

The Role of Care in Economic Decision Making

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Preamble

Introduction

Humanity's most important challenges such as climate change, rising inequality, forced migration and the recent corona pandemic are social dilemmas of a global nature. Solutions to these social dilemmas rely on the willingness to cooperate and require coordination among individuals and nations to take actions that are individually costly, but which have positive effects across borders and generations. The recent reemergence of nationalist tendencies, political polarization and the segmentation of societies are detrimental in this regard. The resulting intergroup conflicts and frustration of some parts of society lead to distrust in institutions and decision makers. This development has been particularly visible in the election of right wing populists such as Donald Trump, Brexit and the ignorance towards the corona pandemic in some parts of society. To understand these phenomena and to design policies that are able to promote a sustainable and inclusive global economy that is resilient and capable of addressing global challenges, it is crucial to include the social context into economic analysis.

Since humans are social beings, every interaction is social in nature and the social context is an important determinant for the outcome of these interactions (Davis, 2015). This is also true for economic transactions, which are typically preceded by social interactions and based on values and narratives (Akerlof and Snower, 2016; Bourdieu, 1987; Benjamin et al., 2016; Doran, 2009). Furthermore, individuals have a need for social belonging and affiliation which makes social groups a central element of social life and a source of individual well-being (Baumeister and Leary, 1995; Lima de Miranda and Snower, 2020). These social groups are typically based on homophily which is the tendency to prefer to engage with similar others, where similarity can be based on common characteristics, values, attitudes and goals (McPherson et al., 2001). The identification of similar others has two important functions. First, it reduces strategic uncertainty about the action of others by means of the projection of preferences, norms and values on similar others or the projection of stereotypes on dissimilar others (Ames, 2004a,b). Second, the identification of similar others can facilitate social learning by imitating their strategies, since similar

others are more likely to encounter similar situations (Riolo et al., 2001; Efferson et al., 2016).

Similarity and therefore social groups can be found in diverse contexts and on societal levels such as families, neighborhoods, colleagues, religious communities and even nations. While social groups can facilitate cooperation, they can also lead to intergroup conflicts and discrimination (Sambanis and Shayo, 2013; Tajfel et al., 1979). This may arise if there is little interaction between groups (Allport et al., 1954), communication bubbles are present (Bliuc et al., 2019) and overarching common goals do not exist or are not salient (Sherif, 1958). As a result, self reinforcement of opinions and ideologies within certain groups may lead to segmentation and isolation of parts of society as seen recently in many Western societies. Therefore, it is crucial to consider social groups and their interrelations in economic analysis to gain a better understanding of underlying motivations of cooperation and how they change with the social context.

A promising approach to include social groups into economic analysis is multilevel selection, recently discussed in the field of evolutionary social biology (Sober and Wilson, 2021; Wilson, 2015; Kerr and Godfrey-Smith, 2002). The theory of multilevel selection challenges the paradigm of methodological individualism, arguing that selection works on different levels of a population rather than exclusively on the individual level. These collective levels are social groups and institutions that promote individual well-being through the collective benefit of cooperation within the respective groups and institutions. This generalization of kin selection¹ on a societal level² is able to explain why altruism can be favored by selection in the presence of intergroup competition resulting in cooperation within groups (Traulsen and Nowak, 2006). In the evolutionary biology literature, altruism refers to an action that promotes evolutionary fitness of others at the expense of own evolutionary fitness³. Such an action of kind towards others is inherent to human behavior, e.g. nurturing of children, charitable giving, and has been studied across disciplines. In economics, the underlying motivation of this behavior is referred to as altruism and prosociality (see Rotemberg (2014) for a comprehensive summary) and in psychology as care, compassion (Heckhausen and Heckhausen, 2010) or prosocial altruism

¹Hamilton (1964) proposes a theory of kin selection to explain the persistence of altruistic behavior in populations. Since it is likely that relatives bear similar genes, he argues that the promotion of fitness on the own expense of relatives favors the selection of own genes. Whereas the benefit of the promotion is decreasing in the degree of relatedness.

²Since kinship is only one dimension of many factors of variance among groups, multilevel selection offers an approach to include other factors that go beyond genetic reproduction to explain the persistence of altruistic behavior (Goodnight, 2005; Okasha, 2020)

³Evolutionary fitness is defined in terms reproduction. Hence, altruism promotes the reproduction of others on the expense of own reproduction.

(Goetz et al., 2010).

When studying cooperation in social dilemmas, multilevel selection can enhance economic analysis. By assuming that individuals pursue both individual and collective goals, it complements the analysis by introducing a meso-level, i.e. social groups, linking individual actions and societal outcomes. A better understanding of the motivations underlying cooperation within social groups is therefore crucial. These insights can, for example, be linked to the core design principles formulated by Ostrom (1990) and become a powerful tool to design policies to appropriately address social dilemmas (Wilson et al., 2013).

In this spirit, this dissertation uses an interdisciplinary approach to investigate the effect of social context on the individual activation of the psychological motive care. The investigation rests on the findings that individuals motivated by care receive utility by increasing the well-being of others even at a personal cost (Goetz et al., 2010; Condon et al., 2013; Batson and Shaw, 1991). Additionally, individuals motivated by care feel responsible for the well-being of others and therefore tend to create a cooperative environment. This tendency is based on the belief of the cooperativeness of others (Crocker and Canevello, 2012). Leaving any higher level investigation to further research, the presented papers contribute to a deeper understanding of how specific contextual primes regarding a counterpart lead to the activation of the psychological care motive and therefore to different outcomes in social interactions. The underlying theoretical framework rests on a vast literature in motivation psychology arguing that individuals are "multi-directed" with regard to their motivations. That is, individuals have access to different motivation systems, which are activated by internal and external stimuli (Heckhausen and Heckhausen, 2010). In economic terms, this implies that individuals have context dependent preferences, which are activated through the appraisal of a social context. A social context is characterized by different aspects, such as (i) the payoff structure, e.g. correlated outcomes, complementarity or externalities (Potters and Suetens, 2009), (ii) the framing (Tversky and Kahneman, 1981) or institutional setting (Peysakhovich and Rand, 2015; Engl et al., 2018) of the situation, (iii) the information available about others in this situation (Dimant, 2020). These aspects of the social context are appraised by the individual and lead to a motivation of an action. In repeated social interactions, outcomes of previous encounters become part of the social context itself and may change the appraisal of the social context and therefore the underlying motivation of action.

In their theoretical model Bosworth et al. (2016) connect psychological motives and economic decision making as follows: (1) All behavior is motivated. (2) People have

access to multiple discrete motives. (3) Each motive is associated with a distinct preference function. (4) Motives are activated by stimuli in a person’s internal or external environment. Based on these assumptions, they derive an equilibrium distribution of a care motive and a self-interest wanting motive in the presence of strategic complementarities and substitutabilities in an evolutionary framework. Their formulation of the utility function of the care motive reads as follows:

$$U_i(x_i, x_j) = (1 - \kappa_i)\pi_i(x_i, x_j) + \kappa_i\pi_j(x_j, x_i), \quad (1)$$

where π_i and π_j are the respective payoffs from a social interaction of individuals i and j . Individual i ’s utility is a combination of the two payoffs, and κ_i measures their relative weights. If $\kappa_i > 0$, an increase in the payoff of individual j increases the utility of the focal individual. Hence, the parameter $\kappa_i > 0$ captures the degree of care towards individual j .

This utility function serves as the basis for the analysis in this dissertation. It is explicitly tested in the first paper, and implicitly assumed in the second and third paper. The focus of the analysis lies on the experimental manipulation of the care parameter κ assuming that $\kappa(T, S)$ is a function of a personal trait (T) and a contextual stimulus (S). Personal traits are crystalized states, in the sense that they result from experiences in previous social interactions and therefore constitute the individual disposition for a motive to be activated. The contextual stimulus is a current situational cue which is evaluated by the individual and leads to a motivation of action that depends on the aforementioned traits. For example, a person who repeatedly encounters social settings which are characterized by status competition, unfairness or violence may be less motivated to thrive for a mutual benefit in a cooperative situation than a person who repeatedly encounters cooperative settings. Based on this theoretical background, the subsequent studies use priming techniques to manipulate the motivational state of participants in accordance with experimental economics methods and investigate how the motivational state influences behavior in a social dilemma game, a dictator game and in social risk taking. A prime is a stimulus that activates associated memories (Callen et al., 2014) or concepts (Cohn and Maréchal, 2016) through situational cues and has been increasingly used in economics (see Bargh and Chartrand, 2000, for a comprehensive summary). In particular, two primes are used to induce the motive of care, (i) norm compliance and (ii) objective similarity to a counterpart.

The empirical analysis rests on state of the art mediation analysis to statistically identify the care motive as the driver of behavior. In general, mediation analysis is a statistical tool to identify mechanisms that underly behavior. To this end, the

treatment effect is decomposed into a direct effect of the treatment on the outcome and an indirect effect that works through intermediary variables, so called mediators, that have been manipulated by the treatment. In this way it is possible to identify the effect of the mediator on the outcome variable (Imai et al., 2010). Specifically, in each study the treatment manipulation of care is evaluated and used as a mediator to estimate the effect of the care motive on the outcome.

Summary of the Papers

In the first paper **Care and Anger Motives in Social Dilemmas** with Patrick Ring and Dennis J. Snower we induce the two antagonistic motives care and anger and find that these motives lead to different beliefs, preferences and perceptions in a social dilemma characterized by strategic complementarity and a positive externality. The motive induction rests on a dictator game with a restricted choice set, while the violation of a fairness norm induces anger and the compliance with the fairness norm induces care towards the dictator. The non-linearity of the social dilemma game adapted from Potters and Suetens (2009) allows us to estimate the care parameter κ based on best response functions derived from the utility function (1). The game is played repeatedly for 16 rounds, in which the subjects state their belief and their contribution to the public good. Additionally, the two motives are validated and preferences are elicited based on the strategy method every five rounds. Using a mediation analysis, we are able to attribute the treatment differences in the social dilemma game to the two aforementioned motives. While subjects motivated by care are more cooperative, have higher beliefs about their partners' contribution and tend to perceive the game as cooperative, subjects motivated by anger are less cooperative, have lower beliefs about the partners' contribution and tend to perceive the game as competitive. The estimation of the best response functions reveals that this is partly attributed to a shift in preferences, i.e., a higher value of κ for individuals motivated by care. Additionally, we find a spillover effect of the motives during the course of the repeated social interactions amplifying cooperation in the care treatment compared to the anger treatment.

In the second paper **The Effect of Perceived Similarity and Social Proximity on the Formation of Prosocial Preferences** I experimentally induce the motive care by manipulating perceived similarity to a partner. Perceived similarity is induced via objective similarity based on pre-elicited Big-Five scores of the subjects and their respective partners. In a subsequent dictator game I find that subjects matched with a similar partner give significantly more than subjects matched with a

dissimilar partner. A mediation analysis shows that this behavior can be explained by an increase in social proximity and care towards the partner. Hence, perceiving someone as similar increases social proximity which activates the motive care and consequently leads to more giving.

In the third paper **The Risk of Walking a Mile in Other People's Shoes: An Experimental Approach to Perceived Similarity and Risk Taking for Others** with Jan Krause and Patrick Ring we study how individuals decide for others in the context of risk. We vary the degree of social distance towards a partner and compare within differences between the decision for oneself and the partner across treatments. The manipulation of social distance rests on the induction of perceived similarity on pre-elicited Big Five scores of the subjects and their respective partners. We find that subjects matched with a similar partner are more risk averse in the gain domain and more loss averse in the mixed domain when deciding for their partner, compared to subjects with a dissimilar partner. This is attributed to a positive target decision gap in the dissimilar treatment, i.e., the within differences between one's own decision and the decision for the partner. We investigate how the two concepts construal level theory and care help explain the differences in the decision gap between the treatments. We find that the results in the gain domain can be explained by a higher construal level, whereas care seems to be the main driver in the mixed domain.

Conclusions

The three papers presented in this dissertation investigate how the care motive can be induced through different contextual primes and show that care leads to more cooperation in a social dilemma, more giving in a dictator game and less excessive risk taking in the presence of losses. Drawing on a vast literature in motivation psychology that argues that individuals are "multi-directed" with regards to their motivation, the presented studies contribute to deeper understanding of the underlying motivations of cooperation in social and economic interaction. In particular, it is shown that the care motive is activated through compliance to social norms and the identification of similar others. Both aspects, social norms and homophily, are essential features of social groups, while both help to coordinate collective action.

Thereby, the dissertation makes three important contributions to the existing literature. First, it presents two novel approaches to induce the care motive which are relevant to the coordination and the stability of social groups, i.e, social norms

and homophily. While social norms offer guidance in social interaction, homophily is a fundamental characteristic of social groups that helps to reduce strategic uncertainty by social projection and facilitates social learning. Second, the presented studies identify the care motive as a relevant motive in social interaction that facilitates cooperation, affects beliefs and the appraisal of the strategic context. Third, by using state of the art mediation analysis, the studies show how motives can be statistically identified as a mechanism for economic decision making in various contexts.

A promising approach to establish a more holistic theory on a micro level that explicitly includes the social context into economic analysis, is to develop a theory that connects different motives to economic decision making. To this end, it will be important to identify further motives that are relevant for economic decision making and study how these motives are activated through contextual stimuli.

Another avenue for future research is to include social groups and their relations into the analysis of economic decision making. So far, studies investigating between-group interaction have emphasized the role of emotions regarding norm violations and perceptions of their group members as cooperators in the presence of intergroup competition (Burton-Chellew et al., 2010; Eckel et al., 2016; Puurtinen and Mappes, 2009). These findings are in line with the presented studies, and indicate that the care motive is an essential motive for generating a mutual benefit in collective decisions. However, these studies typically rely on the minimal group paradigm and therefore cannot account for group specific characteristics. Investigating how specific group settings and compositions activate the care motive in the presence of intergroup competition and intergroup relations should be addressed in future research. For example, varying the degree of similarity within groups and investigating how these groups are able to cooperate in the presence of intergroup competition.

Finally, care is a central human motivation, and caring relationships within social groups are an important source of individual well-being. Therefore, understanding social groups and how specific social contexts activate care is a promising avenue towards a better understanding of social and economic cooperation. Looking through the lens of multilevel selection based on the design principles by Ostrom might help to design policies that are able to address the global challenges that our societies face.

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Papers

Bibliographic Information

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Each co-author contributed significantly to the concept, design, analysis and content of the articles.

I. Care and Anger Motives in Social Dilemmas

by Patrick Ring, Christoph Schütt and Dennis J. Snower

Abstract

This paper provides evidence for the following novel insights: (i) People's economic decisions depend on their psychological motives, which are shaped predictably by the social context. (ii) In particular, the social context influences people's other-regarding preferences, their beliefs and their perceptions. (iii) The influence of the social context on psychological motives can be measured experimentally by priming two antagonistic motives – care and anger – in one player towards another by means of an observance or a violation of a fairness norm. Using a mediation approach, we find that the care motive leads to higher levels of cooperation which are driven by more optimistic beliefs, a different perception of the game as well as by a shift towards more pro-social preferences.

JEL Classification: A12, C71, D01, D62, D64

I.1 Introduction

A large body of empirical and experimental research documents that individuals exhibit other-regarding preferences (Fehr and Schmidt, 1999; Fehr and Fischbacher, 2002; Charness and Rabin, 2002). It is less clear, however, how stable other-regarding preferences are across different social contexts, and a coherent economic theory explaining the context dependence of other-regarding preferences is missing. An important aspect of social contexts is that of shared values or standards, so-called social norms. While norms offer guidance in social interactions and help to facilitate coordination (Bicchieri, 2016; Bicchieri and Dimant, 2019), norm violations can have lasting negative implications on future social interactions by eliciting negative emotional responses (Strang et al., 2016; Pillutla and Murnighan, 1996) and fostering self-interested behavior (Bicchieri et al., 2020). In this paper, we show that other-regarding behavior can be malleable when a fairness norm is violated, and we explore how motivation psychology can help to understand why.

Motivation psychology argues that individuals are “multi-directed,” i.e., they have access to multiple discrete motives. The phenomenon of multidirectedness makes context dependence central to preference formation, since contexts serve to activate motives. In economic terms, individuals do not maximize a single utility function across contexts, but different contextual stimuli lead to different motives, which are associated with different utility functions (Heckhausen and Heckhausen, 2010; Bosworth et al., 2016).

A theoretical background connecting motives to economic decision-making is outlined in Bosworth et al. (2016) as follows: (1) All behavior is motivated. (2) People have access to multiple discrete motives. (3) Each motive is associated with a distinct preference function. (4) Motives are activated by stimuli in a person’s internal or external environment. Based on this theoretical background, Bosworth et al. (2016) argue that motives and the likelihood of their activation based on individual traits are evolutionarily linked to the economic and social context. The economic and social context in Bosworth et al. (2016) is specified in terms of strategic complements and substitutes, the former promoting cooperation, and the latter inducing competition.

Behavioral economics literature acknowledges that contextual differences – particularly those based on framing (Tversky and Kahneman, 1981), priming (Cohn and Maréchal, 2016; Dimant, 2020), perception (Dimant and Hyndman, 2019) and institutional norms (Peysakhovich and Rand, 2015; Engl et al., 2018) – can lead to systematically different behaviors. The literature on priming in behavioral economics focuses primarily on experiments in which behavior and judgment are influenced

by the activation of mental concepts through situational cues. As such, it rests on empirical techniques in experimental psychology.¹ The prime commonly comes in the form of a stimulus that activates associated memories (Callen et al., 2014) or concepts (Cohn et al., 2015). The stimuli may be explicit in the form of words or the recollection of past experiences (Bogliacino et al., 2017) or implicit in the form of music (North et al., 1999), images (Vohs et al., 2006), unscrambling sentences (Bargh et al., 1996), temperature (Williams and Bargh, 2008), odor (Holland et al., 2005), and even subliminal stimuli (McKay et al., 2011). An underlying theory that offers a coherent explanation of these effects with regard to economic decision-making has thus far been absent. This paper contributes to filling this gap by providing a link among motivation psychology, primes and economic behavior.

In this paper, we show that people’s economic decisions depend on their psychological motives, which are shaped predictably by the social context. In particular, the social context influences people’s other-regarding preferences, their beliefs and their perceptions. The influence of the social context on psychological motives can be measured experimentally by priming two antagonistic motives – care and anger – in one player towards another by means of an observance or a violation of a fairness norm.

