Essays on Social Preferences in Economic Experiments

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Chapter 1

Introduction

There exists ample evidence for prosocial behavior in the real world. People donate to charities (Giving USA Foundation, 2014; DZI, 2012), engage in voluntary commitment (BMFSFJ, 2012), donate blood (NHSBT, 2013) or invest into public goods provision (UNEP, 2011). Prosocial behavior is also observed in economic experiments such as dictator, ultimatum, donation, public goods, and trust games.

Due to this, many behavioral models incorporate other-regarding behavior and allow for social preferences. The concept of altruism is based on individuals caring about the payoff of other individuals. Andreoni et al. (2008) define altruism as: the "act must be in consideration of others [. . . and] require[s] that the consequences for someone else affect one's own action" (p.134). In addition, other motives for kind behavior are proposed and tested in laboratory settings. Andreoni (1989) and Andreoni (1990) distinguish pure altruism from warm-glow altruism where giving results in feeling good. Equity or inequity preferences are also brought forward. Bolton and Ockenfels (2000) consider the influence of relative payoff in their model which compares own payoff and others' payoff. Similarly, Fehr and Schmidt (1999) introduce the concept of inequity aversion where disutility arises from an unequal distribution of payoffs. Another approach to explain other-regarding actions (in experiments) is the fairness equilibrium proposed by Rabin (1993) where individuals help (hurt) others who are helping (hurting) them. In strategic interaction the concept of conditional cooperation might apply as well, being grounded in reciprocity and norm adherence. Fischbacher et al. (2001) and Gächter (2007) propose individuals to contribute larger (smaller) amounts towards public goods

if others contribute more (less). The presented list of models is not complete and there exist further explanations of prosocial behavior —but the models have in common that their underlying concepts may be identified in laboratory experiments.

Over the last decades the number of economic experiments increased and experimental economics became a popular tool to investigate individual decision making (Bardsley et al., 2009). In the following, the position of economic experiments and their purpose is briefly discussed to motivate the research vehicles applied in the next chapters. According to the discussion by Croson and Gächter (2010), laboratory experiments are positioned between empirical observations and theory. On the one hand, experiments utilize a controlled setting to investigate observations from the real world. While the field has the problem of potentially existing omitted variables an empiricist cannot capture, these can be tested in the laboratory. On the other hand, experiments are used to address and refine economic theory and the experimental environment can test different parameters that are important for individual decision making. In this light, Croson and Gächter (2010) argue experiments to enable researchers to measure how preferences vary across subject pools or contexts. An example is culture. Henrich et al. (2005) examine different small scale societies and investigate group differences as well as patterns from everyday life reflecting into measured prosocial behavior. In addition, the authors claim individuals' attributes such as gender, age, education, income, and others to matter. Some of these attributes are under-represented in convenient student samples which are usually invited in experimental economics. They incorporate homogeneity with respect to age, education or income and some studies show (prosocial) behavior of students to be positioned at the lower bound compared to non-students (Bellemare and Kröger, 2007).

As an example, student subjects only represent a very narrow age range. In this light, Harrison and List (2004) emphasize to investigate the age-factor because it might be difficult to speculate on individuals beyond a certain age when findings are based on observations from undergraduate students in their 20s. If one would find that age affects prosocial behavior, economic models should be modified accordingly. A further example of how behavior varies across individuals is gender (Gneezy et al., 2009). For social preferences no clear direction of gender has been found (Croson and Gneezy, 2009) and some experimentalists identified females to be more generous while others find this behavior more frequently among males (Eckel and Grossman, 1998; Brown-Kruse and Hummels, 1993). Gender interaction similarly shapes decision making (Ben-Ner et al., 2004; Sutter

et al., 2009) and Croson and Gneezy (2009) conclude that context matters more strongly for female behavior in social preference experiments. The list of individual attributes shaping prosocial behavior is certainly not limited to the examples above. Cognitive abilities, education, personality, economic or family variables could complement behavior and the explanation of prosocial actions further (Boone et al., 1999; Ben-Ner et al., 2004; Ashton et al., 1998).

Lastly, it is proposed that experiments can measure preferences and set their results into relation with natural contexts. This is aimed at by the literature on external validity (Levitt and List, 2007). In social and public goods games Benz and Meier (2008), De Oliveira et al. (2011), or Voors et al. (2012) research real world contexts and the (abstract) laboratory equivalents and the closeness of revealed behavior. Overall, their results on the correlation of laboratory behavior and real world behavior are mixed. The relationship depends on several design parameters which are important for explaining social behavior.

In summary, the task of economic experiments is to fill the gap between theory and empirics (Croson and Gächter, 2010). As experimental economics advanced over the past decades many factors have been investigated. A problem nevertheless remains: as behavior does not always converge, findings might not be generalizable and it is not easy to predict behavioral regularities. Levitt and List (2007) capture this problem and raise issues to be considered when taking prosocial decision making to the laboratory. They dedicate their paper to why it might be difficult to grasp regularities of prosocial behavior present in the real world in the laboratory and emphasize five points which are summarized in the following.

First, they claim moral considerations to matter for prosocial behavior. These considerations are included in early models on interaction claiming that not only monetary parameters shape decision making (Becker, 1974; Bernheim, 1994). Furthermore, they allow for moral costs as well as norms to differ among individuals. Examples could be beliefs, descriptive social norms incorporated in beliefs or other factors such as stakes or scrutiny which they analyze separately.

Second, scrutiny of actions and the awareness of being monitored matter for decisions in experiments. In general, this could include a trade-off between reputational concerns and selfish behavior. Zizzo (2010) hypothesizes varying levels of scrutiny to cause the so

called social experimenter demand effect where individuals exaggerate their prosociality due to social pressure. Several variants of social games prove Zizzo (2010) and the proposed effect to be correct. For example Hoffman et al. (1996) present that varying the degree of observability of actions in dictator games results in behavioral changes.

Third, the context of the experiment matters according to Levitt and List (2007). It often remains unobserved by the experimenter which context is brought to the laboratory. This might be based in experience, repeated participation, or group norms. Furthermore, choice parameters can be crucial. Examples are framing or decision ranges (List, 2007; Bardsley, 2008), endowment and property rights (Oxoby and Spraggon, 2008), returns varied in strategic games (Goeree et al., 2002) and others. How context and experimental instructions are understood may vary by individuals and can be shaped by the experimenter. This effect is known as purely cognitive experimenter demand (Zizzo, 2010) and is based on whether examples or cues are incorporated in instructions and understood by subjects.

Fourth, self-selection of participants matters for experimental outcomes. Participants usually include students who volunteer and therefore, they might be special as to the degree of prosociality (Levitt and List, 2007; Anderson et al., 2013; Slonim et al., 2013; Eckel and Grossman, 2000). This raises a further issue already mentioned above. In the real world not only students engage in social interaction and it appears to be an important question whether there exist attributes –not represented by student subjects– that matter for prosocial behavior.

Fifth, experimental stakes may be an issue. In this light, Harrison and List (2004) summarize findings on varying stakes in the experimental literature and conclude stakes to matter. Due to this, it remains questionable how representative (small) experimental stakes are for real world situations. Even more importantly, it could be possible that substantial versus non-substantial stakes interfere with other factors, for example participants' attributes such as wealth or employment status.

As illustrated by Levitt and List (2007), it is frequently observed that if a situation in the laboratory is varied, i.e., by the experimenter, behavior changes as well. Similarly, a change in the study population may result in differences across subject pools' actions. By these variations in the laboratory, experiments contribute to a better understanding of human behavior, i.e., enhance models or explain field behavior.

Many issues raised so far have been investigated experimentally. Our overall motivation for this thesis is to further extend research on factors influencing decision making in the specific context of social preferences and prosocial behavior in experiments. Next to this common leitmotif, the attention of the specific chapters is directed to different aspects of prosocial behavior. On the one hand, we investigate individual attributes or individual attributes in interaction with design variations. On the other hand, we examine motives underlying behavioral regularities. Furthermore, we apply both abstract as well as context specific experimental settings using real world examples. The following paragraphs summarize the structure of the thesis and the motivation for the main chapters.

Chapters 2–4 focus on prosocial behavior in the dictator game: A common introduction in Chapter 2 provides an overview on the literature and additionally motivates the course of investigation of the individual research projects. Chapter 3 is co-authored by Israel Waichman and may be cited as KETTNER, S.E. AND WAICHMAN, I. (2014) OLD AGE AND PROSOCIAL BEHAVIOR: SOCIAL PREFERENCES OR EXPERIMENTAL CONFOUNDS?, MIMEO, UNIVERSITY OF HEIDELBERG, GERMANY. As the title suggests, the aim of the paper is to explore the relationship of age and prosocial behavior. Specifically, we answer the following research question: *Could it be that more prosocial behavior observed among the elderly compared to students is due to stronger social preferences?* We conduct a dictator game comparing not only two age groups, i.e., young students and the elderly, but also test for the possibility of behavioral differences stemming from experimental confounds. We vary the framing of our decision task, stake size, as well as the level of experience in economic experiments. Further details on treatment variations, design parameters and results are provided in Chapter 3.

The second study on behavior in the dictator game is included in Chapter 4 and a previous version may be found in the AWI working paper series: KETTNER, S.E. AND CECCATO, S. (2014) FRAMING MATTERS IN GENDER-PAIRED DICTATOR GAMES, WORKING PAPER SERIES (No. 557), DEPARTMENT OF ECONOMICS, UNIVERSITY OF HEIDELBERG, GERMANY. The paper examines the influence of gender-pairing and framing on dictator transfers. Instead of matching participants with an androgynous recipient, information on his/her gender is made salient. The main goal is to identify whether this information on recipient gender accommodates framing effects. The specific experimental design, literature and results are provided in the chapter itself.

Chapters 5 and 6 move the focus to behavior in a real world public goods environment. First, we provide a general introduction including the basic public goods game literature. As the research is on a specific real world public good, namely climate change mitigation, we summarize the decision framework of GOESCHL, T., KETTNER, S.E., LOHSE, J., AND SCHWIEREN, C. (2014A) WHAT DO WE LEARN FROM PUBLIC GOOD GAMES ABOUT CLIMATE CHANGE MITIGATION? EVIDENCE FROM AN ARTEFACTUAL FIELD EXPERIMENT, MIMEO, UNIVERSITY OF HEIDELBERG, GERMANY briefly. The paper itself is not included as a chapter in this thesis, nevertheless an overview on the design and results may be insightful as the study serves as the baseline for the investigation presented in Chapter 6. In this light, we outline the experimental environment of the study on contributions towards the real world public good of climate change mitigation. Furthermore, we comment on the external validity of abstract laboratory public goods games for this real world context. The aspects of study population differences and demographic attributes influencing behavior are mentioned as well.

Chapter 6 addresses an experimental manipulation of the climate change contribution task. The work is co-authored by Timo Goeschl, Johannes Lohse and Christiane Schwieren and the citation reads as GOESCHL, T., KETTNER, S.E., LOHSE, J., AND SCHWIEREN, C. (2014B) CHANGING NORMS BY SOCIAL INFORMATION: EXPERIMENTAL EVIDENCE ON CONTRIBUTION MOTIVES, MIMEO, UNIVERSITY OF HEIDELBERG, GERMANY. In the experiment, we introduce social information and observe differences in contribution behavior. As a next step, we investigate the causal mechanism underlying the influence of social information on giving. Therefore, we aim to answer the following research question: *Does social information affect descriptive social norms (beliefs) and, through norms, contribution behavior?* The specific motivation of the study, design parameters, analysis tools as well as results are provided in the chapter.

Finally, Chapter 7 briefly summarizes findings from the different papers and sets them into relation. Furthermore, it includes an outlook on potential extensions of our work.

Chapter 2

Dictator Games

The dictator game is a simple, non-strategic game first implemented by Forsythe et al. (1994). In fact, it may be considered a variant of Kahneman et al. (1986) applying a binary and hypothetical dictator framework. It is played by two players where one is called the dictator (proposer) who may decide how to share an amount of money between himself and a second passive player, the recipient. Average transfers in dictator games are roughly 30% depending on the experimental setting (Engel, 2011). As emphasized in the previous chapter there exist several behavioral models such as altruism, fairness, or inequity preferences, explaining transfers in this game.

Due to its simplicity the dictator game may be used to test for confounding factors, context effects, and individual attribute differences. Engel (2011) provides an extensive review of factors that matter for dictator transfers. Examples stemming from design variations are framing and range effects (Zhang and Ortmann, 2014), property rights (Oxoby and Spraggon, 2008), social distance (Hoffman et al., 1996) and communication (Bohnet and Frey, 1999). Examples for behavioral differences among individuals with different attributes are gender (Bolton and Katok, 1995; Eckel and Grossman, 1998), economic training (Marwell and Ames, 1981), personality (Ben-Ner et al., 2004) and others.

The dictator game is the research vehicle used in Chapter 3 on *Old Age and Prosocial Behavior* co-authored by Israel Waichman. As students –the usual experimental subjects– are only a small share of the population, it is important to direct attention of

research towards aged individuals. The factor of age is important from a political perspective as society in industrial countries is aging. For example, the Federal Statistical Office (2011a) presents data predicting the share of elderly individuals in Germany to increase over the next decades. Similarly, the elderly have large economic power (Federal Statistical Office, 2011b). Our research is motivated by the possibility that social preferences change over the life-time and prosocial behavior among the elderly, in our study individuals above the age of 60, is more pronounced compared to a younger reference group.

Many empirical studies observe the elderly to donate more towards charities as well as to transfer more in dictator games (Engel, 2011). A more extensive summary of the literature investigating behavior of the elderly is provided in the actual chapter, but all studies have in common that one could draw the conclusion that the elderly are in fact more prosocial. We investigate this hypothesis rigorously in the laboratory. The chapter cannot comment on the change in behavior over the life time as it is not possible to observe our participants in the past, but it will enlighten the assertion that the elderly hold stronger social preferences.[1]

In our experiment we compare behavior among two age groups, i.e., young students and the elderly (60+). We measure whether prosocial behavior varies over these subject pools and test for experimental confounds. We conduct a double-anonymous dictator game (Hoffman et al., 1996) and test whether a type of purely cognitive experimental demand effect (Zizzo, 2010) matters by varying the decision framing. Due to wealth differences among the study populations, we also manipulate the experimental stakes and test for differences among the elderly. Furthermore, we distinguish between two sets of student participants, namely inexperienced and experienced ones, to compare behavior in the light of potential confusion or suspicion of subjects.

We initially confirm the behavioral differences between the elderly and the standard student sample. In this light, the elderly behave significantly more generous towards their peers. This result is robust to design variations, i.e., treatments accounting for framing or wealth differences. However, comparing the elderly and inexperienced student

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[1]For theories on behavioral changes in prosocial behavior over the course of life, we refer to Van Lange et al. (1997) and Steverink et al. (1998). They present hypotheses on prosocial-growth and directing attention to social goals. In this light, they emphasize that changes in prosocial behavior could be grounded in experiencing benefits of cooperation, a decrease in competitiveness as an individual goal, or changes in society.

participants does not reveal a difference in observed prosocial behavior. Hence, we cannot rule out that the seemingly stronger social preferences of the elderly are due to confounds (such as confusion and suspicion) associated with the lack of experience in economic experiments.

Chapter 4, *Framing Matters in Gender-Paired Dictator Games*, addresses framing differences in a similar experimental design. In contrast to Chapter 3 on age, the focus of this study lies on gender differences and gender-interaction. Differences between men and women are frequently observed in many domains of interaction. For example helping behavior is differently pronounced among the sexes (Eagly and Crowley, 1986). Similarly, the literature identifies gender differences in prosocial behavior (for a review see Croson and Gneezy (2009)). Not only gender itself but also gender-interaction shapes decisions in everyday life. These interactions are the main course of investigation in our study. A real world example is provided by Miller (2000). He shows that tipping the service in a restaurant depends on the gender of customer and service personal. Males on dates or groups of males tip larger amounts to waitresses while groups of females are less generous. Literature from psychology provides further examples on gender interaction where mating behavior or competitiveness influence preferences in social settings. Farrelly et al. (2007) argue that prosocial behavior could serve as a signal of attractiveness towards the opposite sex.

Most experiments reporting gender differences in prosocial behavior are played with an androgynous match. In these experiments it is unclear what beliefs individuals hold on interaction partners' gender. But if the gender is in fact an important factor, beliefs or gender composition should be introduced as a control (Holm, 2000). Towards this background we explore whether gender-interaction is influenced by social cues. Croson and Gneezy (2009) provide examples where context or frames matter differently for the sexes resulting in behavioral differences. Similar to the first dictator game experiment within this thesis, we vary the decision frame in our study. While framing differences do not appear when interaction includes an androgynous recipient (Dreber et al., 2013), it might be possible that under salient information on the recipients' gender, discrimination or mating behavior is emphasized and framing differences appear.

We utilize a double-anonymous dictator game (Hoffman et al., 1996) and test in varying treatments whether gender-pairing matters for prosocial behavior. Therefore, dictators

are informed about the recipient gender before taking their transfer decision. Additionally, we expand existing literature by investigating whether gender-paired behavior is prone to framing effects.

In anticipation of the actual results, we find transfers in opposite-sex pairs to be higher than in same-sex pairs. But this difference is only significant in the take framing. Hence, we suggest that either gender composition of the sample or (beliefs about) gender-pairing should be controlled for in experiments testing framing and gender differences in social interaction.

Chapter 3

Old Age and Prosocial Behavior: Social Preferences or Experimental Confounds?*

Abstract

Experimental and field evidence indicate a positive link between social preferences and age with the most striking difference between the elderly and young adults. If age affects social preferences then it should be incorporated into theoretical models. However, it is possible that the seemingly positive link between age and preferences stems from confounds of the experimental procedure. In this study we utilize the dictator game to explore the possibility that the elderly participants have stronger social preferences than students. We initially confirm the elderly participants to transfer more money to their peers than a standard sample of student participants. This result holds even in treatments accounting for wealth differences, purely cognitive experimenter demand effects, and social experimenter demand effects. However, we observe no difference in behavior when comparing the elderly participants and students who have not previously participated in economic experiments. Hence, we cannot rule out that the seemingly stronger social preferences of the elderly are due to confounds associated with the lack of experience in economic experiments (such as confusion and suspicion). In addition, we observe no systematic bias comparing incentivized and hypothetical transfer decisions.

3.1 Introduction

It is nowadays well recognized among economists that individuals do not only care about material resources allocated to them, but also about resources allocated to other individuals (e.g., Fehr and Fischbacher, 2002). Relying on the empirical (especially experimental) evidence, theories of social preferences have been developed and further modified (see a review by Fehr and Schmidt, 2006). However, these theories do not consider the possibility that preferences change over time or vary across age cohorts.

As most experiments are conducted with convenient participant pools of students, one issue being underexplored in the laboratory is the relationship between social preferences and age. This gap in the literature exists despite the empirical and theoretical literature in psychology, indicating a positive link between old age and prosocial behavior. For example, socioemotional selectivity theory (Carstensen and Charles, 1998; Carstensen et al., 1999) indicates old age leading "to motivational shifts that direct attention to emotional goals" (Carstensen and Charles, 1998, p.144). In line with this theory, psychologists find that the elderly participants report greater emphatic concerns than their middle-aged and young counterparts (Seider et al., 2011; Sze et al., 2012). Furthermore, the elderly demonstrate more prosocial behavior by donating more money to charity than any other age group (Midlarsky and Hannah, 1989; Sze et al., 2012).[1] Natural field experiments on charity donations also support the positive relationship between social preferences and age. For example, List (2004) observes the mature cohort (49+) to donate more money to charity than their younger counterparts. Bekkers (2007) finds that the elderly (65+) are more likely to donate, and they also donate higher amounts than their younger counterparts. Additional evidence is provided by donation reports from Canada, the UK, and the US.[2]

Complementary evidence emerges from dictator game (DG) experiments, where a player (hereafter "dictator") is endowed with money and can transfer any portion of it to a second (passive) player (hereafter "recipient"). The meta-analysis by Engel (2011) indicates that the elderly transfer more money than middle-aged participants who themselves

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[1]More precisely, Midlarsky and Hannah (1989) find a positive relationship between age and donations except for people aged 75+ who donate less money than those aged 65+. But when donations correspond with volunteering, i.e., an increase in time and effort instead of money, a linear relationship between age and donation is established even when including people aged 75+.

[2]See Turcotte (2012) (Canada); Charities Aid Foundation, CAF (2012) and Foresters (2014) (UK); Havens et al. (2006) (USA).

transfer more money than students. Moreover, transferring nothing is the mode behavior of students, while it is rarely observed among middle-aged participants and never among the elderly. The study by Roalf et al. (2012) reports that the elderly (mean age: 71.30) do not differ from young participants (mean age: 30.14) in the average amount they transfer, but the former are more likely to split the endowment equally than the latter. Finally, Beadle et al. (2013) study a DG, where dictators receive a note from their recipients in advance containing either a neutral message or an empathy-inducing message. The authors observe no difference in transfers under the neutral message condition between the elderly (mean age: 77.92) and young (mean age: 19.83) cohorts. However, under the empathy-inducing message condition, the elderly transfer significantly more money to the recipient than young participants.

If the apparently more prosocial behavior of the elderly in comparison with their younger counterparts stems from inherent factors correlated with age, then economic theory should be modified accordingly. The current study is designated to learn whether the elderly behave more prosocial than student participants and, in particular, to answer the following research question: *Could it be that more prosocial behavior observed among the elderly compared to students is due to stronger social preferences?*

Our research vehicle is the DG described above: First, the DG involves a simple non-strategic decision. Second, it has a similar (unfamiliar) context for both age cohorts. Using the more natural context of donation to charity may have a moderating effect on the age cohorts, since the elderly donate more money to charity than young people. Third, Engel (2011) finds that the elderly transfer more money to the recipient than their young counterparts in DGs, and hence, the DG yields a promising starting point. A final reason to choose the DG is completeness. Martinsson et al. (2011) conduct a DG study in Austria and Sweden with children between 10 and 15 years, indicating social preferences to be different in childhood and adolescence. Studying differences in social preferences among old adults may be even more economically meaningful, as they have much more influence on economic transactions and activities than children.

