827491

Please note the participant ID. You will need it to identify yourself at the beginning of the actual survey. Participation without the participant ID is not possible.

Please complete the request for your personal information by clicking on "Continue."

Screen 3

Your personal information has been stored.

Please use the second link from the email to participate in the actual survey.

A.4.2 Phase 1 - Candidates

Screen 1

Before you can start, please enter your participant ID and your first name (Note: The participant ID cannot be synchronized. An incorrect entry will make the payout impossible; check your entry. Your first name will be synchronized. Please make sure to enter your real first name):

- participant ID:
- first name:

Then click on "Continue" to start the experiment.

Screen 2

On this and the next page, we explain the procedure of the experiment.

Basic decision situation

- Participants of the experiment are divided into different roles.
- You will be shown your role on the next screen.
- Participants with role A receive an endowment of 100 euros.
- Then, each of the participants with role A decides how much of the 100 euros he want to send to an unknown participant in role B. All integer amounts from 0 - 100 euros are allowed.
- The amount sent is the payout of participant B. The remaining amount is the payout of participant A.

You will receive further instructions after the role assignment. Click "Next" to be assigned a role.

Screen 3

You are in the role of participant A.

Please note that the decision you make here will be implemented at least once. However, we will use your decision multiple times in this study. Therefore, it is possible that your decision will be implemented multiple times and thus also leads to multiple payouts.

Please enter the amount of the 100 euros endowment that you would like to send to an unknown participant B. All integer amounts from 0 - 100 euros are allowed.

Screen 4

Please briefly describe how you made your decision.

Do you have any general comments about the study that you would like to share?

A.4.3 Phase 2 - Recipients

Screen 1

Before you can start, please enter your participant ID and your first name (Note: The participant ID cannot be synchronized. An incorrect entry will make the payout impossible; check your entry. Your first name will be synchronized. Please make sure to enter your real first name):

- participant ID:
- first name:

Then click on "Continue" to start the experiment.

Screen 2

On this and the next page, we explain the procedure of the experiment.

Basic decision situation

- Participants of the experiment are divided into different roles.
- You will be shown your role on the next screen.
- Participants with role A receive an endowment of 100 euros.
- Then, each of the participants with role A decides how much of the 100 euros he want to send to an unknown participant in role B. All integer amounts from 0 - 100 euros are allowed.
- The amount sent is the payout of participant B. The remaining amount is the payout of participant A.

You will receive further instructions after the role assignment. Click "Next" to be assigned a role.

Screen 3

You are in the role of participant B.

As participant B, your decision is not directly implemented. You participate in a lottery with up to 49 other participants B. Your decision is only implemented and leads to payouts if you are drawn in the lottery.

Please note that for your own interest, but also for the utilization of the results of this study, it is of utmost importance that you make your decision as if it would be implemented.

Screen 4

As you have already been told, the data collection in this experiment is sequential. In particular, this means that we have already collected participants A decisions.

In a first survey, participants A have already decided what amount of the 100 euros endowment they want to send an unknown participant B. Participants A were informed that their decision will be used multiple times in the study and could potentially result in multiple payouts.

In this second survey, you, as participant B, will receive a list with information about four participants A and decide whom of these four you want most preferably (second-most preferably, third-most preferably, fourth-most preferably) to be assigned to you.

Screen 5

Below you see a list with information about four participants A. One of these participants A will be assigned to you. You determine the probability with which each of the four listed participants A could be assigned to you and would then determine your payout.

The following matching probabilities apply:

- Rank 1: 40%
- Rank 2: 30%
- Rank 3: 20%
- Rank 4: 10%

This means that if you give rank 1 (2, 3, 4) to a participant A, then this participant will be assigned to you with a 40% (30%, 20%, 10%) probability.

Please note that only the decision of the participant A assigned to you becomes relevant for payments and, given that you are drawn in the lottery, determines your payout and the payout of the assigned participant A. The other three participants A, who are not assigned to you, go empty-handed from your decision.

Reminder: Each of the four participants A listed below has decided about the allocation of 100 euros between himself and a participant B unknown to him. You are now in the role of participant B and decide whom of these four participants A you want most (second-most, third-most, fourth-most) preferably to be assigned to you.

Please give each of the four persons a rank from 1 to 4. Note that you can give each rank (1-4) only once. You must give each participant A on the list a rank in order to proceed with the experiment.

Screen 6

What do you believe, who of the four participants A listed below has sent the highest (second-highest, third-highest, fourth-highest) amount?

Please rank each of the four participants A from 1 to 4, where rank 1 (2, 3, 4) means that you believe the highest (second-highest, third-highest, fourth-highest) amount to have been sent by the respective participant A.

Note that you can give each rank (1-4) only once. You must give each participant A on the list a rank in order to proceed with the experiment.

Screen 7

Please briefly describe how you made your decision regarding the matching probabilities.

Please briefly describe how you made your decision regarding your beliefs about the amounts sent.

Do you have any general comments about the study that you would like to share?

A.4.4 Phase 3 - Intermediaries

Screen 1

Before you can start, please enter your participant ID and your first name (Note: The participant ID cannot be synchronized. An incorrect entry will make the payout impossible; check your entry. Your first name will be synchronized. Please make sure to enter your real first name):

- participant ID:
- first name:

Then click on "Continue" to start the experiment.

Screen 2

On this and the next page, we explain the procedure of the experiment.

Basic decision situation

- Participants of the experiment are divided into different roles.
- You will be shown your role on the next screen.
- Participants with role A receive an endowment of 100 euros.
- Then, each of the participants with role A decides how much of the 100 euros he want to send to an unknown participant in role B. All integer amounts from 0 - 100 euros are allowed.
- The amount sent is the payout of participant B. The remaining amount is the payout of participant A.

You will receive further instructions after the role assignment. Click "Next" to be assigned a role.

Screen 3

You are in the role of participant C.

As participant C, your decision is not directly implemented. You participate in a lottery with up to 49 other participants C. Your decision is only implemented and leads to payouts if you are drawn in the lottery.

Please note that for your own interest, but also for the utilization of the results of this study, it is of utmost importance that you make your decision as if it would be implemented.

Screen 4

As participant C, you do not participate directly in the basic decision situation described above but act on behalf of a participant B. For this purpose, a participant B is assigned to you.

As you have already been told, the data collection in this experiment is sequential. In particular, this means that we have already collected participants A and participants B decisions.

In a first survey, participants A have already decided what amount of the 100 euros endowment they want to send an unknown participant B. Participants A were informed that their decision will be used multiple times in the study and could potentially result in multiple payouts.

In a second survey, each participant B received a list with information about four participants A and decided, by ranking from 1 to 4, whom he wanted most (second-most, third-most, fourth-most) to be assigned to him.

In this third survey, you, as participant C, act on behalf of a participant B. For this purpose, a participant B is assigned to you. You will be shown the first name of the participant B assigned to you. You will receive the same list of information about the four participants A, among whom also participant B could choose. On behalf of participant B, you then decide whom of the four participants A you want most (second-most, third-most, fourth-most) preferably be assigned to participant B.

Screen 5: Earnings (depending on incentive scheme)

Screen 5 in WI-Match

Your earnings: For each match between your ranking and the ranking of participant B, you earn 25 euros. For example, if you give a particular participant A rank 1 and if participant B has also given this participant A rank 1, then you earn 25 euros. If you give two participants A the same ranks as also given to them by participant B, then you earn 50 euros. If your ranking completely overlaps with the ranking of participant B, you earn 100 euros. If your ranking does not overlap with the ranking of participant B at all, you earn 0 euros.