Out of the variety of motives discussed in motivation psychology, we study the two antagonistic motives of care and anger. With the care motive, also referred to as compassion (Goetz et al., 2010) or prosocial altruism (Heckhausen and Heckhausen, 2010), individuals receive utility by increasing the well-being of others even at a personal cost (Goetz et al., 2010; Condon and Feldman Barrett, 2013; Batson and Shaw, 1991). Care-motivated individuals feel responsible for the well-being of others and therefore tend to create a cooperative environment. This tendency rests on the belief on the cooperativeness of others (Crocker and Canevello, 2012). With the anger motive, individuals are willing to sacrifice to harm others (Heckhausen and Heckhausen, 2010) or to make anti-social welfare decisions (Small and Lerner, 2008). Additionally, individuals motivated by anger have a desire of wanting and perceive others as competitors (Van Kleef et al., 2008).

Care and anger are common social motives in the sense that they mediate social interactions and that are present in the economics literature. The motive care refers to altruism and prosociality including models on other-regarding preferences, positive reciprocity, guilt aversion and signaling of preferences (see Rotemberg (2014) for a comprehensive summary). The motive anger has been studied theoretically (Battigalli et al., 2019; Akerlof, 2016; Aina et al., 2020; Winter et al., 2016; Brams, 2011;

¹A survey is provided in Bargh and Chartrand (2000).

Passarelli and Tabellini, 2017), in the context of pricing (Anderson and Simester, 2010; Rotemberg, 2005) and experimentally (Gneezy and Imas, 2014; Van Leeuwen et al., 2017; Persson, 2018; Castagnetti and Proto, 2020).

At the beginning of our experiment, one of these two motives is experimentally primed by an observance or a violation of a fairness norm. The primed motive then influences the beliefs, perceptions and preferences that shape the individual's actions in a non-linear public goods game. The outcome of the social interaction, in turn, affects the individual motives. This feedback between motives and social interactions is particularly significant for an explanation of how social motives operate. In practice, naturally, social motives bring people into social interactions, which, in turn, affect their social motives (Bault et al., 2017).

Our research is related to the following literature: In line with Battigalli et al. (2019), we explore how an initial stimulus and the history of interaction between two individuals lead to the endogenous formation of preferences and beliefs under different motives. Similar to Cox et al. (2007), we observe reciprocal behavior, i.e., people treat others as they have been treated, which we interpret as an outcome of the interaction between the motives care and anger. Bartke et al. (2018) induce care and anger motives via autobiographical recall and study subsequent behavior in a one-shot linear public goods experiment. They show that individuals recalling memories associated with the care motive behave more pro-socially than individuals recalling memories associated with the anger motive. While this study analyzes a one-shot interaction without feedback between motives and social interactions, we study a repeated interaction with such feedback. Finally, Chierchia et al. (2017) investigate how motives of care and power, and motives of control and self-efficacy affect cooperation and punishment behavior. They find that individuals with care motives behave more cooperatively, while individuals with power motives are more willing to punish social norm violations. However, the effect of feedback from interpersonal cooperation and punishment on the motives is not studied. Moreover, because this study only recorded unconditional choices, it is unclear whether beliefs, preferences or both are changing. By eliciting conditional choices and thereby controlling for beliefs, we show that motives indeed give rise to different objective functions.

The rest of the paper is structured as follows: In Section II, we outline the experimental methods; in Section III, we present our results; and in Section IV, we conclude with a discussion of our findings.

I.2 Methods

To test the outlined theoretical framework, our experiment consisted of two stages. In the first stage, we experimentally primed one of the two antagonistic motives care and anger in one player toward another. The motive is primed by means of an observance or a violation of a fairness norm via a dictator game with restricted choice set. In contrast to the classical version, here, the dictator cannot choose any distribution of the initial endowment but can only choose from a limited set of alternatives, as done, for example, by Bolton et al. (1998) or Schulz et al. (2014). This feature allows an experimenter to manipulate the frequency of certain outcomes. After a motive validation, which is undertaken at repeated intervals, the two players interact in a context characterized by strategic complements. Because we aimed at testing whether the induced motives give rise to different behavior during the interaction and how beliefs, perceptions and preferences account for the observed differences in behavioral patterns, we elicit unconditional choices, beliefs about the other player's choices, perceptions about the game environment and conditional choices.

I.2.1 Motive prime

We aimed at priming the two antagonistic motives of care and anger via a dictator game with a restricted choice set to study reactions to fairness norms. Perceived fairness about the distribution of the endowment is associated with the induction of the motive care (Singer et al., 2006), while perceived unfairness about the distribution is associated with the induction of the motive anger (Strang et al., 2016; Pillutla and Murnighan, 1996). Based on this approach, we constructed two choice sets consisting of two options (Table I.1). The dictator is endowed with 20 Euros and has to decide how to split this endowment between herself and the receiver according to the choice options. Option 1 is associated with the two treatments. In the care treatment, the endowment is split equally. In the anger treatment, the endowment is split unequally, with the dictator receiving 90 percent of the endowment.

Option 2 is the same in the two choice sets and includes an unfair split in which the dictator keeps only 10 percent of the endowment. Option 2 is therefore unlikely to be chosen by the dictator in both choice sets. The dictator makes decisions for both choice sets. One decision is randomly reported to the receiver, who is unaware of the restriction of the choice set. The revealed decision determines whether the treatment is care or anger. This allows us to conduct both treatments in the same session and avoid any day-specific effects (Howarth and Hoffman, 1984). The dictator is only

told that one of the two decisions is reported to the receiver to exclude a direct effect of the prime on the dictator.

Table I.1: Dictator game with restricted choice sets

	Option 1	Option 2
Care	Keep 10 EUR Send 10 EUR	Keep 2 EUR Send 18 EUR
Anger	Keep 18 EUR Send 2 EUR	Keep 2 EUR Send 18 EUR

I.2.2 Motive validation

During the experiment, we validated the activation and intensity of the motives at various points. This was done via self-reported ratings of motivational states based on words associated with the motives care and anger. For the ratings, we used seven-point Likert scales with words taken from the word atlas assembled by Chierchia et al. (2018), who asked people to assign words to different motives. The words used in this experiment are *outraged* and *upset* for the anger motive, and *likable* and *compassionate* for the care motive. The ratings are formulated directly toward the other player. We averaged the two ratings for each motive to derive a care score and an anger score.

To avoid collinearity in our subsequent analyses, we constructed a motive score, which is the difference between the care and anger scores. Taking the difference is theoretically justified because the two motives are antagonistic. Additionally, in our data, the care and anger scores are negatively correlated ($r(214) = -0.51$, $p < .001$). A high motive score indicates that the care motive dominates, while a negative score indicates that the anger motive dominates.

I.2.3 Social dilemma game

Bosworth et al. (2016) suggest that the care motive has a comparative advantage in a social setting characterized by strategic complements. Such a context should therefore offer care-motivated individuals the opportunity to fully exploit the benefits of this motive. We used the experimental design of Potters and Suetens (2009) with a payoff function that has the following form:

$$\pi_i(x_i, x_j) = a + bx_i + cx_j - dx_i^2 + ex_j^2 + fx_ix_j, \quad (\text{I.1})$$

with $x_i, x_j \geq 0$ denoting the contribution of player i and j respectively and the parameter $b, c, d, f, e > 0$ are the calibration parameters of the payoff function. The game has a positive externality ($\partial\pi_i(x_i, x_j)/\partial x_j > 0$), and the choices are strategic complements ($\partial^2\pi_i(x_i, x_j)/\partial x_i\partial x_j > 0$). Cooperation in this setting is particularly important because of these two properties, which are very common in practice. Examples of such settings are search and matching in labor markets (a worker searches for jobs to increase a firm's profits, *ceteris paribus*, and increases the effectiveness of a firm's search for workers), synergies in the workplace (a worker's willingness to cooperate with a colleague increases the firm's profit and promotes the colleague's productivity due to willingness to cooperate with the worker), and trust under incomplete contracts (one party's trust promotes the surplus of the trading partner and enhances the effectiveness of the trading partner's trust). The unique symmetric Nash equilibrium for individual payoff maximization is as follows:

$$x_i^{Nash} = b/(2d - f). \quad (I.2)$$

The joint payoff maximizing (JPM) choice is given by:

$$x_i^{JPM} = (b + c)/(2(d - e - f)). \quad (I.3)$$

To explore the preference signature resulting from the two induced motives of interest and following Bosworth et al. (2016), we assume that the utility function of an individual in a two-player interaction is represented by

$$U_i(x_i, x_j) = (1 - \kappa_i)\pi_i(x_i, x_j) + \kappa_i\pi_j(x_j, x_i), \quad (I.4)$$

where π_i and π_j are the respective payoffs from a social interaction of players i and j . Player i 's utility is a combination of the two payoffs, and κ_i measures their relative weights. If $\kappa_i > 0$, an increase in the payoff of the other player in the game increases the utility of the focal player. This feature is typically associated with the motive care (Bosworth et al., 2016). If $\kappa_i < 0$, the focal player receives disutility from the payoff of the other player in the game (Heckhausen and Heckhausen, 2010). This feature is typically associated with the motive anger. If $\kappa_i = 0$, the payoff of the other player does not affect the utility of the focal player (Bosworth et al., 2016). This corresponds to the neoclassical motive of selfish wanting. In the following, we refer to κ_i as the preference signature.

Based on the payoff function, we derive the following theoretical best-response func-

tion for each value of the preference signature κ_i :

$$x_i = ((1 - \kappa_i)b + \kappa_i c + f x_j) / (2((1 - \kappa_i)d - \kappa_i e)) \quad (\text{I.5})$$

The best response is increasing in κ_i . Hence, individuals with a higher κ_i value give unconditionally more in the public goods game. This is due to the properties of the payoff function, since the own contribution has a positive effect on the well-being of the other player.

The calibration of the payoff function is adopted from Potters and Suetens (2009) ($a = -28$, $b = 5.474$, $c = 0.01$, $d = 0.278$, $e = 0.0055$ and $f = 0.165$) and the choice range, the equilibrium choices for the Nash equilibrium and the JPM choice as well as the payoffs are summarized in Table I.2, where π^{defect} is the payoff maximizing best response to full cooperation (see Potters and Suetens (2009) for details of the parametrization). Given this calibration, the best-response function increases with x_j , and higher values of κ_i are associated with steeper slopes. The best-response functions for different values of κ_i are plotted in Figure I.1.

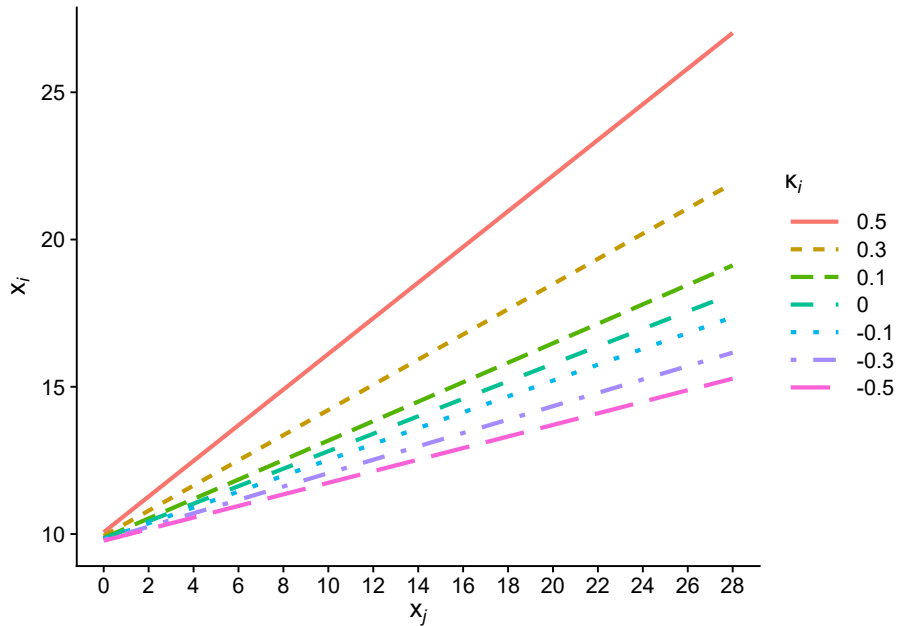
Table I.2: Calibration of the social dilemma game

$Choice^{min}$	0
$Choice^{max}$	28
$Choice^{Nash}$	14
$Choice^{JPM}$	25.5
π^{Nash}	27.71
π^{JPM}	41.94
π^{defect}	60.14

To determine whether individuals with an induced care motive behave more cooperatively and whether this behavior can be explained by a shift in preferences, beliefs or perception, we elicited both unconditional and conditional choices based on the strategy method (Fischbacher et al., 2001)² as well as beliefs and the perception of the game. Controlling for beliefs, the choices from the strategy method enabled us to estimate the individual preference signature of each participant based on the theoretical best-response functions.

²In this method, participants are asked to indicate an action for a range of possible specified actions of their counterpart. The aim is to control for differences in beliefs about one's counterpart's actions.

Figure I.1: Theoretical best-response functions for different values of κ_i



I.2.4 Experimental procedure

The experiment was conducted in the economics laboratory at Kiel University. We recruited 230 participants from the subject pool, which consists of Kiel University students with various scientific backgrounds. The organization and administration were performed using the hroot platform (Bock et al., 2014), and the experiment was set up using the experimental software oTree (Chen et al., 2016). For technical issues, we had to exclude two observations (one couple) because their computers lost their connections to the server and twelve more observations (6 couples) because the dictator’s choices did not reflect the randomly assigned motive treatment, that is, the dictator in the anger treatment opted to keep 2 EUR and send 18 EUR. The remaining 216 participants (96 were female) were distributed across the two treatments, with 102 in the anger treatment and 114 in the care treatment. This corresponds to 108 independent observations.

In each session, the participants were randomly divided into pairs, which stayed the same throughout the experiment. Every participant received the same instructions (see Appendix B). The treatments differed only by the revealed decision in the dictator game, which was randomly determined. In the first stage of the experiment, one of the group members was randomly selected to be the dictator, and the other was the receiver. The dictator made both decisions in a random sequence; only one was randomly revealed to the receiver and, thereby, determined the treatment. The dictator did not know which of the two decisions had been revealed to exclude a

direct effect of the prime on the dictator. In the second stage, the participants played the previously described social dilemma game in the same group for 16 rounds. The first round was a test round and, therefore, was excluded in the final payoff calculation. The payoffs were denoted by points (50 points = 1 EUR) and summed over all rounds.

In each round, the participants were asked to choose a number between 0.0 and 28.0 and to state a belief about the choice of the other player in their group. They were equipped with a payoff table (Appendix B: Figure I.A1) and a calculator, which was implemented as a slider. On this slider, they could enter a hypothetical choice and the belief about the choice of their counterpart. Based on these hypothetical numbers, the participants received information about their own payoff and the payoff of their counterpart. Beliefs were incentivized with 5 points if the stated belief was within ± 1 range of the counterpart's actual choice.

In rounds 1, 2, 6, 11 and 15, the motives were validated as described above, and the participants were asked to state their conditional choices based on the strategy method. This included 15 possible hypothetical choices of the other player: values from 0.00 to 28.00 in steps of two. In these rounds, the choice that received a payoff was randomly selected from the unconditional and conditional choices. If the conditional choice was selected for the payoff, the actual choice was the conditional choice corresponding to the unconditional choice of the other player. If the number was between the predefined hypothetical choices of the counterpart, the conditional choice was linearly approximated between the upper and lower bound as explained to the participants beforehand. At the end of the experiment, participants were asked to state their perception of the nature of the game as either cooperative or competitive.

For the individual payment, it was randomly determined whether the dictator or the social dilemma game was paid out. The sessions lasted between 50 and 75 minutes, and the average payment per participant was 16.33 EUR.

I.2.5 Statistical analysis

The statistical analysis was performed using the computing environment R (R version 4.0.2 (R Core Team, 2016); RStudio version 1.3.1056 (RStudio Team, 2015)). The best-response functions were fitted with the function *mle2* in the *bbmle* package (R Core Team, 2020). Mediation analysis was performed with the *mediation* package (Tingley et al., 2014).

Our dependent variables were as follows:

- Unconditional choices as elicited in the social dilemma game.
- Conditional choices as elicited via the strategy method.
- Beliefs about one’s counterpart’s behavior.
- Perception about the nature of the game.

To study the impact of the motives on those dependent variables, we perform a mediation analysis. The goal of the mediation analysis generally is to decompose the treatment effect into a direct effect of the treatment on the outcome variable and an indirect effect that works through one or more intermediate variables which are manipulated by the treatment. These intermediated variables are called mediators. In economics, mediation analyses have been applied in various settings, such as to evaluate the impact of labor market programs on employment and earnings (Huber et al., 2017), the effect of financial education on financial behavior (Carpena and Zia, 2020), the mechanism of cognitive and non-cognitive skills and the effect of personality traits on health in the Perry Preschool Program (Heckman et al., 2013; Keele et al., 2015; Conti et al., 2016), and the mediating effect of education between growing up poor and economic outcomes in adulthood (Bellani and Bia, 2019).

We first present the results of the traditional mediation approach as developed by Baron and Kenny (1986) and then complement the results with a causal mediation approach as developed by Imai et al. (2010). The traditional approach rests on the following four steps:

- Step 1: We show that the treatment (care vs. anger prime) significantly impacts the dependent variables in the social-dilemma game. This step establishes that there is an effect that may be mediated.
- Step 2: We show that the treatment significantly changes the motive scores. This step establishes that the treatment has an effect on the potential mediator.
- Step 3: We show that the motive scores significantly affect the dependent variables. This, however, is not sufficient to establish mediation because both the outcome variable and the potential mediator are impacted by the treatment.
- Step 4: We perform a multiple regression with the treatment variable and the motive scores, i.e., we control for the direct effect of the treatment on the dependent variables. Mediation is supported if the motive scores continue to have a significant impact on the dependent variables after the treatment is controlled for. If the treatment variable is no longer significant, this finding is called a full mediation, otherwise the finding is called a partial mediation.