To answer the research question, we compare transfers in a DG between a standard sample of student participants and a sample of elderly (60+). Our approach is motivated by Gneezy et al. (2009) in the sense that we study differences in transfers between two populations appearing to be on the two extremes of the spectrum regarding prosocial

behavior (the variable under investigation).[3] We identify five potential explanations for more prosocial behavior (higher transfers) of the elderly in comparison with young students:

1. The elderly have stronger social preferences than young students.

2. The elderly are more prone to "social experimenter demand effect" (social EDE) than students; where social EDE (Zizzo, 2010) corresponds to a bias influencing participants' behavior towards what they perceive as the "correct" social norm. In this respect, the elderly are found to be especially prone to the related "social desirability bias" (Soubelet and Salthouse, 2011).

3. The elderly are more wealthy than students and due to the small experimental stakes in the experiment it might be easier for them to behave generously. In this respect, in an experiment with equal stakes across participant pools, the elderly would exhibit more prosocial behavior.

4. The elderly are more prone to "purely cognitive experimenter demand effect" (purely cognitive EDE) than students; where purely cognitive EDE (Zizzo, 2010) refers to participants being influenced by the wording of the instructions to do the "correct" thing. Zizzo (2010) provides an example where contextual cues of "giving" lead to positive money transfers to peers, whereas instructions allowing for "taking" result in negative transfers.

5. The elderly have not participated in economic experiments before and their behavior may be driven by factors, such as suspicion and confusion, decreasing with experience. As participants gather experience on experiments they may be less suspicions towards the procedure. Since deception is forbidden in economic experiments (Davis and Holt, 1992; Hey, 1991) it is likely that experienced participants trust the instructions (e.g., payment to be determined exactly according to the instructions and confidentiality of payment to be kept). Inexperienced participants, on the other hand, might be still unsure about the procedure. Similarly, confusion might be more pronounced among inexperienced participants as they are not familiar with experimental instructions or the sterile laboratory setting

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[3]Notably, a failure to find differences in transfers between these two age cohorts would not imply that such differences do not exists, since the populations may differ with respect to other factors, and also as the relationship between social behavior and age could be of U-shape.

(even though the DG is an especially easy experiment). In this respect, repetition in a public goods game and in a prisoner's dilemma game seems to diminish prosocial behavior (Ledyard, 1995 and Andreoni and Miller, 1993, respectively).[4] An additional view on the fifth point is that experience might be accompanied by a selection bias. Hence, inexperienced and experienced participants may differ in certain attributes. We address how this possibility may affect the interpretation of the results in the Discussion section.

Following Zizzo (2010), in order to diminish the influence of explanation 2 (social EDE), we are using a double blind procedure. We explicitly test for explanations 3–5 by varying their levels. If we would observe that the elderly behave more prosocially than students even when controlling for explanations 2–5, it would indicate that stronger social preferences of the elderly (explanation 1) is likely to be the source of their more pronounced prosocial behavior.

An additional contribution of this study is the use of the large treatment variation to investigate whether there exists a hypothetical bias in a DG. This hypothetical bias occurs when self-reported measures overstate money transfers to peers compared to situations where participants bear the monetary consequences of their actions. The hypothetical bias was already investigated in the DG by Forsythe et al. (1994), Ben-Ner et al. (2008), and Amir et al. (2012), yielding ambiguous results.[5] Specifically, Forsythe et al. (1994) compare the transfers of dictators endowed with real and hypothetical money, finding transfers to be larger in the hypothetical treatment. Amir et al. (2012) reach a similar result when participants are endowed with either hypothetical money or with a small amount of money. Ben-Ner et al. (2008) conduct an incentivized and a hypothetical DG (albeit providing the dictators with the gender of their recipients) finding no significant hypothetical bias in average behavior of participants. In light of the ambiguous findings, we utilize our large treatment variation (three participant pools, "give" vs. "take" framing, different stake sizes) to further explore the hypothetical bias hypothesis. An interesting novelty of our study is that we conduct the hypothetical DG in a double blind procedure. A primary reason for emergence of a hypothetical bias is

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[4]In contrast to the DG, the public goods game and the prisoner's dilemma game are strategic games, but in these games it is also the dominant strategy to keep all the money.

[5]There are other related studies comparing incentivized and hypothetical decisions in a binary DG (e.g., Dana et al., 2007 and Bühren and Kundt, 2013) or in a donation to charity (e.g., Alpizar et al., 2008b). However, as the action set and context may moderate the effect of hypothetical vs. incentivized treatments, we only focus on the standard (not binary) DG experiments.

social EDE and our double blind procedure is applied to reduce it. We thus hypothesize no difference between the incentivized treatments and their corresponding hypothetical treatments.

Our findings are as follows: We initially confirm that the elderly participants transfer more than the standard sample of (experienced) student participants. This result holds when we increase the stake size for the elderly participants and when we change the framing from "give" to "take." However, we do not observe a difference in transfers when comparing the elderly participants and students who did not participate in economic experiments before. Thus, we cannot exclude that what drives the difference in behavior between the elderly and young students are confounds associated with the lack of experience in economic experiments. In addition, we observe no systematic bias when comparing incentivized and hypothetical transfer decisions.

3.2   Sample, Procedure, and Treatments

We conducted a DG with three distinct participant pools: elderly (60+), "standard" student participants (i.e., who are experienced with economic experiments), and students who did not participate in an economic experiment before. Furthermore, we varied the framing of the DG task, as well as experimental stakes to test for confounding factors. In the following, we present the recruitment procedure, sample properties, and treatments.

3.2.1   Recruitment and sample properties

The elderly participants were recruited via an advertisement in the regional newspaper, flyers, as well as posts in public places. Interested participants contacted a research assistant and were assigned to the experimental sessions. Student participants were recruited via ORSEE (Greiner, 2004) and filtered for experience.

The experiment was conducted at the AWI Lab of the University of Heidelberg. Overall, 22 sessions were run, each lasting 30 minutes. Each session included participants from one sample only (i.e., there was no interaction between samples). In total, we collected

data from 274 participants with 167 participants aged 60+, 57 standard student participants, and 50 inexperienced student participants.[6] Table 3.1 presents the sample properties.

TABLE 3.1: Sample Properties

Demographic Variables	Elderly participants	Inexperienced students	Standard students
Age	68.54 (5.22)	21.78 (3.08)	23.75 (2.83)
Female (%)	49.70%	52.00%	42.11%
Individual Income (Category)	3.52 (1.24)	1.78 (0.62)	1.61 (0.56)
Household Income (Category)	4.13 (1.43)	n.a.	n.a.
Assets (%)	68.86%	n.a.	n.a.
Relationship (%)	62.28%	16.00%	15.79%
Marital status (%)			
Single	7.19%	84.00%	84.21%
Relationship	3.59%	16.00%	15.79%
Married	58.68%		
Divorced	17.96%		
Widowed	12.57%		
Children	1.57 (1.30)	0 (0)	0 (0)
Difficulty	1.50 (0.67)	1.40 (0.64)	1.27 (0.52)
Begin of Studies (Year)	n.a.	2010.3 (0.38)	2010.14 (0.28)
Game Theory (%)		40.00[§]	31.58

Note: These measures were obtained in the post-experimental questionnaire. The entries are means (std. dev.) in the open-ended items and category (multiple-choice) items, and % in the indication items (gender, married, etc.). [§] We did not ask student participants about this attribute in the first inexperienced student sessions. Therefore, we do not have this measure for all observations.

3.2.2 Procedure

The procedure is as follows: Upon arrival, participants receive a €5 show-up fee. Next, the participants draw a number or a letter indicating who will be assigned the rules of dictators and recipients.[7] Dictators and recipients are guided into separate rooms and sit in isolated cubicles according to the drawn number or letter. Instructions are provided

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[6]In fact 310 participants underwent the experimental procedure, but we analyze 274 valid observations, excluding 36 observations. Exclusion criteria include not making a decision or being experienced in economic experiments although participating in the inexperienced participant sessions.

[7]If a participant failed to show up to the experiment and we had an uneven number of participants, we assigned the additional participant to the dictator room and carried out his/her decision according to the experimental instructions. His/her decision was administered to a recipient in the other room by means of an additional lottery, i.e., a volunteer in the recipient room drew a random number to appoint the additional recipient. We had to use such a procedure as refusing to allow an invited elderly individual to participate in the experiment may be uncomfortable for the individual and also adversely impact on the other participants.

and read aloud. In each cubicle participants find two distinct envelopes, one labeled "your personal envelope" and the other labeled "other participant's envelope". In the "give" ("take") framing, 10 coins of ¢50 together with 10 metal washers are positioned in the envelope labeled "your personal envelope" ("other participant's envelope"), while the second envelope labeled "other participant's envelope" ("your personal envelope") is empty.[8] The decision is implemented by participants splitting the coins and washers among the two envelopes. Hence, a participant taking the whole endowment for himself/herself and transferring nothing to the recipient should have 10 coins of ¢50 in the envelope labeled "your personal envelope", and 10 metal washers in the envelope labeled "other participant's envelope". Similarly, a participant dividing the endowment equally between himself/herself and the recipient should have 5 coins of ¢50 and 5 metal washers in each of the two envelopes. After taking the decision, participants are asked to seal the envelopes, place their "personal envelope" in their pockets, and drop the envelope labeled "other participant's envelope" into a collection box. Neither experimenters nor other participants are able to observe how much a dictator transfers to the recipient because each envelope contains 10 coins or washers which are similar in weight, diameter, and the noise they make. When all dictators drop their sealed envelopes into the box, it is carried into the recipient room. In the mean time, the dictators are asked to fill in a short demographic questionnaire. The experimenter in the recipient room, receives the box, opens it and distributes the envelopes to the respective recipients. The matching is implemented by the random number/letter drawn at the beginning of the experiment and is not revealed to participants. The experimenter then moves recipient-by-recipient, opens the envelopes, and records the amount transferred by the dictators.

The procedure is mirrored in the recipient room albeit participants take a hypothetical, i.e., non-incentivized, decision. Similarly, to the incentivized procedure, "hypothetical dictators" find two labeled enveloped in their cubicles ("your personal envelope" and "other participant's envelope").[9] Depending on the framing the envelopes contain a paper slip where participants can indicate their hypothetical transfer. After taking the decisions, the "other participant's envelopes" are carried to the other room and the decisions are revealed to the recipients one-by-one (equivalently to the procedure described above). Participants know that matched participants are informed about their decision, but when taking their decisions neither "real dictators" nor "hypothetical

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[8]In treatments with increased stakes (€20) the 10 coins of ¢50 are replaced by 10 coins of €2.

[9]Notably, another DG study in which recipients take hypothetical decisions is Dana et al. (2007).

dictators" are aware that their counterparts in the other room are taking a similar but hypothetical or incentivized decisions, respectively. Only when the collection boxes arrive to the respective rooms, the experimenters inform participants about (real or hypothetical) decisions in the other room.

3.2.3 Treatments

Our study utilizes the control achieved in laboratory experiments to investigate the conjecture that the elderly hold stronger social preferences than young students. The starting point is to examine whether transfers of the elderly are larger than those of a standard experimental participant pool of students, i.e., of participants who are registered in the recruitment system of the economics department and receive invitations to experiments.

If the answer is yes, our next step is to investigate whether this result holds when testing for the alternative explanations specified in the Introduction. In order to reduce the possible confounds of explanation 2 (social EDE), the experiment is conducted in a double blind procedure.[10] This procedure prevents the experimenters from linking decisions to the respective dictators' identities.

Explanation 3 indicates that more prosocial behavior of the (wealthier) elderly may stem from the small stakes. From the follow-up questionnaire we in fact find average household income of the elderly to be between €2000 and €4000 a month. Furthermore, 68% of them own an apartment or a house, which may imply that the elderly have higher discretionary income than students. Hence, comparing transfers of the elderly and young participants under equal endowment may be conceptually incorrect.[11] To account for the case of considerably larger stakes, we conduct treatments in which the elderly are endowed with a considerably larger amount of €20 (in comparison with the benchmark endowment of €5).[12]

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[10]We essentially use the "Double Blind 2" procedure by Hoffman et al. (1996).

[11]This information is in line with the German average disposable income per month of people age 55+ which is €3500 (Federal Statistical Office, 2011b, 2014). Students declare an average individual income between 0 and €1000, while the elderly declare an average individual income between €1000 and €3000. These self-report items should be taken cautiously, as they may not perceive all aspects of wealth, like financial support by parents or other wealthy family members.

[12]The amount of €20 for half an hour would correspond to almost triple the average hourly gross-wage in Germany.

Explanation 4 suggests that higher transfers of the elderly might be due to a stronger sensitivity to purely cognitive EDE (Zizzo, 2010). To control for this explanation, we conduct treatments in a "take" framing, besides the standard DG in a "give" framing (see Bardsley, 2008, List, 2007, and recently Dreber et al., 2013).

Explanation 5 proposes that since elderly participants are not experienced with economic experiments, confounding factors fading away with experience (e.g., suspicion, confusion) are responsible for their more prosocial behavior compared to experienced students. In fact, data from our follow-up questionnaire indicates that elderly participants rate the instructions as significantly more difficult than the standard student participants ($p = 0.01$, two-sided Mann-Whitney-U test). To this end, we conduct treatments with student participants who did not participate in an experiment before (but the percentage of students who took a game theory course is not different between the standard and inexperienced samples ($p = 0.72$, two-sided Fisher's exact test)). The inexperienced students' rating of instructions' difficulty is not significantly different from the elderly participants ($p = 0.28$, two-sided Mann-Whitney-U test).

To sum up, Table 3.2 presents the design: There are three cohorts (elderly, standard (experienced) students, inexperienced students) each playing the DG under either "give" or "take" frame. In addition to these six treatments, we conducted "give" and "take" treatments with an endowment of €20 with elderly participants.

TABLE 3.2: Experimental Treatments

Participant Pool	Endowment	Frame	Observations (real/hypothetical)
Standard Students	€5	give	14 / 11
Standard Students	€5	take	15 / 17
Inexperienced Students	€5	give	13 / 13
Inexperienced Students	€5	take	13 / 11
Elderly	€5	give	26 / 21
Elderly	€5	take	23 / 27
Elderly	€20	give	18 / 14
Elderly	€20	take	20 / 18

Note: A difference in real and hypothetical observations within several treatments stems either from an uneven number of participants joining a session or exclusion of observations due to incorrectly indicating the transfer decision.

3.3 Results

This section portrays the results. We start with the main findings on whether the elderly have stronger social preferences than younger students. We then present our result on the hypothetical bias. Figure 3.1 displays the average transfer in the incentivized and hypothetical treatments.
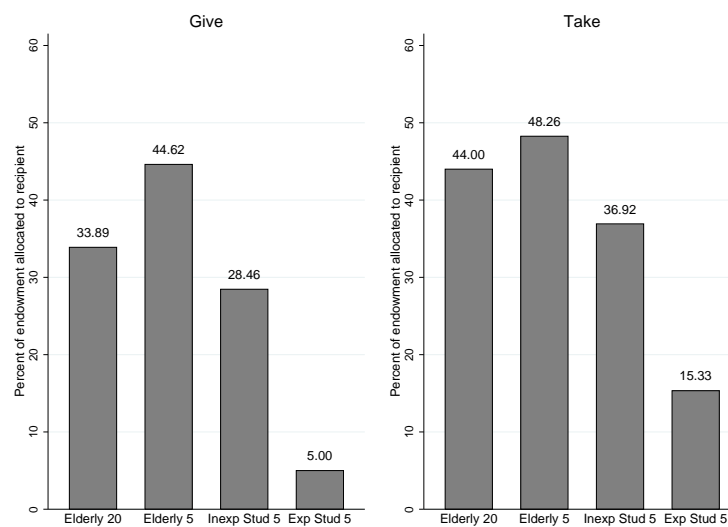
(a) Real Stakes
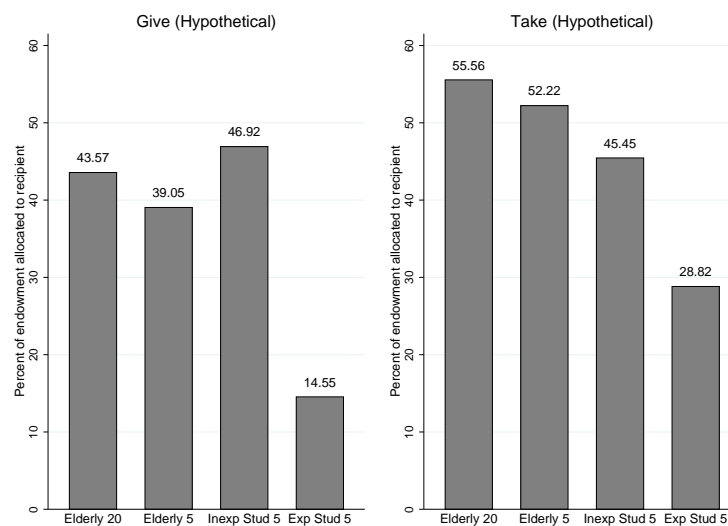
(b) Hypothetical Stakes

FIGURE 3.1: Average Transfers by Treatment

3.3.1 Social preferences or confounds?

We start with the non-parametric analysis where we report the results from Mann-Whitney-U tests (MWU) comparing average transfers across treatments.[13] Additionally, we provide Fisher's exact tests comparing rates of dictators transferring nothing, and half of the initial endowment, respectively. Then we show the results of Tobit model estimations including further demographic controls from the follow-up questionnaire.

Average transfers of the elderly in the (standard) "give" frame with €5 endowment are significantly larger than the transfers by the standard student participants (MWU test, $p \leq 0.01$). In line with this result, the percentage of dictators who contribute nothing (half of the endowment) is significantly smaller (larger) among the elderly compared to the standard student sample (Fisher's exact test $p \leq 0.01$ for both, transferring nothing, and half of the endowment). Hence, the first analysis suggests that the elderly might have stronger social preferences than students.

In order to investigate this preliminary result, we further examine the alternative explanations (3–5) that might confound behavior. If behavior stems from explanation 3 and for the (wealthier) elderly participants the price of giving is lower when endowed with same amount as student participants, we would observe differences in rate of endowment transferred when the elderly are endowed with €20 and with €5. However, the difference is insignificant (MWU test, $p = 0.16$). Moreover, elderly endowed with €20 transfer a significantly larger percentage of the endowment than standard student participants endowed with €5 (MWU test, $p \leq 0.01$).

Next, we inspect explanation 4 on purely cognitive EDE being the source of behavioral differences among the samples. However, transfers in the "take" treatments do not differ from those in the "give" treatments for all three age cohorts (elderly $p = 0.64$, standard students $p = 0.10$, inexperienced students $p = 0.30$, MWU test).[14] This result re-affirms Dreber et al. (2013) who also find that framing does not significantly affect decision behavior of any of the participant pools in a DG.[15]

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[13]In the following, we report the p-values of two-sided tests. We were also using a Kolomogorov-Smirnov test to compare the distributions of the pairwise samples, but as the results are equivalent under both tests, we refrain from reporting them in the manuscript.

[14]In this respect, the elderly transfer more than standard student participants also under the "take" frame (when the elderly are endowed with either €5 or €20) at $p \leq 0.01$ (MWU test).

[15]It is also in line with the meta-analysis results published recently by Zhang and Ortmann (2014) where normalizing transfers (i.e., considering taking €3 as a giving of €2 for a €5 endowment) leads to no significant effect of framing in the meta-regression.

Lastly, we investigate explanation 5 suggesting that more prosocial behavior of the elderly stems from confounds associated with a lack of experience in economic experiment (e.g., suspicion or confusion). To this end, we conduct treatments using student participants who did not who participate in an experiment before, finding them to contribute significantly more than the standard (experienced) student participants (MWU test, $p \leq 0.01$ in both "give" and "take" treatments). Furthermore, we observe only a marginal difference between elderly participants and inexperienced student participants in the €5 "give" treatment (MWU test, $p = 0.07$), which becomes insignificant in the €5 "take" treatment (MWU test, $p = 0.25$). Similarly, there does not exist a difference in transfers between the elderly endowed with €20 and inexperience students endowed with €5 ($p = 0.66$ in the "give" frame and $p = 0.19$ in the "take" frame, MWU test). The results of the MWU tests as to explanations 3–5 are mirrored at the extensive margin by Fisher's exact tests comparing rates of dictators transferring nothing or exactly half the endowment, respectively.[16]

As a further step, we utilize the data collected on demographic attributes of participants to investigate transfer behavior. Table 3.3 presents the results of a Tobit model estimation with robust standard errors.[17] Specification (1) includes the elderly participant sample. The only variable that is found to marginally explain transfer decisions is age (i.e, the older the participant the more he/she transfers). Other variables like gender, reported income, framing ("give" or "take"), stake size, and family variables do not affect the transfer decisions. Specification (2) includes only the (standard and inexperienced) student participant sample. In line with our non-parametric analysis, the only variable explaining transfer decisions is experience in economic experiments. Other variables such as gender, age, reported income, framing, and relationship do not affect the transfer decisions. Finally, specification (3) includes the whole sample of (elderly and student) participants. Consistent with our non-parametric analysis, experience in economic experiments is the only significant variable (negatively) affecting transfers. Hence, we can formulate our result regarding the main research question:

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[16]The results from these tests are provided in the Appendix.

[17]As two elderly participants fail to report their income and one fails to report his/her number of children, our regression contains less observations than the non-parametric analysis (N=142).

Result 1: *The elderly behave more prosocially than standard student participants. However, we find no significant difference in behavior between elderly and inexperienced student participants.*

3.3.2   Is there a hypothetical bias?

In this section we compare results of the incentivized and hypothetical treatments. Our contribution, on top of previous studies, is in applying a double blind procedure for the hypothetical decisions within the large treatment variation (varying the participant pool, framing, and stake sizes).