Please note that your decision determines not only your own payout but also the payout of participant B and the four participants A. The three participants A not matched go empty-handed.

Screen 5 in WI-Same

Your earnings: Your earnings equal the earnings of participant B. This means that not only participant B receives the amount that the matched participant A has sent, but also that you receive the exact same amount.

Please note that your decision determines not only your own payout but also the payout of participant B and the four participants A. The three participants A not matched go empty-handed.

Screen 5 in WI-Fix

Your earnings: You earn 50 Euros fixed.

Please note that your decision determines not only your own payout but also the payout of participant B and the four participants A. The three participants A not matched go empty-handed.

Screen 6

The first name of the participant B assigned to you is [NAME] (male).

Below you see a list with information about four participants A. One of these participants A will be assigned to [NAME]. You determine on behalf of [NAME] the probability with which each of the four listed participants A could be assigned to [NAME] and would then determine the payout of [NAME].

The following matching probabilities apply:

- Rank 1: 40%
- Rank 2: 30%
- Rank 3: 20%
- Rank 4: 10%

This means that if you give rank 1 (2, 3, 4) to a participant A, then this participant will be assigned to [NAME] with a 40% (30%, 20%, 10%) probability.

Please note that only the decision of the participant A assigned to [NAME] becomes relevant for payment and, given that you are drawn in the lottery, determines the payout of [NAME] and the payout of the assigned participant A. The other three participants A, who are not assigned to [NAME], go empty-handed from your decision.

Reminder: Each of the four participants A listed below has decided about the allocation of 100 euros between himself and a participant B unknown to him. You are now in the role of participant C, who decides on behalf of [NAME], whom of these four participants A you want most (second-most, third-most, fourth-most) preferably to be assigned to [NAME].

Please give each of the four persons a rank from 1 to 4. Note that you can give each rank (1-4) only once. You must give each participant A on the list a rank in order to proceed with the experiment.

Screen 7 or 8 or 9

What do you believe to whom of the four participants A [NAME] has allocated the highest (second-highest, third-highest, fourth-highest) matching probability (rank)? That is, whom of the four participants A would he want mostly (second-mostly, third-mostly, fourth-mostly) to be assigned to him?

Please rank each of the four participants A from 1 to 4, where rank 1 (2, 3, 4) means that you believe that [NAME] has given the respective participant A rank 1 (2, 3, 4).

Note that you can give each rank (1-4) only once. You must give each participant A on the list a rank in order to proceed with the experiment.

Screen 7 or 8 or 9

What do you believe [NAME] believes, who of the four participants A listed below has sent the highest (second-highest, third-highest, fourth-highest) amount?

Please rank each of the four participants A from 1 to 4, where rank 1 (2, 3, 4) means that you believe that [NAME] believes the highest (second-highest, third-highest, fourth-highest) amount to have been sent by the respective participant A.

Note that you can give each rank (1-4) only once. You must give each participant A on the list a rank in order to proceed with the experiment.

Screen 7 or 8 or 9

What do you believe, who of the four participants A listed below has sent the highest (second-highest, third-highest, fourth-highest) amount?

Please rank each of the four participants A from 1 to 4, where rank 1 (2, 3, 4) means that you believe the highest (second-highest, third-highest, fourth-highest) amount to have been sent by the respective participant A.

Note that you can give each rank (1-4) only once. You must give each participant A on the list a rank in order to proceed with the experiment.

Screen 10

Please briefly describe how you made your decision regarding the matching probabilities.

Please briefly describe how you made your decision regarding your own beliefs about the amounts sent.

Please briefly describe how you have formed your beliefs about the beliefs of [NAME] regarding the amounts sent.

Please briefly describe how you formed your beliefs about the allocation of the matching probabilities by [NAME].

Do you have any general comments about the study that you would like to share?

A.4.5 Demographics Questionnaire - all participants

Screen 1

Finally, we would like to ask you to answer a few more questions.

How old are you?

What is your professional status?

- student
- working full-time
- working part-time
- retired
- unemployed

In case you are studying or have studied: what is/was your field of study?

- Economics
- Education
- Law
- Humanities (incl. medical science)
- Natural sciences
- Technical sciences
- Other field of study

What is your highest level of education?

- No degree
- Lower educational type secondary school degree (Hauptschulabschluss)
- Middle educational type secondary school degree (Realschulabschluss)
- Higher educational type secondary school degree - A level (Fach-/allg. Hochschulreife)
- Bachelor
- Master
- PhD

What is your monthly disposable income?

Screen 2

The experiment is now finished. As soon as the data collection is completed, we will pay out your earnings in the way you have specified. This may take several weeks.

We would like to thank you for your participation!

B Appendix to Chapter 3

B.1 Pictures

Figure B.1: Picture of the debranded backpack used in both studies

Figure B.2: Picture of the stand used for data collection in the first study

B.2 Supplementary tables

Table B.1 and Table B.2 present the average valuations by type of participant in Study 1 and Study 2, respectively. They show that lower and higher pre-valuation participants react with an identical absolute increase in their average valuation to the FN intervention with functionality-related information only. At the same time, the increase caused by ECO&SCL&FN is larger for higher than for lower pre-valuation participants.

Table B.1: Study 1 valuations (by participant type)

Type of participant	Treatment	Pre-intervention valuation (in €)	Post-intervention valuation (in €)	Change in valuation (in €)
All participants	FN	35.30	42.78	7.48
	ECO&SCL&FN	32.79	50.25	17.46
	Treatment difference	-2.51	7.47	9.98
Lower pre-valuation participants	FN	14.64	22.11	7.49
	ECO&SCL&FN	15.12	27.80	12.68
	Treatment difference	0.48	5.69	5.19
Higher pre-valuation participants	FN	52.64	60.11	7.48
	ECO&SCL&FN	54.30	77.58	23.28
	Treatment difference	1.66	17.47	15.80

Table B.2: Study 2 valuations (by participant type)

Type of participant	Treatment	Pre-intervention valuation (in €)	Post-intervention valuation (in €)	Change in valuation (in €)
All participants	ECO&FN	38.67	53.37	14.70
	SCL&FN	43.41	58.28	14.87
	Treatment difference	4.74	4.91	0.17
Lower pre-valuation participants	ECO&FN	22.89	34.09	11.20
	SCL&FN	21.58	33.13	11.55
	Treatment difference	-1.31	-0.96	0.35
Higher pre-valuation participants	ECO&FN	57.72	76.66	18.93
	SCL&FN	64.56	82.66	18.09
	Treatment difference	6.84	6.00	-0.84

Columns (1) and (2) of Table B.3 present regression results from regressing the pre-valuation on the treatment dummy ECO&SCL&FN. Column (1) shows that there is no significant treatment difference in pre-valuations. Column (2) shows that liking the backpack's design and reporting to currently need a backpack are positively associated with having a higher pre-valuation for the backpack. Column (3) regresses the post-valuation on the treatment dummy ECO&SCL&FN. CSR intervention significantly increases the post-valuation. Column (4) shows that liking the backpack's design and reporting to currently need a backpack are positively associated with having a higher post-valuation. In addition, column (4) also shows that participants with a higher pre-valuation also tend to have a higher post-valuation. Column (5) replicates the main result from the study that the increase in valuation caused by the CSR intervention is stronger than the increase caused by the control intervention in FN. Further, column (6) shows a positive relationship between the increase in valuation and needing a backpack and higher pre-valuation. In Table 3.3, we show that the latter comes solely from CSR information causing higher pre-valuation participants to increase their valuation stronger than lower pre-valuation participants. Column (6) also points to a negative association of age with an increase in valuation.