To confirm the statistical significance of the indirect effect via the mediator, we complement these results with insights from modern causal mediation techniques (Imai et al., 2010). The causal mediation approach is based on a counterfactual framework. Let Y_i be the outcome variable, T_i the treatment status and M_i the value of the mediator variable for individual i . The observed outcome, Y_i , equals then $Y_i(T_i, M_i(T_i))$, where $M_i(T_i)$ is the observed value of the mediator in the given treatment. The total unit treatment effect is then given by $\tau_i = Y_i(1, M_i(1)) - Y_i(0, M_i(0))$. This effect can be decomposed into a) the *causal mediation effect*, also referred to as the indirect effect,

$$\delta_i = Y_i(t, M_i(1)) - Y_i(t, M_i(0)) \quad (\text{I.6})$$

for the treatment status $t = 0, 1$ and b) the *direct effect* which accounts for all other causal mechanisms

$$\zeta_i = Y_i(1, M_i(t)) - Y_i(0, M_i(t)) \quad (\text{I.7})$$

for the treatment status $t = 0, 1$ (Pearl, 2014; Robins and Greenland, 1992). These two effects sum up to the *total effect*

$$\tau_i = \bar{\delta}_i t + \bar{\zeta}_i (1 - t). \quad (\text{I.8})$$

The *average causal mediation effects (ACME)* $\bar{\delta}_i$ and the *average direct effects (ADE)* $\bar{\zeta}_i$ represent the corresponding population averages. The identification of *ACME* and *ADE* rests on the assumption of *Sequential Ignorability* proposed by Imai et al. (2010). *Sequential Ignorability* consists of the following three conditions:

$$\{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp T_i \mid X_i = x; t, t' \in \{0, 1\} \quad (\text{i})$$

$$Y_i(t', m) \perp\!\!\!\perp M_i(t) \mid T_i = t, X_i = x; t, t' \in \{0, 1\} \quad (\text{ii})$$

$$0 < Pr(T_i = t \mid X_i = x) \text{ and } 0 < Pr(M_i = m \mid T_i = t, X_i = x), t \in \{0, 1\}, \forall x, m \in \text{supp}(X_i, M_i) \quad (\text{iii})$$

where X_i is a vector of the observed pre-treatment confounders for individual i . The first condition (i) of *Sequential Ignorability* implies that the treatment assignment is assumed to be ignorable, i.e., statistically independent of potential outcomes and potential mediators. This is ensured by design in our study, since treatments are assigned randomly. Condition (ii) implies that the mediator is ignorable given the observed treatment and pre-treatment confounders. This condition is quite strong and

will be violated if there are (measured or unmeasured) post-treatment confounders or unmeasured pre-treatment confounders that influence both the mediator and the outcome variable. We address this issue in more detail in section I.3.8. Condition (iii) is a support condition that allows the estimation of treatment effects. Imai et al. (2010) show that under this set of assumptions, it becomes possible to estimate the *ACME* and *ADE* with the following two equations.

$$M_i = \alpha_1 + \beta_1 T_i + \epsilon_{1i} \quad (\text{I.10})$$

$$Y_i = \alpha_2 + \beta_2 T_i + \gamma_2 M_i + \epsilon_{2i} \quad (\text{I.11})$$

The *ACME* is then given by $\hat{\beta}_1 \cdot \hat{\gamma}_2$ and the *ADE* by $\hat{\beta}_2$. The standard errors and confidence intervals can be derived from a quasi-Basian Monte Carlo approximation as implemented by Tingley et al. (2014).

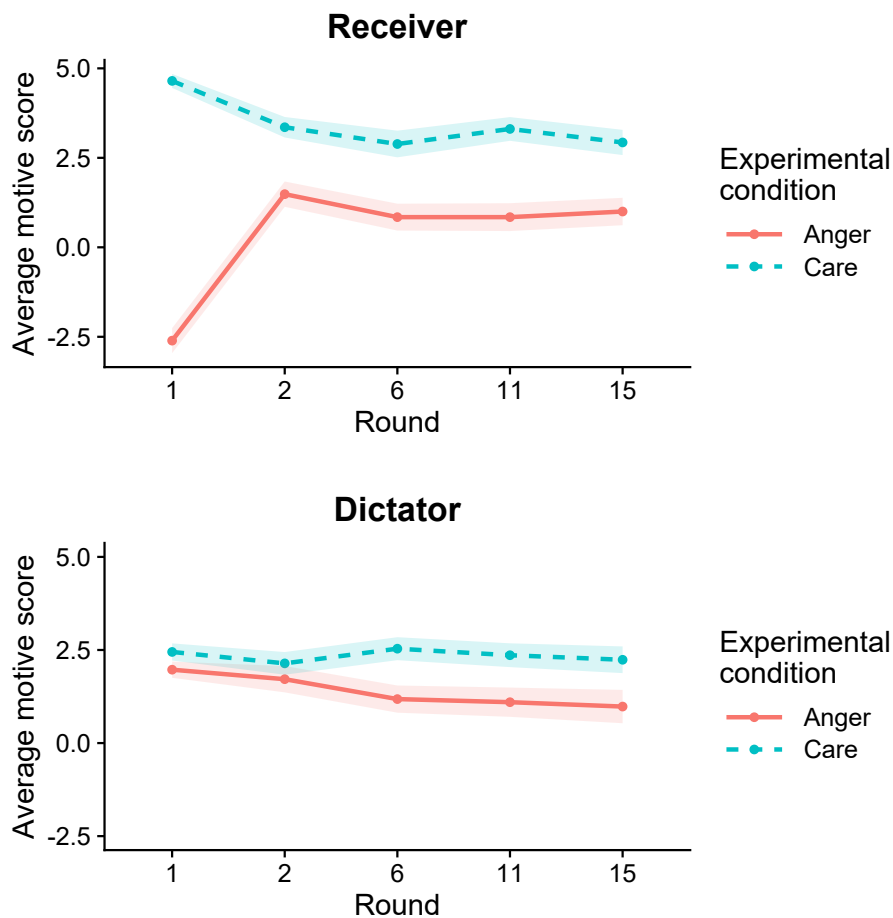
I.3 Results

I.3.1 Motive induction

First, we determine whether the dictator game with a restricted choice set indeed reliably primed the predicted motives of care and anger. In Figure I.2, the average motive scores for the dictator and the receiver by experimental condition and round number are presented. For the receiver in round one, a t-test reveals that the motive score is significantly higher in the care treatment ($M = 4.65$, $SE = 0.20$) than in the anger treatment ($M = -2.61$, $SE = 0.35$; $t(106) = 18.63$, $p < .001$). This finding suggests that the manipulation was successful. Using ordinary least-squares regression with cluster-robust standard errors at the individual level and excluding the first round, we found that the motive scores were persistently higher in the care treatment than they were in the anger treatment ($\beta = 2.08$, $SE = 0.45$, $p < .001$, see Table I.A1). Where it is appropriate, we include the reciprocal of the round number ($1/round$ as `Round_rec`) as an explanatory variable as suggested by Moffatt (2015) in our models. This variable is thought to capture learning effects or other temporal dynamics that could potentially arise as participants become more familiar with the task or for other reasons move to another long-term equilibrium. A positive coefficient for this variable would indicate a decrease in the dependent variable during the experiment. In our analysis, we observe a general decrease in the motive score for the receiver ($\beta = 1.02$, $SE = 0.50$, $p = .046$, see Table I.A1).

As expected, the motive score in the first round is not significantly different for the dictator, who was not informed of which decision was revealed to the receiver (care:

Figure I.2: Average motive scores by experimental condition and round. Shaded areas indicate the standard error.



$M = 2.45$, $SE = 0.23$; anger: $M = 1.97$, $SE = 0.22$; $t(106) = 1.49$, $p = .138$. During the experiment, however, we observe that the motive score was significantly higher in the care treatment than in the anger treatment when the first round is excluded ($\beta = 1.07$, $SE = 0.44$, $p = .017$, see Table I.A1). This result suggests that there was a motivational spillover effect from the receiver to the dictator during their interaction.

To further investigate this effect, in Table I.A2, we describe how the motive score of the receiver in round t affects the motive score of the dictator in round $t+1$. An ordinary least-squares regression with cluster-robust standard errors at the individual level confirms a positive spillover effect ($\beta = 0.19$, $SE = 0.07$, $p = .009$, see Table I.A2). The channel through which this effect operates seems to be the choices of the receiver in the corresponding rounds, as shown in Table I.A2 ($\beta = 0.12$, $SE = 0.03$, $p < .001$, see Table I.A2).

In a nutshell, our analysis shows that the motive priming was successful in elic-

iting the targeted motives. Because we are interested in how the primed motives affect choices, beliefs and preferences, the subsequent analysis is based on mediation analyses.

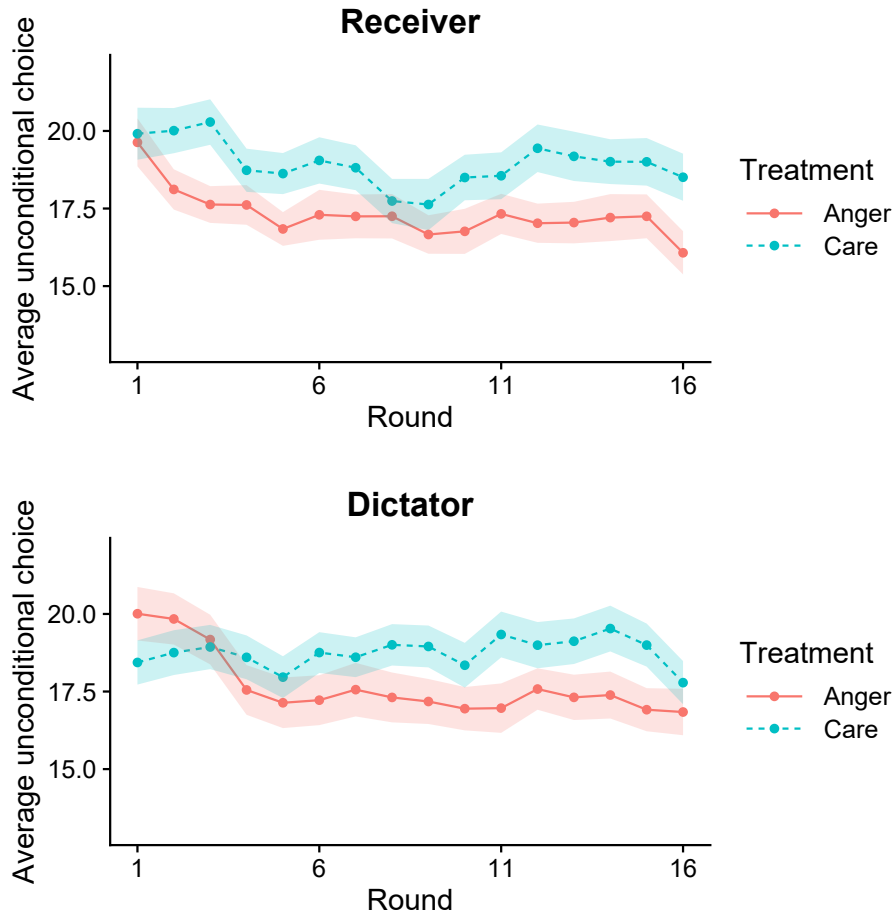
I.3.2 Unconditional choices

Our theory predicts that with the care motive, individuals internalize the positive externalities of their actions to a greater degree than they do with the anger motive. Therefore, the level of cooperation measured by the average number chosen should be greater. Figure I.3 shows the average choices for the receiver and the dictator throughout the experiment separately by treatment. A visual inspection of the data suggests that the average unconditional choices and, therefore, the levels of cooperation for the receiver are higher in the care treatment than in the anger treatment throughout the experiment. In Table I.A3, this visual impression is statistically confirmed by an ordinary least-squares regression with cluster-robust standard errors at the individual level ($\beta = 1.63$, $SE = 0.75$, $p = 0.032$). We also confirm statistically that the motive scores significantly affect the unconditional choices ($\beta = 0.32$, $SE = 0.08$, $p < .001$). When running a multiple regression including both the treatment variable and the motive scores, we find that the motive scores continue to significantly influence the unconditional choices ($\beta = 0.28$, $SE = 0.11$, $p = .017$). The treatment dummy is no longer significant ($\beta = 0.51$, $SE = 0.78$, $p = .510$), which suggests a full mediation of its impact on choices via the motive scores. The significance of the indirect effect is confirmed using the causal mediation approach ($ACME = 0.87$, $95\% CI = [0.17, 1.65]$, $p = .014$, Table I.A4). In all models, we observe a significant positive temporal trend, which suggests that the unconditional choices decrease throughout the experiment ($p < .01$).

For the dictator, we do not observe significant differences in unconditional choices by treatment ($\beta = 1.08$, $SE = 0.78$, $p = .170$, see Table I.A5). For completeness, we report all statistical models necessary to confirm mediation, but given the previous finding, mediation appears unlikely.

Unconditional choices inform us about the level of cooperation with each primed motive. However, it is unclear whether beliefs, preferences or both drive the difference in behavior. To disentangle the effects of beliefs and preferences on observed behavior, we analyze them separately.

Figure I.3: Average unconditional choices by treatment and round. Shaded areas indicate the standard error.

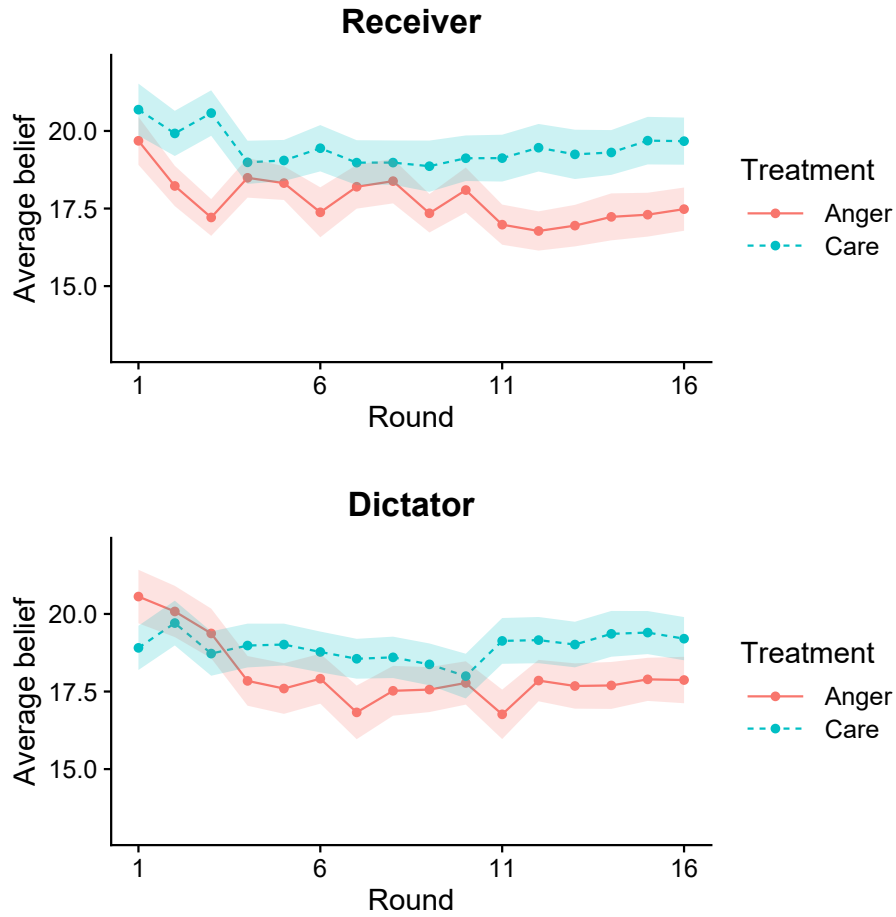


I.3.3 Beliefs

In the next step, we analyze how beliefs about the behavior of one’s counterpart change by treatment and motive. Since we are in a context characterized by strategic complements, individuals have incentives to cooperate more when they believe that their counterparts select larger numbers. Beliefs were incentivized and elicited at the same time when the unconditional choices were recorded. Figure I.4 shows the time course of the elicited beliefs by treatment. The Figure reveals that receivers in the care treatment believe that their counterparts choose higher numbers as compared to individuals in the anger treatment.

This is statistically confirmed in Table I.A6 ($\beta = 1.69$, $SE = 0.78$, $p = .032$). Again, this relation is mediated by changes in motive scores ($\beta = 0.24$, $SE = 0.12$, $p = .049$). The causal mediation analysis in Table I.A7 supports the statistical significance of the indirect effect ($ACME = 0.76$, $95\% CI = [0.01, 1.58]$, $p = .045$).

Figure I.4: Average unconditional choices by treatment and round. Shaded areas indicate the standard error.



For the dictator, we do not observe a significant effect of the treatment on beliefs ($\beta = 0.88$, $SE = 0.78$, $p = .253$, Table I.A8) and mediation therefore appears unlikely.

Please note that we observe a strong correlation between beliefs and unconditional choices ($r(214) = 0.92$, $p < .001$). This correlation holds for both treatments, although the correlation is stronger in the care treatment than in the anger treatment (care: $r(112) = 0.96$, $p < .001$, anger: $r(100) = 0.87$, $p < .001$). Given the nature of the experimental task, which is characterized by strategic complements, the positive correlation between beliefs and unconditional choices is not surprising. As a consequence of the strong correlation, however, we refrain from estimating models including both beliefs and unconditional choices to avoid problems of multicollinearity.

I.3.4 Perception

At the end of the experiment, we asked the participants whether they perceived the nature of the experimental game as a) cooperative or b) competitive. Evidence suggests that a cooperative priming (Drouvelis et al., 2015) and a cooperative perception of a situation (Bartke et al., 2018) increase pro-social behavior. Acknowledging that perceptions matter for economic behavior (Weber, 2004) and that anger (care) is associated with competition (cooperation) (Van Kleef et al., 2008; Chierchia et al., 2017), we hypothesize that individuals motivated by the care motive tend to perceive the experimental game as cooperative, whereas individuals motivated by anger should tend to perceive the game as competitive. In line with these hypotheses, we find that a significantly larger fraction of receivers describe the game environment as cooperative in the care treatment as compared to the anger treatment ($\chi^2 = 8.01$, $p = .005$). For the dictators, we do not observe such a tendency ($\chi^2 = 1.05$, $p = .305$).