Figures 3.1 (a) and (b) provide a visual comparison of the incentivized and hypothetical treatments. MWU tests reveal no systematic hypothetical bias, i.e., no difference in average incentivized and hypothetical transfers. The only exceptions can be found in the "elderly-take-€20" and the "inexperienced-students-give-€5" conditions ($p = 0.04$ and $p = 0.05$). Interestingly, in neither of the standard student treatments a hypothetical bias is observed. At the extensive margin this result is further strengthened. The rate of dictators transferring nothing, or half of the endowment, respectively, does not differ between any of the incentivized and their corresponding hypothetical treatments (i.e., Fisher exact tests indicate $p > 0.10$)

Result 2: *There exists no systematic hypothetical bias in prosocial behavior.*

TABLE 3.3: Tobit Model Estimations

	Elderly (1)	Students (2)	Elderly and Students (3)
Elderly Participant			40.11
			(24.93)
Give	2.629	-9.640	-9.626
	(12.16)	(11.86)	(12.11)
Give x Elderly Participant			7.906
			(16.42)
High Stakes	9.490		1.477
	(12.27)		(11.09)
Give x High Stakes	-26.99		-16.82
	(17.00)		(15.49)
Experienced		-30.15***	-33.23***
		(10.74)	(12.31)
Give x Experienced		-12.62	-14.38
		(16.78)	(19.11)
Age	1.375*	-0.457	
	(0.810)	(1.114)	
Female	-4.136	-2.449	-5.312
	(8.288)	(8.135)	(6.080)
Children	-5.320		
	(4.156)		
Relationship	8.845	-5.792	4.069
	(37.28)	(9.500)	(6.667)
Income	-9.198	2.312	1.277
	(9.131)	(7.481)	(7.821)
Income x Relationship	1.554		
	(9.708)		
Income x Elderly Participant			-8.026
			(8.391)
Constant	-13.70	43.69	33.62*
	(72.14)	(29.75)	(18.15)
Sigma			
Constant	35.23****	26.57****	32.97****
	(4.407)	(3.477)	(3.341)
N	84	55	140
Pseudo R^2	0.0187	0.0639	0.0481

Note: Robust standard errors in parentheses;
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$;
Variables: Elderly Participant (0 = Student Participant, 1 = Elderly Participant), Give (0 = Take Frame, 1 = Give Frame), High stakes (0 = €5 Endowment, 1 = €20 Endowment), Experienced (0 = Inexperienced Participant, 1 = Experienced Participant), Age (in years), Female (0 = Male, 1 = Female), Children (number of children), Relationship (0 = single, 1 = in a relationship or married), Income (categorical variable ranging from <€500 to >€5000 per month, for students monthly individual income, for elderly monthly household income).

3.4 Discussion

This study centers on whether the apparent difference in prosocial behavior could stem from stronger social preferences or rather from confounds in the experimental procedure. Our vehicle is the dictator game and prosocial behavior is measured by money transfers to the recipient. Our approach relies on Gneezy et al. (2009) in that we compare behavior of two participant populations (elderly and young students) that are expected to reveal distinct prosocial behavior. In this respect, the design is directed to test whether the result of elderly behaving more prosocially than students stems from confounds of the experimental procedure. Even if we would have rejected this hypothesis, this result would be only suggestive, requiring further investigation as there are possibly important factors differing between the two populations.

We find that elderly participants transfer more money to their peers than the standard student participants who are used to participate in economic experiments. Furthermore, this result holds also when elderly participants are endowed with significantly more money than students, and when the dictator "takes" money from the recipient's envelope, rather than inserts money into the recipient's envelopes. However, transfers are not different between elderly participants and students who have not participated in an economic experiment before (under both give and take framing). Thus, we cannot exclude that the apparent difference in prosocial behavior stems from confounds correlated with experience in economic experiments.

Another possible interpretation of the differences in behavior between standard and inexperienced student participants is that those two participant pools are inherently different due to a strong selection bias (i.e., experienced student participants are a subgroup of the total student participants differing with regard to social preferences to inexperienced students).[18] In this case, the relevant comparison is between inexperienced student and elderly participants treatments which does not support the 'different social preferences' hypothesis.

In sum, our findings imply that the current experimental evidence indicating higher social preferences of elderly should be taken with caution. In particular, this study highlights the need to control for experience in experiments when comparing between

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[18]However, the inexperienced and standard student participants do not differ in attributes like the gender composition, income, beginning of their studies, and relationship status.

participant pools. In this sense, it is plausible that the finding of the meta-study by Engel (2011) where old age significantly affects transfer decisions in a DG stems from experience of students, rather than from a pure "age effect".

Finally, we add to Ben-Ner et al. (2008) in that, despite what seems to be the prevailing view since Forsythe et al. (1994), hypothetical decisions in a dictator game are not statistically different from their corresponding incentivized decisions. Our findings could be due to the double blind procedure which is designated to reduce social EDE, arguably a major source of the hypothetical bias. Hence, such a procedure is especially important in studies employing non-incentivized decisions.

3.5 Appendix

Instructions

In what follows we present the translated instructions of the Give Real (page 29), Take Real (page 31), Give Hypothetical (page 35) and Take Hypothetical (page 38) treatments.

First Page: All Treatments

General Information

Dear participant,

Thank you for participating in this study on decision making. In the following you will be informed about the rules and procedures. Every participant has received the same printed instructions as you did. Please take your time and read the instructions carefully.

No communication with other participants

All decisions in this study are private. Please do not communicate with the other participants. Otherwise, we are forced to exclude you from the experiment and you will have to forgo your payment. If you have any questions, please raise your hand. The experimenter will answer your question quietly.

Anonymous matching

In this study, you will be randomly matched with another participant from the other room. The randomization is carried out according to the number you drew during registration at the beginning of this study. The matching will not be made public and no participant can reconstruct which other participant he is matched to.

This experiment is completely anonymous. Your identity will not be made public and you will not receive information about the identity of the other participants in this room and the other participants in the other room.

In order to simplify readability we forgo the female form. We would like to note that the use of the male form should be understood as gender-independent.

Treatment: Give Real

General information about the decision task

Both you and the matched participant received 5€ for your participation at the beginning. In addition to this, you have another 5€ which is in *your personal envelope*, on the table in front of you. The other matched participant has nothing (0€).

You can now leave the amount you just received unchanged or reduce it, and increase the amount of the participant you have been matched with.

How to make your decision

On the table in front of you, you see two envelopes: one is your personal envelope and the other envelope belongs to the other participant. In order to distinguish between the envelopes, they are marked: your personal envelope is marked "YOUR PERSONAL ENVELOPE"; the envelope of the other participant is marked "ENVELOPE OF OTHER PARTICIPANT".

Content of the two envelopes

Your personal envelope contains a total of 20 coins, out of which ten are 50 cents coins (5€) and ten are worthless coins (metal washers). The washers have the purpose of keeping your decision completely anonymous with respect to other persons including the experimenters.

The envelope of the other participant is empty.

Please make sure, that your personal envelope contains ten 50 cents coins and ten worthless washers by emptying the contents onto the table in front of you.

Receipt 2: This receipt is only for accounting purposes. After you signed the receipt, we ask you to place it in the sealed collection box and continue with your decision. The sealed box is used so that the experimenters cannot see the name written on the receipt. All participants in this room sign the second receipt. The participants in the other room will not sign such a receipt.

The decision

After you have emptied the contents of your personal envelope on the table in front of you and signed the receipt, please put exactly ten coins/washers back in your personal

envelope. Similarly, put exactly ten coins/washers into the envelope of the other participant. In the appendix, we present all possible decisions.

Completing the decision and sealing the envelopes

As soon as you have made your decision, put your personal envelope into your pocket (coat etc.). Please seal the envelope of the other participant (i.e., use the flap-tape to seal the envelope) and place it in the box located behind you, on the floor. *(Important: Please do not hand the envelope to another person or to the experimenter, but place it directly in the collection box.)*

After all participants in this room have made their decision, an experimenter will carry the box to the other room in which a second experimenter will take over the box and distribute the envelopes to the assigned participants. Nobody in the other room is informed about your identity.

Anonymity

We have planned the experiment in a way which guarantees your anonymity at all times.

1. Your identity is never revealed to another person.

2. The experimenter who distributes the envelopes to the participants in the other room was not present at the time you made your personal decision. He or She and the other participants do not know from whom they received the envelope.

3. After the decision we will ask you to fill in an anonymous questionnaire. The questions are used for the evaluation of the study and none of your answers can be linked to your identity.

Thank you very much for your support!

Treatment: Take Real

General information about the decision task

Both you and the matched participant received 5€ for your participation at the beginning. In addition to this, the other participant you are matched with has another 5€ in the envelope labeled *envelope other participant*. It is found on the table in front of you. You have nothing (0€).

You can now leave the amount he just received unchanged or reduce it, and increase your amount.

How to make your decision

On the table in front of you, you see two envelopes: one belongs to the other participant and the other is your personal envelope. In order to distinguish between the envelopes, they are marked: the envelope of the other participant is marked "ENVELOPE OF OTHER PARTICIPANT"; your personal envelope is marked "YOUR PERSONAL ENVELOPE".

Content of the two envelopes

The envelope of the other the participant contains a total of 20 coins, out of which ten are 50 cents coins (5€) and ten are worthless coins (metal washers). The washers have the purpose of keeping your decision completely anonymous with respect to other persons including the experimenters.

Your personal envelope is empty.

Please make sure, that the envelope of the other participant contains ten 50 cents coins and ten worthless washers by emptying the contents onto the table in front of you.

Receipt 2: This receipt is only for accounting purposes. After you signed the receipt, we ask you to place it in the sealed collection box and continue with your decision. The sealed box is used so that the experimenters cannot see the name written on the receipt. All participants in this room sign the second receipt. The participants in the other room will not sign such a receipt.

The decision

After you have emptied the contents of the envelope of the other participant on the

table in front of you and signed the receipt, please put exactly ten coins/washers back in the envelope of the other participant. Similarly, put exactly ten coins/washers into your personal envelope. In the appendix, we present all possible decisions.

Completing the decision and sealing the envelopes

As soon as you have made your decision, put your personal envelope into your pocket (coat etc.). Please seal the envelope of the other participant (i.e., use the flap-tape to seal the envelope) and place it in the box located behind you, on the floor. *(Important: Please do not hand the envelope to another person or to the experimenter, but place it directly in the collection box.)*

After all participants in this room have made their decision, an experimenter will carry the box to the other room in which a second experimenter will take over the box and distribute the envelopes to the assigned participants. Nobody in the other room is informed about your identity.

Anonymity

We have planned the experiment in a way which guarantees your anonymity at all times.

1. Your identity is never revealed to another person.

2. The experimenter who distributes the envelopes to the participants in the other room was not present at the time you made your personal decision. He or She and the other participants do not know from whom they received the envelope.

3. After the decision we will ask you to fill in an anonymous questionnaire. The questions are used for the evaluation of the study and none of your answers can be linked to your identity.

Thank you very much for your support!

TABLE 3.4: Last Page of the Instruction: Give Real Treatment

You	Other partici-pant	Return to your personal envelope	Place in envelope of other partici-pant
5€	0€	10 x 50 cents coins and 0 x washers	0 x 50 cents coins and 10 x washers
4.5€	0.5€	9 x 50 cents coins and 1 x washers	1 x 50 cents coins and 9 x washers
4€	1€	8 x 50 cents coins and 2 x washers	2 x 50 cents coins and 8 x washers
3.5€	1.5€	7 x 50 cents coins and 3 x washers	3 x 50 cents coins and 7 x washers
3€	2€	6 x 50 cents coins and 4 x washers	4 x 50 cents coins and 6 x washers
2.5€	2.5€	5 x 50 cents coins and 5 x washers	5 x 50 cents coins and 5 x washers
2€	3€	4 x 50 cents coins and 6 x washers	6 x 50 cents coins and 4 x washers
1.5€	3.5€	3 x 50 cents coins and 7 x washers	7 x 50 cents coins and 3 x washers
1€	4€	2 x 50 cents coins and 8 x washers	8 x 50 cents coins and 2 x washers
0.5€	4.5€	1 x 50 cents coins and 9 x washers	9 x 50 cents coins and 1 x washers
0€	5€	0 x 50 cents coins and 10 x washers	10 x 50 cents coins and 0 x washers

TABLE 3.5: Last Page of the Instruction: Take Real Treatment

Other participant	You	Return to other participant envelope	Place in your envelope
5€	0€	10 x 50 cents coins and 0 x washers	0 x 50 cents coins and 10 x washers
4.5€	0.5€	9 x 50 cents coins and 1 x washers	1 x 50 cents coins and 9 x washers
4€	1€	8 x 50 cents coins and 2 x washers	2 x 50 cents coins and 8 x washers
3.5€	1.5€	7 x 50 cents coins and 3 x washers	3 x 50 cents coins and 7 x washers
3€	2€	6 x 50 cents coins and 4 x washers	4 x 50 cents coins and 6 x washers
2.5€	2.5€	5 x 50 cents coins and 5 x washers	5 x 50 cents coins and 5 x washers
2€	3€	4 x 50 cents coins and 6 x washers	6 x 50 cents coins and 4 x washers
1.5€	3.5€	3 x 50 cents coins and 7 x washers	7 x 50 cents coins and 3 x washers
1€	4€	2 x 50 cents coins and 8 x washers	8 x 50 cents coins and 2 x washers
0.5€	4.5€	1 x 50 cents coins and 9 x washers	9 x 50 cents coins and 1 x washers
0€	5€	0 x 50 cents coins and 10 x washers	10 x 50 cents coins and 0 x washers

Treatment: Give Hypothetical

General information about the decision task

Both you and the matched participant received 5€ for your participation at the beginning. Now imagine that in addition to this you received a second payment of 5€ and it is contained in *your personal envelope* on the table in front of you. The other matched participant has nothing (0€).

You can now leave your second, hypothetical amount of 5€ unchanged or reduce it, and increase the amount of the participant you have been matched with.

How to make your decision

On the table in front of you, you see two envelopes: one is your personal envelope and the other envelope belongs to the other participant. In order to distinguish between the envelopes, they are marked: your personal envelope is marked "YOUR PERSONAL ENVELOPE"; the envelope of the other participant is marked "ENVELOPE OF OTHER PARTICIPANT".

Content of the two envelopes

Your personal envelope contains a paper-slip with 5€ written on it.
The envelope of the other participant contains a paper-slip with 0€ written on it.

The decision

Now, please imagine the 5€ were real and not hypothtical.

If you wish to reduce your payment and increase the one of the other participant, denote the amount (in increments of 50 cent) on the paper-slips; i.e., you denote the amount by which you wish to increase the other participants payment on the paper-slip contained in the other participant's envelope. On your personal paper-slip you change the amount as well.

Example:

You wish to decrease your amount of 5€ by X€ (i.e., either by 0€, 0.50€,... or 5€) by

which you increase the other participants amount by X€(i.e., either 0€, 0.5€,... or 5€): Then you write on the empty paper-slip contained in the other participants envelope: X€ (i.e., you write either 0€, 0.50€, ... or 5€).

Additionally, you write on the paper-slip contained in your personal envelope: the rest that you wish to leave in your envelope (i.e., you either write 5€, 4.50€, ... or 0€). Please make sure that the amounts on both paper-slips sum up to 5€.

Completing the decision and sealing the envelopes

As soon as you have made your decision, put your personal envelope into your pocket (coat etc.). Please seal the envelope of the other participant (i.e., use the flap-tape to seal the envelope) and place it in the box located behind you, on the floor. *(Important: Please do not hand the envelope to another person or to the experimenter, but place it directly in the collection box.)*

After all participants in this room have made their decision, an experimenter will carry the box to the other room in which a second experimenter will take over the box and distribute the envelopes to the assigned participants. Nobody in the other room is informed about your identity.

Anonymity

We have planned the experiment in a way which guarantees your anonymity at all times.

1. Your identity is never revealed to another person.

2. The experimenter who distributes the envelopes to the participants in the other room was not present at the time you made your personal decision. He or She and the other participants do not know from whom they received the envelope.

3. After the decision we will ask you to fill in an anonymous questionnaire. The questions are used for the evaluation of the study and none of your answers can be linked to your identity.

Thank you very much for your support!

Your Personal Envelope
5€
Please denote here which amount you would like to leave in this envelope: ⎯⎯⎯⎯
(i.e., you either write 5€, 4.50€,... 0€)
Other Participant Envelope
0€
Please denote here which amount you would like to leave in this envelope: ⎯⎯⎯⎯
(i.e., you either write 0€, 0.50€,... 5€)

Treatment: Take Hypothetical

General information about the decision task

Both you and the matched participant received 5€ for your participation at the beginning. Now imagine that in addition to this the other participant received a second payment of 5€ and it is contained in the *envelope other participant* on the table in front of you. You have nothing (0€).

You can now leave the hypothetical 5€ amount of the other participant unchanged or reduce it, and increase your amount.

How to make your decision

On the table in front of you, you see two envelopes: one belongs to the other participant and the other is your personal envelope. In order to distinguish between the envelopes, they are marked: the envelope of the other participant is marked "ENVELOPE OF OTHER PARTICIPANT"; your personal envelope is marked "YOUR PERSONAL ENVELOPE".

Content of the two envelopes

The envelope of the other participant contains a paper-slip with 5€ written on it.
Your personal envelope contains a paper-slip with 0€ written on it.

The decision

Now, please imagine the 5€ were real and not hypothetical.

If you wish to reduce the payment of the other participant and increase yours, denote the amount (in increments of 50 cent) on the paper-slips; i.e., you denote the amount by which you wish to increase your payment on the paper-slip contained in your personal envelope. On the other participant's paper-slip you change the amount as well.

Example:

You wish to decrease the amount of the other participant of 5€ by X€ (i.e., either by 0€, 0.50€,... or 5€) by which you increase your personal amount by X€(i.e., either 0€, 0.5€,... or 5€):
Then you write on the empty paper-slip contained in your personal envelope: X€ (i.e.,

you write either 0€, 0.50€, ... or 5€).

Additionally, you write on the paper-slip contained in the other participant's envelope: the rest that you wish to leave in the other participants envelope (i.e., you either write 5€, 4.50€, ... or 0€).

Please make sure that the amounts on both paper-slips sum up to 5€.

Completing the decision and sealing the envelopes

As soon as you have made your decision, put your personal envelope into your pocket (coat etc.). Please seal the envelope of the other participant (i.e., use the flap-tape to seal the envelope) and place it in the box located behind you, on the floor. *(Important: Please do not hand the envelope to another person or to the experimenter, but place it directly in the collection box.)*

After all participants in this room have made their decision, an experimenter will carry the box to the other room in which a second experimenter will take over the box and distribute the envelopes to the assigned participants. Nobody in the other room is informed about your identity.

Anonymity

We have planned the experiment in a way which guarantees your anonymity at all times.

1. Your identity is never revealed to another person.

2. The experimenter who distributes the envelopes to the participants in the other room was not present at the time you made your personal decision. He or She and the other participants do not know from whom they received the envelope.

3. After the decision we will ask you to fill in an anonymous questionnaire. The questions are used for the evaluation of the study and none of your answers can be linked to your identity.

Thank you very much for your support!

Other Participant Envelope

5€

Please denote here which amount you would like to leave in this envelope: _____

(i.e., you either write 5€, 4.50€,... 0€)

Your Personal Envelope

0€

Please denote here which amount you would like to leave in this envelope: _____

(i.e., you either write 0€, 0.50€,... 5€)