Columns (1) and (2) of Table B.4 show the specific dimension of CSR does not affect pre-valuations. However, pre-valuations in Study 2 are positively associated with being a business or economics major, liking the backpack's design, needing a backpack, and reporting having previously paid premiums for ecologically responsible products. They are negatively associated with reporting having previously paid premiums for socially responsible products. Columns (3) and (4) show that the CSR dimension does not affect post-valuations. Higher post-valuations are strongly associated with being a female and having a higher pre-valuation. Columns (5) and (6) show that the CSR dimension also does not affect the change in valuation. Increases in valuation are positively associated with being a female and having a higher pre-valuation but negatively associated with being a business or economics major.

Table B.3: Study 1 regression results (with and without controls)

	Bid 1 (1)	Bid 1 (2)	Bid 2 (3)	Bid 2 (4)	Bid 2-Bid 1 (5)	Bid 2-Bid 1 (6)
ECO&SCL&FN	-2.509	-4.315	7.473**	10.010***	9.982***	9.492***
	(2.790)	(2.857)	(3.417)	(2.605)	(1.480)	(1.697)
Age		0.359		-0.364		-0.298*
		(0.384)		(0.333)		(0.174)
Female		-5.564*		-2.055		0.186
		(2.938)		(2.592)		(1.589)
Student		3.716		3.921		-0.352
		(4.537)		(3.695)		(2.202)
University degree		2.263		-1.051		-0.463
		(3.102)		(2.835)		(1.760)
Business/Econ. Major		-1.511		-1.135		-1.681
		(3.384)		(2.528)		(1.602)
Liking Design		4.871***		1.271		0.552
		(1.650)		(1.585)		(1.109)
Needing backpack		3.446**		4.091***		2.074**
		(1.627)		(1.473)		(0.957)
Market knowledge		4.471		4.775*		0.120
		(2.996)		(2.646)		(1.434)
Buying ECO		-0.809		3.721		0.700
		(2.938)		(2.810)		(2.181)
Buying SCL		-0.724		-4.508		-1.231
		(3.403)		(3.026)		(2.021)
Premium ECO		-2.017		-0.503		-0.555
		(3.003)		(2.733)		(1.309)
Premium SCL		1.534		1.920		0.983
		(2.858)		(2.531)		(1.250)
Higher pre-valuation				42.273***		4.293***
				(2.600)		(1.652)
Constant	35.302***	5.787	42.784***	8.323	7.482***	8.308
	(1.937)	(11.713)	(2.130)	(10.369)	(0.774)	(5.843)
Observations	366	327	366	327	366	327
R^2	0.002	0.092	0.013	0.514	0.111	0.155

Notes: The table presents results from ordinary least squares (OLS) regressions with robust standard errors in parentheses. Column (1), (3), and (5) regresses the pre-valuation (Bid 1), the post-valuation (Bid 2), and the difference between post- and pre-valuation (Bid 2 - Bid 1), respectively, on the treatment dummy ECO&SCL&FN. In columns (2), (4), and (6), control variables are added. "Higher pre-valuation" is a dummy based on the categorization described in Section 3.4.2. The remaining control variables are self-reported from the post-experimental questionnaire described in Section 3.2. The number of observations differs because some participants did not answer all questions from the questionnaire. Statistical significance is indicated as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.4: Study 2 regression results (with and without controls)

	Bid 1	Bid 1	Bid 2	Bid 2	Bid 2-Bid 1	Bid 2-Bid 1
	(1)	(2)	(3)	(4)	(5)	(6)
SCL&FN	4.739	1.315	4.909	1.451	0.170	0.945
	(4.623)	(4.445)	(5.818)	(4.035)	(2.921)	(2.699)
Age		-0.323		-0.465		-0.180
		(0.400)		(0.294)		(0.224)
Female		7.962		13.807***		6.000**
		(4.928)		(4.056)		(2.605)
Student		-9.151		-7.969		-4.808
		(6.409)		(7.292)		(5.344)
University Degree		4.188		-0.796		-4.800
		(5.124)		(4.495)		(3.067)
Business/Econ. Major		9.669*		-0.802		-6.745**
		(5.056)		(4.476)		(3.182)
Liking Design		6.500**		1.379		-0.006
		(2.851)		(2.918)		(2.081)
Needing Backpack		8.102***		-0.158		-2.859
		(2.519)		(2.190)		(1.731)
Market Knowledge		2.673		8.304		2.797
		(4.263)		(5.071)		(3.048)
Buying ECO		-1.283		-1.967		0.867
		(4.474)		(3.806)		(3.023)
Buying SCL		4.038		6.117		0.904
		(4.865)		(4.039)		(3.167)
Premium ECO		6.613**		-4.420		-2.844
		(3.069)		(4.745)		(2.973)
Premium SCL		-5.494*		-0.446		0.754
		(3.000)		(4.298)		(2.832)
Higher pre-valuation				48.042***		9.576***
				(4.459)		(2.947)
Constant	38.672***	0.000	53.375***	27.138	14.703***	21.551
	(2.713)	(21.229)	(3.732)	(20.832)	(1.837)	(14.216)
Observations	127	116	127	116	127	116
R^2	0.008	0.206	0.006	0.587	0.000	0.203

Notes: The table presents results from ordinary least squares (OLS) regressions with robust standard errors in parentheses. Column (1), (3), and (5) regresses the pre-valuation (Bid 1), the post-valuation (Bid 2), and the difference between post- and pre-valuation (Bid 2 - Bid 1), respectively, on the treatment dummy SCL&FN. In columns (2), (4), and (6), control variables are added. "Higher pre-valuation" is a dummy based on the categorization described in Section 3.4.3. The remaining control variables are self-reported from the post-experimental questionnaire described in Section 3.2. The number of observations differs because some participants did not answer all questions from the questionnaire. Statistical significance is indicated as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B.3 Instructions

This is a consumer survey conducted by the University of Cologne. You can win money or a backpack in a lottery. If you are drawn in the lottery, your reward depends on your decisions as well as on chance.

If you are drawn in the lottery, you will receive this backpack. You have the possibility to sell it back directly. The reselling is done according to the following mechanism:

Step 1: You place a bid. This bid is not the price at which you will sell the backpack.

Step 2: The price will be drawn by random number.

Step 3: There are two options:

a. If this random number equals your bid or is larger, you get money equal to this random number and you have to give up the backpack for it.

b. If this random number is smaller than your bid, you keep the backpack and get no money for it.

Therefore, the best strategy for you is to think of the smallest amount you would still be willing to sell the backpack for and bid that amount.

Example: You have this candy bar and you are supposed to place your bid such that it equal the lowest price you would sell it for. Let's say this candy bar is worth 50 cents to you.