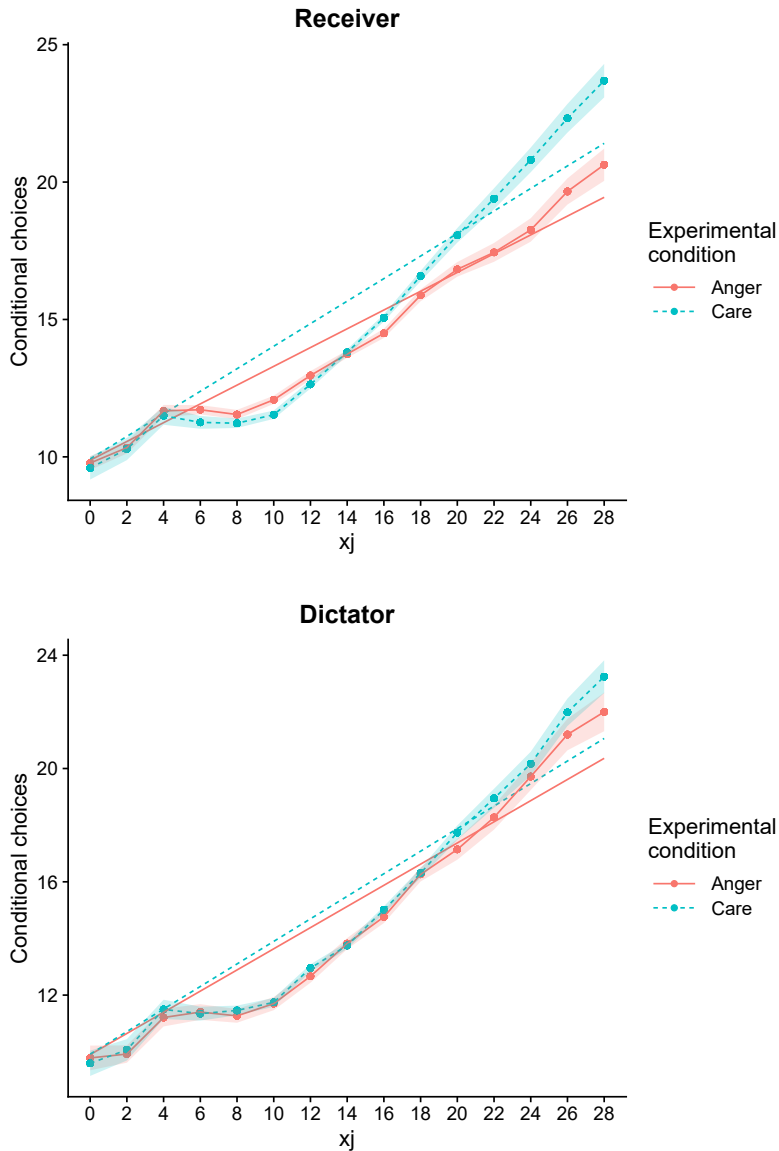
I.3.5 Conditional choices

Finally, we look at conditional choices. By controlling for beliefs, we aim to determine whether the different motives are indeed linked to different preference signatures, i.e., different objective functions. In Figure I.5, we show the average conditional choices by treatment (care vs. anger) and for the receiver and the dictator separately. First, we observe that the conditional choices increase with the counterpart's choice. This finding is not surprising given the nature of the game, which is characterized by strategic complements. Therefore, individuals have stronger incentives to choose higher numbers if their counterparts do so. This effect is significant for both players, as shown in Table I.A9 and Table I.A11 (receiver: $\beta = 0.45$, $SE = 0.02$, $p < .001$; dictator: $\beta = 0.47$, $SE = 0.02$, $p < .001$). The receiver's conditional contributions are significantly higher in the care treatment than in the anger treatment ($\beta = 0.72$, $SE = 0.25$, $p = .004$), and this effect is fully mediated by motive scores ($\beta = 0.11$, $SE = 0.03$, $p < .001$). The significance of the indirect effect is confirmed using the causal mediation approach ($ACME = 0.36$, 95% $CI = [0.17, 0.58]$, $p < .001$, Table I.A10).

For the dictator, there is no significant treatment difference in conditional contributions ($\beta = 0.31$, $SE = 0.28$, $p = .270$, see Table I.A11) making mediation unlikely. As the final step in our analysis, we estimate the preference signatures κ resulting from the induced motives via the conditional contributions. We apply a Maximum Likelihood Estimation technique under the assumption that the residuals are nor-

mally distributed with zero mean. In a nutshell, our procedure aims at finding the κ -value that provides the best fit of the individual best-response function given the observed data. We base this analysis on the individual data from all five measurement points where the conditional choices were elicited to obtain stable estimates.

Figure I.5: Average conditional choices by treatment



The value of κ indicates the degree to which the individuals internalize the well-being of their counterpart. Higher values of κ are associated with more pro-social objectives. The best-response functions based on the estimated values of κ are shown in Figure I.5. In Table I.A12, we observe that receivers have significantly higher values of κ in the care treatment than in the anger treatment ($\beta = 0.20$, $SE = 0.07$, $p = .027$) with a full mediation via the motive scores ($\beta = 0.04$, $SE =$

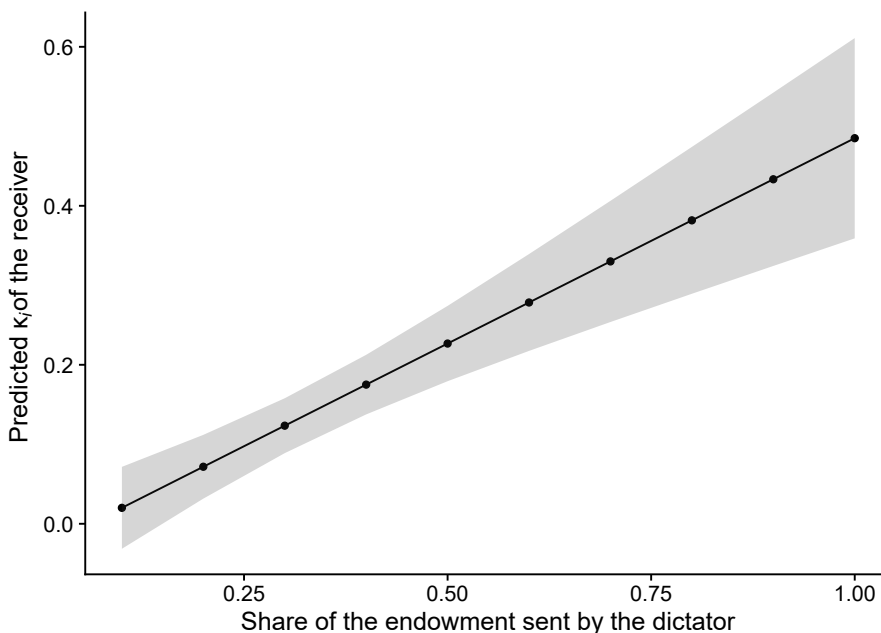
0.02, $p = .017$). Again, the significance of the indirect effect is confirmed by the causal mediation analysis ($ACME = 0.12$, $95\% CI = [0.02, 0.52]$, $p = 0.012$, see Table I.A10)

For the dictator in Table I.A14, we do not observe significant differences in the value of κ between the treatments ($\beta = 0.14$, $SE = 0.10$, $p = .171$) making mediation unlikely.

I.3.6 Quantifying the priming effect

In this section, we quantify the effect of the prime on the preference signature κ . For this purpose, we replace the treatment dummy (Anger = 0; Care = 1) with a continuous variable indicating the percentage shared by the dictator in the initial dictator game (Anger = 0.1; Care = 0.5). We then assume a linear relationship between different endowment splits (0-1) and thereby can obtain predictions for splits that were not included in our treatment variation.

Figure I.6: Predicted κ -values for different endowment splits



In Figure I.6, we find that a selfish dictator who is not sharing any endowment with the receiver results in a predicted κ -value of the receiver which is close to zero. This suggests that as a result of an unequal split at the beginning of the experiment, receivers are not receiving utility by making their partners better off in the subsequent social dilemma game; or in terms of motivation psychology, they are not caring about their partners. On the other side of the spectrum, a full share of the

initial endowment results in receivers that give equal weight to their partners' and their own payoffs ($\kappa=0.5$), and thereby jointly maximize the outcome. Please note, however, that this prediction relies on the assumption about a linear relationship between the κ -values and the shared endowment which needs to be verified in future studies.

I.3.7 Earned points in the social dilemma game

Please note that individuals in the care treatment earned more points ($M = 522.11$, $SE = 10.28$) in the social dilemma game than individuals in the anger treatment ($M = 479.02$, $SE = 16.04$; $t(214) = 2.31$, $p = .022$). However, it was mainly the dictators profiting from the care motive induction (dictator - care: $M = 525.03$, $SE = 14.20$ vs. dictator - anger: $M = 449.57$, $SE = 27.05$; receiver - care: $M = 519.20$, $SE = 14.97$ vs. receiver- anger: $M = 508.48$, $SE = 16.54$).

I.3.8 Robustness

In this section, we discuss the robustness of our results in light of the assumption of *Sequential Ignorability*. Since *Sequential Ignorability* is a strong assumption, it is necessary to understand how our results change due a violation of this assumption. The crucial part of the assumption is condition (ii) which implies that there are no (measured or unmeasured) post-treatment confounders or unmeasured pre-treatment confounders that affect both the mediator and the outcome. We employ the sensitivity analysis proposed by Imai et al. (2010) to evaluate the effect of a violation of this condition on our results. This sensitivity analysis is based on the correlation between the error terms of the equation II.1 and equation II.2 ($\rho = corr(\epsilon_{1i}, \epsilon_{2i})$). A higher correlation indicates a larger bias of the *ACME*. The idea is to relax the assumption of $\rho = 0$ and find the value of ρ at which the *ACME* is zero. For this end, the mediation analysis is calculated with different values of ρ . A high value of ρ at which the *ACME* is zero indicates that the estimate are rather robust to a violation of *Sequential Ignorability*, while small values of ρ indicate a high sensitivity to a violation.

It appears that all four variables are sensitive to a violation of *Sequential Ignorability* ($\rho_{unconditional} = 0.14$, $\rho_{belief} = 0.12$, $\rho_{conditional} = 0.1$ and $\rho_{kappa} = 0.21$). Generally speaking, the results for the preference signature κ are less sensitive than the results for the other variables. One critical point with respect to potential pre-treatment confounders might be that certain personality types could react more or less to the prime than other personality types. For example, an individual with a pro-

social trait may react more to the care prime than an individual with a selfish trait. Therefore, the results of the sensitivity analyses need to be taken into account when interpreting the *ACME* results.

I.4 Discussion

In this paper, we show (1) that motives can be experimentally primed by means of an observance or a violation of a fairness norm and (2) that the primed motives give rise to different behavior in a social dilemma game. The behavior that we observe in our social dilemma game is driven by different beliefs, perceptions as well as different preferences due to the social norm violation. More specifically, we find that individuals with care motives are more pro-social than individuals with anger motives because they internalize the externalities of their actions to a higher degree. Overall, inducing a care motive appears to be one way to achieve more socially desirable outcomes in social dilemmas where individual actions have positive externalities. In summary, we show that other-regarding behavior can be malleable and that motivation psychology can help to explain why.

We acknowledge that other economic models of social behavior such as the belief-dependent reciprocity models of Dufwenberg and Kirchsteiger (2004) and Falk and Fischbacher (2006) offer explanations for the behavior observed in our experiment: The dictator in the care condition treats the receiver kindly in the first stage of the experiment and – as a response – the receiver reciprocates in the subsequent interaction. We believe, however, that motivation psychology offers an explanation of why we observe this kind of correlation in behavior. We utilize the framework of motivation psychology and show that the relations between the initial prime and the subsequent social interaction are mediated by changes in motive scores. Moreover, motivation psychology offers tools to make the initial prime quantifiable. Using motives scores, we are able to quantify the initial priming effect and predict how other endowment splits may impact the subsequent game.

Some of our findings need further elaboration: We find that the motive score for the receiver in the anger treatment is negative after motive induction. This result suggests that the anger motive indeed is activated at that point in time. During the course of the experiment, however, the motive score increases and becomes positive, which suggests that the care motive is activated (although significantly less strongly than it is after the care motive has been primed). This finding is actually in line with the theoretical predictions outlined by Bosworth et al. (2016). Here, a social context characterized by strategic complements favors and activates the care

motive. Therefore, the social interaction within this context potentially overrides the initial anger motive. This effect also plausibly explains why the average value of κ for receivers in the anger treatment is not negative (but is close to zero), as our hypothesis suggests it should be with an anger motive. For receivers in the care treatment, we observe that κ , on average, is 0.23. This finding is in line with our theoretical prediction for the preference signature underlying the care motive. It suggests that while those individuals take the well-being of their counterparts into account, they still put more weight on themselves. For the dictators, all the effects of the treatments, on average, go in the same direction as for the receivers, but they do not reach the significance threshold in all cases. It appears, however, plausible that the effects for the dictators are less pronounced because they are not exposed to the initial strong signal through the dictator game.

Any form of deception should be avoided in experimental economics (Davis and Holt, 1993) and it is debated whether omitting relevant information is deception or not; so far no consensus has been reached (Krawczyk et al., 2013). In our experiment, we withheld information about the exact choice set of the dictator. It was necessary to omit this information, because otherwise we most likely would not have induced the motives of interest. We would like to highlight, however, that the receivers were informed that the dictators would choose from a pre-defined, yet unknown choice set. In that sense, participants held all relevant procedural information and knew the limits of their knowledge.

There are several important differences between our study and the existing literature on the role of motives in economic decision making, such as Bartke et al. (2018) and Chierchia et al. (2017). First, our motive induction method does not rely on any motivational carryover from an unrelated situation to the situation of interest. Our method, in contrast, induces a motive that is directed toward the person the player interacts with later. Second, we study a repeated interaction, whereas previous studies focused on one-shot situations. The repeated interaction allows us to study the dynamics of motives and in particular, how motives change through social interaction and how they affect the motive of one’s counterpart. Third, by controlling for beliefs, we show that motives give rise to different preference signatures.³

Several limitations of our study need to be mentioned. First, our motive validation, which is based on self-reported measures, relies on the assumption that participants are able to be introspective. Without this ability, it would not be possible for

³Chierchia et al. (2017) did not control for beliefs, and therefore, it appears unclear whether beliefs are changing or indeed preferences. Bartke et al. (2018) provide a type analysis based on conditional contributions and observe more pro-social types in the care treatment than in the anger treatment. Their type analysis is in line with our observations.

them to report their current motivational state. Within the literature, the extent to which individuals have this ability has been debated (Zajonc, 1980). Moreover, it is always possible that the participants do not answer truthfully, which also challenges the validity of the self-reported measures. Due to this criticism, it is desirable to introduce new techniques for motive validation. One potential way to do this might be physiological reactions, such as skin conductance or heart rate, because empirical evidence suggests that motives have distinct neurobiological footprints. The care motive, for example, is characterized by a decrease in physiological arousal such as lower skin conductance or inhibition of heart rate (Eisenberg et al., 1988, 1991). The anger motive, in contrast, is associated with heightened physiological arousal, such as higher skin conductance or accelerated heart rate (Levenson et al., 1990; Herrero et al., 2010).

A second limitation of our experimental design might be a wealth effect due to the use of a dictator game with a restricted choice set. It has been shown that social preferences can be affected by such a wealth effect (Chowdhury and Jeon, 2014). In our experimental design, however, we effectively mitigate this effect as only one of the two games is paid out.

Third, it is important to think carefully about how our laboratory results are related to situations that people commonly encounter in practice. In particular, the question of how motives can be induced in the real world appears pressing. There seems to be evidence that motives can be induced via narratives (Akerlof and Snower, 2016), compassion training (McCall et al., 2014; Condon et al., 2013) and supportive social contexts (Crocker and Canevello, 2015).

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Appendix A: Regression Tables

Table I.A1: Regression analysis of motive scores by treatment

	<i>Dependent variable:</i>	
	Motive score	
	Receiver	Dictator
	(1)	(2)
Treatment (Anger = 0, Care = 1)	2.08*** (0.45)	1.07* (0.44)
Round_rec	1.02* (0.50)	0.52 (0.49)
Constant	0.83* (0.37)	1.14** (0.39)
Nr. of observations	432	432
Nr. of clusters	108	108
R ²	0.14	0.04

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A2: Regression analysis of lagged motive scores

	<i>Dependent variable:</i>	
	Motive score of the dictator (at t+1)	
	(1)	(2)
Lagged motive score of the receiver (at t)	0.19** (0.07)	
Lagged choice of the receiver (at t)		0.12*** (0.03)
Round_rec	0.35 (0.54)	0.51 (0.53)
Constant	1.27*** (0.31)	-0.53 (0.70)
Nr. of observations	324	324
Nr. of clusters	108	108
R ²	0.04	0.06
<i>Note:</i>	+ p<.1; * p<.05; ** p<.01; *** p<.001	

Table I.A3: Regression analysis of unconditional choices for the receiver

	<i>Dependent variable:</i>			
	Unconditional choice	Motive score	Unconditional Choices	
	(1)	(2)	(3)	(4)
Treatment (Anger = 0, Care = 1)	1.63* (0.75)	3.11*** (0.39)		0.51 (0.78)
Motive score			0.32** (0.11)	0.28* (0.11)
Round_rec	2.30** (0.73)	-0.74+ (0.38)	2.14** (0.72)	2.11** (0.72)
Constant	16.83*** (0.55)	0.58+ (0.35)	17.25*** (0.46)	17.07*** (0.57)
Nr. of observations	1728	540	540	540
Nr. of clusters	108	108	108	108
R ²	0.03	0.25	0.05	0.05
<i>Note:</i>	+ p<.1; * p<.05; ** p<.01; *** p<.001			

Table I.A4: Mediation analysis of unconditional choices for the receiver.