Further figures
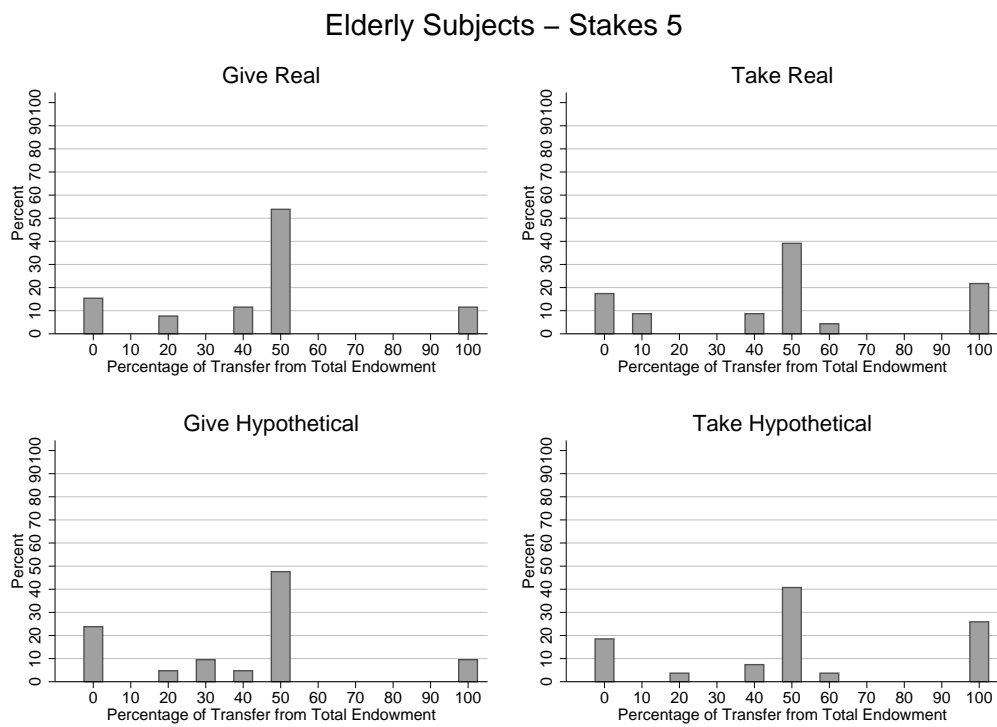
FIGURE 3.2: Histogram Elderly Participants (€5)
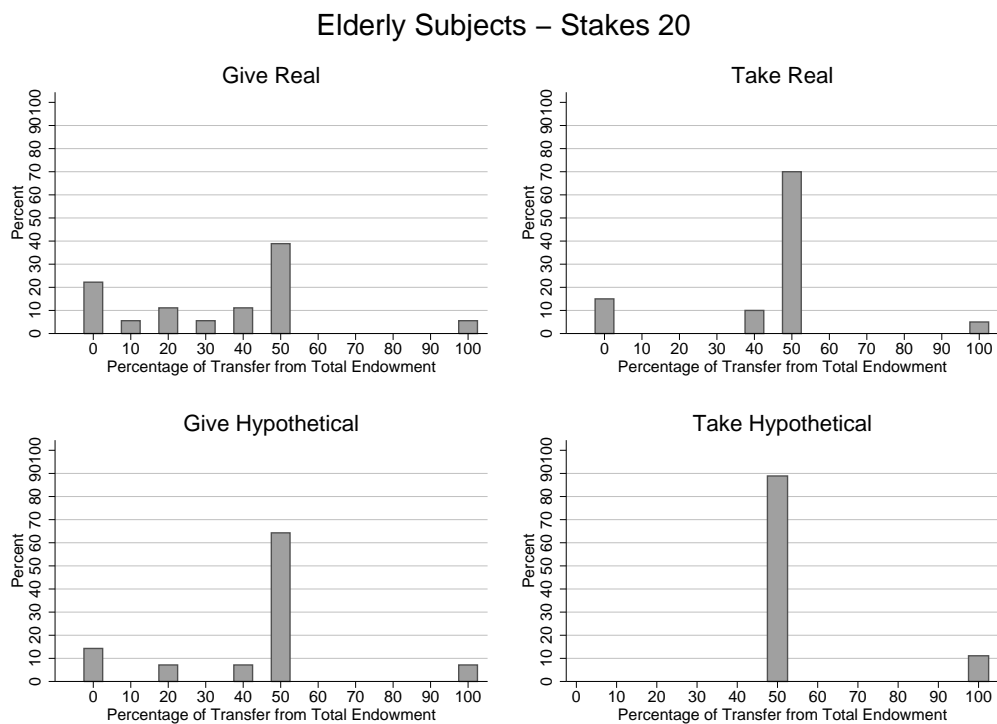
FIGURE 3.3: Histogram Elderly Participants (€20)
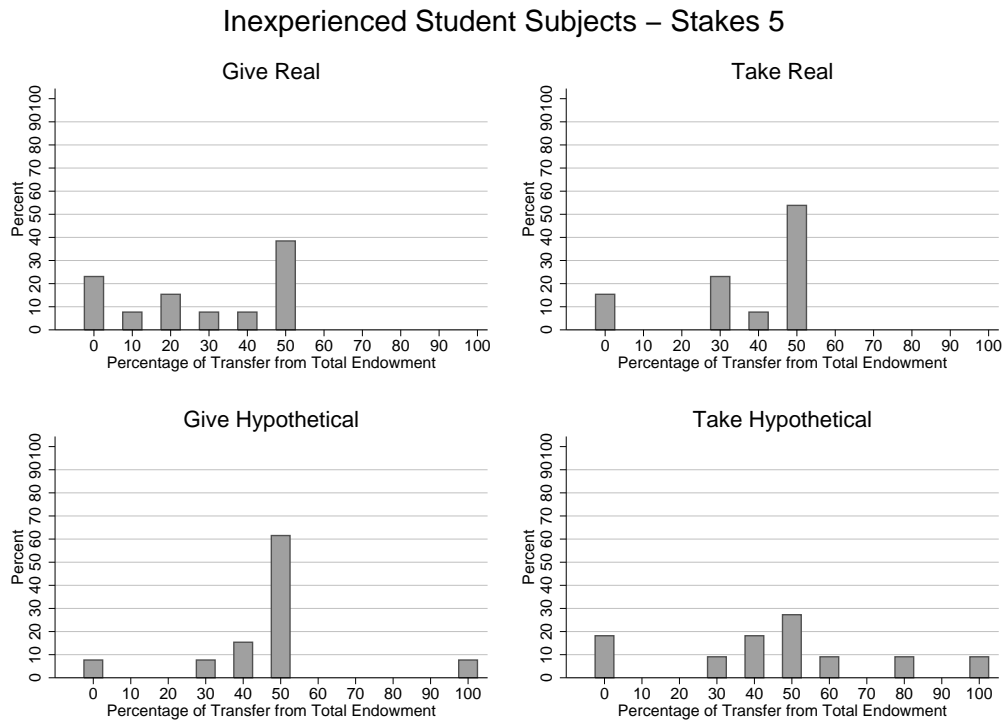
FIGURE 3.4: Histogram Inexperienced Student Participants (€5)
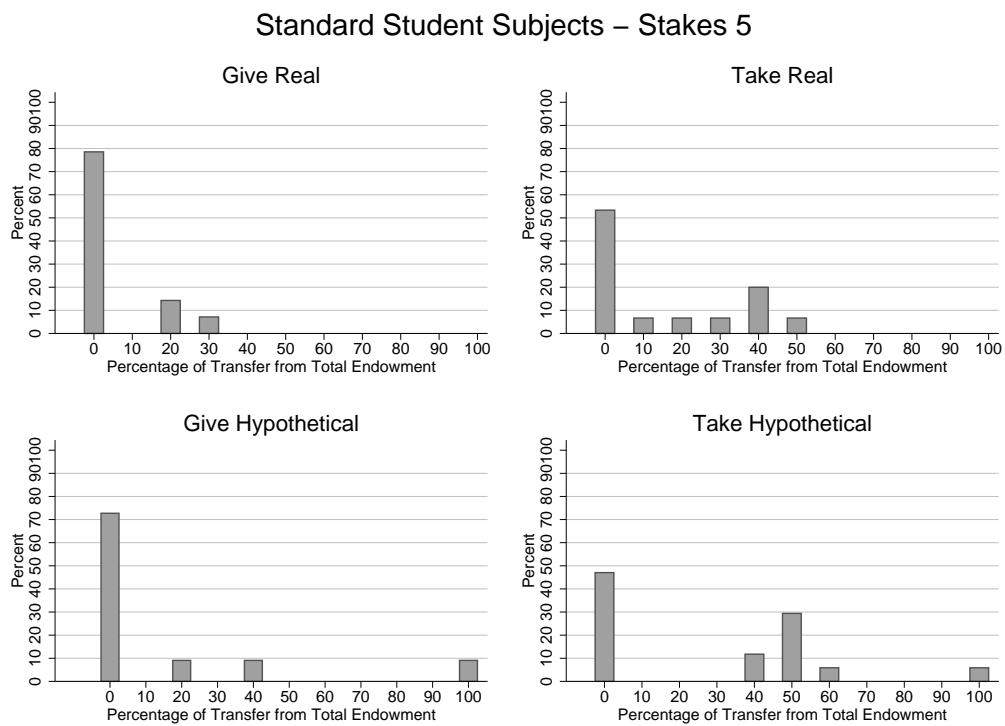
FIGURE 3.5: Histogram Standard Student Participants (€5)

Further results on individual comparisons

TABLE 3.6: Fisher Exact Tests: Extensive Margin and Transferring Half of the Endowment

Compared Treatments		Extensive Margin	Transferring Half
Elderly Give €5	Experienced Give €5	≤ 0.01	≤ 0.01
Elderly Give €5	Elderly Give €20	= 0.70	= 0.37
Elderly Give €20	Experienced Give €5	≤ 0.01	= 0.01
Elderly Give €5	Elderly Take €5	= 1.00	= 0.39
Elderly Give €20	Elderly Take €20	= 0.69	= 0.10
Experienced Give €5	Experienced Take €5	= 0.25	= 1.00
Inexperienced Give €5	Inexperienced Take €5	= 1.00	= 0.70
Elderly Take €5	Experienced Take €5	= 0.03	= 0.06
Elderly Take €20	Experienced Take €5	= 0.03	≤ 0.01
Experienced Give €5	Inexperienced Give €5	≤ 0.01	= 0.02
Experienced Take €5	Inexperienced Take €5	= 0.06	= 0.01
Elderly Give €5	Inexperienced Give €5	= 0.67	= 0.50
Elderly Take €5	Inexperienced Take €5	= 1.00	= 0.49
Elderly Give €20	Inexperienced Give €5	= 1.00	= 1.00
Elderly Take €20	Inexperienced Take €5	= 1.00	= 0.47

Note: *Experienced* refers to Experienced Student Participants, *Inexperienced* refers to Inexperienced Student Participants; p-values are rounded to the second decimal.

Chapter 4

Framing Matters in Gender-Paired Dictator Games[*]

Abstract

We show that social context matters in gender-paired dictator decisions. Our experiment investigates the influence of gender-pairing and framing on monetary transfers in a 2x2x2 design where sender gender, recipient gender, and frame, i.e., give or take, are varied. We are the first to combine all three variables and uncover that giving information about the gender of the recipient accommodates framing effects. If each of the three variables were to be analyzed independently, our data would confirm previous findings where females transfer more than males and framing has no effect (Eckel and Grossman, 1998; Dreber et al., 2013). However, we investigate the variables in interaction and find that framing matters when information about recipient gender is salient. For both genders, transfers in opposite-sex pairs are always higher than in same-sex pairs, but significantly higher in the take frame. We suggest that the gender composition of the sample or (beliefs about) gender-pairing should be controlled for in experiments testing framing and gender differences in social interaction.

4.1 Introduction

The comparison between decision makers' genders has been explored in experimental economics[1], but gender-paired interaction has been rarely considered. Most real-life interactions do not occur with an androgynous match, because individuals have either information, expectations, or beliefs about their counterpart. For instance, counterpart gender has been shown to mediate outcomes in strategic and non-strategic settings where the payoffs depend on the courtesy of others. In restaurants it is frequently observed that males on dates and groups of males are more generous towards waitresses than groups of females (Miller, 2000). Research in evolutionary psychology argues that preferential behavior towards the opposite sex has a signaling role and can be explained by the sexual selection hypothesis (Farrelly et al., 2007). Farrelly et al. (2007) show that interacting partners behave more cooperatively to increase their attractiveness. This courtship behavior is meant to appear to be altruistically motivated particularly since it is costly and non-reciprocal.

Evidence also suggests that sharing is lower within same-sex pairs (Buunk and Massar, 2012). With mate competition for reproduction and survival being the norm, aggression within genders is higher (Trivers, 1972, ch. 7) and this is especially prominent for men (Buunk and Massar, 2012). But also women see other women as competitors when gender is salient (Buss, 1999; Campbell, 1999; Kanazawa, 2005) and they become more competitive in single-sex environments (Gneezy et al., 2003). The study by Holm (2000) similarly identifies discrimination of females in a battle of the sexes game where the better outcome is selected for oneself and the worse outcome is selected for the matched female. Lastly, Houser and Schunk (2009) show that even school-age girls are already sensitive to gender-paired interactions.

Given that gender considerations appear to be embodied in social interaction, we maintain that the gender composition of the experimental sample, gender-pairing, or beliefs about it are an important factor for economic decision making and experimental methodology. Additionally, behavior in social games is often context dependent (Fehr and Schmidt, 1999) and context further interacts with gender (Croson and Gneezy, 2009) which is why we decide to analyze the interplay of social context and gender-pairing.

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[1]For a summary see Croson and Gneezy (2009).

Our study is the first to focus on the interaction of decision maker's gender, his/her counterpart's gender, and framing. The most promising setting for this investigation is a non-strategic, anonymous environment because it elicits prosocial behavior in the absence of most exogenous demands. The remainder of the paper is organized as follows: in Section 4.2 the existing literature is summarized. Section 4.3 presents the experimental design, followed by the hypotheses in Section 4.4. The results and the regression analysis are discussed in Section 4.5 and we conclude in Section 4.6.

4.2 Existing Literature

Individuals exhibit other-regarding preferences (Camerer, 2003, chap. 2) and this is observed even under conditions of double-blindness (Hoffman et al., 1996) and in contexts where reciprocity cannot be a motivational concern (Johannesson and Persson, 2000). Other-regarding preferences are cross-cultural and have been motivated by constructs such as inequality aversion (Fehr and Schmidt, 1999), (impure) altruism (Andreoni, 1989), Rawlsian "social welfare" preferences (Charness and Rabin, 2002), or other non-monetary benefits (Aknin et al., 2013).

In analyzing giving behavior in dictator games, female gender has been positively correlated with the amount transferred (Eckel and Grossman, 1998; Engel, 2011). However, gender effects seem to be context dependent (Andreoni and Vesterlund, 2001; Bolton and Katok, 1995) since females' preferences appear to be more sensitive to social cues (Croson and Gneezy, 2009). One factor influencing social interaction is gender-pairing: offers are affected by information regarding the responder gender. In an ultimatum game by Solnick (2001) amounts proposed to men are larger, especially those by women. Sutter et al. (2009) expand on this finding in a power-to-take game, reporting that in same-sex pairs the amounts proposed are lower than those in opposite-sex pairs. Finally, in a gender-paired dictator game, females transfer less to females than to males (Ben-Ner et al., 2004). However, others find that there is no effect of gender-paired transfers, i.e., women transfer higher amounts regardless of responders' gender (Eckel and Grossman, 2001). We think that these mixed results are due to different design approaches, such as the social distance between the participants and the decision elicitation method. In this paper we propose examining non-strategic gender interaction in a double-anonymous setting.

Another important aspect regarding social games is framing. Bardsley (2008) and List (2007) report that dictator giving might be an artefact of the experimental design while others report that framing matters. Evidence in this direction exists in public goods and bargaining research (Andreoni, 1995; Park, 2000; Leliveld et al., 2008). Yet other authors find no framing effect in dictator games (Dreber et al., 2013), in public goods contexts (Brandts and Schwieren, 2007), or donation games (Grossman and Eckel, 2012).

However, when the decision maker's gender is considered, framing potentially creates differential effects. A recent study by Alevy et al. (2014) demonstrates that men and women react differently to dictator game framing when anonymity is manipulated. In contrast, Dreber et al. (2013) report an overall gender effect regardless of frame with females being more generous. Fujimoto and Park (2010) find that there are no gender differences in contributions under a positive public goods frame, but women are more generous under a negative one.

So far studies only considered combinations of gender and framing in interaction with an androgynous match. Likewise, gender-pairing in the dictator game has only been investigated in the give frame pointing at preferential behavior towards the opposite sex. But it remains to explore whether gender-pairing effects are further influenced by framing transfers. Hence, we investigate the additional influence of recipient gender on framed giving behavior. To our knowledge, we are the first to have employed a combination of the three aspects: gender, gender-pairing, and framing.

4.3 Experimental Design and Implementation

We conducted a double-anonymous dictator game (Hoffman et al., 1996) where gender-pairing and framing are combined. In a 2x2x2 between-subjects design a) the framing of the decision is either give or take, b) the dictator gender is varied, and c) the recipient gender is varied. The basic experimental procedure, except gender-pairing, is a replication of Kettner and Waichman (2014).[2]

The experiment was organized as follows: all participants were invited to the same room, signed up and received a €5 show-up fee. They thus became aware of the gender composition of the sample, and the fact that everybody had received the same show-up

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[2]Instructions are provided in the Appendix.

fee. Next, the way the experiment unfolded depended on the gender composition of the sessions' samples. For same-sex sessions, the participants drew a number or letter indicating the room and seat to which they were randomly assigned (numbers to dictators and letters to recipients). For opposite-sex sessions, participants were directed towards different rooms and there drew the seat number.[3] It is possible that separating participants by gender into different rooms might create additional distance but it was the method we chose to maintain full anonymity towards experimenters and other subjects.

After everyone had been assigned to a room and seated in a cubicle, the experiment began. In both rooms there were always two experimenters present –a male and a female– as experimenter gender might influence decisions (Innocenti and Pazienza, 2006). The first page of the instructions, including explanations on random matching and anonymity, was read aloud. The remainder was read by the participants in private. Dictators found two envelopes in their cubicles, one labeled "Your Personal Envelope" and the other labeled "Other [male/female][4] Participant's Envelope". In the give framing, €5 (ten €0.50 coins) together with ten metal washers were in the "Your Personal Envelope" and the "Other Participant's Envelope" was empty. In the take framing, the €5 (ten €0.50 coins) together with ten metal washers were in the "Other Participant's Envelope" and the "Your Personal Envelope" was empty. The metal washers ensured anonymity, as they mimic €0.50 coins in weight, diameter, and the noise they make. After being asked to count the contents of the envelopes and check that both ten €0.50 pieces and ten metal washers were in the correct envelope, participants made their decision. They were instructed to place ten coins or washers in the "Other Participant's Envelope". In this way they could transfer from €0 to €5 in increments of €0.50. Neither the experimenters nor other participants could observe decisions or connect a particular identity to a decision. Once a dictator had made a decision, he/she placed the sealed envelope labeled "Other Participant's Envelope" in a collection box and started filling in demographic and psychometric questionnaires.

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[3]When participants who had signed up did not arrive on time and the group was uneven in number or in terms of gender, we proceeded as follows: if the missing participant belonged to the recipient group, the participant without a partner was assigned a dictator role and his/her decision was carried out and paired up by means of a lottery. If the missing participant belonged to the dictator group, the participant without a partner could not join the experiment.

[4]Gender is embedded in the inflection in the German language. It was therefore not required to further emphasize the recipient's gender.

TABLE 4.1: Number of Independent Observations and Demographic Details per Treatment

Give Framing				
Treatment	MM	MF	FM	FF
Observations	N=24	N=23	N=26	N=24
Age	23.52 (24)	22.48 (22)	22.27 (22)	23.17 (23)
Individual Income in €	678.26 (750)	691.30 (750)	601.92 (750)	650.00 (750)
Single	73.91%	82.61%	69.23%	50.00%
Household Size	3.61 (3)	2.96 (3)	2.69 (2)	2.54 (2)
Economics Major	45.83%	39.13%	15.38%	41.67%

Take Framing				
Treatment	MM	MF	FM	FF
Observations	N=26	N=25	N=23	N=24
Age	23.54 (22.5)	22.52 (23)	22.59 (22.5)	22.96 (22.5)
Individual Income in €	628.85 (750)	682.00 (750)	622.73 (750)	664.58 (750)
Single	80.77%	72.00%	72.73%	70.83%
Household Size	3.54 (3)	3.48 (3)	2.64 (2)	2.42 (2)
Economics Major	57.69%	44.00%	13.04%	45.83%

Note: tables include mean values (where applicable, median values) and percentage frequencies.

When all dictators had placed their sealed envelopes in the box, it was carried into the recipient room.[5] The experimenters in the recipient room, who were at no point present while the dictators made their decisions, randomly distributed the envelopes to the recipients. The recipients opened the envelopes and counted the contents. The experimenters in the recipient room recorded the amounts transferred while the participants filled in demographic and psychometric questionnaires

The experiment took place at the AWI Lab of the University of Heidelberg in February, April, and June 2013. Data was collected in thirty-three sessions with an average duration of thirty minutes. 376 participants were recruited from the ORSEE-student-pool (Greiner, 2004) after filtering for experience in similar experiments. Five subjects were excluded owing to their advanced age, two were excluded because they had misunderstood the instructions. After exclusion, the sender sample contained a total of 195 independent observations.[6] The mean age of this sample was 22.89 and ranged from 18 to 33 years. 50.14% of the participants were females and 37.95% were majoring in

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[5]The ten coins/washers that were not transferred by the dictator were placed in the personal envelope and taken home. Therefore, dictators could not reveal their decision to the experimenter by returning the metal washers.

[6]The notation used to abbreviate the eight treatments is as follows: framing is either G (Give) or T (Take); gender-pairing is denoted with a combination of sender gender and recipient gender, i.e., M (Male) and/or F (Female).

economics. Further demographic details and the number of observations per treatment are displayed in Table 4.1.

4.4 Hypotheses

In the analysis that follows, we first test the robustness of previously reported results – i.e., in Section (4.5.1) we check whether females transfer and receive more than males, and framing does not matter. In a second step, we uncover the interaction of the three manipulated variables and test the following hypotheses in Section (4.5.2):

Hypothesis 1: Framing does not affect higher transfers from and to females.

Hypothesis 2: Transfers in opposite-sex pairs are different from transfers in same-sex pairs; this holds independent of framing.

4.5 Results

4.5.1 Average amount transferred and frequency of non-zero transfers

The average amount transferred in the pooled decisions is 20.92% of initial endowment. For pairwise comparisons we use Mann-Whitney-U tests and report significance levels. Dictator gender plays a significant role in the distribution of transfers, while framing does not. Female dictators transfer 24.12%, while male dictators transfer 17.76% ($p = 0.06$). Recipient's gender does not influence transfers significantly, as females receive 21.04% and males 20.81% ($p = 0.65$). Finally, the framing of the decision does not play a role ($p = 0.68$). In the give frame 19.69% of the endowment is transferred; in the take frame 22.14% of the endowment is send to the recipient.

For the extensive margin, we analyze the percentage of participants transferring a non-zero amount and compare it between genders and frames through χ^2-tests. 54.87% of the senders choose to transfer some money to the recipient. However, a split by dictator gender underlines that females transfer more frequently than males: 61.86% of the females and 54.08% of the males transfer at least 50 cents ($p = 0.05$). Between recipient genders and frames no difference is found: females receive a non-zero amount

in 58.33% of the cases, while males receive a non-zero amount in 51.52% of the cases ($p = 0.34$). Give and take transfer frequencies are not significantly different either (55.67% versus 54.08%; $p = 0.82$).

In summary, females are more likely to transfer non-zero amounts and, on average, transfer higher amounts than males. In what concerns the recipient, males and females receive similar average amounts and do not differ at the extensive margin. Finally, average transfers and transfer frequencies do not differ between frames. In the following, we put our design to test and analyze the interaction of the three manipulated variables.

4.5.2 Treatment comparisons and interaction effects

Figure 4.1 displays the average transfers in percent for each of the eight experimental treatments. As shown on the left side (give framing), females transfer higher amounts than males regardless of recipient gender (on average 24.60% versus 14.47%; $p = 0.02$). The difference is due to higher transfers to the opposite sex in the case of females and lower transfers towards the same sex in the case of males (26.54% versus 11.67%; $p = 0.05$). This disparity, driven by the sender's gender, disappears in the take frame (right side of Figure 4.1) where females and males transferred similar amounts (23.62% versus 20.78%; $p = 0.59$). The finding stems from opposite-sex "befriending" behavior: females transfer significantly more to males than to females (31.30% versus 16.25%; $p = 0.05$) and males transfer significantly more to females than to males (27.60% versus 14.23%; $p = 0.03$). Non-parametric tests comparing contributions between all experimental treatments are displayed in Table 4.2.