Two scenarios:

1) If you place a bid below your value, say 40 cents, you have to give the candy bar up when 45 cents is drawn as the price afterwards. This means that by underbidding, you have given up the opportunity to keep that candy bar and will only get 45 cents, even though it is actually worth 50 cents to you.

2) If you bid above your value, say 60 cents, you have to keep the candy bar if a price of 55 cents is drawn afterwards, even if you could have sold it for 5 cents more than it is actually worth to you.

So if you bid something other than what the candy bar is really worth to you, you will get a worse result in two cases:

1) You bid less than the candy bar is actually worth to you: Then you may have to sell the bar at a price that is lower than it is actually worth to you.

2) You bid more than the candy bar is actually worth to you: Then it can happen that you have to keep the bar in the end, even if you could have been paid more than it is actually worth to you.

So your best strategy is to offer exactly what the candy bar is really worth to you. This value may differ from what others would offer for it, because products can be worth different amounts to different people.

Because the mechanism is not quite easy, we'll do a short training round with this apple. [A training round with an apple followed. Only if participants fully understood the mechanism, she/he could they continue. Otherwise, the experimenter explained the mechanism again, and the training round was repeated.]

You will place two bids for the backpack. However, you will only participate in the lottery with one of the bids. Which bid is binding will be determined by chance.

Inspect the backpack on display at the stand before you place your bid.

[First bid was placed and a random price drawn]

[Then, depending on the treatment, information on the backpack was provided as described in Section **??**.]

[Second bid was placed and a random price drawn]

[Then, the following questionnaire followed]

Questionnaire

[1.] Please indicate how important the following features of the backpack were to your decision of your minimum selling price. Put them in order of importance. Start with 1 = most important and end with 15 = least important.

[Number of features differed according to the treatment. For example, in the control treatment without CSR information, no CSR features were included.]

[Features displayed in ECO&SCL&FN treatment: occupational safety review; training for employees; no child labor; long, transparent partnership with suppliers; fabrics made from 100% recycled PET bottles; backpacks free of PFCs harmful to the environment and health; continuous reduction of negative environmental impacts in production; certification/verification by independent third parties (i.e., Fair Wear Foundation and bluesign®); expandable volume; padded laptop compartment; 2 accesses to main compartment; ergonomic shoulder straps; padded back section; removable and adjustable chest strap; water repellent]

[2.] Do you know what brand the backpack is from? [Yes, from ... ; No]

[3.] If you know the brand: how much do you know about the sustainability measures of the brand? [Very much; Much; Somewhat; Little; Nothing]

[4.] Do you like the design of the backpack? [Totally; Rather yes; Neither yes nor no; Rather no; Not at all]

[5.] Do you currently need a new backpack? [Totally; Rather yes; Neither yes nor no; Rather no; Not at all]

[6.] Do you currently need a new backpack? [Totally; Rather yes; Neither yes nor no; Rather no; Not at all]

[7.] How do you assess your knowledge of the current backpack market? [Very good; Above average; Average; Below average; Very bad]

[8.] What is your gender? [male; female; other; I don't want to say]

[9.] What is your gender? [male; female; other; I don't want to say]

[10.] How old are you? [...]

[11.] What is your vocational status? [student; work in fulltime; work in parttime; retirement; unemployed]

[12.] If you study or have studied, what is your field of study? [...]

[13.] What is your highest educational attainment? [No degree; Certificate of Secondary Education; General Certificate of Secondary Education; Higher Education Entrance Qualification; University Degree]

[14.] I buy clothes and accessories made under fair working conditions. [Always; Often; Sometimes; Seldom; Never]

[15.] I buy clothes and accessories that have been made in an environmentally sustainable way. [Always; Often; Sometimes; Seldom; Never]

[16.] In the past, I paid more for clothes and accessories that I knew were made under fair working conditions. [Correct; Rather correct; Sometimes; Rather not correct; Not correct]

[17.] In the past, I paid more for clothes and accessories that I knew were made in an environmentally conscious way. [Correct; Rather correct; Sometimes; Rather not correct; Not correct]

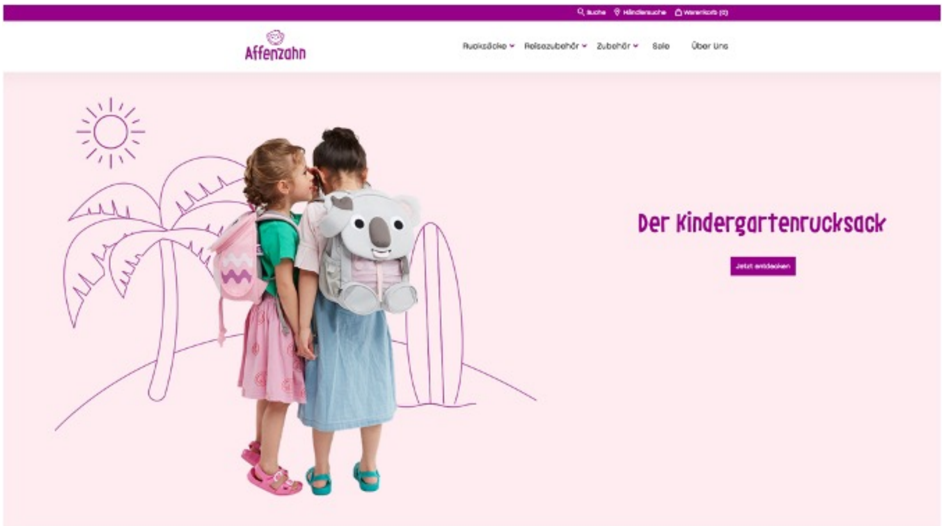
[18.] Do you know the Fair Wear Foundation? [Yes; No]

[19.] Do you know bluesign®? [Yes; No]