	Estimate	95%-CI Lower	95%-CI Upper	p-value
$\bar{\delta}$ (<i>ACME</i>)	0.87	0.17	1.65	.014*
$\bar{\zeta}$ (<i>ADE</i>)	0.52	-.94	2.02	.508
$\bar{\tau}$ (<i>ATE</i>)	1.39	-0.07	2.88	.063+
Proportion mediated	0.60	-1.36	3.95	.076+
Nr. of observations				540
Nr. of clusters				108
Nr. simulations				5.000

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A5: Regression analysis of unconditional choices for the dictator

	<i>Dependent variable:</i>			
	Unconditional choice	Motive score	Unconditional Choices	
	(1)	(2)	(3)	(4)
Treatment (Anger = 0, Care = 1)	1.08 (0.78)	0.95* (0.39)		0.38 (0.73)
Motive score			0.31* (0.12)	0.30* (0.12)
Round.rec	1.55+ (0.81)	0.52* (0.24)	1.20 (0.76)	1.20 (0.76)
Constant	17.36*** (0.61)	1.20*** (0.36)	17.51*** (0.51)	17.33*** (0.61)
Nr. of observations	1728	540	540	540
Nr. of clusters	108	108	108	108
R ²	0.01	0.04	0.03	0.03

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A6: Regression analysis of beliefs for the receiver

	<i>Dependent variable:</i>			
	Beliefs	Motive score	Beliefs	
	(1)	(2)	(3)	(4)
Treatment (Anger = 0, Care = 1)	1.69* (0.78)	3.11*** (0.39)		1.10 (0.79)
Motive score			0.33*** (0.09)	0.24* (0.12)
Round_rec	1.98* (0.84)	-0.74+ (0.38)	2.27** (0.78)	2.21** (0.79)
Constant	17.34*** (0.63)	0.58+ (0.35)	17.42*** (0.38)	17.03*** (0.59)
Nr. of observations	1728	540	540	540
Nr. of clusters	108	108	108	108
R ²	0.03	0.25	0.05	0.05

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A7: Mediation analysis of beliefs for the receiver

	Estimate	95%-CI Lower	95%-CI Upper	p-value
$\bar{\delta}$ (<i>ACME</i>)	0.76	0.02	1.58	.045*
$\bar{\zeta}$ (<i>ADE</i>)	1.10	-0.38	2.64	.160
$\bar{\tau}$ (<i>ATE</i>)	1.86	0.38	3.36	.012*
Proportion mediated	0.41	-0.01	1.58	.055+
Nr. of observations				540
Nr. of clusters				108
Nr. simulations				5.000

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A8: Regression analysis of beliefs for the dictator

	<i>Dependent variable:</i>			
	Beliefs (1)	Motive score (2)	Beliefs (3)	Beliefs (4)
Treatment (Anger = 0, Care = 1)	0.88 (0.77)	0.95* (0.39)		0.35 (0.71)
Motive score			0.21* (0.10)	0.20 (0.13)
Round_rec	1.88* (0.81)	0.52* (0.24)	1.56* (0.73)	1.57* (0.79)
Constant	17.65*** (0.60)	1.20*** (0.36)	17.95*** (0.38)	17.79*** (0.61)
Nr. of observations	1728	540	540	540
Nr. of clusters	108	108	108	108
R ²	0.01	0.04	0.02	0.02

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A9: Regression analysis of conditional choices for the receiver

	<i>Dependent variable:</i>			
	Conditional choices	Motive scores	Conditional choices	
	(1)	(2)	(3)	(4)
x_j	0.45*** (0.02)		0.45*** (0.02)	0.45*** (0.02)
Treatment (Anger = 0, Care = 1)	0.72** (0.25)	3.11*** (0.39)		0.36 (0.25)
Motive score			0.14*** (0.03)	0.11*** (0.03)
Round_rec	0.07 (0.14)	-0.74* (0.38)	0.21 (0.16)	0.18 (0.16)
Constant	8.19*** (0.28)	0.58+ (0.35)	8.22*** (0.25)	8.10*** (0.28)
Nr. of observations	8100	540	8100	8100
Nr. of clusters	108	108	108	108
R ²	0.62	0.25	0.62	0.62

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A10: Mediation analysis of conditional choices for the receiver

	Estimate	95%-CI Lower	95%-CI Upper	p-value
$\bar{\delta}$ (ACME)	0.36	0.17	0.58	< .001***
$\bar{\zeta}$ (ADE)	0.36	-0.12	0.84	.143
$\bar{\tau}$ (ATE)	0.72	0.24	1.19	.002**
Proportion mediated	0.50	0.21	1.45	.002**
Nr. of observations				8.100
Nr. of clusters				108
Nr. simulations				5.000

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A11: Regression analysis of conditional choices for the dictator.

	<i>Dependent variable:</i>			
	Conditional choices	Motive scores	Conditional choices	
	(1)	(2)	(3)	(4)
x_j	0.47*** (0.02)		0.47*** (0.02)	0.47*** (0.02)
Treatment (Anger = 0, Care = 1)	0.31 (0.28)	0.95* (0.39)		0.17 (0.26)
Motive score			0.15*** (0.05)	0.15*** (0.04)
Round_rec	0.53* (0.26)	0.64** (0.24)	0.43* (0.24)	0.43* (0.24)
Constant	7.95*** (0.37)	1.09** (0.36)	7.87*** (0.33)	7.79*** (0.38)
Nr. of observations	8100	8100	8100	8100
Nr. of clusters	108	108	108	108
R ²	0.58	0.04	0.58	0.58

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A12: Regression analysis of κ -values for the receiver

	<i>Dependent variable:</i>			
	κ	Motive scores	κ	
	(1)	(2)	(3)	(4)
Treatment (Anger = 0, Care = 1)	0.20*** (0.07)	3.11*** (0.39)		0.08 (0.09)
Motive score			0.05*** (0.01)	0.04** (0.02)
Constant	0.03 (0.05)	0.31 (0.29)	0.04 (0.04)	0.01 (0.05)
Nr. of observations	108	108	108	108
R ²	0.07	0.37	0.11	0.12

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A13: Mediation analysis of κ -values for the receiver

	Estimate	95%-CI Lower	95%-CI Upper	p-value
$\bar{\delta}$ (<i>ACME</i>)	0.12	0.02	0.21	.012*
$\bar{\zeta}$ (<i>ADE</i>)	0.09	-0.02	0.20	.129
$\bar{\tau}$ (<i>ATE</i>)	0.20	0.06	0.35	.004**
Proportion mediated	0.58	0.19	1.28	.012*
Nr. of observations				108
Nr. simulations				5.000

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table I.A14: Regression analysis of κ -values for the dictator

	<i>Dependent variable:</i>			
	κ	Motive scores	κ	κ
	(1)	(2)	(3)	(4)
Treatment (Anger = 0, Care = 1)	0.14 (0.10)	0.95** (0.38)		0.12 (0.11)
Motive score			0.03 (0.03)	0.02 (0.03)
Constant	0.06 (0.08)	1.39*** (0.28)	0.08 (0.07)	0.03 (0.08)
Nr. of observations	108	108	108	108
R ²	0.02	0.05	0.01	0.02

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Appendix B: Instructions

General Information

You are participating in a study on economic decision making, and in the following you will be asked to make a number of decisions. Please read the following instructions carefully. You have the opportunity to earn money that will be paid out in private and cash at the end of the study. During the study, you are not allowed to talk to the other participants. If you have questions, please raise your hand and one of the experimenters will approach you and answer your question.

At the beginning of the study, another participant will be assigned to you. This participant remains the same during the entire experiment. His identity will not be revealed.

The study consists of two parts. At the end of the study, one of the two parts will be randomly selected as payoff relevant, i.e., this part will be paid out according to your decisions and the decisions of the other participant. Since you do not know which part of the study is payoff relevant, you should always behave as if the current part would be paid out.

Dictator game

In the first part of the study one of you will participate as participant 1 and the other as participant 2. Before you have to make a decision, you will be informed about your role which is randomly assigned.

In this part of the study, 20 Euros have to be divided. Participant 1 can decide which amount he wants to keep for himself from a pre-defined set of alternatives. The remaining amount is transferred to participant 2.

Social dilemma game: General Information

In part II of the study, you continue playing with the same participant which has been assigned to you in part I. In the following, he/she will be referred to as participant B.

In this part of the study, your payoff depends on your decision and the decision of the other participant. It consists of 16 rounds, whereas the first round is a test round.

In each round, you choose a number between 0.0 and 28.0. Participant B also chooses a number between 0.0 and 28.0. Your payoff depends on both decisions.

The table attached gives information about your payoff depending on your own decision and the decision of participant B in steps of two. The columns contain

your possible payoffs for the respective numbers of participant B. Participant B receives the same table.

Additionally, you can calculate your own payoff and the payoff of participant B in more detail with a calculator on your screen. Here you can enter your hypothetical decision and a hypothetical decision of participant B to calculate the hypothetical payoffs.

Please enter your final decision and the expectation regarding the decision of participant B in the corresponding fields. Then click next.

The final payoff of this part of the study is determined by summing up all collected points. If you correctly guessed the decision of participant B (+1), you additionally receive 5 points. The points will be converted into Euros according to the following rate: 50 points = 1 Euro.

You have about 1 minute to enter your decision.

Social dilemma game: Test round

In the first round, you have the opportunity to get familiar with the task and the calculator. This round is a test round and therefore not payoff relevant.

The payoff calculator is implemented as a slider, which can be handled with the mouse or the arrow keys on your keyboard. On the upper slider, you can enter your own hypothetical decision and on the lower slider you can enter a hypothetical decision of participant B. Your payoff and the payoff of participant B will be displayed accordingly.

Use this round to understand how your decision affects your payoff and the payoff of participant B. Then enter your decision and the expectation about the decision of participant B in the corresponding fields and click on next.

In several rounds, you will be asked additionally which number you would choose based on hypothetical decisions of participant B. Fifteen predefined hypothetical decisions of participant B will be displayed. Your task is to state which number you would choose for each hypothetical decision of participant B.

Again, you can use a slider to calculate the payoffs. The fields will be filled automatically with the slider. Please click next when you have entered all decisions.

In these rounds, your actual payoff will be randomly determined from your hypothetical decisions and your actual choice. If your hypothetical decision is payoff relevant, your hypothetical decision will be chosen based on the actual decision of participant B. If this number is between the predefined hypothetical decisions of participant B, your number will be linear approximated between the relevant predefined hypothetical decisions of participant B.

Since you do not know which decision will be payoff relevant, you should always behave as if each decision will be paid out.

Figure I.A1: Payoff table for the social dilemma game

	Decision of participant B															
	0	2	4	6	8	10	12	14	16	18	20	22	24	26	28	
Your decision	0	-28,00	-27,96	-27,87	-27,74	-27,57	-27,35	-27,09	-26,78	-26,43	-26,04	-25,60	-25,12	-24,59	-24,02	-23,41
2	-18,16	-17,46	-16,72	-15,93	-15,09	-14,21	-13,29	-12,33	-11,32	-10,26	-9,16	-8,02	-6,84	-5,61	-4,33	
4	-10,55	-9,19	-7,78	-6,33	-4,84	-3,30	-1,72	-0,09	1,58	3,29	5,05	6,85	8,70	10,59	12,52	
6	-5,16	-3,14	-1,08	1,03	3,19	5,39	7,63	9,91	12,24	14,62	17,04	19,50	22,00	24,55	27,15	
8	-2,00	0,68	3,41	6,18	8,99	11,85	14,75	17,70	20,69	23,72	26,80	29,92	33,09	36,30	39,55	
10	-1,06	2,28	5,67	9,10	12,57	16,09	19,65	23,26	26,91	30,60	34,34	38,12	41,95	45,82	49,73	
12	-2,34	1,66	5,70	9,79	13,93	18,11	22,33	26,59	30,90	35,26	39,66	44,10	48,58	53,11	57,69	
14	-5,85	-1,19	3,52	8,27	13,06	17,90	22,78	27,71	32,68	37,69	42,75	47,85	53,00	58,19	63,42	
16	-11,58	-6,26	-0,90	4,51	9,97	15,47	21,01	26,59	32,22	37,90	43,62	49,38	55,18	61,03	66,93	
18	-19,54	-13,56	-7,53	-1,46	4,65	10,81	17,01	23,26	29,55	35,88	42,26	48,68	55,15	61,66	68,21	
20	-29,72	-23,08	-16,39	-9,66	-2,89	3,93	10,79	17,70	24,65	31,64	38,68	45,76	52,89	60,06	67,27	
22	-42,12	-34,82	-27,48	-20,09	-12,65	-5,17	2,35	9,91	17,52	25,18	32,88	40,62	48,40	56,23	64,11	
24	-56,75	-48,79	-40,78	-32,73	-24,64	-16,50	-8,32	-0,09	8,18	16,49	24,85	33,25	41,70	50,19	58,72	
26	-73,60	-64,98	-56,32	-47,61	-38,85	-30,05	-21,21	-12,33	-3,40	5,58	14,60	23,66	32,76	41,91	51,11	
28	-92,68	-83,40	-74,07	-64,70	-55,29	-45,83	-36,33	-26,78	-17,19	-7,56	2,12	11,84	21,61	31,42	41,27	

II. The Effect of Perceived Similarity and Social Proximity on the Formation of Prosocial Preferences

by Christoph Schütt

Abstract

Homophily, the tendency of interacting with similar others, has been found to be an important determinant of the existence and stability of social groups, whereby social relationships within these groups vary in the degree of social proximity. In this paper, I investigate how perceived similarity affects social proximity and care towards a partner and altruistic giving. In a between subjects design, subjects are matched with an either similar or dissimilar partner and play a dictator game. Similarity is induced via objective similarities on pre-elicited personality profiles based on Big-Five personality traits. I show that perceiving someone as more similar increases giving in a dictator game. A mediation analysis shows that social proximity and care towards the partner mediate the treatment effect. That is, subjects in the similar treatment feel a higher degree of social proximity to their partner leading to a higher degree of care towards the latter, and therefore, give more in the dictator game.

JEL Classification: A12, C71, D01, D62, D64

Keywords: Social Proximity, Care, Dictator Game, Other-regarding Preferences

II.1 Introduction

Social relations and social belonging are central human needs and are crucial to cooperation and well-being among humans and within societies (Baumeister and Leary, 1995). The absence of social relations, i.e., social isolation and loneliness have severe consequences for physical and mental health, which has been particularly visible in the recent corona pandemic (Kearns et al., 2015; Ford, 2021). A central dimension that characterizes social relations is social proximity, which structures social and economic life and varies from family, close friends, colleagues and distant acquaintances to incidental encounters (Gächter et al., 2015). In the behavioral economics literature, social proximity has been found to be an important determinant of prosocial behavior and cooperation in various contexts. In dictator games, the literature shows that social proximity toward the experimenter (Hoffman et al., 1996, 1998), the identification of the counterpart (Bohnet and Frey, 1999; Charness and Gneezy, 2008) and the presence of social networks in the lab (Branas-Garza et al., 2010; Goeree et al., 2010) and in the field (Candelo et al., 2018; Bezu and Holden, 2013; Binzel and Fehr, 2013; Leider et al., 2009) have a significant impact on giving. A central claim is that social proximity increases care towards the other person. This might be due to an identifiable victim effect (Bohnet and Frey, 1999) or the motivation of promoting the well-being of close others (Sober and Wilson, 2021). Since social proximity is an I-centric concept, it allows the internalization of others well-being into ones own preferences. Therefore, the mutual benefit becomes part of the individual maximization of well-being.

Social proximity might be static in a societal context in terms of social classes or social status (Akerlof, 1997; Snower and Bosworth, 2016) or change dynamically in an interpersonal context through social interaction. Sharing a common experience or interest, having the same profession or even sharing a cabin in a train may increase social proximity based on the perception of similarity. Such social interactions may facilitate cooperation and might reduce prejudices against other social or ethnic groups (Allport et al., 1954; Grimalda et al., 2021). The associated concept - homophily - which describes the phenomenon that individuals prefer to engage with similar others is a well established finding across a variety of dimensions such as age, ethnicity, class, religion, personality and interests. As such, homophily is the basis for friendships, partner selection and the formation of social networks (McPherson et al., 2001). Besides stable relationships, perceived similarity and homophily are important in incidental interactions that may or may not be repeated. It has been shown that perceived similarity predicts voting behavior (Caprara et al., 2007; Vecchione et al., 2011), persuasion in contracting (Pandelaere et al., 2010) and consumer

behavior (Jiang et al., 2010) and also has a stronger effect on giving compared to group assignments by means of the minimal group paradigm (Carrarini and Mengel, 2016).

Indeed, sorting things into the categories of similar and dissimilar is inherent to human thinking and has two important functions. First, it helps to deal with uncertainty by projecting own preferences onto others if they are similar or using stereotyping in the case of dissimilar others (Ames, 2004a,b). Second, similarity can facilitate social learning by identifying similar others and imitating their strategy (Riolo et al., 2001; Efferson et al., 2016).

In this context, I present a minimal design to test the mechanism that increasing perceived similarity increases social proximity towards a partner. The increase in social proximity activates a motive of care that leads to more giving in a dictator game. Perceived similarity is induced via personality profiles based on the actual, pre-elicited Big-Five scores of the subjects. In a mediation analysis, I statistically identify this mechanism and support the finding that perceiving someone as more similar indeed changes the motivation of giving in the dictator game.

The contribution of this paper is threefold. First, we provide evidence for the effect of homophily on altruistic giving. That is, we show that being paired with a more similar partner increases giving in a dictator game. Second, I provide a mechanism for this effect via social proximity and care. The literature on social proximity and social preference typically argues that social proximity affects giving by increasing care, however, this mechanism has never been explicitly tested. My results give strong evidence in favor of such a mechanism. Third, on a methodological level, I present a novel approach to induce perceived similarity, based on objective similarity that does not rely on personal taste or attitude in accordance with experimental economics practices.

The remainder of the paper is structured as follows. In section 2, the methods are presented. Section 3 presents the results and section 4 concludes with a discussion of the results.

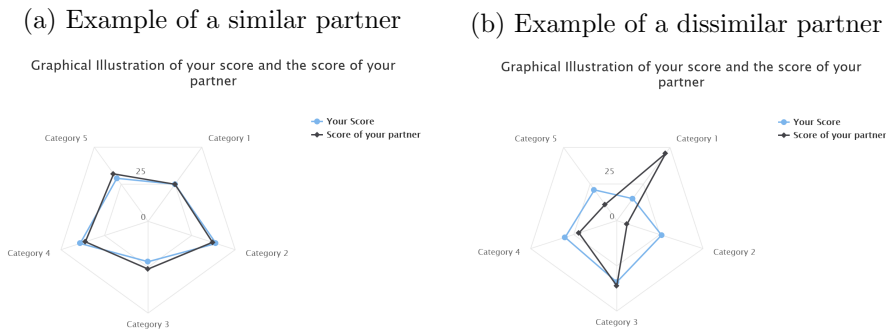
II.2 Methods

II.2.1 Experimental design

The experiment consisted of two stages, a recruitment stage and the experimental stage. In the recruitment stage, subjects were recruited from a student pool at Kiel University via Hroot ((Bock et al., 2014)) and completed a personality questionnaire based on the Five-Factor Model (Costa and McCrae, 1992). 657 subjects completed

the questionnaire and formed the matching pool. In the experimental stage, 110 subjects¹ from this pool were invited to the Kiel Experimental Economics Laboratory (KEEL) and were randomly assigned to one of the two treatments². In the *similar* treatment, 56 (34 female) subjects were matched with the person from the matching pool whose personality profile had the smallest Euclidean distance to their own, while in the *dissimilar* condition 54 (24 female) subjects were matched with the person from the matching pool whose personality profile had the largest distance. The respective partner was not present during the experiment, but was paid out afterwards according to the respective decisions. At the beginning of the experiment, the own personality profile and the personality profile of the partner were displayed in a spider plot (Figure III.2). To prevent subjects from forming lay theories about their partners from individual personality traits, the axes of the spider plot read only “category 1-5”. No other personal information about their partner was revealed.