Figure 4.2 presents non-zero transfer frequencies for the eight experimental treatments. In the give frame females are on average more likely to transfer non-zero amounts (66.00% versus 44.68%; $p = 0.04$) and none of the genders discriminates one of the two recipient genders (females give to females 70.83% and females give to males 61.54%; $p = 0.48$; males give to males 41.67% and males give to females 47.83%; $p = 0.67$). In the take frame transfer frequencies analysis reveals the same pattern as the analysis at the intensive margin. On average, females and males are equally likely to transfer non-zero amounts (57.45% versus 50.98%; $p = 0.52$). When we consider gender-pairing we find again that in opposite-sex pairs the frequency of non-zero transfers is significantly higher than in same-sex pairs. This result is robust across dictator genders: females transfer

TABLE 4.2: Treatment Comparison

Gender-Pairing within Give and Take Framing

Give Framing		Take Framing	
MM vs MF	$p = 0.43$	MM vs MF	$p = 0.03$ (**)
MM vs FM	$p = 0.05$ (**)	MM vs FM	$p = 0.02$ (**)
MM vs FF	$p = 0.03$ (**)	MM vs FF	$p = 0.60$
MF vs FM	$p = 0.22$	MF vs FM	$p = 0.66$
MF vs FF	$p = 0.29$	MF vs FF	$p = 0.07$ (*)
FM vs FF	$p = 0.76$	FM vs FF	$p = 0.05$ (**)

Gender-Pairing and Framing Interaction

	TMM	TMF	TFM	TFF
GMM	$p = 0.93$	$p = 0.02$ (**)	$p = 0.01$ (**)	$p = 0.56$
GMF	$p = 0.48$	$p = 0.12$	$p = 0.08$ (*)	$p = 0.83$
GFM	$p = 0.06$ (*)	$p = 0.76$	$p = 0.50$	$p = 0.15$
GFF	$p = 0.07$ (*)	$p = 0.50$	$p = 0.39$	$p = 0.18$

Note: Results from Mann-Whitney U Tests; p-values rounded to the second decimal; stars in parentheses indicate * $p < 0.10$, ** $p < 0.05$

non-zero amounts more frequently to males than to females (69.57% versus 45.83%; $p = 0.10$) and males transfer non-zero amounts more frequently to females than to males (68.00% versus 34.62%; $p = 0.02$).

Result 1a: In the give frame, females are generally more likely to transfer non-zero amounts and, on average, transfer higher amounts than males.

Result 1b: In the take frame, transfer frequencies and average transfers are not different between dictator genders.

Result 2: Framing matters in gender-paired decisions: opposite-sex preferences are stronger when the endowment belongs to the recipient. Average transfers are higher in opposite-sex pairs, but only significantly higher in the take frame.

The interplay of the three binary variables, i.e., framing, dictator gender, and recipient gender, is further scrutinized in a regression analysis.
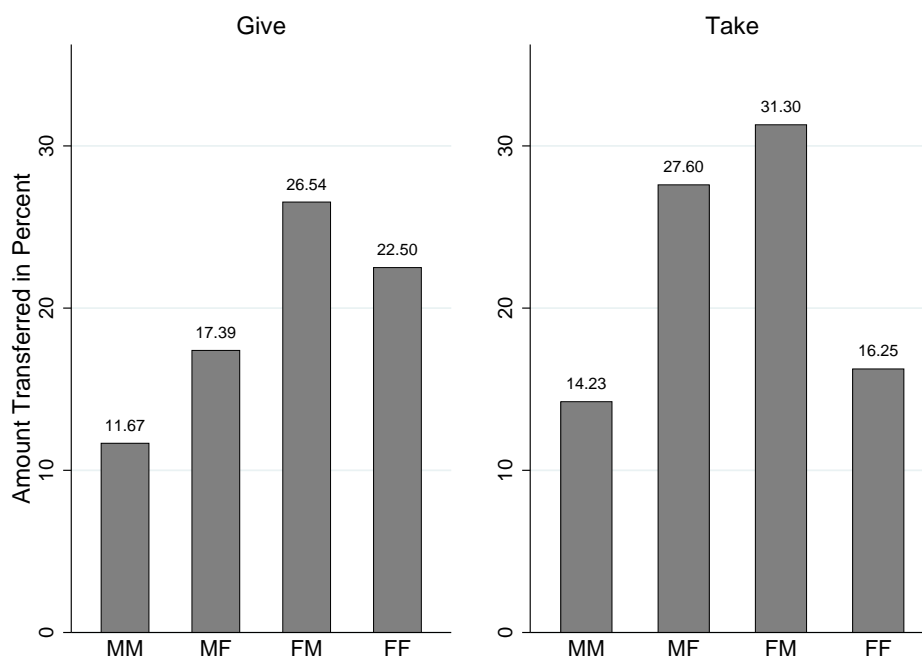
FIGURE 4.1: Average Amount Transferred in Percent by Treatment
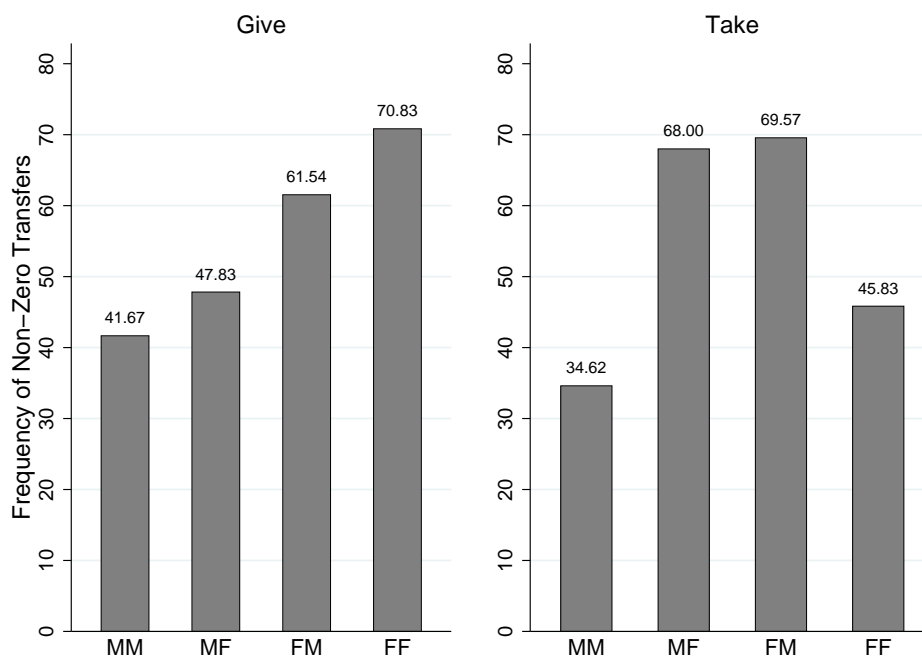
FIGURE 4.2: Non-zero Transfers: Frequencies by Treatment

4.5.3 Regression analysis

In Table 4.3 we specify five regression models to explain the amounts transferred in our experiment and thus confirm the results of the pairwise tests. Since our data is censored, a Tobit estimation with robust standard errors is the most applicable way of testing our models (Engel, 2011).[7]

The first model solely includes the *framing* (0=Take, 1=Give), a dummy for the *sender's gender* (0=Male, 1=Female), and a dummy for the *recipient's gender* (0=Male, 1=Female). Model (2) expands with the interactions of the three dummies, and Model (3) adds the triple interaction. Further, to test the robustness of our results, Model (4) includes the following demographic controls: age in years, income category, relationship status (0=Relationship, 1=Single), and household size. Lastly, Model (5) controls for the additional effect of studying economics as we expect that trained participants will adopt a selfish behavior more often (Marwell and Ames, 1981; Frank et al., 1993).

Our results can be summarized as follows: in the limited Model (1) only the *sender's gender* reveals a positive, significant coefficient. Therefore, controlling for framing and recipient gender, we confirm the previously discussed findings where females transfer significantly more than males. In Model (2) the *sender's gender* maintains a positive, significant coefficient, while the recipient's gender coefficient also becomes positive and significant. This indicates that females receive higher transfers. *Framing*, in line with Dreber et al. (2013), does not affect transfers. However, the interaction of *sender and recipient gender* reveals a significant, negative coefficient. This shows that the recipient's gender negatively affects the positive effect of the sender's gender on the amount transferred. Plainly, the fact that females transfer generally more is reversed when the recipient is another female. This supports the results explained in the previous subsection, where in female-female pairs (and also in male-male pairs) transfers are lower than in mixed pairs. In this model, *framing* interacted with either *sender* or *recipient gender* does not reveal a change in the first difference. When we control for the triple interaction in Model (3), we see a weakly significant indication of how framing affects the interplay of sender and recipient genders: the second difference decreases when framing changes from take to give.

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[7]The presented results remain valid in an OLS-model (the significance of the triple interaction effect is lower).

Model (4) supports these results and further demonstrates that they are independent of age, income, relationship status, and household size.[8] Model (5) additionally shows that holding everything else constant, education in economics influences transfers negatively.

To parallel the extensive margin tests, we additionally apply a logistic model with robust standard errors. Explanatory variables included in Models (6)–(10) are equivalent to Models (1)–(5) and results, i.e., marginal effects, are presented in Table 4.4. We re-confirm the previous findings: in the limited model the *sender's gender* weakly influences transfer probabilities, revealing that females are more likely to transfer non-zero amounts. This persists and is even more pronounced in the later models, where we introduce more controls. In Model (7) the coefficient of the recipient's gender increases, although it remains insignificant. The interaction of *sender* and *recipient gender* is negative and significant, allowing us to restate that the second difference affects the initial *sender's gender* effect. *Framing* and its interactions are insignificant and, at this point, an overall influence of framing can be rejected. Finally, the triple interaction effect in Model (8) reveals the influence of framing on the second difference of gender-pairing. This should be read as follows: higher transfer frequencies exist in mixed pairs, but this effect of gender-pairing is more pronounced in the take framing. Models (9) and (10) strengthen our findings, proving that the effects we claim are mediated by education, but hold independent of age, income, relationship status, and household size.

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[8]We must specify that most of our participants were single and they are thus driving the results. If we re-run the models in the small subsample of individuals in a relationship, the effects we report disappear. This does not mean there are no effects for committed individuals, but only that this subsample is too small to draw any definitive conclusions. Our data suggests that the influence of marital status should be analyzed in a larger sample.

TABLE 4.3: Tobit Regression Results

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Framing (0 = Take, 1 = Give)	-3.085 (5.858)	-11.36 (10.63)	-0.469 (12.59)	1.311 (12.42)	-0.232 (12.60)
Gender Sender (0 = Male, 1 = Female)	12.20** (5.902)	21.63** (10.58)	31.60** (12.71)	34.80*** (12.60)	26.33** (12.66)
Gender Recipient (0 = Male, 1 = Female)	2.485 (5.814)	16.96* (9.864)	26.58** (11.57)	27.60** (11.58)	24.62** (11.67)
Framing x Gender Sender		12.70 (11.41)	-7.355 (17.06)	-10.91 (16.95)	-9.285 (16.67)
Framing x Gender Recipient		3.622 (11.46)	-17.05 (16.69)	-19.23 (16.47)	-19.33 (16.32)
Gender Sender x Gender Recipient		-31.61*** (11.47)	-51.21*** (16.73)	-54.53*** (16.76)	-45.63*** (16.80)
Framing x Gender Sender x Gender Recipient			39.66* (22.94)	44.23* (22.51)	43.49* (22.13)
Age				0.690 (1.038)	0.213 (1.031)
Income Category				0.965 (5.511)	0.839 (5.396)
Relationship Status (0 = Relationship , 1 = Single)				5.011 (6.778)	6.238 (6.761)
Household Size				-0.0946 (1.446)	-0.285 (1.558)
Economics Major (0 = No, 1 = Yes)					-16.65*** (6.119)
Constant	2.804 (6.340)	-1.136 (8.550)	-6.223 (9.671)	-27.63 (28.44)	-6.398 (29.20)
sigma Constant	37.62**** (2.450)	36.46**** (2.399)	36.22**** (2.368)	35.76**** (2.360)	34.88**** (2.356)
N	195	195	195	193	193

Note: robust standard errors in parentheses;
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

TABLE 4.4: Logistic Regression Results

	Model (6)	Model (7)	Model (8)	Model (9)	Model (10)
Framing (0 = Take, 1 = Give)	0.0122	-0.0785	0.0739	0.0981	0.0850
	(0.0723)	(0.127)	(0.144)	(0.144)	(0.153)
Gender Sender (0 = Male, 1 = Female)	0.139*	0.199*	0.347***	0.390***	0.323**
	(0.0711)	(0.119)	(0.134)	(0.134)	(0.146)
Gender Recipient (0 = Male, 1 = Female)	0.0690	0.190	0.331**	0.341**	0.352**
	(0.0720)	(0.117)	(0.131)	(0.133)	(0.141)
Framing x Gender Sender		0.149	-0.162	-0.221	-0.208
		(0.137)	(0.206)	(0.206)	(0.209)
Framing x Gender Recipient		0.0270	-0.277	-0.308	-0.313
		(0.146)	(0.191)	(0.189)	(0.194)
Gender Sender x Gender Recipient		-0.272**	-0.519****	-0.553****	-0.507****
		(0.135)	(0.137)	(0.132)	(0.152)
Framing x Gender Sender x Gender Recipient			0.447****	0.473****	0.476****
			(0.116)	(0.0999)	(0.0995)
Age				0.00223	-0.00280
				(0.0138)	(0.0139)
Income Category				-0.00852	-0.00749
				(0.0709)	(0.0720)
Relationship Status (0 = Relationship , 1 = Single)				0.0862	0.106
				(0.0878)	(0.0920)
Household Size				0.00223	-0.0000612
				(0.0183)	(0.0203)
Economics Major (0 = No, 1 = Yes)					-0.181**
					(0.0819)
N	195	195	195	193	193

Note: marginal effects reported; robust standard errors in parentheses;
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

4.6 Conclusion

We have shown that in a non-strategic game gender-pairing matters and social context influences preferential money transfers towards the opposite sex. Generally, both females and males transfer more to the other gender. Yet, common favoritism is statistically significant only when context implies that the endowment, but not the decision, belongs to the recipient. We conclude that framing matters for gender-paired dictator games.

Gender considerations are important in social interactions and social interactions are rarely context-free. Thus, the interaction between gender-pairing and framing should not be ignored. If we overlook this interaction, our data confirms that females transfer more than males and framing does not matter. But, as pointed out before, we discovered that gender-paired dictator game transfers can be context-sensitive. That is, females take significantly more from females than from males and give less to females than to males when the money is theirs. In a similar fashion, males take significantly more from males than from females and give less to males than to females when the endowment belongs to them.

For now we cannot disentangle the motives behind this behavior, but we would like to point at the importance of gender-pairing for social games and its interaction with framing. Additional insights into the motives might be gained in settings where money transfers are publicized, the marital status is revealed, or the allocation of the endowment is also determined by an effort task in both frames.

4.7 Appendix

Instructions

In what follows we present the translated instructions of the Give Male to Female treat-ment. By changing the sender or recipient gender all other give treatments were con-structed. Please note that the translation highlights the gender of both sender and re-cipient. In German this is not required since gender is embedded in the inflection. The original German instructions, except for the gender-pairing details, were used by Kettner and Waichman (2014).

General Information

Dear (male) participant,

Thank you for participating in this study on decision making. In the following you will be informed about the rules and procedures. Every (male) participant has received the same printed instructions as you did. Please take your time and read the instructions carefully.

No communication with other (male) participants

All decisions in this study are private. Please do not communicate with the other (male) participants. Otherwise, we are forced to exclude you from the experiment and you will have to forgo your payment. If you have any questions, please raise your hand. The (male or female) experimenter will answer your question quietly.

Anonymous matching

In this study, you will be randomly matched with another (female) participant from the other room. The randomization is carried out according to the number you drew during registration at the beginning of this study. The matching will not be made public and no (male) participant can reconstruct which other (female) participant he is matched to.

This experiment is completely anonymous. Your identity will not be made public and you will not receive information about the identity of the other (male) participants in this room and the other (female) participants in the

other room.

General information about the decision task

Both you and the matched (female) participant received 5€ for your participation at the beginning. In addition to this, you have another 5€ which is in *your personal envelope*, on the table in front of you. The other matched (female) participant has nothing (0€). **You can now leave the amount you just received unchanged or reduce it, and increase the amount of the (female) participant you have been matched with.**

How to make your decision

On the table in front of you, you see two envelopes: one is your personal envelope and the other envelope belongs to the other (female) participant. In order to distinguish between the envelopes, they are marked: your personal envelope is marked "YOUR PERSONAL ENVELOPE"; the envelope of the other (female) participant is marked "ENVELOPE OF OTHER (FEMALE) PARTICIPANT".

Content of the two envelopes

Your personal envelope contains a total of 20 coins, out of which ten are 50 cents coins (5€) and ten are worthless coins (metal washers). The washers have the purpose of keeping your decision completely anonymous with respect to other persons including the (male and female) experimenters.

The envelope of the other (female) participant is empty.

Please make sure, that your personal envelope contains ten 50 cents coins and ten worthless washers by emptying the contents onto the table in front of you.

Receipt 2: This receipt is only for accounting purposes. After you signed the receipt, we ask you to place it in the sealed collection box and continue with your decision. The sealed box is used so that the (male and female) experimenters cannot see the name written on the receipt. All (male) participants in this room sign the second receipt. The (female) participants in the other room will not sign such a receipt.

The decision

After you have emptied the contents of your personal envelope on the table in front of

you and signed the receipt, please put exactly ten coins/washers back in your personal envelope. Similarly, put exactly ten coins/washers into the envelope of the other (female) participant. In the appendix, we present all possible decisions (for the appendix see last page of instructions).

Completing the decision and sealing the envelopes

As soon as you have made your decision, put your personal envelope into your pocket (coat etc.). Please seal the envelope of the other (female) participant (i.e., use the flaptape to seal the envelope) and place it in the box located behind you, on the floor. *(Important: Please do not hand the envelope to another person or to the (male or female) experimenter, but place it directly in the collection box.)*
After all (male) participants in this room have made their decision, a (male or female) experimenter will carry the box to the other room in which a second (male or female) experimenter will take over the box and distribute the envelopes to the assigned (female) participants. Nobody in the other room is informed about your identity.

Anonymity

We have planned the experiment in a way which guarantees your anonymity at all times.

1. Your identity is never revealed to another person.

2. The (male or female) experimenter who distributes the envelopes to the (female) participants in the other room was not present at the time you made your personal decision. He or She and the other (female) participants do not know from whom they received the envelope.

3. After the decision we will ask you to fill in an anonymous questionnaire. The questions are used for the evaluation of the study and none of your answers can be linked to your identity.

Thank you very much for your support!

[Translated Instructions of the Take Female from Female treatment. By changing the sender or recipient gender all other take treatments were constructed.]

General Information

Dear (female) participant,

Thank you for participating in this study on decision making. In the following you will be informed about the rules and procedures. Every (female) participant has received the same printed instructions as you did. Please take your time and read the instructions carefully.

No communication with other (female) participants

All decisions in this study are private. Please do not communicate with the other (female) participants. Otherwise, we are forced to exclude you from the experiment and you will have to forgo your payment. If you have any questions, please raise your hand. The (male or female) experimenter will answer your question quietly.

Anonymous matching

In this study, you will be randomly matched with another (female) participant from the other room. The randomization is carried out according to the number you drew during registration at the beginning of this study. The matching will not be made public and no (female) participant can reconstruct which other (female) participant she is matched to.

This experiment is completely anonymous. Your identity will not be made public and you will not receive information about the identity of the other (female) participants in this room and the other (female) participants in the other room.

General information about the decision task

Both you and the matched (female) participant received 5€ for your participation at the beginning. In addition to this, the other (female) participant you are matched with has another 5€ in the envelope labeled *envelope other (female) participant*. It is found on the table in front of you. You have nothing (0€).

You can now leave the amount she just received unchanged or reduce it, and

increase the your amount.

How to make your decision

On the table in front of you, you see two envelopes: one belongs to the other (female) participant and the other is your personal envelope. In order to distinguish between the envelopes, they are marked: the envelope of the other (female) participant is marked "ENVELOPE OF OTHER (FEMALE) PARTICIPANT"; your personal envelope is marked "YOUR PERSONAL ENVELOPE".

Content of the two envelopes

The envelope of the other (female) participant contains a total of 20 coins, out of which ten are 50 cents coins (5€) and ten are worthless coins (metal washers). The washers have the purpose of keeping your decision completely anonymous with respect to other persons including the (male and female) experimenters.
Your personal envelope is empty.
Please make sure, that the envelope of the other (female) participant contains ten 50 cents coins and 10 worthless washers by emptying the contents onto the table in front of you.

Receipt 2: This receipt is only for accounting purposes. After you signed the receipt, we ask you to place it in the sealed collection box and continue with your decision. The sealed box is used so that the (male and female) experimenters cannot see the name written on the receipt. All (female) participants in this room sign the second receipt. The (female) participants in the other room will not sign such a receipt.

The decision

After you have emptied the contents of the envelope of the other (female) participant on the table in front of you and signed the receipt, please put exactly ten coins/washers back in the envelope of the other (female) participant. Similarly, put exactly ten coins/washers into your personal envelope. In the appendix, we present all possible decisions (for the appendix see last page of instructions).

Completing the decision and sealing the envelopes

As soon as you have made your decision, put your personal envelope into your pocket

(coat etc.). Please seal the envelope of the other (female) participant (i.e., use the flap-tape to seal the envelope) and place it in the box located behind you, on the floor. *(Important: Please do not hand the envelope to another person or to the (male or female) experimenter, but place it directly in the collection box.)*

After all (female) participants in this room have made their decision, a (male or female) experimenter will carry the box to the other room in which a second (male or female) experimenter will take over the box and distribute the envelopes to the assigned (female) participants. Nobody in the other room is informed about your identity.

Anonymity

We have planned the experiment in a way which guarantees your anonymity at all times.

1. Your identity is never revealed to another person.

2. The (male or female) experimenter who distributes the envelopes to the (female) participants in the other room was not present at the time you made your personal decision. He or She and the other (female) participants do not know from whom they received the envelope.

3. After the decision we will ask you to fill in an anonymous questionnaire. The questions are used for the evaluation of the study and none of your answers can be linked to your identity.

Thank you very much for your support!