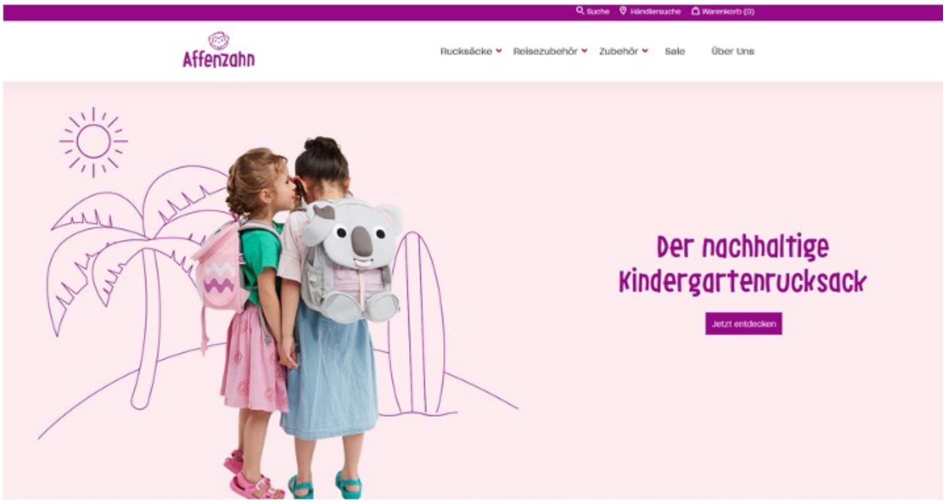
C Appendix to Chapter 4

C.1 Screenshots of webshop

Figure C.1: Screenshots of webshop: Landing page

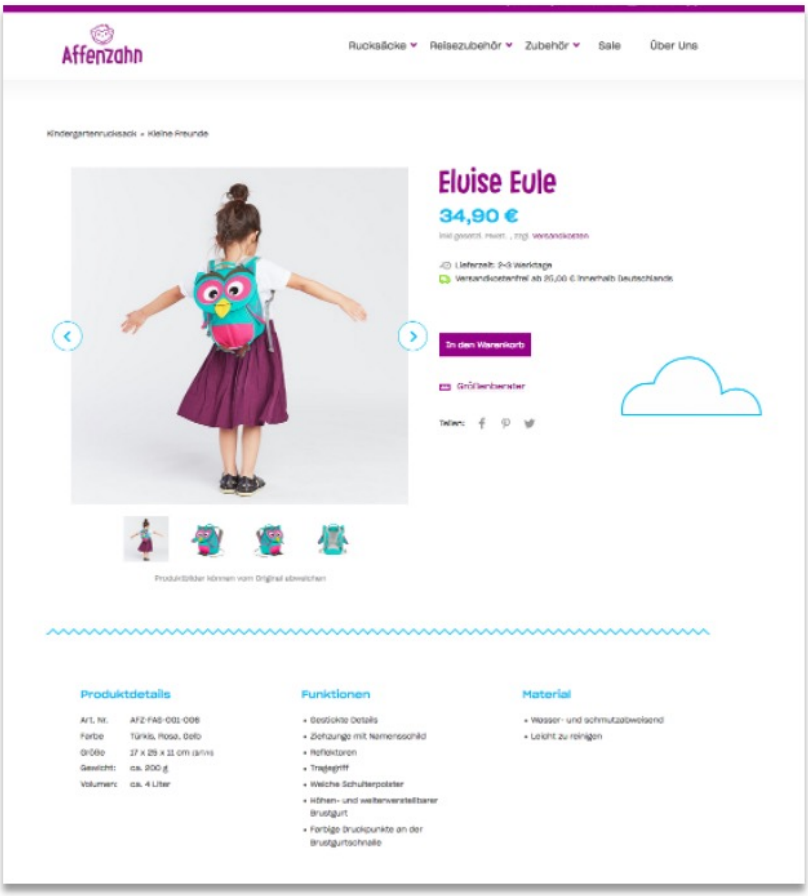


(a) Control Treatment

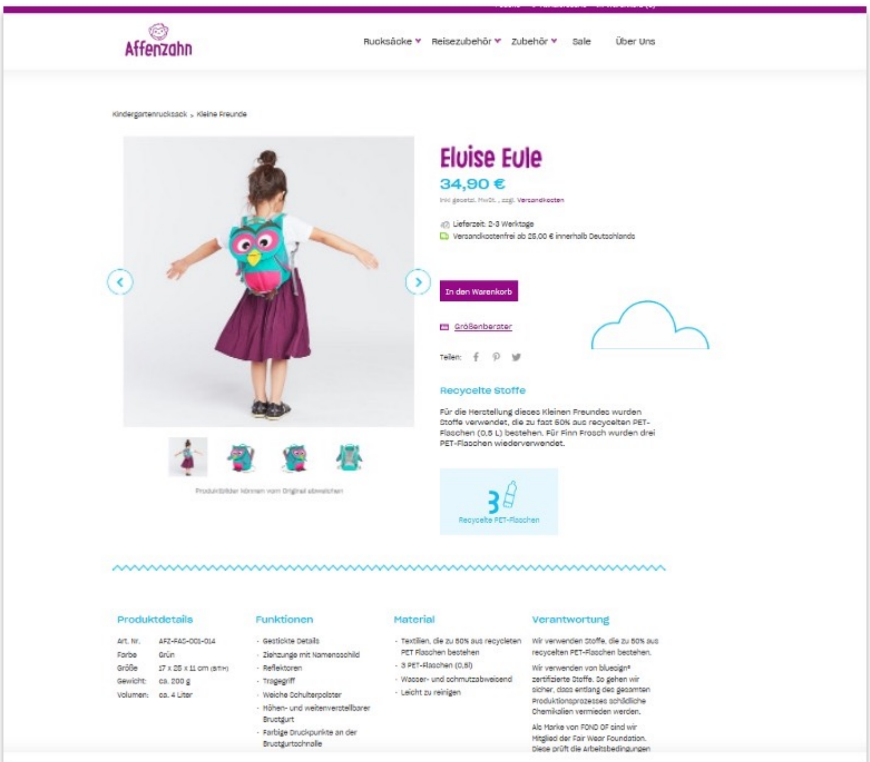


(b) CSR Treatment

Figure C.2: Screenshots of webshop: Product details page (I)