Figure II.1: Graphical illustration of the partner matching



In the first part of the experiment, subjects completed two risk tasks: one in the gain domain taken from Holt and Laury (2002) and one in the mixed domain taken from Gächter et al. (2007), for themselves and for their partner and for both of them³. In the second part, the subjects played a dictator game with their partner. In the dictator game, the subjects received €10 and had to decide how much they want to transfer to their partner. In the post-experimental questionnaire subjects rated their perceived social proximity based on the *Inclusion of Others in the Self (IOS)* scale (Gächter et al., 2015; Aron et al., 1992) and care towards their partner based on words associated with the motive care. The *IOS* scale is a graphical representation based on overlapping circles, on which subjects could state their perceived social proximity to their partner on a scale from 1 to 7 (see Figure III.A9 in Appendix

¹Due to a server error, data on the IOS scale for 27 subjects was lost.

²To maintain anonymity, each subject generated an individual subject code in the recruitment stage. The invited subjects had to enter this subject code before the experimental session.

³Results are reported in Krause et al. (2021)

A). The words that were used to validate the activation of the motive care are taken from the word atlas by Chierchia et al. (2021) consisting of words that people associate with different motives. Specifically, *care* and *compassionate* as well as the importance of the partners' payoff were used, for which subjects rated themselves on seven-point Likert scales. To exclude that any treatment differences are driven by sample differences in empathy, the *Basic Empathy Scale* in a German validated version (Heynen et al., 2016) was elicited.

II.2.2 Statistical analysis

The aim of this paper is to test whether (i) perceived similarity increases social proximity to a partner, (ii) the associated higher social proximity leads to a higher degree of care towards the partner and (iii) if the higher degree of care leads to more giving in a dictator game. To identify this mechanism, a serial multiple mediation analysis is conducted.

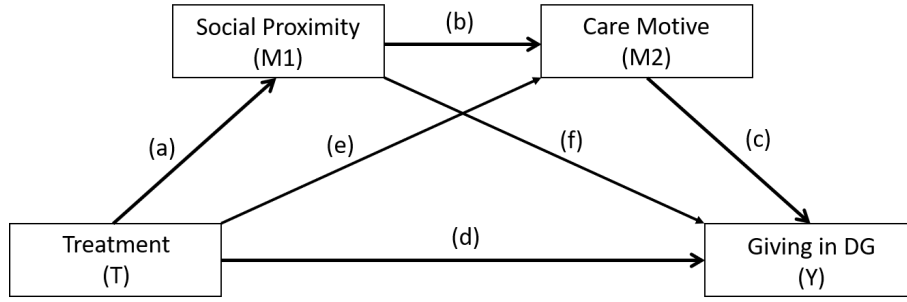
Mediation analysis is typically used in psychology. However, it has recently been used in economics to identify the effectiveness of training programs and personality traits on economic outcomes (e.g. Keele et al., 2015; Heckman et al., 2013; Huber et al., 2017a,b; Carpena and Zia, 2020; Conti et al., 2016) as well as the effect of motives on cooperation in social dilemmas (Ring et al., 2021).

In general, a mediation analysis decomposes the treatment effect into a direct and an indirect effect. The direct effect is the pure treatment effect on the outcome variable, hence the effect of perceived similarity on giving in the dictator game. The indirect effect measures the effect of intermediate variables, in this case social proximity and care, on the outcome variable, which are manipulated by the treatment.

In particular, a serial multiple mediation analysis assumes a sequential relationship between the mediators. Hence, the proposed indirect effect works through social proximity and care sequentially, rather than separately as represented by the path diagram depicted in Figure II.2. The sequential relationship is theory driven and statistically justified, since perceived social proximity and care are correlated ($r(91) = .36, p < .001$), which makes a parallel design unlikely.

The common approach to mediation analysis is structural equation modelling. In contrast to a regression analysis proposed by Baron and Kenny (1986), structural equation modelling fits a theoretical structure to the data by simultaneously estimating a system of equation (Hayes, 2017; Preacher et al., 2007). This has the advantage, that the mediation effect can be quantified and determined even in the presence of effects that go in opposite directions (Keele et al., 2015). The system of equations represents the sequential nature of the model, and is given by the following

Figure II.2: Mediation Path



three equations:

$$M_1 = \alpha_1 + \beta_1 T + \zeta_1 X + \epsilon_1 \quad (\text{II.1})$$

$$M_2 = \alpha_2 + \beta_2 T + \gamma_2 M_1 + \zeta_2 X + \epsilon_2 \quad (\text{II.2})$$

$$Y = \alpha_3 + \beta_3 T + \gamma_3 M_1 + \delta_3 M_2 + \zeta_3 X + \epsilon_3 \quad (\text{II.3})$$

The effect of the treatment (T) on social proximity (M_1) is estimated by equation II.1. The degree of care (M_2) is determined by the treatment (T) and social proximity (M_1) in equation II.2. And finally, equation II.3 determines giving in the dictator game (Y) by the treatment (T), social proximity (M_1) and care (M_2). The variable X is a control variable that captures the empathy score of the subjects. ϵ_1 , ϵ_2 and ϵ_3 represent the error terms and α_1 , α_2 and α_3 are the constants of the respective equations. The estimation results in a direct effect ($\hat{\beta}_3$) corresponding to the path (d) in Figure II.2 and five partial indirect effects depicted as path (a)-(e). The total indirect effect that corresponds to the proposed mechanism ((a)-(c)) is given by the product of coefficient $\hat{\beta}_1 \cdot \hat{\gamma}_2 \cdot \hat{\delta}_3$ (MacKinnon et al., 2002). This is a valid estimate under the following assumptions: (i) The relationship between the variables is indeed linear and additive, (ii) the effect of the mediators on the outcome does not interact with the treatment status and (iii) Sequential Ignorability holds (Jo, 2008). Sequential Ignorability implies that the treatment assignment is statistically independent from potential outcomes and that there are no pre- and post-treatment confounds that affect both mediators and outcome. The first part of Sequential Ignorability is satisfied by design, since treatments are assigned randomly. The second part is a rather strong assumption (see Heckman et al., 2013; Imai and Yamamoto, 2013, for a detailed discussion).

The findings of the estimation is called a full mediation if the total indirect effect ((a)-(c)) is significant, while the direct effect (d) and the partial mediation (e) and (f) are not.

The analysis is conducted in R with the *laavan* package (Rosseel, 2012). Confidence intervals are calculated with the bootstrap method.

II.3 Results

II.3.1 Manipulation Check

To confirm that the treatment manipulates perceived similarity, subjects were asked to state how similar they had perceive their partner after they received the information on the Big-Five Scores of their partner. For this purpose, they rated themselves on a Five-point Likert-scale ranging from "not similar at all" to "very similar". A Wilcoxon-rank-sum test reveals that subjects in the *similar* treatment ($M = 3.79$, $SE = 0.089$) reported a significantly higher perceived similarity to their partner than subjects in the *dissimilar* treatment ($M = 0.94$, $SE = 0.055$) ($z = -9.471$, $p < 0.001$). This indicates that the similarity manipulation worked.

II.3.2 Main results

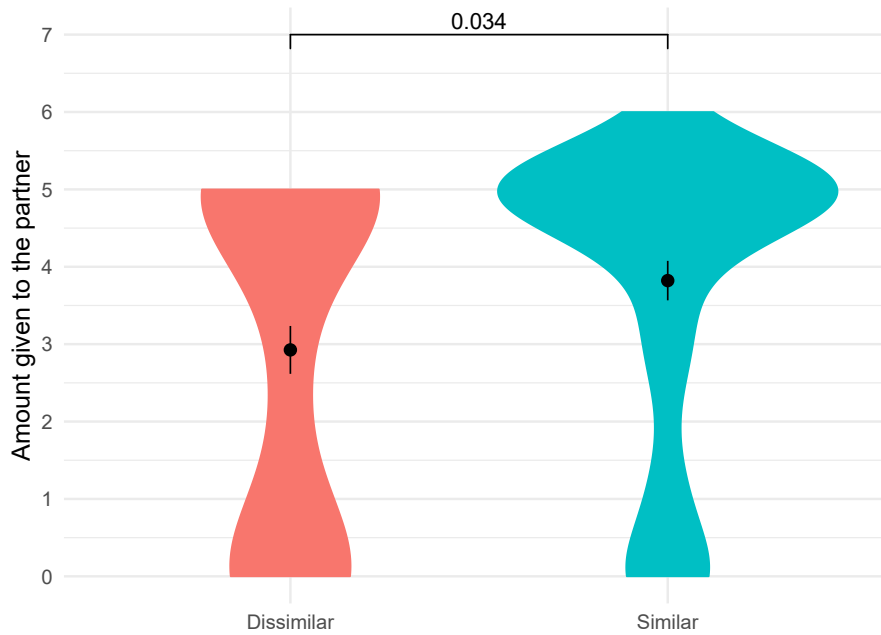
As expected, subjects give significantly more in the *similar* treatment ($M = 3.82$, $SE = 0.255$) compared to subjects in the *dissimilar* treatment ($M = 2.93$, $SE = 0.309$). Figure II.3 presents the average treatment effect on giving in the dictator game, which amounts to almost one Euro difference between the treatments. This effect is statistically significant at the 5%-level (Wilcoxon-rank-sum test: $z = -2.123$, $p = 0.034$).

II.3.3 Mediation analysis

To establish a mediation, it is necessary to show that the mediators, social proximity ($M1$) measured on the *IOS* and care ($M2$) based on the self-rating on the associated words, are manipulated by the treatment. A visual inspection of Figure II.4 suggests that subjects in the *similar* treatment perceive their partner as socially closer and have a higher degree of care towards their partner, compared to subjects in the *dissimilar* treatment. A Wilcoxon-rank-sum test confirms this impression for both, social proximity ($z = -5.497$, $p < 0.001$) and care ($z = -2.977$, $p = 0.003$). Hence, both mediators are manipulated by the treatment in the predicted way.

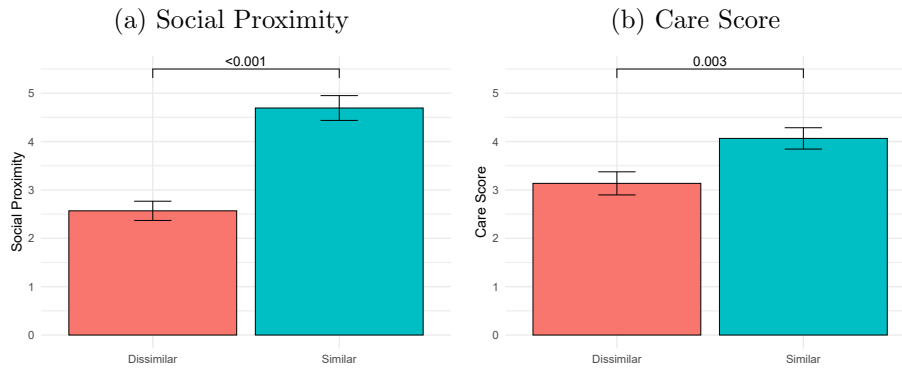
In the final step, the serial multiple mediation analysis is conducted to test the proposed mechanism statistically. Figure II.5 shows the coefficients and significant levels that correspond to the respective paths in Figure II.2. As presented earlier, the

Figure II.3: Giving in the Dictator game by Treatment.



Note: Mass and distribution of subjects' contributions in the dictator game. The p-value corresponds to a Wilcoxon-rank-sum test with $N = 110$.

Figure II.4: Care Score and Social Proximity

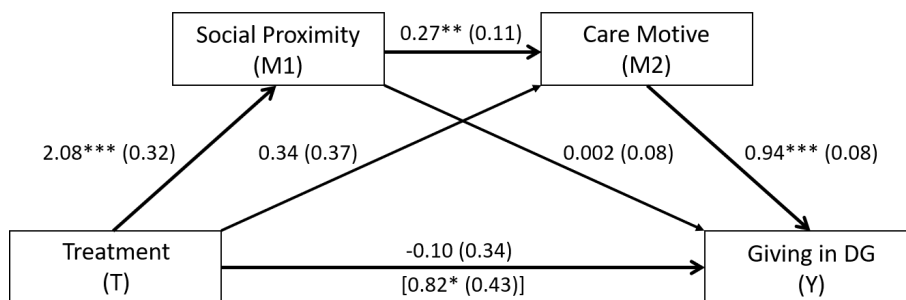


Note: The p-values correspond to a Wilcoxon-rank-sum test with $N = 93$ in panel (a) due to the server error and $N = 110$ in panel (b).

treatment significantly influences social proximity towards the partner. This effect is replicated and confirmed by equation II.1 in the mediation analysis ($\hat{\beta}_1 = 2.08$, $SE = 0.33$, $p < 0.001$). Based on equation II.2 it can be shown that social proximity ($\hat{\gamma}_2 = 0.27$, $SE = 0.108$, $p < 0.013$) significantly affects care controlling for the treatment. Finally, the estimation of equation II.3 confirms that care significantly influences giving in the dictator game ($\hat{\delta}_3 = 0.94$, $SE = 0.08$, $p < 0.001$) controlling for both

social proximity and the treatment. The remaining partial mediation effects, namely the direct influence of the treatment on the motive care ($\hat{\beta}_2 = 0.340$, $SE = 0.362$, $p < 0.347$) and the direct influence of social proximity ($\hat{\gamma}_3 = 0.002$, $SE = 0.08$, $p = 0.982$) on giving in the dictator game, are not significant. Calculating the total indirect effect reveals that the indirect path significantly affects giving in the dictator game ($\hat{\beta}_1 \cdot \hat{\gamma}_2 \cdot \hat{\delta}_3 = 0.53$, $SE = 0.23$, $p = 0.024$), while the direct effect becomes insignificant ($\hat{\beta}_3 = -0.10$, $SE = 0.36$, $p = 0.780$). This indicates a full mediation of the treatment effect through the proposed theoretical mechanism (see Table II.A1 in Appendix A).

Figure II.5: Mediation Analysis



Note: Coefficients and significance levels are based on the SEM estimation of the proposed mediation model (see Table II.A1 in Appendix A). Standard errors are in parenthesis. The estimate in the square brackets corresponds to a OLS Regression of giving in the dictator game on the treatment with $N=93$ ($\beta = 0.82$, $SE = 0.43$, $p = 0.064$). The significant levels are: + $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

II.4 Conclusion

I presented a minimal design to statistically identify the mechanism that increasing perceived similarity leads to an increase in social proximity and thereby increases care towards a partner which results in more giving in a dictator game. In a between subjects design, subjects were matched with either a similar or a dissimilar partner. Social proximity was elicited using the *IOS* scale, and care based on self-rating on words associated with the latter motive. A serial multiple mediation analysis revealed that this mechanism is significant, and explains the treatment variation in giving controlling for prosocial traits. This finding supports the idea that arbitrary similar characteristics lead to different degrees of care towards a partner. Furthermore, the design offers a tool to induce similarity based on real characteristics rather than tastes or random group assignments, and thereby maintaining anonymity among subjects. There are several limitations to the study. First, the

representation of the spider plots is rather similar to the self-rating on the *IOS* scale, which could lead to an experimenter demand effect. However, since the experiment consisted of three tasks and the *IOS* elicitation was done in the post-experimental questionnaire, there was a substantial amount of time between the visualization of the personality profiles, and the *IOS* elicitation. Therefore, I am confident that a possible experimenter demand effect is at most minor. Secondly, the elicitation of the motive care is based on self-reporting. This implies that subjects are able to correctly assess their motivation and that they report them truthfully. If this is indeed possible is controversially debated in the literature (Zajonc, 1980). One possibility to overcome this problem is to use physical markers, such as skin conductance or heart rates, that are associated with the motive care (Ring et al., 2021).

Overall, the results have important implications. If it is indeed the case that similarity increases care among people, which implies that others are integrated into their own utility maximization cooperation in specific situations within societies, could be increased and discrimination and polarization could be reduced by highlighting similarities or creating common experiences. This might be achieved by the creation of common narratives (Akerlof and Snower, 2010; Warner et al., 2020; Ordabayeva, 2019) or by creating places of exchange such as volunteering (Grimalda et al., 2021).

Acknowledgments

Financial support by the Institute for New Economic Thinking is gratefully acknowledged. I also thank Dennis Snower, Steven J. Bosworth, Patrick Ring, Jan Krause, Katharina Lima de Miranda, Felix Gelhaar and Simon Bartke, Menusch Khadjavi Pour, Ulrich Schmidt, Gianluca Grimalda and Ute Losch for comments on the design and on an earlier version of the manuscript.

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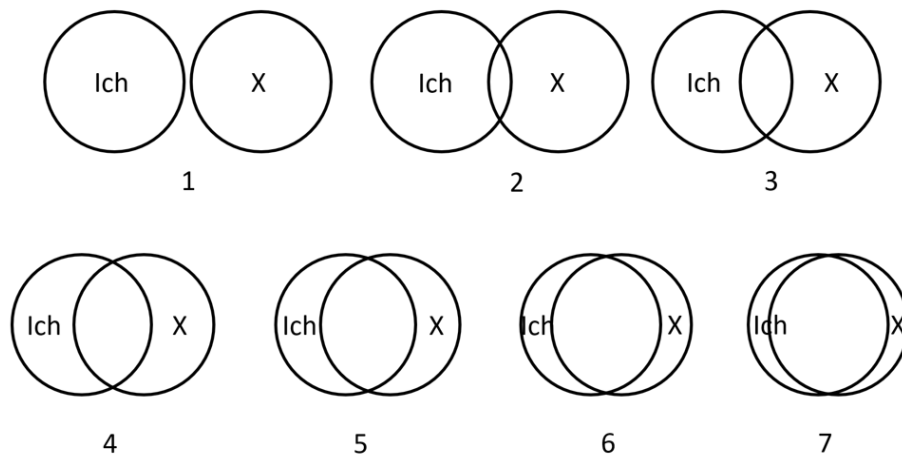
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Appendix A: Additional Tables and Figures

Table II.A1: Regression table of the mediation analysis. The paths (a)-(f) correspond to the paths in Figure II.2.

	Path	Estimate	Std. Err.	z-value	p-value
<u>Social Proximity (M1)</u>					
Treatment (T)	(a)	2.08	0.32	6.48	.000
Empathy Score		0.03	0.03	0.93	.352
<u>Care Motive (M2)</u>					
Treatment (T)	(e)	0.34	0.37	0.91	.360
Social Proximity (M1)	(b)	0.27	0.11	2.45	.014
Empathy Score		0.03	0.03	0.79	.430
<u>Giving in DG (Y)</u>					
Treatment (T)	(d)	-0.10	0.34	-0.29	.772
Social Proximity (M1)	(f)	0.00	0.08	0.02	.982
Care Motive (M2)	(c)	0.94	0.08	11.91	.000
Empathy Score		0.02	0.04	0.52	.606
Total Indirect Effect	(a)-(c)	0.53	0.22	2.35	.019
Total Direct Effect	(d)	-0.10	0.34	-0.29	.772
Nr. of Observations					93
Nr. Simulations					1000

Figure II.A1: Inclusion of the other in the Self Scale



Appendix B: Instructions

You are participating in a study on economic decision making, and in the following you will be asked to make a number of decisions. Please read the following instructions carefully. You have the opportunity to earn money that will be paid out in private and cash at the end of the study. During the study, you are not allowed to talk to the other participants. If you have questions, please raise your hand and one of the experimenters will approach you and answer your question.