TABLE 4.5: Last Page of the Instruction: Give Male to Female

You	Other (female) participant	Return to your personal envelope	Place in envelope of other (female) participant
5€	0€	10 x 50 cents coins and 0 x washers	0 x 50 cents coins and 10 x washers
4.5€	0.5€	9 x 50 cents coins and 1 x washers	1 x 50 cents coins and 9 x washers
4€	1€	8 x 50 cents coins and 2 x washers	2 x 50 cents coins and 8 x washers
3.5€	1.5€	7 x 50 cents coins and 3 x washers	3 x 50 cents coins and 7 x washers
3€	2€	6 x 50 cents coins and 4 x washers	4 x 50 cents coins and 6 x washers
2.5€	2.5€	5 x 50 cents coins and 5 x washers	5 x 50 cents coins and 5 x washers
2€	3€	4 x 50 cents coins and 6 x washers	6 x 50 cents coins and 4 x washers
1.5€	3.5€	3 x 50 cents coins and 7 x washers	7 x 50 cents coins and 3 x washers
1€	4€	2 x 50 cents coins and 8 x washers	8 x 50 cents coins and 2 x washers
0.5€	4.5€	1 x 50 cents coins and 9 x washers	9 x 50 cents coins and 1 x washers
0€	5€	0 x 50 cents coins and 10 x washers	10 x 50 cents coins and 0 x washers

TABLE 4.6: Last Page of the Instruction: Take Female from Female

Other (female) participant	You	Return to other (female) participant envelope	Place in your envelope
5€	0€	10 x 50 cents coins and 0 x washers	0 x 50 cents coins and 10 x washers
4.5€	0.5€	9 x 50 cents coins and 1 x washers	1 x 50 cents coins and 9 x washers
4€	1€	8 x 50 cents coins and 2 x washers	2 x 50 cents coins and 8 x washers
3.5€	1.5€	7 x 50 cents coins and 3 x washers	3 x 50 cents coins and 7 x washers
3€	2€	6 x 50 cents coins and 4 x washers	4 x 50 cents coins and 6 x washers
2.5€	2.5€	5 x 50 cents coins and 5 x washers	5 x 50 cents coins and 5 x washers
2€	3€	4 x 50 cents coins and 6 x washers	6 x 50 cents coins and 4 x washers
1.5€	3.5€	3 x 50 cents coins and 7 x washers	7 x 50 cents coins and 3 x washers
1€	4€	2 x 50 cents coins and 8 x washers	8 x 50 cents coins and 2 x washers
0.5€	4.5€	1 x 50 cents coins and 9 x washers	9 x 50 cents coins and 1 x washers
0€	5€	0 x 50 cents coins and 10 x washers	10 x 50 cents coins and 0 x washers

Chapter 5

Public Goods Games

Public goods are by definition non-excludable and non-rivalrous goods potentially leading to an inefficient provision (Samuelson, 1954). Standard theoretical models propose underprovision to stem from free-riding being the dominant strategy in public good interaction. Nevertheless, the voluntary provision of public goods has been observed frequently. Examples include local or global as well as inter- or intragenerational public goods such as animal disease control, weather forecasts, removal of toxic waste, climate change mitigation or knowledge (Kaul et al., 1999; Sandler, 1999).

The public goods dilemma has been tested experimentally in the laboratory as well as in the field. In the laboratory participants are usually put in an abstract environment[1] and interact in a public goods group task. They are endowed with an amount of money and may invest a share of their endowment towards a "group account" (the public good). While the amount not invested in the group account is paid privately, the contribution towards the public good is multiplied by a certain factor, usually < 1, and contributions are added up for all group members. The total sum of contributions is then evenly distributed among the group. Given this structure mimicking the public goods dilemma, individuals have an incentive to free-ride on the contributions of others. Nevertheless, experimentalists frequently observe positive average contributions between 40 and 60% (Ledyard, 1995). There exist several theories explaining contribution behavior. As mentioned in the first chapter, they include forms of altruism, fairness and inequity preferences. Reciprocity and norm adherence are further motives where individuals base

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[1]An early example of a laboratory public goods game is Isaac et al. (1985). For an extensive review on laboratory public goods games see Ledyard (1995).

their giving on others leading to the frequently observed pattern of conditional cooperation (Fischbacher et al., 2001; Gächter, 2007). In Chapter 6 we focus on contributions conditioned on others' behavior.

Many laboratory experiments use the basic public goods framework to test parameters that are relevant for giving behavior. They include varying returns from the group account (Goeree et al., 2002), changes in group size or matching in repeated decisions (Isaac and Walker, 1988b; Andreoni, 1988). Communication (Isaac and Walker, 1988a) as well as punishment options (Fehr and Gächter, 2000) append the list of design parameters altering observed behavior. Furthermore, individual attributes have been shown to matter for giving. These include gender (Brown-Kruse and Hummels, 1993) or study major and economic training (Marwell and Ames, 1981).

Research on public goods is frequently performed in the field as well. Field experiments are conducted in a realistic context and participants may contribute towards real world public goods. Examples are contributions towards local public goods such as trees, parks or radio stations (Laury and Taylor, 2008; Alpizar et al., 2008a; Shang and Croson, 2009), and positive giving is observed. For these experiments factors such as demographic attributes or design variations similarly appear to matter for contribution behavior. In our research, we explore contributions towards a real world public good as well, scrutinizing parameters that influence contribution motives.

We study giving towards the global and intergenerational public good of climate change mitigation. Due to increased greenhouse gas emissions, climate change is likely to cause severe problems. The Intergovernmental Panel on Climate Change (IPCC, 2014) dedicates its work on the investigation of the climate change phenomenon and discusses potential impacts in detail. As cooperation on providing climate change mitigation at an international or state level is sometimes difficult, voluntary contributions by individuals may be necessary. Climate change mitigation has several characteristics that should be kept in mind when examining contributions towards it. First, it is global and investment benefits affect every individual on the planet. Second, the marginal return or benefit from contributing approaches zero. Third, the costs and benefits from investment into mitigation do not coincide and it is considered an intergenerational public good. Due to the inertness of the climate system, contributing today will therefore only result in benefits several decades from now.

Two other experimental studies provide evidence on voluntary contributions towards climate change mitigation. Diederich and Goeschl (2013) and Löschel et al. (2012) study the individual demand for mitigation efforts and observe positive giving. We take the climate change context to the laboratory and conduct an artefactual field experiment (Harrison and List, 2004) to investigate contribution motives. We are able to benefit from the control of the laboratory environment and can test a broad study population in the contribution context.

The experiment by Goeschl et al. (2014a) represents the basic framework and is the starting point of our analysis of contribution behavior towards climate change mitigation. As it is not included in this thesis, we summarize its findings briefly.[2] In Goeschl et al. (2014a) we provide information on participants' behavior and investigate whether free-riding is observed in general. In order to understand behavioral regularities in the contribution context, we further make use of several treatment variations: The within-subject design includes two tasks. The first task is a real contribution task where participants may use their endowment to reduce global CO_2 emissions.[3] The second task is an abstract laboratory public goods game based on Goeree et al. (2002). In ten independent decisions it varies group size, marginal per capita return and payoff symmetry. In addition, the experiment includes two distinct subject pools, namely that of students and non-students recruited from the general population holding a broader set of demographic attributes. As mentioned previously, the analysis of student subjects alone may not be fully representative for the behavior of the population contributing towards climate change mitigation in the real world. In this line, factors such as age, education or wealth –not varying strongly among students– potentially could interact with behavior.

The analysis of Goeschl et al. (2014a) includes four steps to investigate contribution behavior: first, it provides evidence on average giving; second, it analyzes differences in subject pools; third, it compares the two task formats; fourth, it combines findings from task and population differences.

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[2]An interested reader is referred to Goeschl, T., Kettner, S.E., Lohse, J., and Schwieren, C. (2014a). *What do we learn from public good games about climate change mitigation? Evidence from an artefactual field experiment.* Mimeo, University of Heidelberg, Germany.

[3]As this task corresponds to the baseline decision of Chapter 6 the complete instructions may be found in the Appendix of Chapter 6.

Average contributions in both task formats are positive (real task 28%, abstract task 34% (average over ten distinct decisions)). In contrast to free-riding predictions, we observe that individuals are in fact willing to contribute their money towards climate change mitigation. These findings are in line with the two other experiments investigating mitigation contributions, albeit in a different setting (Diederich and Goeschl, 2013; Löschel et al., 2012). The results reveal that students and non-students differ in their behavior. Student subjects are found to be more homogeneous in their choices and their contributions are lower than those of non-students. Hence, they may be classified as being at the lower bound of non-student behavior. When comparing the two task formats for an individual, we find no significant correlation between average contributions. Correlations of real contributions and single-round abstract task contributions depend on the design parameters varied in the abstract task. Larger correlations are observed in decisions with low marginal per capita return, asymmetric payoffs, or large group size. Free-riding behavior in the two tasks is weakly correlated. The consistency between tasks, i.e., correlations between real and abstract decisions, is more strongly pronounced among students.

These findings provide evidence that behavior in the abstract task is only partially informative for contribution behavior towards climate change mitigation. Hence, it might not be possible to apply findings from the extensive abstract public goods game literature using student subjects to our specific setting. The reported results are not only relevant from a methodological point of view but findings in Goeschl et al. (2014a) constitute important contributions for policy makers. First of all, observing positive contributions may be considered good news for politics organizing mitigation programs, and extending research in this field could help to identify contribution motives. Second, behavior of non-students is more generous. As the population contributing towards climate change mitigation in the real world does not consist of students alone, further research should include participants with a broad set of demographic attributes. This research could identify additional factors that influence giving behavior.

A contribution motive for real world public goods and climate change mitigation in specific is norm adherence and conformity where individuals base their own contributions on others' contributions (Bernheim, 1994; Croson et al., 2009). The basic framework from Goeschl et al. (2014a) is an ideal candidate for testing norm adherence by extending its design.

To motivate the following chapter, we provide a brief summary of *Changing Norms by Social Information*. The chapter itself presents results on our investigation of contribution behavior and motives in the climate change mitigation context. As pointed out before, many studies observe conditional cooperation in the public goods context (Gächter, 2007). One explanation of this behavior could be grounded in descriptive social norms and adherence to them. Descriptive social norms are defined as an individual's belief about the contribution behavior of others (Cialdini et al., 1990; Croson et al., 2009). In order to investigate the relationship of descriptive social norms and behavior, we introduce social information and add it to the contribution task of Goeschl et al. (2014a). We expand the literature by testing a potential mechanism through which social information works. Therefore, we test the hypothesis that providing social information updates descriptive social norms and, through norms, behavior. We make use of a study population with a broad set of demographic attributes and test whether there exist demographic influences altering behavior. This could reveal whether certain groups of individuals are more prone to behavioral changes under social information.

In anticipation of the actual results presented in the chapter, we in fact provide evidence in favor of the hypothesized mechanism. Descriptive social norms, measured as subjects' beliefs, are mediating social information and giving. Additionally, our method and analysis allows us to identify why, for certain groups of individuals, social information does not affect contribution behavior. Another novelty of the study is the test of results towards a potential psychological confound, namely anchoring, that might interfere with behavior (Ariely et al., 2003; Cason and Mui, 1998). By this, we are able to exclude anchoring as an explanation of social information effects in our context.

Chapter 6

Changing Norms by Social Information: Experimental Evidence on Contribution Motives[*]

Abstract

Contributions to a public good could be motivated by preferences for norm conformity. Existing experiments have linked individual contributions to the contributions of other participants, treating the underlying mechanism as a given. This paper thus provides first evidence on the mechanism that links social information to contributions via changing the perception of social norms. In an artefactual field experiment participants can contribute towards a real world public good under an exogenous variation of social information. By eliciting information on the participants' beliefs regarding the contribution behavior of others, we observe whether participants provided with social information update their perception of descriptive social norms and, through that change, actual behavior. Reactions to the display of social information could, however, be unrelated to norm conformity and instead be the result of a potential psychological anchoring effect. We test for this alternative by an irrelevant information treatment. In this treatment the provided information does not allow inference on a contribution norm. Our results reveal that beliefs are in fact a mediator between social information and contribution behavior. Anchoring, on the other hand, does not affect contributions and thus, the social content is behaviorally relevant.

6.1 Introduction

The behavioral regularity of positive contributions to public goods, observed both in lab games and in field studies, has been attributed to preferences for unconditional altruism but also to motives of reciprocity or norm conformity expressed as forms of conditional cooperation (for a review see Gächter (2007)). In this light, conditional cooperation assumes a relationship between own and others' giving.

Experimental tests of this relationship make peer contributions salient by providing *social information.* These studies, at large, establish a positive relationship between the provision of social information and contribution behavior: Shang and Croson (2009) conduct an experiment on radio station funding and find that the possibility to observe high contributions of another listener positively impacts individual contribution behavior. In an experiment on student fund donations Frey and Meier (2004) show donor rates to be higher if the students are informed that a large share of their fellow students has made a donation in the past. Charness and Cheung (2013) investigate charitable giving in a restaurant and provide evidence of social information influencing giving. Alpizar et al. (2008a) investigate contributions towards a national park in Costa Rica and observe higher average giving if subjects are confronted with others' high donations.

These studies provide strong evidence for a causal relationship between social information and giving behavior. A plausible explanation for this relationship could be based on the theory of conformity (Bernheim, 1994). According to this theory, deviating from a norm results in disutility to the transgressor. Yet, by construction, the existing studies are no test of this explanation, as they leave the process of norm formation unexplored. In other words, these studies establish that social information works but remain silent about the mechanism determining behavioral changes. If conformity is in fact a valid explanation, the presentation of social information would need to lead to an updating of the perceived descriptive norm (Croson et al., 2009; Cialdini et al., 1990). Figure 6.1 displays the schematic view of this potential causal mechanism where norms mediate between reference contributions and actual behavior.

This paper provides a first step into validating this intuitively appealing but as yet empirically underexplored mechanism. Specifically, we ask the following research question: *Does social information affect the perception of a descriptive social norm (beliefs) and,*
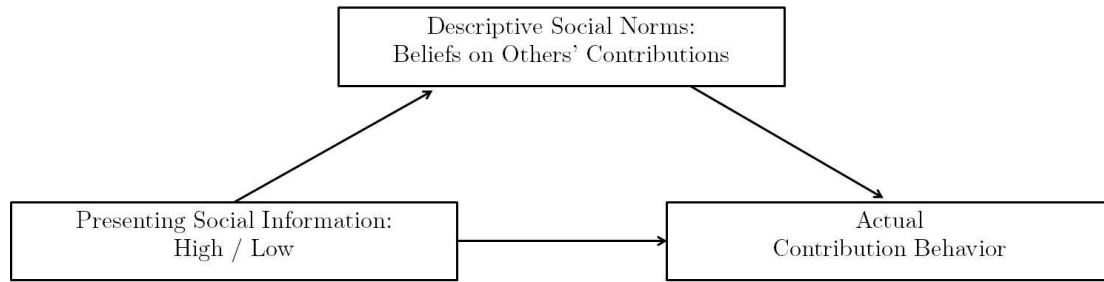
FIGURE 6.1: Proposed Causal Mechanism

through norms, contribution behavior? There exists one other study scrutinizing this mechanism, albeit in a hypothetical context (Croson et al., 2009). The authors provide social information and, by mediation analysis, explore the role of changing beliefs. They detect a relationship and show how social information affects hypothetical giving behavior through updating beliefs. Given this evidence, we conduct a more rigorous experimental investigation including incentivized decisions.

Overall, our experimental design builds on three elements, i.e., the manipulation of social information, the elicitation of beliefs, and a specific public good. Social information, as common in the literature, is given in the form of a high or low contribution average observed for different groups of subjects in the baseline. In contrast, information on participants' beliefs, elicited in an incentivized fashion, is rarely available in the other studies. We use this information as a basis for exploring the underlying mechanism. To make belief elicitation feasible, in contrast to classical field studies of Shang and Croson (2009) or Frey and Meier (2004), we conduct an artefactual field experiment that benefits from the controlled lab conditions. Thirdly, the specific public good employed has the purpose to rule out reciprocity as an alternative channel driving conditional cooperation. Therefore, it is not possible to conduct a standard (repeated) public goods game in which reciprocity and norm adherence cannot be separated as contribution motives (Fischbacher et al., 2001). Instead, as in the existing field studies, we give subjects the option to contribute to a real world public good. In the case of local public goods, such as radio stations or parks (Shang and Croson, 2009; Alpizar et al., 2008a), reciprocity might not be irrelevant, either. Hence, in our study participants contribute to a global and intergenerational public good, namely climate change mitigation. Due to its attributes, reciprocity is an unlikely motivation as the delay of mitigation benefits leads to a temporal separation of contributors and potential beneficiaries.

Based on these design elements we use two different empirical tests for the proposed mechanism. As in Croson et al. (2009) we apply a mediation analysis. This analysis is supplemented by a novel approach (Costa-Gomes et al., 2010; Smith, 2013) that uses random treatment variations as instruments by design. Thereby, we address a potential endogeneity problem that might be present in the standard regression and mediation approach.

One potential concern could be that behavioral responses to social information do not reflect the social content of the information and are instead the result of a psychological confound, namely anchoring. For instance in the context of dictator games it has been shown how an irrelevant number, made present in short term memory by recalling a birth date, influences giving (Cason and Mui, 1998). Therefore, motives for behavioral changes could be unrelated to social information and anchoring might bias behavior. To validate our results against this concern, we introduce an irrelevant information treatment in which the reference to a contribution norm is removed.

Our results can be summarized as follows: We replicate the positive link between social information and contributions. The proposed (causal) mechanism underlying this link via beliefs qualifies as an explanation for our results. Specifically, we demonstrate that an exogenous variation of social information influences beliefs on others' contributions (descriptive social norm) and, through this channel, actual contribution behavior. In addition, we are able to show that the causal relationship of social information, beliefs, and contributions is differently pronounced among the two sexes and confirm the robustness of our results in the light of the demographic heterogeneity of subjects. Lastly, we complement the existing literature on social information by demonstrating that irrelevant information and anchoring do not matter and do not qualify for explaining changes in behavior. The remainder is organized as follows: Section 6.2 provides the experimental design, followed by the hypotheses in Section 6.3. In Section 6.4 we present the results and conclude in Section 6.5.

6.2 Experimental Design and Implementation

The experiment took place between November 2011 and July 2012 at the AWI Experimental Lab of the University of Heidelberg and was implemented in z-Tree (Fischbacher,

2007).[1] Overall, 23 sessions with up to 15 subjects were conducted. Before subjects started the session, they were seated randomly at one of the available computer terminals. A printed version of the general instructions was handed out at the beginning of every session and read out loud. Communication between participants was not allowed and questions addressed to the experimenter were answered quietly. All sessions were conducted under full anonymity. Subjects were not able to observe decisions made by other subjects. Communication before the experiment was avoided as well by installing a separate check-in room and different arrival times. Therefore, contact of subjects was minimized and common waiting times were reduced. In the check-in room subjects also generated a personal code to be used in the experiment. Subjects were informed up-front that their personal code had the purpose of guaranteeing their anonymity. Anonymous payment was also assured through this code: at the end of each session the experimenters provided sealed envelopes with earning receipts which subjects picked up. The payment itself was conducted in a different room by a research assistant who was not present at any time of the experimental sessions. Therefore, subjects were aware that their decisions and their overall earnings were not observable by the experimenter and that their identity was not revealed after decisions. Sessions lasted around 75 minutes and average payment for all tasks was €15.47 ranging from €1.05 to €26.00.[2]

Contribution task (baseline)

At the beginning of the session subjects were informed that they receive €10 as a reward for taking part in the experiment. Subsequently, subjects were given the choice to contribute any share of these €10 towards a common account which was used by the experimenters to offset global CO_2 emissions.[3] Before subjects made their decision, it was assured that each subject had at least the same basic information level on the public good offered. In a neutral and brief text they were informed about greenhouse gas emissions, the functioning of the offset scheme, and the amount of CO_2 that could be reduced by €1. Additionally, it was explained that offsetting emissions has a global impact. The delay between costly reduction and potential benefits occurring in the future was mentioned as well. Subjects were also informed that the total amount contributed

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[1]The procedure as well as a translation of the z-Tree instructions are provided in the Appendix.

[2]Total earnings encompass the earnings from a second task subsequent to the contribution decision that is not analyzed here.

[3]As in Goeschl et al. (2014a), Diederich and Goeschl (2013) and Löschel et al. (2012) the contribution was used to retire CO_2-permits via the EU ETS.

would be made public by an aggregate emission certificate of the German Emission Trading Registry at the end of the last session.

Contribution task with high and low social information

The two social information treatments are equivalent to the baseline in all stages except one: before the actual decision, an additional screen containing information on past subjects' contributions was displayed. In the high social information treatment it was pointed out that the experiment had been conducted before and average contributions of fifty participants were €7 of the initial €10.[4] Similarly, in the low social information treatment the presented average from fifty participants was €1.[5] The translated information text read as follows:

"In previous sessions of the experiment 50 participants gave €7 / €1 on average to retire CO_2-permits"

The social information we employed differs from previous studies in two aspects. While Shang and Croson (2009) refer only to one previous donor, we presented information on contributions of a larger group of individuals. By this we aimed to augment the perception of a common contribution norm. Compared to Alpizar et al. (2008a) who refer to a norm from tourists originating from many different countries, we used a group of people that decided to participate in the same experiment. As discussed by Goldstein et al. (2008) this could enhance the impact of social information because of the closer proximity towards the reference group.

Contribution task with irrelevant information

In order to control for potential cognitive anchoring effects, we introduced the irrelevant information treatment. It included the number from the high social information treatment, i.e., 7, but did not deliver social information. Before taking the actual decision,

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[4]The amount is drawn from a subset of baseline participants taking their decision several months earlier. €7 was the highest possible average from fifty previous observation.

[5]Again the average is based on actual baseline decisions. €1 is the lowest possible integer –below median behavior of previous participants– that can be drawn from the sample.

subjects received information on the place of residence of fifty previous participants, i.e., living within a 7 kilometer radius around the experimental location.[6]

Belief elicitation and follow-up questions

After the contribution decision we elicited subjects beliefs about their peers' behavior. They were asked to estimate the mean contribution of the other participants joining their experimental session and received additional payment for correct estimates.[7] An estimate within the range of - €0.5 and + €0.5 around the true session's average yielded a payment of €2. An absolute deviation of €1.5 from the true average yielded €1. Larger deviations from the true average did not result in additional payments. Feedback on earnings and the session's contribution average was only provided at the very end to avoid biasing other decisions or reported data.

The end of the experiment consisted of several follow-up questions on standard demographics and climate change perception.

Sample

Our experiment includes a broad study population. As previous field experiments in the social information literature used the general public as participants, we also invited a heterogeneous sample to the laboratory. Overall, we recruited N=274 subjects. The baseline contains 144 observations, the high and low reference treatment 47 subjects each, and the irrelevant information treatment contains 36 subjects. Table 6.1 gives an overview on demographic characteristics.

As expected from the random treatment assignment, there exist no difference in most demographic factors across treatments. An exception is income where we find a significant difference between conditions. In the analyses below we control for these differences in a regression framework.[8] Subjects also do not differ with respect to their stated climate

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[6]The information is based on the residences past of baseline participants. In order to avoid deception we did not apply a treatment of low irrelevant information.