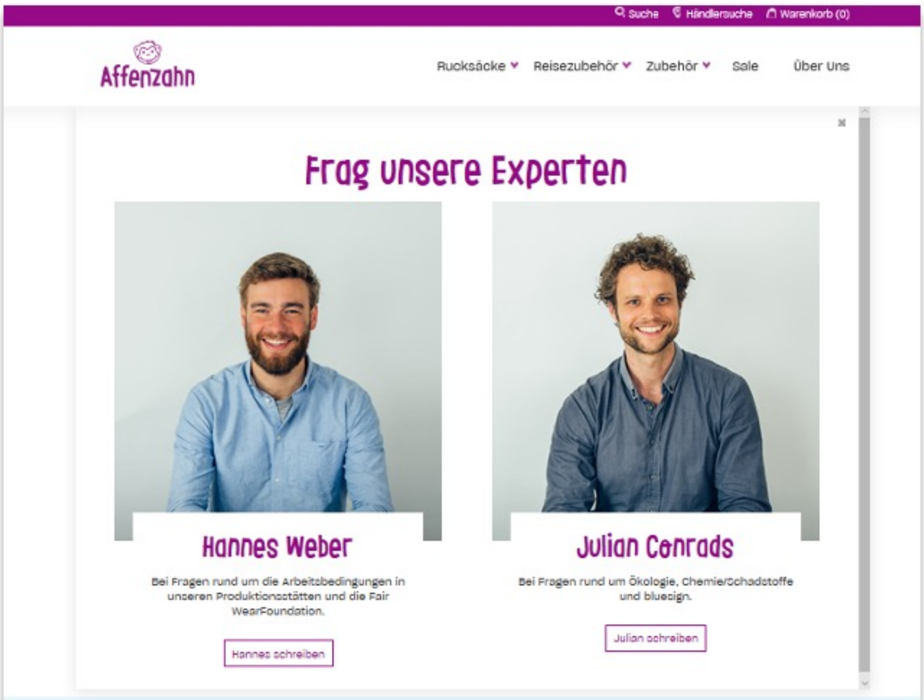
(a) Control Treatment



(b) CSR Treatment

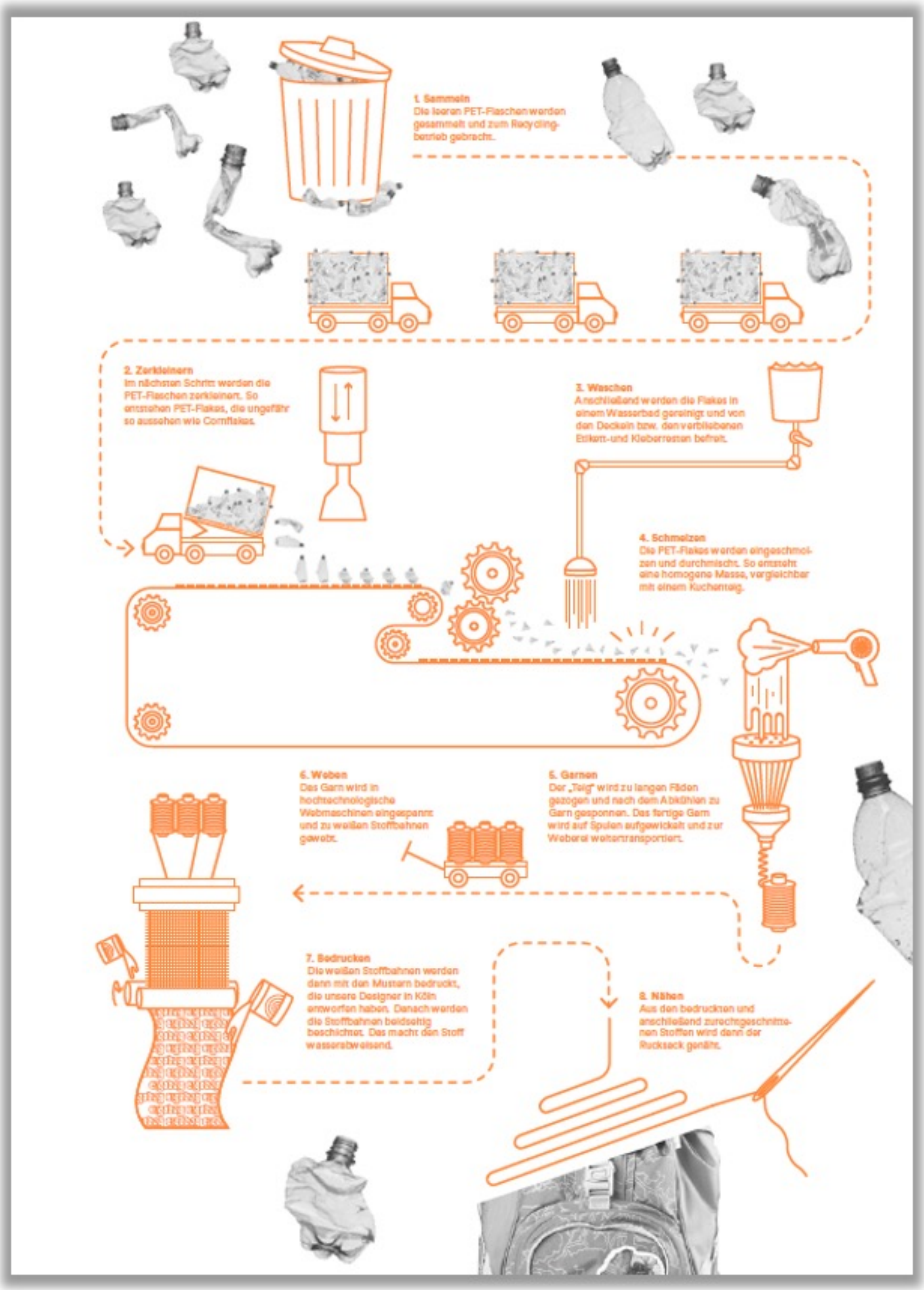
Figure C.3: Screenshots of webshop: Product details page (II) - only in CSR treatment