Before the study, 656 other participants completed the "Big Five" personality questionnaire as you did. The specific questions are associated with one of the five dimensions, namely: Neuroticism, Openness to experience, Extraversion, Conscientiousness and Agreeableness. This classification has been evaluated as meaningful in numerous studies, and is used among others in professional coaching and assessment centers. Hence, your personality profile matches your personality very well. At the end of the study, you receive a personalized analysis of your personality profile. At the beginning of the study, you are matched with the person out of the pool of 656 participants who is the the most similar/dissimilar to you based on the "Big Five" personality questionnaire.

This study consists of two parts. In both parts you are asked to take decisions for you and for your partner you are matched with. Part I consists of two subparts. To determine your finale payment at the end of the study, either part I or part II are randomly determined and you and your partner are paid out according to your decisions in the respective part. Please note that only you can take decisions, while your partner is not able to influence his/her own payment.

[Instruction for part I consisting of two risk tasks entered here.]

As mentioned before you are matched with one of the 656 participants who is the most similar to you based on the "Big Five" questionnaire. In the following graph your personality profile and the personality profile of your partner are presented.

[Figure III.2 entered here.]

Please note that that highest score for each dimension is 48 and the lowest 0.

[Here the participants are invited to reflect on their partner. The page was displayed for 1 minute and could not be skipped.] As mentioned before, you are matched with one of the 656 participants who is the most similar/dissimilar to you based on the

”Big-Five” questionnaire. In the following graph, your personality profile and the personality profile of your partner are presented.

Please think about your partner.

Which characteristics does your partner have?

[Part I starts here.]

Part II of the study

In this part of the study, you receive €10 which you can distribute between you and your partner. Your partner receives nothing. If this part is paid out, your partner receives the amount you transferred to her. You get the remaining amount. If this part is randomly chosen to be paid out, you and your partner only receive the amounts you chose in this task.

III. The Risk of Walking a Mile in other People's Shoes: Perceived Similarity and Risk Taking for Others

by Jan Krause, Patrick Ring and Christoph Schütt

Abstract

Recent studies on risk taking for others report either a “risky” or a “cautious shift” i.e., more or less risk taking for a partner compared to oneself. In addition to contradicting results, there exists no consensus about the channels involved. This study provides an experimental test of Construal Level Theory and an explanation for difference in risk taking for oneself and a partner. By varying the perceived similarity to the partner, we manipulate the construal level while keeping information about the partner's risk preferences constant. We find that subjects take significantly more risk for a dissimilar partner than for a similar partner. Considering loss aversion, we find that the prosocial motive of care towards the partner is the main driver of decision making in the mixed domain.

JEL classification: A12, A13, C91, D01, D81, D90, D91

Keywords: Social Distance, Other-regarding preferences, risk-taking, Loss aversion

III.1 Introduction

Most economic and social decisions are taken under uncertainty and have severe consequences for others. This ranges from political decisions on environmental protection, financial decisions of investors on behalf of their clients, CEO’s decisions on business strategies to doctors who decide about the treatments for their patients. In the recent Corona pandemic, the perception of risk and the behavior towards others has become a central element of social life. ”Social distancing”, that is the reduction of contact to family and friends, has become a norm to minimize the risk of an infection of beloved people. Furthermore, in public spaces, adhering to hygiene rules has become crucial to reduce infection risks. In this context, the question arises how people decide for others and what is the process behind these decisions. Hence, should I visit my parents and risk an infection which might be detrimental for them or should schools stay open to reduce the burden of isolation on children and families? In such decisions, the social context is central to decision making under uncertainty and has long been neglected in the economic literature, which exclusively focused on individual decision making (Holt and Laury, 2002). Recently, a small but growing body of literature addresses the issue of responsibility in decision making under risk (Charness et al., 2007; Sutter, 2009; Füllbrunn et al., 2020) and how decisions change with the presence of others (Friedl et al., 2014; Li et al., 2017). However, the results are still inconclusive (Polman and Wu, 2019) and no general agreement on the channels influencing the process has been reached.

The quantity of interest in this literature is the target decision gap, which captures the differences between the decision for oneself and another person. In this context, a ”risky shift” describes situations in which a person takes more risk for another person than for themselves, while a ”cautious shift” constitutes the opposite behavior. Both effects are observed in the literature and might be explained by different factors. According to a meta-analysis by Polman and Wu (2019), some of the target decision gap can be explained by the identity of the target, the decision frame (for example gain vs. loss or social vs. financial) or the theoretical underpinning of the study (for example responsibility aversion). Whether studies observe a “risky” or a “cautious shift” depends crucially on the design of the study (between- or within-subjects design). Polman and Wu (2019) report a small-to-medium risky shift in between-subjects designs ($d = 0.253$) and a small cautious shift in within-subjects designs ($d = -0.072$). Instead of focusing exclusively on the target decision gap - a within difference between oneself and a partner - we compare different target decision gaps between subjects, by varying the identity of the partner. Specifically, we manipulate social distance towards a partner in order to study the size and sign of the decision

target gap. We investigate whether Construal Level Theory (CLT) is able to explain how target decision gaps depend on the perceived similarity to the target.

CLT links the level of mental construal, i.e., abstraction, to the level of psychological distance (Trope and Liberman, 2010). This link is bidirectional, that is, a high psychological distance leads to a high degree of mental abstraction and vice versa. The mental construal describes the extent to which an object is perceived as abstract or concrete. At a high-level construal, objects are perceived as more abstract, general and de-contextualized, while at a low-level construal they are described as concrete, contextual and incidental (Liberman and Trope, 1998, 2008; Trope and Liberman, 2003). Psychological distance, on the other hand, is defined as the subjective experience that something is far away from oneself (Trope and Liberman, 2010) and may vary in four dimensions: the temporal, spatial, social and hypothetical/probability dimension (Bar-Anan et al., 2006; Liviatan et al., 2008; Trope et al., 2007). In this paper, we focus on social distance as a conceptualization of the social dimension of psychological distance (Liberman et al., 2007).

Social distance, or the degree of perceived social proximity, is an important property of any human relationship and varies from very close relationships such as within families and between friends to rather loose relationships such as between colleagues, neighbors or incidental encounters. In the economics literature, social distance is mainly studied in the context of social preferences and strategic interactions. For example, social distance has been found to be an important determinant of prosocial behavior leading to more giving in Dictator Games (Schütt, 2021; Dimant, 2019; Bohnet and Frey, 1999; Charness and Gneezy, 2008), better coordination in Coordination Games (Chierchia and Coricelli, 2015; Chierchia et al., 2020) and a higher level of contributions in Public Goods Games (Eckel and Grossman, 2005; Bochet et al., 2006; Charness et al., 2014).

Since social distance is by definition I centric (Trope and Liberman, 2010) –that is the level of social distance to oneself is close to zero, and the social distance between oneself and another person is always strictly positive– it makes it central to decision making for others. The manipulation of social distance is usually achieved by varying the identity of the decision target. There are two common approaches: (i) making decisions for oneself vs. for another (Andersson et al., 2016; Chakravarty et al., 2011; Polman, 2012; Sun et al., 2017) or (ii) making decisions for multiple others, e.g. friend vs. stranger (Montinari and Rancan, 2018; Michael et al., 2018; Sun et al., 2017).

The target pairs for decision making for others under risk suffer from a common drawback. By varying social distance, these studies also change the available infor-

mation about the targets' risk preferences. When deciding for themselves, subjects have complete information about their preferences, for close friends social distance is low and subjects have a good idea about how much risk their friends are willing to take. Giving seemingly relevant information about a stranger's characteristics, e.g. earlier choices (Bolton et al., 2015) or gender (Daruvala, 2007; Lima de Miranda et al., 2019), decreases social distance but also allows decision-makers to form more precise beliefs about the target's preferences. Changing social distance and preference information simultaneously does not allow to clearly identify the responsible channel.

We resolve the identification problem by comparing self-stranger within-differences for varying strangers while keeping the amount of information about strangers' risk preferences constant. Specifically, we manipulate social distance by varying the objective similarity along a fundamental dimension that does not provide any information about risk preferences to the decision-maker, namely personality traits, and consequently perceived similarity to a target. Miller et al. (1998) show that social distance decreases with higher perceived similarity (see also Heider 1958; Liviattan et al. 2008).

We report results from two experiments where subjects have to make decisions once for themselves and once for their partner. Risk preferences are elicited in the gain as well as in the mixed domain. In Experiment 1, subjects' own risk preferences are elicited before they are aware of the existence of a partner. Afterwards, they make a decision for their partner. In Experiment 2, the order of decisions is randomized, as order effects have influenced results in earlier studies (Montinari and Rancan 2018). In both experiments, we find a positive treatment effect on the target decision gap in the gain domain, i.e., more debiased decisions for a dissimilar than for a similar partner. Specifically, we find a negligible target decision gap for similar partners and a positive target decision gap, i.e., a risky shift, for a dissimilar partner. In the mixed domain, we do not observe a treatment effect but instead find an overall lower level of loss aversion for partners.

There exist at least two pathways for social distance to influence risk preferences. Primarily, we consider the effect of social distance on CLT, however one might be tempted to speculate that social distance has a motivational influence on decisions for others as well. In the absence of monetary incentives, decreased social distance might motivate subjects to make more of an effort when making decisions for a partner, i.e., motivate them to make "better" decisions. However, when deciding between different risks, it is not at all obvious, what a "better" decision is, since lotteries involve trade-offs between probabilities and outcomes. While such a care

motive seems unlikely to have a clear influence in the gain domain, increased care could increase loss aversion. Therefore, we control for the prosocial motive of care as an alternative mechanism for decision making for others.

We find that the similarity manipulation activates the prosocial motive of care yet only influences decisions in the mixed domain. Specifically, decisions in the mixed domain are driven by a higher degree of care towards the partner, whereas care does not explain the decision target gap in the gain domain.

The remainder of the paper is structured as follows: In Section 2, we develop our hypothesis based on CLT; in Section 3 we outline our estimation strategy; in Section 4 and Section 5 we present Experiment 1 and Experiment 2. We first describe the experimental methods and then the results for the gain and mixed domain separately; in Section 6 we report the findings on the alternative explanation of care in the pooled sample; and in Section 7, we conclude with a discussion of our results.

III.2 Hypotheses

In the following, we develop our hypothesis based on the predictions of CLT for both experiments. As described above, CLT links the mental construal to psychological distance. That is, a higher level of construal is associated with a higher psychological distance and vice versa. In psychology, CLT has been used in explaining, for example, discrimination (Milkman et al., 2012), consumer psychology (Fiedler, 2007), discrepancies between consumers' attitudes and behavior regarding sustainable consumption (van Dam and van Trijp, 2016) and saving decisions (Ülkümen and Cheema, 2011), as well as risk taking (Polman, 2012; Trautmann and van de Kuilen, 2012).

In general, people are risk averse for gains and exhibit loss aversion in the mixed domain. Accordingly, we restrict the derivation of our hypothesis to risk and loss averse decision-makers. In this context, it has been shown that a high-level construal makes participants focus more on the desirability of an outcome and less on the feasibility (Liberman and Trope, 1998). That is, safety and the attainability of the good are evaluated to a lesser degree (Sagristano et al., 2002). This leads to an increase in risk seeking for gains and risk aversion for losses, as evidenced by numerous studies (e.g. Lerner et al., 2015, 2016).

Deciding for another person does naturally involve a positive psychological distance. As stated by CLT, an increase in psychological distance leads to debiased decision making, i.e. decisions closer to risk neutrality (Trautmann and van de Kuilen, 2012). Accordingly, risk averse subjects are less risk averse for their partner than

for themselves (Polman, 2012; Sun et al., 2017). As the psychological distance and therefore the construal level is higher for a dissimilar than a similar partner, subjects take more risk for the former than for the latter.

H1: Risk and loss averse decision-makers take more risk for a dissimilar partner than for a similar partner.

The manipulation of (objective) similarity, based on relevant characteristics, leads to a manipulation of perceived similarity towards a partner. Specifically, when the manipulated characteristics are considered as highly relevant, the social distance towards a similar partner is minimal. Thus, when making decisions for such a partner, there is no need for taking a different perspective and considering the problem more abstractly. CLT predicts that a low level of construal, similar to the level when deciding for oneself, will result in negligible differences in risk taking, i.e., a negligible target decision gap. For a dissimilar partner, a high construal level predicts larger differences in risk taking for a partner than for oneself. This holds independently of whether subjects are aware of the partner matching in advance or not. Therefore, we hypothesize:

H2: For risk and loss averse decision-makers, the target decision gap is larger for a dissimilar partner than for a similar partner. Specifically, the target decision gap is positive for a dissimilar partner and negligible for a similar partner.

Decisions for oneself and one's partner are measured by the same task and are thus objectively similar. Due to the knowledge of such a similarity, subjects will anticipate (remember) their decisions and compare the decisions for themselves and their partner. Specifically, subjects will consider how they would (did) decide for their partner when making decisions for themselves. This might lead to an information effect and subjects' construal level could be influenced by knowing about the existence of a partner (Li et al., 2017). When matching subjects with a dissimilar partner, the more abstract mindset might carry over to one's own risk preferences, leading to debiased decision making for oneself. When matching subjects with a similar partner, the level of psychological distance is at most small, leading to a low level of mental construal (Liberman and Trope 1998). This is particularly relevant in Experiment 2, since the partner matching was carried out before the decisions and the decision order was randomized. Consequently, we hypothesize:

H3: Risk and loss averse decision-makers who are informed about the existence of their partner take more risk for themselves when matched with a dissimilar partner than with a similar partner.

Since the manipulation of social distance has previously been found to activate a prosocial motive of care, the decision for the partner might be influenced by the latter. We therefore formulate the following exploratory hypothesis on whether differences in the decision gap might be explained by the activation of the care motive induced by the similarity manipulation.

H4: For risk and loss averse decision-makers, caring for the partner leads to a smaller target decision gap.

It must be noted that, in the mixed domain, the existence of losses might dominate the proposed effects. Trautmann and van de Kuilen (2012) find that effects proposed by prospect theory, for example loss aversion, have a stronger impact on risk taking for oneself than those implied by theories of psychological distance like CLT. Hence, we are agnostic about possible effect differences between the gain and mixed domain. The preceding hypotheses for decision making in the pure gain and mixed domain were derived for risk averse subjects only. We will not speculate on the behavior of risk and loss loving decision makers, as they represent only a small minority in typical subject pools. In order to investigate hypotheses about risk and loss seeking subjects, sample sizes need to be very large to allow statistical inference. While our samples are by no means small, they do contain only a small number of these individuals.

III.3 Estimation Strategy

Based on our hypothesis, the subsequent analysis is structured as follows. First, we determine if the similarity induction and the manipulation of perceived similarity and social distance was successful. In the next step, we test the general implications of construal level theory based on hypothesis *H1* and *H2*. The results are reported separately for the gain and mixed domain.

Additionally, we test a possible information effect as specified in *H3* in Experiment 2. In a final step, we pool the data of both experiments and check the robustness of our results and investigate the care motive as an alternative pathway.

When comparing decision making for oneself and a similar partner, we hypothesize for negligible differences. Since the non-significance of an effect based on means does not necessarily imply the absence of an effect, we conduct equivalence tests to corroborate the equivalence of the two means. Equivalence testing is a well-established tool in the fields of medical science and psychology and is increasingly recognized in economics (Kim and Robinson, 2019). The term equivalence in this

context means equality except for practically irrelevant differences. Hence, in this sense a proof of equivalence means that we are able to exclude any effect sizes that are larger or equal to a small effect size, as measured by *Cohen's d*. More precisely, we provide results of a Two One-Sided Test (TOST) (Schuirmann, 1987). The TOST calculates a confidence interval for the effect size and determines if it is entirely contained within the interval $(-0.3, 0.3)$ (Lakens, 2017).

III.4 Experiment 1

III.4.1 Methods and Procedures

The subjects in this experiment had to make decisions for themselves as well as for a partner (Factor: self/other). Between subjects, we manipulated whether subjects were matched with an objectively similar or dissimilar partner (Factor: similar/dissimilar). The design structure is illustrated in Figure III.1. We begin by

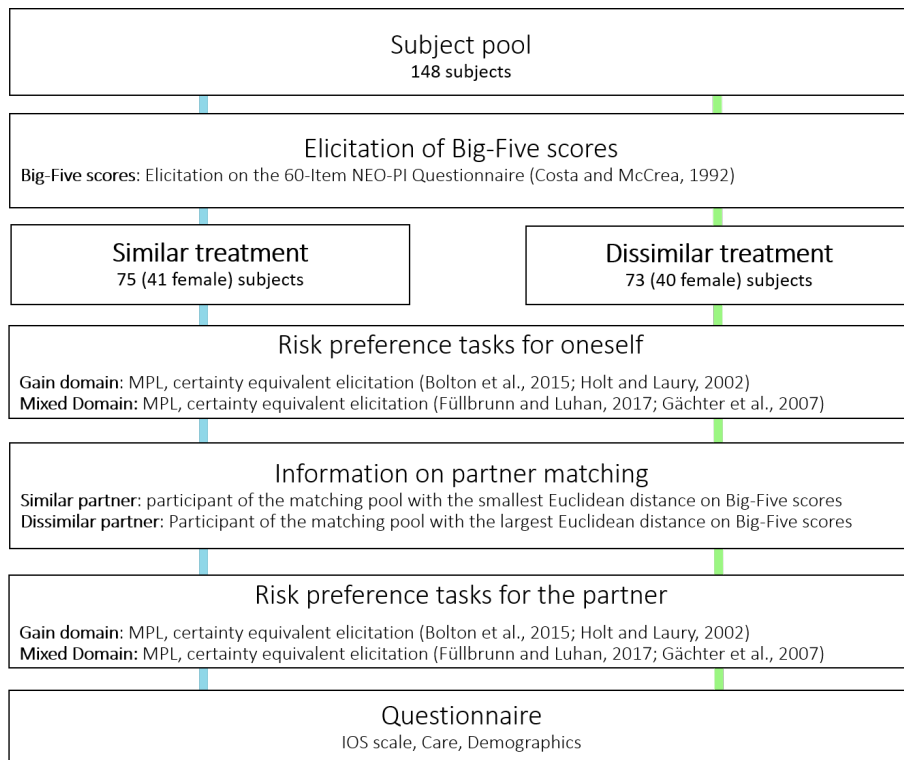


Figure III.1: Experimental design Experiment 1

describing how subjects were matched to their partner and how (dis-)similarity was operationalized. Afterwards, the outcome variables and procedures are presented.