[7]In a few, early baseline sessions belief elicitation was not incentivized. To increase the reliability of estimates we added an incentive mechanism in later baseline sessions, as well as all information sessions.

[8]As self-reported income might not capture the wealth status of an individual, we further report participants assets. It is a binary variable indicating whether participants own a house or an apartment. With respect to reported assets there exists no significant difference between the four treatments.

TABLE 6.1: Sample Properties

	Baseline N=144	High N=47	Low N=47	Irrelevant N=36	Pooled N=274
Age (Years)	48.79	44.60	46.53	50.03	47.85
Female (%)	62.94%	46.81%	48.94%	55.56%	56.78%
Education (Years)	14.15	13.81	14.39	14.11	14.12
Monthly Net Income (€)	1232.80	1404.65	1748.39	1818.97	1409.87
Assets (%)	38.89%	29.79%	40.43%	44.44%	38.32%
Household Size	2.11	1.79	1.89	2.06	2.02
Number of Children	0.92	0.85	0.79	0.88	0.88
Climate Change Attitude	5.72	5.77	5.86	5.49	5.72

Note: table reports mean values; climate change attitude corresponds to a mean score of four questionnaire-items retrieved from a 7-point Likert-scale; it includes belief in the existence of climate change, perceived consequences of climate change arising, and concern for oneself and future generations; the scales range from 1 (not convinced/concerned) to 7 (very convinced/concerned).

change attitude. Compared to the general population, we slightly over-sample females and well-educated subjects with the majority holding at least a high school degree.

The sample was recruited via advertisements in two different local newspapers.[9] As a further recruitment tool, leaflets about the experiment were posted in all neighborhoods and public places of the city of Heidelberg. The response to all recruitment tools was similar and no significant differences in demographic attributes and decisions within the experiment can be found. Participants contacted a research assistant for further information and were invited to a session. It was assured that none of the subjects had previous experience with economic experiments.

6.3   Hypotheses

In the subsequent section we present our hypotheses based on observations from other studies which will guide the course of investigation in Section 6.4. First, we develop hypotheses on the effects of high and low social information on actual contributions. Second, we provide our hypotheses on the causal mechanism and anchoring effects.

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[9]The "Rhein-Neckar-Zeitung" is sold at a price of 1.40 € and has 88,649 sold copies within the Heidelberg region. The "Wochen-Kurier" is distributed for free to all households in the Heidelberg region with a run of 74,000 copies.

6.3.1 Social information as a driver

Theory conveys two opposing directions of how social information could affect behavior –namely crowding-in and crowding-out of giving (Shang and Croson, 2009). If individuals only care about the aggregate level of climate change mitigation, then according to the crowding-out hypothesis, originally formulated by Roberts (1984), the presentation of a high reference contribution leads to a reduction in giving. In contrast, under the crowding-in hypothesis individuals are expected to contribute larger amounts when others contribute as well. The studies closest to ours, i.e., Alpizar et al. (2008a) and Shang and Croson (2009) presenting high references to subjects, provide evidence that social information is positively related to contributions. Hence, we expect similar behavior in our context:

Hypothesis 1a: *If individuals are presented with information on high contributions of others, they contribute larger amounts compared to a setting without social information.*

As Croson and Shang (2008) identify a downward shift in contributions when provided information incorporates a references below intended giving, we propose a second hypothesis for the low social information treatment:

Hypothesis 1b: *If individuals are presented with information on low contributions of others, they contribute smaller amounts compared to a setting without social information.*

6.3.2 Mechanisms by which social information affects behavior

A potential channel through which social information works could be the change in perception of (descriptive) social norms. In accordance with the theory of conformity by Bernheim (1994), utility from contributions is affected by the deviation of giving from the normative giving standard. Therefore, we propose the following mechanism which is also investigated in Croson et al. (2009): subjects use the presented information to update their beliefs on others' contribution behavior. Subsequently, they adapt their own contributions (i.e., increase or decrease) to conform with the updated norm.

Hypothesis 2: *Social information affects descriptive social norms (beliefs) and through norms behavior.*

Our data also allows us to test behavior towards the background of a confounding effect. Anchoring might affect behavior whereas the social content of presented information is not influential. Evidence from experimentation suggests that such anchoring effects exist. If participants base their contribution on a heuristic assessment of the decision environment, they potentially recall a figure from their short term memory (Ariely et al., 2003). For example, Cason and Mui (1998) observe a relationship of dictator transfers and irrelevant information, i.e., date of birth. Our anchoring-hypothesis reads as follows:

Hypothesis 3: *If individuals are presented with (high) irrelevant information –providing a large figure from the contribution range– they contribute larger amounts compared to the setting without additional information.*

6.4 Results

In the following, we present results and chronologically test the three hypotheses. Additionally, we expand the analysis and test for robustness of results with respect to socio-demographic factors.

6.4.1 Social information matters

Our findings on contribution behavior in the baseline and the two social information treatments can be summarized as follows: In the baseline average contributions are 31% of the initial endowment (median 20%) and 61% of the subjects contribute a non-zero amount.[10] In the high social information treatment subjects contribute 44% of their endowment with a median of 50% and a contribution frequency of 70%. In the low social information treatment average giving is 27% (median 10%) and non-zero contributions are observed in 60% of the cases. Figure 6.2 displays these results.

In accordance with Hypothesis 1a, a Mann-Whitney test reveals contributions in the high social information treatment to be significantly larger than in the baseline (p=0.04).

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[10]Overall, these results lie in the range of observed behavior in three similar studies: Diederich and Goeschl (2013) and Löschel et al. (2012) study the same public good but vary the price of contributions and therefore are not fully comparable. They observe non-zero contribution frequencies of roughly 40% that are lower than in our experiment. The study by Carpenter et al. (2008) uses a similar method to ours but allows participants to donate towards their first-choice charity. They observe average donations of 68% which exceed contributions in our design.
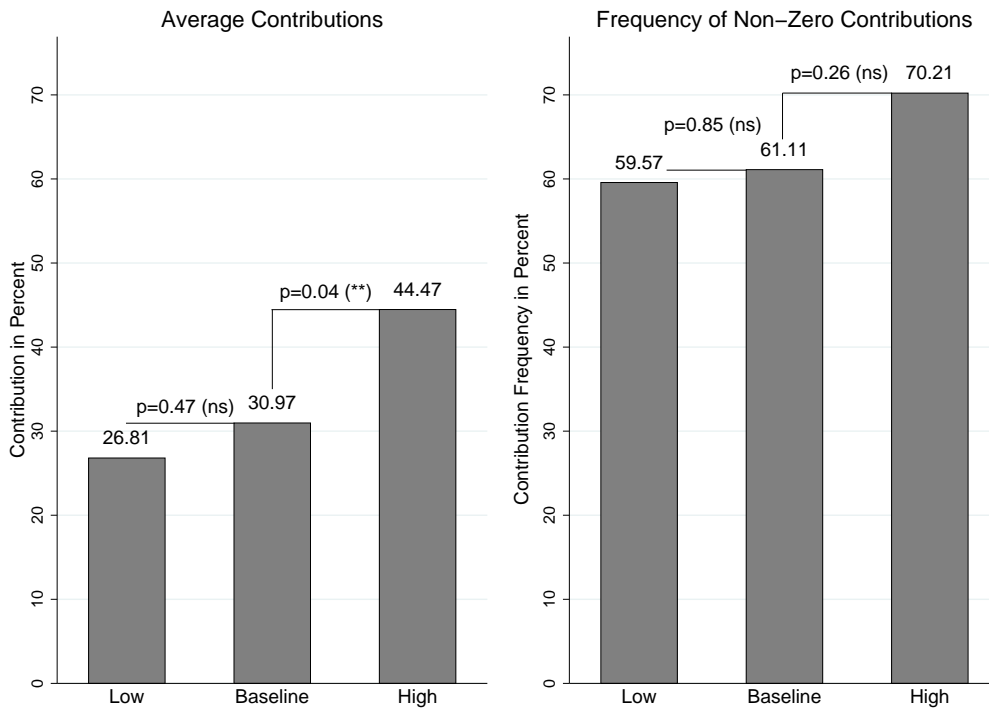
FIGURE 6.2: Average Contributions and Frequency of Non-zero Contributions by Treatment

Hence, contributions under the high reference treatment exceed baseline contributions significantly by 43%. Contributions frequencies are higher as well, although not significantly. With respect to Hypothesis 1b on the downward effect of low social information, we do not find supporting evidence. Contributions are smaller but neither average contributions, nor the frequency of non-zero contributions differ compared to the baseline (Mann-Whitney test $p = 0.47$, Fisher exact test $p = 0.85$).

Result 1a: *Compared to the baseline, contributions are significantly larger when subjects are presented with high social information.*

Result 1b: *Compared to the baseline, contributions smaller but not significantly smaller when subjects are presented with low social information.*

6.4.2 Mechanism

6.4.2.1 Social norms

Hypothesis 2 proposes social information to affect descriptive social norms (beliefs) and through these norms contribution behavior. If the contribution decision corresponds with the desire to conform with a social norm, presenting social information would induce subjects to update or infer the social norm in the underlying decision environment. We utilize data on subjects' beliefs drawn from an incentivized estimation question and look at the relationship between presenting information, beliefs, and behavior.

Social information significantly affects beliefs about the contribution behavior of other subjects present in the session: In the baseline the average estimate of others' contributions is 38%, while estimates in the low and high social information treatment are 24% and 51% respectively. When comparing the means of the two social information treatments to the baseline, we observe a significant difference at the 1%-level. Figure 6.3 summarizes these findings.

FIGURE 6.3: Beliefs about Average Contributions by Treatment

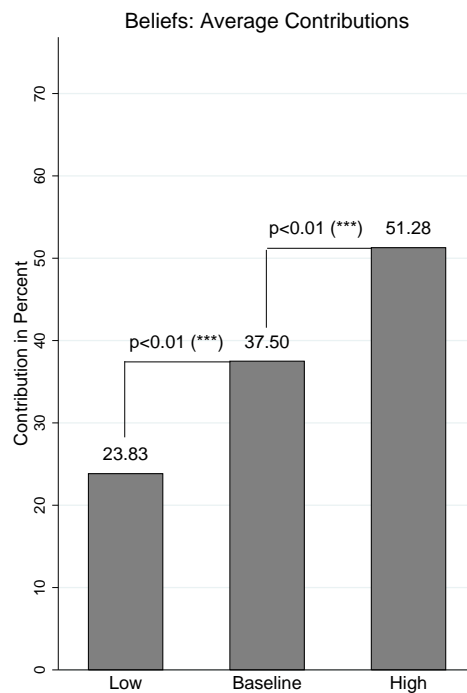**Result 2a:** *Beliefs about peer contributions differ across treatments. Compared to the baseline, estimates in the high social information treatment are significantly larger and estimates in the low social information treatment are significantly smaller.*

By investigating the correlations of actual and estimated contributions, we observe a significant relationship in all treatments. The correlation in the baseline is $\rho = 0.44$ ($p < 0.01$). In the low social information treatment it is similarly strong with $\rho = 0.43$ ($p < 0.01$) and even more pronounced in the high social information treatment $\rho = 0.58$ ($p < 0.01$). To further elaborate these findings, we specify a regression framework focusing on the relationship of treatments, beliefs, and contributions. It applies the steps of a mediation analysis (Baron and Kenny, 1986) and is expanded by an IV regression. Results are presented in Table 6.2.

TABLE 6.2: Regression Results: Relationship of Treatments, Beliefs, and Contributions

	(1)	(2)	(3)	(4)
		1st Stage		*2nd Stage*
Dependent Variable	Contributions	Beliefs	Contributions	Contributions
High (0 = No, 1 = Yes)	13.50**	13.78***	2.736	
	(6.093)	(3.104)	(5.824)	
Low (0 = No, 1 = Yes)	-4.164	-13.67***	6.514	
	(5.865)	(3.439)	(5.891)	
Beliefs			0.781***	0.644***
			(0.129)	(0.244)
Constant	30.97***	37.50***	1.682	8.644
	(2.889)	(1.643)	(4.417)	(9.452)
N	238	238	238	238
R^2	0.029	0.164	0.213	0.203
Prob>F / χ^2	0.0405	< 0.01	< 0.01	< 0.01

Note: contributions as well as beliefs are reported in percentage of endowment; robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As a first step, we investigate the path from social information to contribution behavior. Specification (1) includes the social information treatments as dummy variables. We can restate Results 1a and 1b on the impact of social information on contributions. The coefficient of the high treatment dummy is positive and significant while the coefficient of the low treatment dummy is negative but insignificant. Second, we analyze the path from social information to beliefs. In Specification (2) beliefs are regressed on high and low social information treatments. We show that beliefs are significantly affected by

the exogenous treatments where the coefficient of high social information is positive and the coefficient of low social information is negative. Lastly, we include both social information and beliefs in a common regression predicting contributions. Specification (3) displays outcomes and we observe a moderating effect. While the coefficients for both treatment dummies are insignificant, the coefficient of beliefs is positive and highly significant. A separate mediation analysis (Sobel-Goodman-test) confirms that the mediator (beliefs) carries the influence of treatment-variation to our dependent variable. In particular, a Sobel-test reveals that the reduction in treatment effects is significant for both the high and the low treatment condition ($p < 0.01$). In the low treatment condition the sign of the coefficient even reverses. This provides first evidence that social information affects behavior through beliefs. Nevertheless, this finding should be interpreted with care as the mediator variable is potentially endogenous.

To control for this endogeneity, we rely on an IV-framework similar to Smith (2013). Random treatment assignment is used as an exogenous instrument for beliefs. Under the exclusion restriction assumption, this identifies the causal effect of beliefs on behavior (ibid). Specification (2) corresponds to the first stage of the Two-Stage Least Squares (2SLS) estimation. As emphasized previously, beliefs are significantly affected by exogenous social information. A large F-statistic ($F(2, 235) = 23.73$) further indicates that instruments are not weak. Specification (4) displays the second stage where contributions are regressed on (first stage) beliefs. While the coefficient of beliefs is slightly smaller than in specification (3) it remains highly significant indicating a causal relationship between beliefs and contributions. Furthermore, an endogeneity test indicates that an endogeneity problem is not likely ($p = 0.65$).

Result 2b: *Evidence suggests a causal relationship between beliefs and actual contributions.*

6.4.2.2 Anchoring

To test for potential anchoring effects, we compare average behavior in the baseline (31%) and the irrelevant information treatment (33%). As displayed in Figure 6.4 we do not observe a difference in mean contributions (Mann-Whitney test: p=0.87). Similarly, the median corresponds to 20% of the initial endowment in both treatments. When comparing non-zero contribution frequencies no differences arise either.

Result 3: *Compared to the baseline, contributions do not differ when subjects are presented with irrelevant information.*
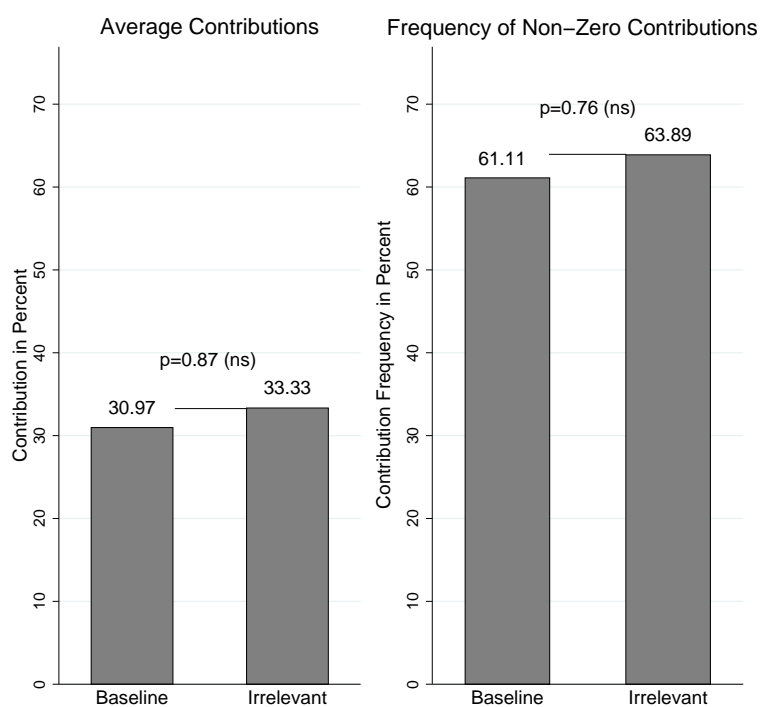
FIGURE 6.4: Panel A: Average Contributions by Treatment;
Panel B: Frequency of Non-zero Contributions by Treatment

For completeness we compare beliefs in the baseline and irrelevant information treatment as well. The average contribution estimate does not reveal a significant difference (see Figure 6.5). Overall, we take this as evidence that anchoring is not a confounding factor when we analyze the potential mechanism through which social information affects behavior in the contribution decision.
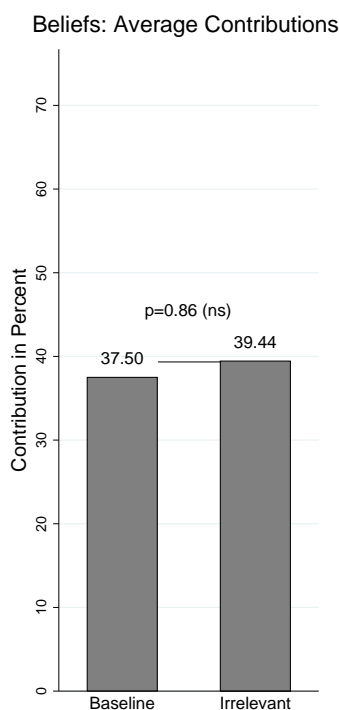
FIGURE 6.5: Beliefs about Average Contributions by Treatment

6.4.3   Demographic effects

In the analysis above we find evidence for a treatment effect of presenting high social information and identify changes of beliefs as a potential mechanism while anchoring is ruled out. Despite random treatment assignment these findings could be moderated by individual characteristics or attitudes towards climate change. In the following, we check the robustness of results and control for further demographic factors.

6.4.3.1   Composition effects

In Table 6.3 we display the regression results with different sets of control variables.[11] We are able to confirm the treatment effects observed in the non-parametric analysis: presenting high social information reveals a positive, highly significant coefficient. The effect of presenting low social information and irrelevant information, on the other hand, is insignificant. The coefficient of age is positive and significant confirming age effects observed in field studies or donation games (List, 2004; Carpenter et al., 2008). Nevertheless, this relationship is surprising as we are analyzing contributions towards climate

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[11]As our data is potentially censored a Tobit regression qualifies for analysis as well. We refrain from reporting the Tobit regression output as it mirrors the results presented in the OLS set-up in Table 6.3.

change mitigation which is characterized by a delay of mitigation rewards. Additionally, we observe a positive effect of gender with females contributing more than males. The results are discussed in more detail in the next section. Specifications (6) and (7) expand the set of controls and reveal positive coefficients of household size and climate change attitude while education, number of children, as well as income/assets are not correlated with behavior.

TABLE 6.3: Regression Results: Treatment Effects and Demographic Attributes

	(5)	(6)	(7)
Dependent Variable	Contributions	Contributions	Contributions
High (0 = No, 1 = Yes)	17.47***	17.34***	18.27***
	(5.521)	(6.079)	(5.489)
Low (0 = No, 1 = Yes)	-1.367	-7.014	-2.479
	(5.901)	(6.875)	(6.439)
Irrelevant (0 = No, 1 = Yes)	2.726	4.514	4.152
	(6.924)	(7.375)	(6.720)
Age (Years)	0.481***	0.633***	0.519***
	(0.118)	(0.170)	(0.156)
Female (0 = No, 1 = Yes)	11.34***	7.438	7.553*
	(4.241)	(4.846)	(4.538)
Education (Years)		0.570	0.632
		(0.723)	(0.677)
Monthly Net Income (€)		-0.00157	
		(0.00265)	
Household Size		4.179**	4.446**
		(1.984)	(1.929)
Number of Children		-0.886	-0.776
		(2.547)	(2.395)
Climate Change Attitude (1-7)		6.116***	6.501***
		(1.699)	(1.604)
Assets (0 = No, 1 = Yes)			2.200
			(5.006)
Constant	0.242	-52.73***	-54.70***
	(6.645)	(15.54)	(14.64)
N	273	226	257
R^2	0.098	0.174	0.171
Prob>F	< 0.01	< 0.01	< 0.01

Note: contributions are reported in percentage of endowment; robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.4.3.2 Gender effects

Croson and Gneezy (2009) report females to be more sensitive to context variations or cues in experimental settings investigating social preferences. In contrast, Croson et al. (2010) find males to react more strongly towards social information, albeit in a hypothetical choice experiment. Given this mixed evidence, we employ an additional analysis to explore whether females and males reveal systematic differences in contribution behavior under the information treatments. As a second step we repeat the IV approach from before and investigate whether there exists an interaction of gender and the causal mechanism.

Table 6.4 (Specification (8)) reports the regression results including treatment dummies, gender, as well as their interaction. Specification (9) repeats the analysis including additional demographic controls as utilized in Table 6.3. In both specifications an effect of gender is observed. Regression results reveal females to contribute significantly larger amounts when provided with high social information whereas the coefficient is insignificant for males.[12]

From this result the question arises whether the difference between the two sexes stems from (a) a difference in belief updating under social information or (b) a difference in adherence to the perceived social norm translating into actual behavior. Therefore, we repeat the IV approach and interact beliefs and gender. If the first explanation (a) is correct, we would observe a gender difference in the first stage of the IV regression. As shown in Specification (10) the exogenous treatment variation significantly impacts beliefs for both females and males. Testing the treatment coefficients of the two sexes individually reveals at most a weak difference (Male-High vs. Female-High, $F(1, 231) = 0.01$, $p = 0.93$; Male-Low vs. Female-Low, $F(1, 231) = 3.26$, $p = 0.07$).[13] In order to investigate explanation (b), we compare the effect of (updated) beliefs on actual giving in the second stage (Specification (11)). Our analysis reveals that females expose a positive, significant effect of beliefs on contribution behavior, while it is insignificant for

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[12]Additional testing shows the treatment difference between high social information and baseline decisions to be highly significant for females (Mann-Whitney test, $p < 0.01$) and insignificant for males ($p = 0.71$).

[13]Similarly, testing treatment differences in average beliefs separately for females and males results in significant effects when comparing high/low social information to the baseline. Hence, both males and females adapt their beliefs under social information.