(a) CSR Treatment



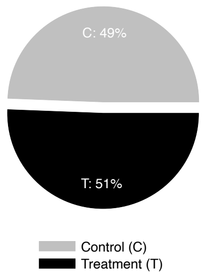
(b) CSR Treatment

Figure C.4: Screenshots of webshop: CSR subpage - only in CSR treatment

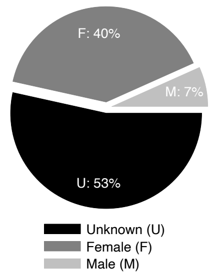


C.2 Supplementary figures and tables

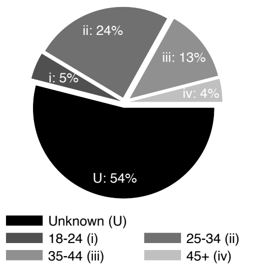
Figure C.5: Visits by category



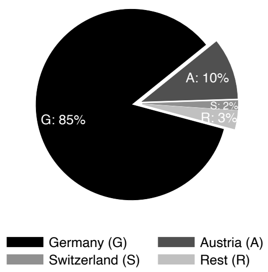
(a) Visits by treatment



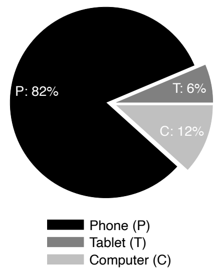
(b) Visits by gender



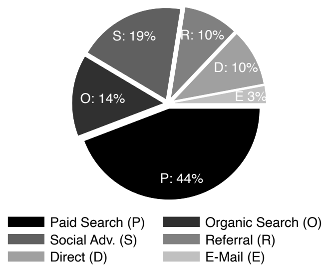
(c) Visits by age category



(d) Sessions by location

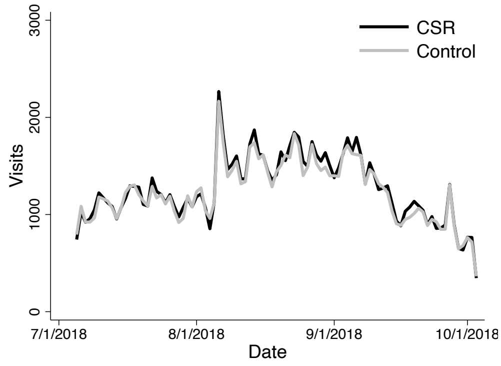


(e) Sessions by device

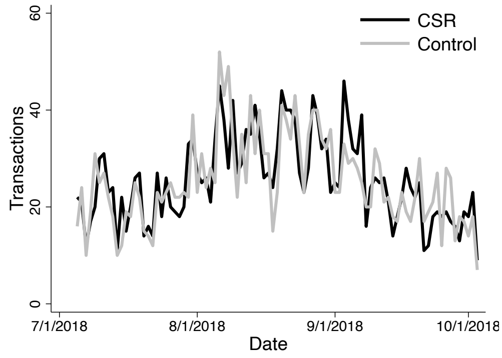


(f) Visits by source

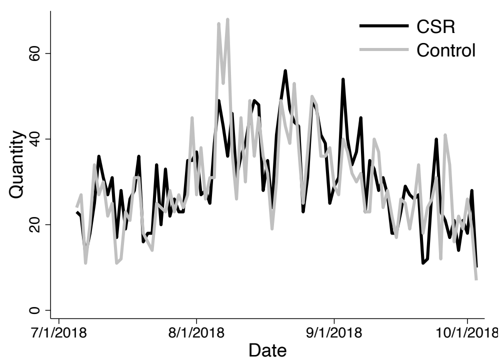
Figure C.6: Sales data over time



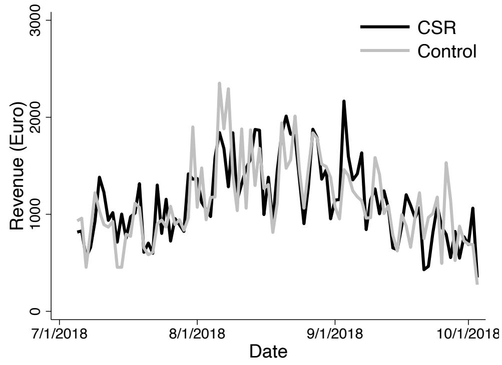
(a) Visits



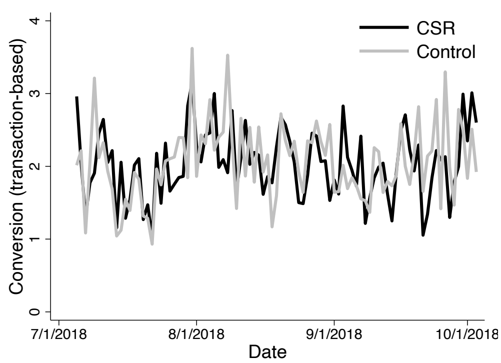
(b) Transactions



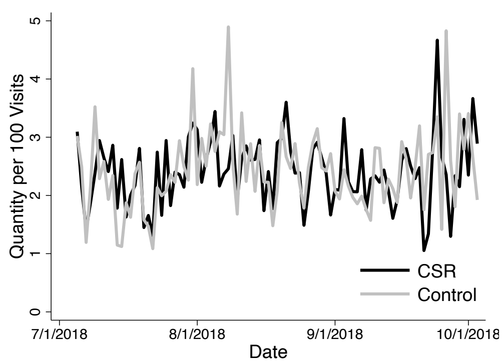
(c) Quantity



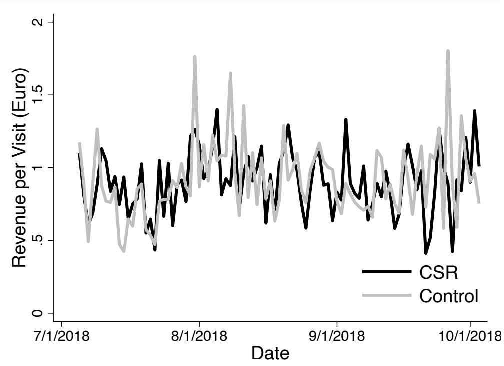
(d) Revenue



(e) Conversion rate



(f) Quantity per 100 visits



(g) Revenue per visit

Table C.1: The effect of CSR on sales (conditional on visitors' number of visits)

	Single-time visitors			Multiple-time visitors		
	Transactions	Quantity	Revenue	Transactions	Quantity	Revenue
	(1)	(2)	(3)	(4)	(5)	(6)
Control	0.0140	0.0164	0.6025	0.0268	0.0318	1.2097
(constant)	(0.0005)	(0.0007)	(0.0232)	(0.0007)	(0.0009)	(0.0336)
Treatment	-0.0010	-0.0013	-0.0342	0.0000	0.0004	-0.0079
	(0.0007)	(0.0009)	(0.0325)	(0.0009)	(0.0013)	(0.0470)
95% Confidence Interval						
Lower bound constant	0.0131	0.0151	0.5570	.0255	0.0301	1.1438
Upper bound constant	0.0150	0.0177	0.6479	.0282	0.0335	1.2755
Lower bound treat. coef.	-0.0024	-0.0031	-0.0979	-.0018	-0.0021	-0.0990
Upper bound treat. coef.	0.0003	0.0004	0.0295	.0018	0.0029	0.0842
Observations	110,486	110,486	110,486	117,169	117,169	117,169
R^2	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Columns (1), (2), and (3) present results from ordinary least squares (OLS) for the effect of CSR information on the conversion of visits into transactions, quantity sold, and revenue, respectively, for visitors documented to have visited a single time. Columns (4), (5), and (6) repeat the analysis for visitors documented to have visited multiple times. Robust standard errors are in parentheses. Statistical significance is indicated as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.2: Distributions sales data by treatment, device and type of visitor

		Phone	Computer	Tablet	Total
Overall Both Treatments	Visits	82%	12%	6%	227,655
	Transactions	63%	27%	10%	4,638
	Quantity	61%	29%	10%	5,488
	Revenue	62%	28%	10%	€205,937
Overall Control	Visits	82%	11%	7%	112,449
	Transactions	63%	27%	11%	2,311
	Quantity	61%	28%	11%	2,726
	Revenue	62%	26%	11%	€102,495
Overall CSR Treatment	Visits	82%	12%	6%	115,206
	Transactions	63%	28%	9%	2,327
	Quantity	61%	30%	9%	2,762
	Revenue	62%	29%	9%	€103,442
Single-time visitors Both Treatments	Visits	80%	14%	6%	110,486
	Transactions	45%	44%	11%	1,493
	Quantity	44%	44%	13%	1,735
	Revenue	45%	43%	12%	€64,674
Single-time visitors Control	Visits	80%	14%	6%	55,221
	Transactions	44%	43%	12%	775
	Quantity	43%	43%	15%	904
	Revenue	44%	42%	14%	€33,269
Single-time visitors CSR Treatment	Visits	80%	14%	6%	55,265
	Transactions	46%	44%	10%	718
	Quantity	45%	44%	11%	831
	Revenue	46%	44%	10%	€31,405
Multiple-time visitors Both Treatments	Visits	83%	10%	7%	117,169
	Transactions	71%	20%	9%	3,145
	Quantity	69%	22%	9%	3,753
	Revenue	70%	20%	10%	€141,262
Multiple-time visitors Control	Visits	84%	9%	7%	57,228
	Transactions	72%	19%	10%	1,536
	Quantity	70%	20%	10%	1,822
	Revenue	71%	19%	10%	€69,226
Multiple-time visitors CSR Treatments	Visits	83%	11%	6%	59,941
	Transactions	70%	21%	9%	1,609
	Quantity	67%	24%	9%	1,931
	Revenue	69%	22%	9%	€72,036

D General appendix

D.1 Declaration of contribution to the chapters of the dissertation

It follows a description of the authors' contributions to Chapters 3 and 4. Chapter 2 is single-authored. Any work described in the following to be done jointly is to be understood as conducted "to equal parts" by the respective persons. See the first footnote in the respective Chapters for additional acknowledgments of help from students and student helpers.

Chapter 3. I initiated the project and asked Julian Conrads, Alexandra Eyberg, and Bernd Irlenbusch to join. Together we developed an initial experimental design of mine into the final design presented in Chapter 3. Alexandra collected the data for the first experiment. Under my supervision and in consultation with Julian and Bernd, she conducted the initial data analysis described in her master's thesis. I organized data collection in the second experiment. I conducted the final data analysis, including data from both projects. I wrote the manuscript, which was then greatly improved by feedback from the others.

Chapter 4. I joined the project at the suggestion of Bernd Irlenbusch. Julian Conrads, Bernd Irlenbusch, Dirk Sliwka, and I together developed the experimental design described in Chapter 4. Julian implemented the experiment in cooperation with the partner firm. The firm gathered sales data. In tedious work, I retrieved additional data about subjects' behavior on the webshop from the firm's web analytics tool. I cleaned the data and prepared the final data set. I analyzed the data under the consultation of the others. I wrote the manuscript, which was improved by feedback from the others.