TABLE 6.4: Regression Results: Treatment Effects, Gender, and the Causal Mechanism

	(8)	(9)	(10) *1st Stage*	(11) *2nd Stage*
Dependent Variable	Contributions	Contributions	Beliefs	Contributions
High (0 = No, 1 = Yes)	2.408 (7.853)	-0.299 (8.170)	8.921* (4.639)	
Low (0 = No, 1 = Yes)	-3.042 (8.763)	-7.573 (10.30)	-16.18*** (5.369)	
Irrelevant (0 = No, 1 = Yes)	5.083 (11.48)	-4.517 (11.00)		
Female (0 = No, 1 = Yes)	6.430 (5.997)	-1.961 (6.530)	-2.013 (3.363)	-24.37 (19.24)
Female*High	26.19** (11.47)	35.29*** (11.29)	9.867 (6.124)	
Female*Low	-0.180 (11.87)	-0.980 (13.62)	4.730 (6.913)	
Female*Irrelevant	-3.805 (14.64)	16.53 (14.66)		
Beliefs				0.212 (0.359)
Beliefs*Female				0.923* (0.494)
Constant	26.79*** (4.705)	-51.40*** (15.45)	38.68*** (2.579)	18.81 (14.01)
Demographic Controls	No	Yes		
N	273	226	237	237
R^2	0.065	0.211	0.173	0.187
Prob>F	0.011	< 0.01	< 0.01	< 0.01

Note: contributions as well as beliefs are reported in percentage of endowment; demographic controls include age, education in years, monthly net income, number of children, climate change attitude; robust standard errors in parentheses;
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

males. Thus, male participants change their beliefs according to the presented social information but they are less prone to adapt their behavior accordingly.

6.5 Discussion and Conclusion

In our research we provide evidence on the impact of social information in a real world public good context and identify a potential mechanism of how such information reflects on behavior. First, we demonstrate that providing high reference information leads to participants contributing higher amounts compared to a situation where no social information is salient. This result is in line with findings by Shang and Croson (2009) and Alpizar et al. (2008a). Low reference information results in lower average contributions but the effect is not significant.

Second, we analyze a (causal) mechanism underlying observed behavior. We find individuals' beliefs on others' contribution behavior to be a mediator of contributions. Interestingly, we observe females to be more sensitive to this mechanism.

Third, we conduct a robustness check against a potential confound. As our proposed mechanism, as well as interpretations of other social information studies, rely on the assumption that the social content of presented information influences behavior, we add an irrelevant information treatment. This treatment variation leads to no behavioral differences and we thus conclude that anchoring does not drive results.

Overall, our results are relevant from different perspectives. We show not only that there exists an effect from the treatment variable, social information, on actual contributions, but also uncover a channel through which this causal relationship could arise. Knowing more about this underlying mechanism is not only of methodological interest. It could similarly help to identify situations in which the provision of information on social norms is an effective tool and in which situations it is ineffective. If such display is not effective, it could result from two distinct effects: either the presented information does not change the perception of the norm or a change in norm perception does not reflect on actual behavior.

Our study provides some evidence that the second effect can be observed among male participants: compared to the baseline, they do not reveal significantly different behavior when receiving high (or low) social information. Nevertheless, they change their perception of others' giving significantly. The second link from changes in beliefs to changes in actual behavior, on the other hand, is not actionable.

Similarly, there could be other contextual factors or individual attributes that alter the reaction to social information and its transmission through social norms. In situations where no behavioral response to social information is observed, this might be due to the fact that information does not change the individuals' perception of the norm.

In this context it would be interesting to extend our study, by exogenously varying the observability of social information. In our experiment it is easy and cheap for participants to observe others' behavior. They are provided with concise information immediately before taking their contribution decision. In reality it might be more difficult or even costly to observe others. In addition, individuals sometimes have to interpret others' noisy behavior which complicates inference on the underlying social norm.

Another contextual factor that could affect the role of social information is its source. In our study, we provide participants with information on fifty unspecified participants from previous sessions. It is unclear to the decision makers who these other individuals are and thus whether they are a subjectively relevant reference group for setting a social norm. Plausibly this could be an important factor. For instance, findings from Goldstein et al. (2008) suggest that preferences for conformity could be much stronger if the decision maker receives information from a demographically similar group of individuals. In this light, it would be worthwhile to investigate whether varying proximity has an influence on behavior and, in specific, on the separate paths of the causal mechanism.

6.6 Appendix

Procedure

Check-in	Check-in room:
	Sign-in and generation of personal code
Experiment	Laboratory:
	Random seat assignment
	General instructions read out loud *(page 94)*
	Tasks implemented in z-Tree
	• Contribution to climate change mitigation *(page 95)* • Belief elicitation *(page 98)* • Questionnaire on climate change attitudes • Laboratory public goods game *(page 99)* • Questionnaire on demographics
	Payment receipt distributed according to personal code
Payment	Check-in room:
	Subjects exchange payment receipt for cash

General Instructions

General Instructions were handed to participants as a print-out.

General Information

Dear participant,

Thank you for participating in this study. In the following you will be informed about the rules and procedures. You have the opportunity to earn money. The payment depends on your decisions during the experiment. Every participant has received the same printed instructions as you did. Please take your time and read the instructions carefully.

No communication with other participants

Please do not communicate with the other participants. Otherwise we are forced to exclude you from the experiment and you will have to forgo your payment. If you have any questions, please raise your hand. The experimenter will answer your question quietly.

Procedure

Please make sure that you created your personal code. During today's experiment, you will be asked to insert your personal code. Your personal code ensures your decisions during the study to be anonymous. The experiment is taking place at the computer and your tasks are explained step-by-step. Please read the instructions on the screen thoroughly. If amounts of money are mentioned for a task, these amounts refer to real payments being paid out –depending on your decisions– at the end of the experiment. It is important that you answer all questions; your data is treated anonymously.

Thank you!

Real Contribution Task

General Instructions

Dear participant,

Thank you for supporting our research. On this screen you receive general instructions on the procedure. You will take several tasks. Please follow the instructions on the screen.

At the end of today's experiment you receive your payment. At several points you can influence your payment by your own decisions. Whenever this is the case you will be informed on the screen and you will receive information on how to make your decisions. Your decisions are anonymous. This is guaranteed by your personal code. In addition, you receive your payment at the end in room 00.005a (check-in room). Therefore, the experimenters will not receive information on your decisions and payments.

− − −

For your participation you receive **ten Euro**.

These **ten Euro** are paid to you at the end of today's experiment in cash.

Alternatively, we offer you to use a share of these ten Euro to reduce global CO_2 emissions.

In the following we explain how it is possible for us to reduce global CO_2 emissions.

− − −

What is CO_2?

CO_2 is a gas which is emitted by burning oil, coal, or fuel. It accrues from the manufacturing of goods or the production of electricity as well as from travel by car or airplane.

Why does one try to reduce CO_2?

The more CO_2 gets into earth's atmosphere, the more likely becomes the occurrence of the environmental problem climate change. Scientists expect climate change to cause consequences such as the rise of sea levels, the stronger spread of tropical diseases, or smaller yields in agriculture.

How is it possible to reduce CO_2 emissions?

Within the European Union a binding limit has been installed which constitutes how much CO_2 may be emitted by large industrial companies. In order to emit CO_2, these companies need emission permits. These permits can be purchased from the emission-trading-registry of the Federal Environmental Agency. After purchase these permits are not available to companies anymore. In this way, European CO_2 emissions are reduced

by the amount of purchased permits. As the climatic system reacts inertly to a change in CO_2 emissions, the reduction action contributes only in approximately 50 years towards noticeable climate change mitigation.

What do we offer to you?

As soon as you completed reading the informational text, we offer you to purchase permits from the German emission-trading-registry of the Federal Environmental Agency from your ten Euro. For each Euro you can mitigate emissions of approximately 70 kg CO_2, i.e., with your ten Euro you can reduce CO_2 emissions by a total of 700 kg. For example, 70 kg correspond to CO_2 emissions arising from a drive from Frankfurt am Main to Hamburg by car.

On average a German citizen emits 9 tons of CO_2 per year (one ton equals 1000 kg). Therefore, 700 kg, which may be reduced with your 10 Euro, correspond to a little less than the monthly CO_2 emissions of an average German.

How can you verify that your contribution was used to retire CO_2 permits?

As permits for CO_2 emissions are purchased through the emission-trading-registry of the Federal Environmental Agency, the procedure can be monitored transparently. At the end of this study a certificate of reduction –issued by the emission-trading-registry– will be posted at the notice board of the Chair of Behavioral Economics (Prof. Dr. C. Schwieren).

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Purchasing CO_2 permits

On the following screen you may indicate the share of your ten Euro you would like to spend on CO_2 permits.

Social Information Treatments: In previous sessions of the experiment 50 participants gave €7 / €1 on average to retire CO_2 permits.

Irrelevant Information Treatment: In previous sessions of the experiment 50 participants lived within a 7km radius around Heidelberg.

– – –

On this screen you may purchase emission permits from a share of your ten Euro.

- Please insert into the blue field how much money you would like to use to retire CO_2 permits and thus reduce global CO_2 emissions.

- You are free to indicate every integer between 0 and 10 Euro, i.e., you may fill in whole numbers without decimal place (period or comma).

- Each Euro you are not using to purchase CO_2 permits, you can take home at the end in cash.

<insert decision>
<summary screen displayed>

− − −

Instructions Estimates

In the following we ask you to appreciate the behavior of the other participants in the room.

You should estimate how the other participants in the room decided on purchasing CO_2 permits.

Depending on the accuracy of the estimate, you receive additional money.

$- - -$

Payment:

For your estimate you may receive additional payments. The more accurate your estimate of the other participants, the more money you receive.

Calculation:

If your estimate deviates by 0.5 Euro (above or below) from the true value of today's session, you receive 2 Euro.

If your estimate deviates by 1.5 Euro (above or below) from the true value of today's session, you receive 1 Euro.

Question:

What do you think: How much did other participants on average give to purchase CO_2 permits?

<insert estimate>

$- - -$

Payment:

For your estimate you may receive additional payments. The more accurate your estimate of the other participants, the more money you receive.

Calculation:

If your estimate deviates by 5 percentage points (above or below) from the true value of today's session, you receive 2 Euro.

If your estimate deviates by 15 percentage points (above or below) from the true value of today's session, you receive 1 Euro.

Question:

What do you think: How many percent of the other participants used a share of their endowment to purchase CO_2 permits?

<select radio-button ranging from 0% to 100%>

Laboratory Public Goods Game

Instructions Computer Task 1/3

Explanation:

In this task you have the possibility to receive further payments, in addition to the ten Euro you already received at the beginning. Furthermore, during this task you interact with the other participants in this room. These will be matched to you randomly and you cannot reconstruct who these participants are.

Payment:

Your own decisions determine how much money you receive at the end. In addition, the decisions of the other matched participants influence your payment.

This part of the study contains a total of 12 decisions.

As soon as you took all decisions, a random mechanism will determine which of the 12 decisions will be relevant for payment at the end of the study. For the other decisions which are not selected, you will not receive payment. Each decision will be chosen with the same probability. Therefore, each decision is equally important for your final payment.

– – –

Instructions Computer Task 2/3

Possible Decisions:

In the following 12 decisions you can distribute 20 balls between two bowls which are labelled A and B.

Bowl A is filled by you only.

Bowl B is filled by you and the other matched participants.

It is not possible to observe how many balls are placed in bowl B by the other matched participants.

Anonymous Matching:

For this task the computer will make an anonymous matching. This matching determines the other participants who can place balls in bowl B.

In some decisions you will execute the task with two other participants (i.e., in total three); in other decisions with eleven other participants (i.e., in total twelve).

If you are interacting with two other participants, you and the others cannot observe

who these participants are. How many participants interact is displayed for every deci-sion.

− − −

Instructions Computer Task 3/3

Calculation of Payment:

This numerical example illustrates how payments in the decision task are determined. The indicated cent-amounts only hold within the example and can differ in and within the actual 12 decisions.

You and the other participants can distribute 20 balls between bowl A and bowl B:

Each participant fills his own bowl A.

Bowl B is filled by you and all other matched participants.

Bowl A: For each ball placed in bowl A you receive 20 cent and the other matched participants receive 0 cent.

Bowl B: For each ball placed in bowl B you receive 5 cent and the other matched participants receive 15 cent each.

The calculation is the same for all participants: Hence, all other participants can also distribute 20 balls.

Bowl A: For each ball another participant places in his/her own bowl A, he/she receives 20 cent and you receive 0 cent.

Bowl B: For each ball another participant places in bowl B, he/she receives 5 cent and all other matched participants **(including yourself)** receive 15 cent each.

− − −

Example:

Please choose how many balls you would like to place in bowl B. Remember, balls which are not placed in bowl B are automatically placed in bowl A.

This is only an example.

Bowl A: This bowl is only filled you. You receive 20 cent per ball. The other participants receive 0 cent per ball.

Bowl B: This bowl is filled by you and the other (two or eleven) matched participants. You receive 5 cent per ball. The other participants receive 15 cent per ball each.

Your choice:

Please indicate in the field how many of the 20 balls you would like to place in **bowl B**. The remaining balls are automatically placed in **bowl A**.

<insert choice for example>

− − −

Your decision

You decided to place *<example choice>* of 20 balls in bowl B. Hence, you placed the remaining *<20 minus example choice>* in bowl A.

Per ball placed in bowl A you receive 20 cent.

Per ball placed in bowl B you receive 5 cent and the other participants receive 15 cent.

Calculation of Payment:

Please indicate how much you would receive for the decision.

In the example you placed *<20 minus example choice>* in bowl A. Hence, you receive from bowl A: *<insert calculation for example>*

In the example you placed *<example choice>* in bowl B: You receive *<insert calculation for example>*

In the example you placed *<example choice>* in bowl B: Hence, every other participant receives *<insert calculation for example>*

In addition, your own payment may change depending on how much the other participants place in bowl B. For each ball another participant places in bowl B, the other matched participants (including yourself) receive 15 cent per ball.

<feedback screen on calculation of example. If correct, continue. If incorrect, repeat example>

− − −

The numerical examples are completed. The actual task is presented in a table.

− − −

Example

Column 1	Column 2	Column 3	Column 4	Column 5	Column 6
Decision	Bowl A – per ball you receive	Bowl B – per ball you receive	Bowl B – per ball the other participants receive	Bowl B – number of participants	Your decision
1	20	5	15	3	

Example for table:

The above table is an example and corresponds to the display of the following decision task.

The above table includes only one row. The actual decision table will include twelve rows. Each row corresponds to a decision.

Explanation of table:

In this explanation you receive information on the (numbered) columns in the table.

Column 2   In this column the amount in cent is displayed which you receive for each ball remaining in bowl A.

Column 3   In this column the amount in cent is displayed which you receive for each ball placed in bowl B.

Column 4   In this column the amount in cent is displayed which each other matched participant receives for a balls placed by you into bowl B.

Column 5   In this column the number of participants is displayed who can place balls in in bowl B. This number includes yourself.

Column 6   In this column you indicate how many balls you would like to place in bowl B.

$- - -$

You completed the examples. Now the actual task begins! All decisions are relevant for payment. One of the 12 decisions is chosen randomly at the end and determines your payment.

$- - -$

Decision Task

The table displays the 12 decisions. Each row corresponds to a new decision.

Decision	Bowl A – per ball you receive	Bowl B – per ball you receive	Bowl B – per ball the other participants receive	Bowl B – number of participants	Your decision
A	20	2	9	12	*<insert choice>*
B	20	2	2	12	*<insert choice>*
C	20	4	4	3	*<insert choice>*
D	20	4	4	12	*<insert choice>*
E	20	16	16	3	*<insert choice>*
F	20	12	8	3	*<insert choice>*
G	20	8	12	3	*<insert choice>*
H	20	8	8	3	*<insert choice>*
I	20	8	6	3	*<insert choice>*
J	20	3	2	12	*<insert choice>*
K	20	1	1	12	*<insert choice>*
L	20	2	3	12	*<insert choice>*

Please indicate in the blue fields how many balls you would like to place in bowl B. The remaining balls are placed in bowl A.

Chapter 7

Discussion and Conclusion

Finally, we provide a brief summary of results. In particular, we emphasize on findings with respect to potential extensions and denote an outlook for future research.

Chapters 2 to 4 focus on behavior in the dictator game. In the first study, we experimentally test and compare student and elderly subjects. In addition, we introduce several treatment variations to examine whether aged individuals behave more prosocial due to stronger social preferences or whether this behavior stems from confounds of the experiment. Our results within the age groups are robust to framing and stake variations. Additionally, we find no systematic differences between real and hypothetical treatments. However, an important factor identified in our experiment is that experience matters for decision making. Without discriminating between experienced and inexperienced student participants, we would have observed a different result. Average transfers of students, i.e., pooled over experienced and inexperienced subjects, are significantly below those of the elderly. But this result is in fact driven by the experienced student participants while inexperienced students and the elderly do not differ in their average transfer behavior.

Extensions of our experiment could enlighten the issue of experience in experiments further. One potential extension might be a test of experienced elderly. In this respect, one could invite aged participants repeatedly and analyze whether their transfers remain at the same average level or deteriorate as well. Additionally, it could be worthwhile to investigate changes in behavior over time. In our study, we only observe participants in their 20s or above the age of 60 at one point in time. But in the light of development

psychology theories (Van Lange et al., 1997; Steverink et al., 1998), longitudinal observations could inform research on age and prosocial behavior further. Overall, our paper has added upon the experimental literature in presenting results on elderly individuals. While previous studies detected larger contributions by the elderly, these findings should be taken with caution before concluding that aged individuals hold stronger social preferences. Certainly, investigating the elderly is important and should be further extended to other types of experiments. However, one should not loose focus on the student control group before commenting on effects.

The study presented in Chapter 4 is on a different aspect, namely framing in gender-paired dictator games. In particular, we conduct a double-anonymous dictator game where information on recipient gender is salient. By this, we compare behavior in same- and opposite-sex pairs. Furthermore, we vary framing of the task to identify whether framing influences behavior in gender-paired settings. If we analyze gender and frame individually as if participants were interacting with an androgynous match, we confirm that females are more generous than males and framing does not matter (Dreber et al., 2013). However, when recipient gender is considered as well, we find differences among the frames. Transfers to the opposite sex are larger than to the same sex. But this effect is only significant when participants are taking from their match instead of giving. In the real world individuals do not interact with an androgynous match, either, and the information on the gender of an interaction partner can be observed easily and reflects on behavior. Hence, we suggest to control for beliefs about recipient gender and the gender composition of a session or to make gender information salient.

Several factors might further interact with gender-paired behavior. Personality or cognitive abilities and education potentially could affect transfers (Ben-Ner et al., 2004). In the corresponding chapter we briefly emphasize on another aspect that might play a role for gender-interaction, namely relationship status. As pointed out, our results are driven by singles who constitute the vast majority in our sample. If this aspect is influential for the mating behavior hypothesis which could explain preferential transfers towards the opposite sex, it would be worthwhile to extend our design. Therefore, one could increase the observations on participants in a relationship and investigate whether they behave in a similarly preferential way towards the recipient –even though they already have a significant other. Furthermore, one could make relationship status information on the recipient salient.

In addition, relaxing the anonymity in our design could influence decisions. Mating behavior might be further strengthened with an audience because individuals would like to signal altruistic behavior to an interaction partner (Farrelly et al., 2007). In this light, one could combine our design with a similar study by Alevy et al. (2014) observing audience effects in framed dictator environments.

Chapters 5 and 6 focus on a more specific context, namely climate change mitigation contributions. From the summary of Goeschl et al. (2014a) it can be seen that context is important for examining real world public goods. Overall, we find correlations of abstract and specific tasks to be low and dependent on several design parameters. Furthermore, the study population matters for giving and, as students are not representative for the population contributing in the real world, one should invite a sample with broader demographic attributes.

Following the framework of Goeschl et al. (2014a) we take the climate change contribution task as a baseline for the study in Chapter 6. An artefactual field experiment (Harrison and List, 2004) is most suited for investigating contribution motives. In our study on norms and social information, participants contribute towards climate change mitigation and social information on other participants is introduced and varied. We are able to identify a mechanism through which social information reflects on behavior. Our results show that presenting social information on other participants updates participants' beliefs, i.e., descriptive social norms (Croson et al., 2009), and through beliefs, their behavior changes. Additionally, we show that results are robust to anchoring and that it is in fact the social content of the information resulting in behavioral differences.

Our design is helpful for testing and explaining situations where social information does not influence behavior. Therefore, it is possible that social information updates beliefs but beliefs do not reflect on contributions. In our experiment we identify this behavior for men. Potentially, one could apply our method to further expand on factors that impact the sensitivity to the mechanism. As discussed in the chapter, one could also manipulate the effectiveness of the mechanism by design. We propose varying the degree of proximity to the reference groups and changing the costs of social information.

Lastly, our research is not only interesting from an academic perspective but also for policy-makers and fund-raisers in the domain of climate change mitigation. As options

for policy intervention are limited by issues of international agreement, fostering voluntary private contributions can be essential. In all treatments participants contribute on average positive amounts towards mitigation which is in line with the positive demand found by Diederich and Goeschl (2013) and Löschel et al. (2012). We show that social information might be an effective tool and provide evidence that policies could rely on the establishment of descriptive social norms. Additionally, the results indicate that certain individual attributes matter for contribution behavior and policy interventions could be more successful by targeting individuals most prone to social information. Variables elicited from the follow-up questionnaire reveal that individuals being more aware of the topic, i.e., convinced of the climate change phenomenon, contribute larger fractions. Informing individuals on the issues could therefore boost cooperation. Age similarly appears to be connected to contribution behavior. Given the time structure of the climate change dilemma one would expect aging individuals to contribute less. Our results point in a different direction and show that elderly individuals are willing to invest into a large scale intergenerational environmental problem such as climate change.

In conclusion, the chapters share a common link of prosocial behavior and social preferences. In each experiment individuals on average transfer positive amounts. This finding is certainly not surprising given the vast literature on experiments identifying sharing behavior. This thesis comprises several extensions on experimentally examining prosocial behavior, and we investigate factors such as age, gender interaction as well as context and norm adherence. Taking our research to the experimental laboratory has several advantages such as the control of data collection. Nevertheless, combining our findings with data from the field could further contribute to the understanding of prosocial behavior among individuals.

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