Köln, 15.08.2022

D.2 Curriculum vitae

Maivand Sarin

Curriculum Vitae as of August 14, 2022

Moselstr. 4, D-50674 Cologne
+49-174-47103710 | maivand.sarin@gmail.de
Citizenship: Afghanistan and Germany

Research Interest

Behavioral Ethics, Behavioral Economics, Experimental Economics

Education

Ph.D. (Dr. rer. pol.) in Economics, University of Cologne	since 10/2016
Visiting Ph.D. Student (invited by John List), University of Chicago	09/2021 – 11/2021
Master of Science (M.Sc.) in Economics, University of Cologne	10/2013 – 04/2016
Master of Business Administration (MBA), UIBE Business School, Beijing (CN)	03/2014 – 06/2015
Bachelor of Science (B.Sc.) in Business Administration, University of Cologne	09/2009 – 03/2013
A level *(best in cohort)*, Mercator High School, Moers, Germany	06/2009
Entered German education system	08/1999

Work Experience

Research Associate, Seminar for Corporate Development and Business Ethics, University of Cologne	05/2016 – 04/2022
Bayer HealthCare China: Internship in Strategy Management, Beijing (CN)	09/2014 – 10/2014
Bayer China Headquarters: Internship in Controlling, Shanghai (CN)	06/2014 – 07/2014
Bayer AG Global Headquarters: Internship in Corporate Finance, Leverkusen	03/2013 – 09/2013

Teaching Experience

Business Ethics (lecturer, undergrad level)	winter term (wt) 20 & 21
Business Ethics (tutor, undergrad level)	wt 16–20 & st 19–21
CEMS Business Project with Beiersdorf (supervisor, grad level)	st 19
Business Project with Fonds of Bags (supervisor, grad level)	st 18
Compliance Management (tutor, grad level)	st 16 & st 17
Supervision of Bachelor (#37) & Master (#5) theses	ongoing

Grants, Awards, and Scholarships

International Travel Grant for a research stay at the University of Chicago: *C-SEB*	09/2021 – 11/2021
Junior Start-Up Grant for conducting economic experiments: *C-SEB*	07/2020
Student Research Grant for conducting economic experiments: *C-SEB*	12/2016
German Government Scholarship (Deutschlandstipendium): *University of Cologne*	09/2014 – 04/2016
Dean's Award (best in MBA cohort): *UIBE Business School*	06/2015
Chinese Government Scholarship (for MBA): *China Scholarship Council (CSC)*	03/2014 – 06/2015
Promos Scholarship: *German Academic Exchange Service (DAAD)*	02/2012 – 07/2012
Haniel Scholarship for short-term studies in Asia: *Haniel Foundation*	09/2011 – 12/2011
Dean's Award: *WiSo-Faculty, University of Cologne*	06/2011

Presentations, Workshops and External Lectures

Presentations	New York University Abu Dhabi (research seminar)	06/2022
	ESA Asia Pacific Meeting 2022	03/2022
	ESA Special Meeting 2022 by JILAEE	02/2022
	University of Chicago, brown-bag with John List	12/2021
	Southern Economic Association 91st Annual Meeting	11/2021
	ESA 2021 North American Conference	10/2021
	University of Chicago, John List's Experimental Lunch Seminar	09/2021
	2021 Meeting of the Committee for Organizational Econ. (VfS)	09/2021
	ESA 2021 Global Online Around-the-Clock Conference	07/2021
	WEAI Virtual 96th Annual Conference	06/2021
	6th Maastricht Behavioral Economic Policy Symposium (M-BEPS)	06/2021
	University of Erfurt (research seminar)	12/2020
	University of Innsbruck (eeecon research seminar)	12/2020
	University of Cologne (C-SEB research seminar)	11/2020
	University of Cologne (brown-bag seminar)	11/2018
	Thurgau Experimental Economist Meeting (conference)	04/2018
	The Economics of Corruptions (workshop at the University of Passau)	10/2016
	University of Cologne (brown-bag seminar)	03/2016
	University of Cologne (brown-bag seminar)	12/2015
External Lectures	Mini-Course in Experimental Economics by *John List (University of Chicago)*	10/2021
	Field Experiments (ICPSR Summer Program 2018) by *Eline de Rooij (Simon Fraser University),* *Florian Foos (King's College London),* *Alexander Coppock (Yale University)*.	07/2018
	Social Identity and Narratives by *Erin Krupka, University of Michigan*.	07/2018
	LEOH – Lectures on Behavioral Organizational and Industrial Econ. by *Paul Heidhues, Düsseldorf Institute for Competition Economics (D.I.C.E.)*.	09/2017
	Incorporating More Realistic Psychology into Economic Analysis by *Matthew Rabin, Harvard University*.	08/2017
	Mini course on structural estimation and experiments by *Charles Bellemare, Université Laval*.	02/2017
	The Economics of Corruptions by *Johann Graf Lambsdorff, University of Passau*.	10/2016
	LEOH – Lectures on Incomplete Contracts and the Firm by *Oliver Hart, Harvard University*.	09/2016

Language and Computer Skills

Language Skills	*native:* German, Pashto (Afghan 1), Dari (Afghan 2) *fluent:* English *advanced:* Bulgarian *basic:* Chinese (Mandarin), Spanish
Computer Skills	*advanced:* STATA, LaTeX *basic:* zTree, R

Köln, 15.08.2022

D.3 Eidesstattliche Erklärung

Eidesstattliche Erklärung nach Paragraph 8 Abs. 3 der Promotionsordnung vom 17.02.2015

Hiermit versichere ich an Eides Statt, dass ich die vorgelegte Arbeit selbstständig und ohne die Benutzung anderer als der angegebenen Hilfsmittel angefertigt habe. Die aus anderen Quellen direkt oder indirekt übernommenen Aussagen, Daten und Konzepte sind unter Angabe der Quelle gekennzeichnet. Bei der Auswahl und Auswertung folgenden Materials haben mir die nachstehend aufgeführten Personen in der jeweils beschriebenen Weise entgeltlich/unentgeltlich geholfen: –

Weitere Personen, neben den ggf. in der Einleitung der Arbeit aufgeführten Koautorinnen und Koautoren, waren an der inhaltlich-materiellen Erstellung der vorliegenden Arbeit nicht beteiligt. Insbesondere habe ich hierfür nicht die entgeltliche Hilfe von Vermittlungs- bzw. Beratungsdiensten in Anspruch genommen. Niemand hat von mir unmittelbar oder mittelbar geldwerte Leistungen für Arbeiten erhalten, die im Zusammenhang mit dem Inhalt der vorgelegten Dissertation stehen. Die Arbeit wurde bisher weder im In- noch im Ausland in gleicher oder ähnlicher Form einer anderen Prüfungsbehörde vorgelegt. Ich versichere, dass ich nach bestem Wissen die reine Wahrheit gesagt und nichts verschwiegen habe. Ich versichere, dass die eingereichte elektronische Fassung der eingereichten Druckfassung vollständig entspricht. Die Strafbarkeit einer falschen eidesstattlichen Versicherung ist mir bekannt, namentlich die Strafandrohung gemäß Paragraph 156 StGB bis zu drei Jahren Freiheitsstrafe oder Geldstrafe bei vorsätzlicher Begehung der Tat bzw. gemäßParagraph 161 Abs. 1 StGB bis zu einem Jahr Freiheitsstrafe oder Geldstrafe bei fahrlässiger Begehung.

Köln, 15.08.2022

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