III.4.1.1 Recruitment and matching process

Subjects were recruited from a student pool at Kiel University via Hroot (Bock et al. 2014) in a two-stage procedure. In the first stage, subjects completed a personality questionnaire based on the Five-Factor Model (Costa and McCrae 1992). The 657 subjects who completed the personality inventory became the matching pool. In the second stage, additional subjects were invited to the Kiel Experimental Economics Laboratory (KEEL) and completed the same personality inventory. These subjects made up our subject pool. Subjects were randomized into two conditions. In the similar condition, subjects were matched with the person whose personality profile had the smallest Euclidean distance to their own, while in the dissimilar condition they were matched with the person whose personality profile had the largest distance. Both the own personality profile and the personality profile of the partner were displayed in a spider plot (Figure III.2) to induce a comparison of their personalities. To prevent subjects from forming lay theories about their partners from individual personality traits, the axes of the spider plot read only “category 1-5”. No other personal information about their partner was revealed.

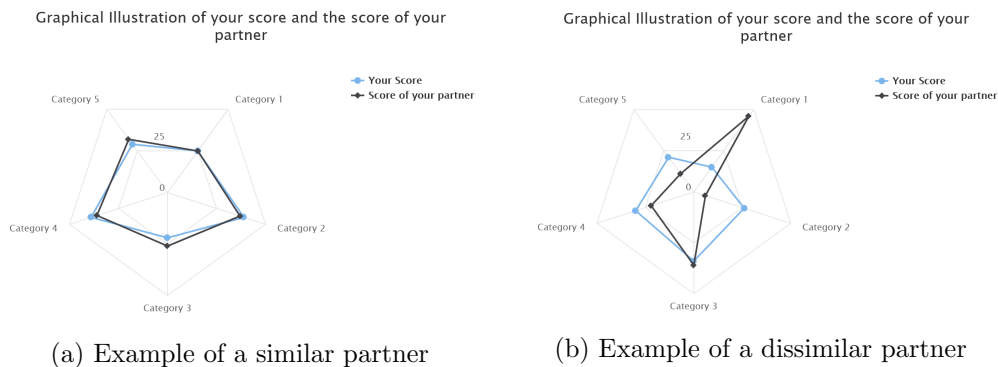


Figure III.2: Graphical illustration of the partner matching

III.4.1.2 Design and Procedure

Subjects had to complete two risk preference tasks, once for themselves (self) and once for their partner (other). In this experiment subjects completed the decision task for themselves first and were then informed about the existence and personality profile of their partner. Risk preferences were elicited in the gain and mixed domain. For the gain domain, the task of Bolton et al. (2015) and Holt and Laury (2002) was used. In this task, subjects had to decide repeatedly between a safer and a riskier pure gain lottery represented by a Multiple Price List (see Table III.A5 in Appendix III.7). While the outcomes in each lottery were constant, the probability of the

higher outcome increased from 0 to 1 in steps of 0.1. After ten binary decisions, subjects were presented with a summary table of their choices and had the chance to revise them. For consistency reasons, subjects were restricted to at most one switching point from the safe to the risky lottery. For the mixed domain, we chose the task described in Füllbrunn and Luhan (2017) and Gächter et al. (2007). In this task, subjects had to decide repeatedly whether to accept or reject a mixed outcome lottery. Subjects received an initial endowment of 7 Euros prior to the task, from which losses were deducted. In each lottery, there was an equal chance of winning 6 Euros or lose a certain amount of money. The amount that could be lost increased from 2 Euros to 7 Euros (see Table III.A6 in Appendix C). After subjects made six binary decisions, they were again presented with a summary table and had the chance to revise them. The tasks were followed by a questionnaire on subjects' perceptions of their risk preferences, feelings of responsibility, care and empathy towards their partner, as well as sociodemographic variables.

As a manipulation check, subjects stated the perceived similarity to their partner on a scale of 0 to 4 directly after the matching procedure. Manipulation of the perceived psychological distance, specifically social distance, was measured on the seven-point *Inclusion of Other in Self Scale (IOS, 1-7)* (Gächter et al., 2015) at the end of the experiment.

To determine the final payoff of the subjects and their partners, one decision was randomly, independently and anonymously chosen and paid out. Subjects were guaranteed that their partner was paid, but they were not informed about the amount. Overall, 148 students participated in the experiment. 75 (41 female) subjects were in the similar treatment and 73 (40 female) subjects were in the dissimilar treatment. Sessions lasted for around 45 minutes and the mean payoff was €12.38, including a show-up fee of €5. The experiment was conducted with oTree (Chen et al., 2016).

III.4.2 Results

III.4.2.1 Manipulation check

Matching subjects with an objectively similar or dissimilar partner should lead to differences in perceived similarity and social distance. A Wilcoxon Rank Sum Test (WRST) reveals that subjects perceived their partner as significantly more similar when matched with a similar ($M_S = 3.44$, $SE = 0.062$) compared to a dissimilar partner ($M_D = 0.64$, $SE = 0.050$) (WRST: $z = -14.889$, $p < 0.001$). Subjects also reported a significantly lower social distance measured by a higher score on the

IOS scale in the similar treatment ($M_S = 4.60$, $SE = 0.159$) than in the dissimilar treatment ($M_D = 2.64$, $SE = 0.088$) (*WRST*: $z = -8.404$, $p < 0.001$). Thus, the manipulation was successful.

III.4.2.2 Gain domain

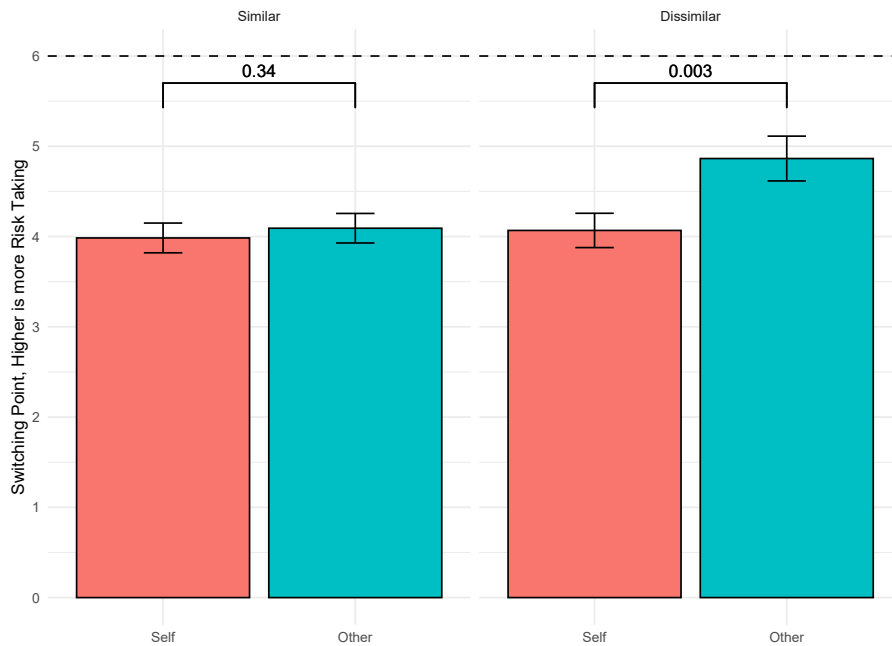


Figure III.3: Switching point in the gain domain.

Considering only pure gain lotteries, a higher switching point (scale: 0-10) corresponds to a higher degree of risk taking; a switching point at 6 constitutes risk neutrality. In the overall sample, 24 subjects (similar:10, dissimilar:14) were risk-seeking and 124 subjects (similar:57, dissimilar:50) were risk averse. As described above, we restrict our analysis to the 124 risk averse subjects and report the results for the risk seeking subjects as well as the entire sample in Appendix A. Restricting the analysis to the risk averse subjects does not change the observed effects.

The average switching points for the decision for oneself and the partner in the two treatments similar and dissimilar are summarized in Figure III.3.

We do not find a significant treatment difference in the decision for oneself (*WRST*: $M_{S,S} = 3.98$, $SE = 0.165$, $M_{D,S} = 4.07$, $SE = 0.190$, $z = -0.606$, $p = 0.544$). This is not surprising given that the subjects were not informed about the existence and characteristics of their partner before they made the decision for themselves.

In the following, we test our hypotheses. Hypothesis *H1* states that being paired with a dissimilar partner leads to a higher level of construal. Therefore, the switch-

ing point in the decision for the partner should be closer to risk neutrality in the dissimilar treatment than in the similar treatment. This effect is confirmed and highly statistically significant ($WRST : M_{S,P} = 4.09, SE = 0.164, M_{D,P} = 4.86, SE = 0.248, z = -2.699, p = 0.007$). Accordingly, we find a larger target decision gap (Δ^G) in the dissimilar treatment ($\Delta_D^G = 0.80, SE = 0.244$) compared to the similar treatment ($\Delta_S^G = 0.11, SE = 0.139$) ($WRST : z = -2.462, p = 0.014$). As visually suggested in Figure III.3, the target decision gap in the dissimilar treatment is positive ($WRST : z = -2.973, p = 0.003$), whereas we do not find significant differences between risk taking for oneself and a similar partner ($WRST : z = -0.954, p = 0.340$). Indeed, we measured a negligible effect size of $d = 0.081$. Running a TOST in order to rule out effect sizes of 0.3 (in absolute terms) and larger, we conclude that the means are significantly equivalent ($TOST : t(64) = -1.643, p = 0.053$). This corroborates hypothesis $H2$ in the gain domain.

III.4.2.3 Mixed domain

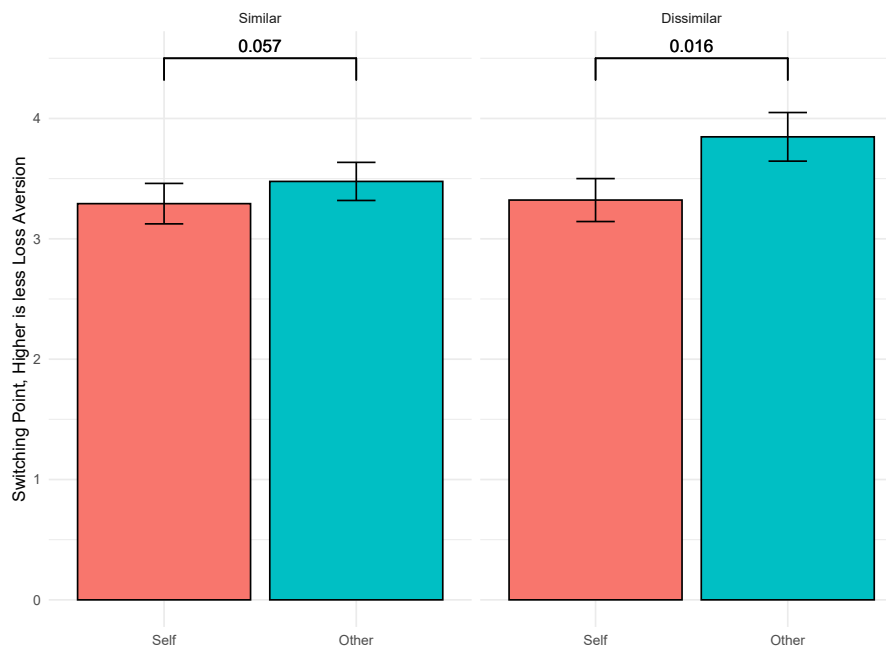


Figure III.4: Switching point in the mixed domain

Considering mixed lotteries, a higher switching point corresponds to less loss aversion. Figure III.4 shows the average switching points for oneself and the partner in both treatments. Again, we do not find a treatment difference for the decision for oneself ($WRST : M_{S,S} = 3.30, SE = 0.168, M_{D,S} = 3.32, SE = 0.178, z = -0.163, p = 0.871$).

Analogous to the gain domain, we hypothesize that being paired with a dissimilar partner leads to debiasing, and therefore a lower degree of loss aversion for the partner compared to subjects who were paired with a similar partner. A visual inspection of Figure III.4 confirms the direction of this effect, but we do not find a statistically significant difference (*WRST*: $M_{S,P} = 3.48$, $SE = 0.158$, $M_{D,P} = 3.85$, $SE = 0.202$, $z = -1.243$, $p = 0.214$). Accordingly, the decision gap (Δ^M) is significantly positive for both a dissimilar partner (*WRST* : $\Delta_D^M = 0.53$, $SE = 0.204$, $z = -2.419$, $p = 0.016$) and a similar partner (*WRST* : $\Delta_S^M = 0.18$, $SE = 0.093$, $z = -1.906$, $p = 0.057$) and are not significantly different from each other (*WRST* : $z = -1.212$, $p = 0.225$). For completeness, we test the equivalence of the decision for oneself and a similar partner. We measured an effect size of $d = 0.140$. Based on a TOST, we cannot rule out small effect sizes of 0.3 (in absolute terms) and larger (*TOST*: $t(64) = -0.428$, $p = 0.335$). We therefore cannot confirm the equivalence of the two decisions. There is a clear tendency in favor of hypothesis $H1$ and $H2$. The results suggest that CLT might not be the only driver in the mixed domain. This is especially true for $H2$, as loss aversion is also significantly decreased for a similar partner contrary to the predictions.

III.4.3 Summary of the results

In Experiment 1, our similarity manipulation was successful and the results indicate that we were able to manipulate the mental construal based on social distance. In the gain domain, we find strong support for construal level theory. Subjects in the dissimilar exhibit a risky shift towards risk neutrality, while subjects with a similar partner do not. In the mixed domain, we find a risky shift, irrespective of the treatment. Nevertheless, there is a clear tendency for a larger target decision gap in the dissimilar treatment. The results suggest that the similarity manipulation did not only manipulate the level of mental construal, but might have altered care towards the partner. The alternative pathway of care is explored in section 6. To test the robustness of our results, we will report the results of a second experiment to control for a possible information and order effect.

III.5 Experiment 2

III.5.1 Methods and Procedures

Since order effects have been found to have a significant effect on decision making for others (Montinari and Rancan, 2018), we conducted a second experiment to

test the robustness of our results. Subjects in Experiment 2 had to make the same decisions for themselves, as well as for a partner (Factor: self/other). Again, we manipulated whether subjects were matched with an objectively similar or dissimilar partner (Factor: similar/dissimilar) in a between-subjects design. In contrast to Experiment 1, subjects were first informed about the existence of their partner and their characteristics. The order of the decisions for oneself and the partner was then randomized (Factor: Self First/Other First). The design structure is illustrated in Figure III.5.

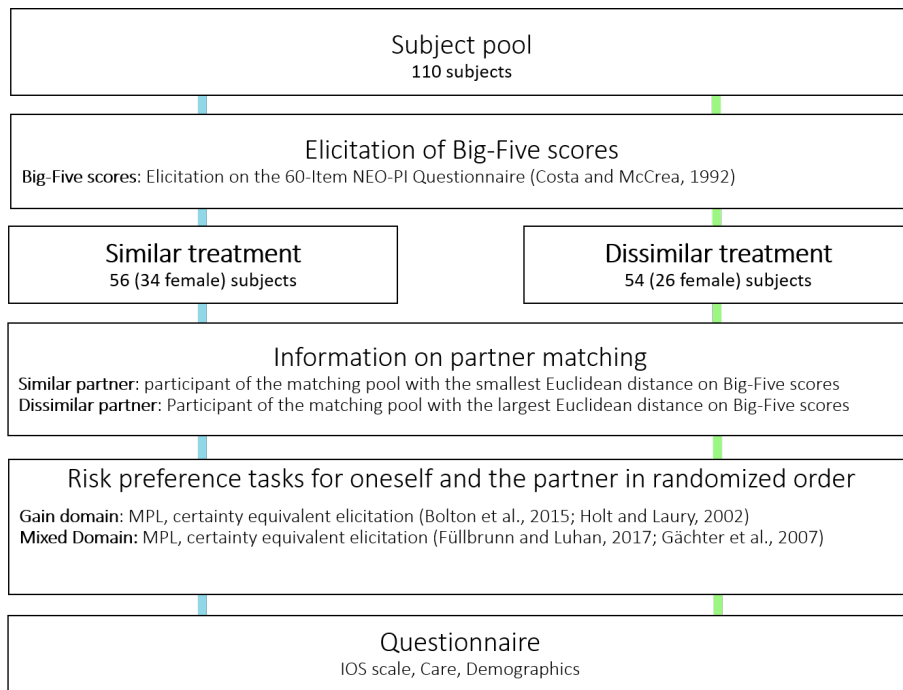


Figure III.5: Experimental design Experiment 2

Randomizing the decision order does also change the information structure of the experiment. Therefore, we report the results for the second experiment separately and identify the relevant effects based on hypotheses $H1$ and $H2$ to check the robustness of our previous results. We invited 110 students¹ from the matching pool of Experiment 1 to the Kiel Experimental Economics Laboratory (KEEL), who made up our subject pool. Of those 110 subjects, 54 (26 female) subjects were randomly assigned to the dissimilar treatment and 56 (34 female) subjects were randomly assigned to the similar treatment. At the beginning of the experiment, subjects entered their study code to retrieve the information of their personality profile. Afterwards, they were randomly assigned to the similar or dissimilar treatment and matched with a partner whose personality profile had the smallest or largest Euclidean distance to

¹Due to technical issues, individual items of questionnaire data for 27 subjects was lost.

their own. After the presentation of the partner and the visualization of the personality profiles by means of a Spider plot (Figure III.2), subjects completed the same risk task in the gain and mixed domain as in Experiment 1 (see Table III.A5 and III.A6 in Appendix C). We used the same scales of perceived similarity and perceived social distance as manipulation checks². Sessions lasted for around 45 minutes and the mean payoff was €12.37 including a show-up fee of €5. The experiment was conducted with oTree (Chen et al., 2016). Analogously to Experiment 1, we present the results separately for the gain and the mixed domain.

III.5.2 Results

III.5.2.1 Manipulation check

In the second experiment, we are able to replicate the manipulation of perceived similarity and perceived social distance. Subjects perceive their partner as significantly more similar (on a scale from 0-4) when matched with a similar ($M_S = 3.79$, $SE = 0.039$) compared to a dissimilar partner ($M_D = 0.94$, $SE = 0.063$) (*WRST*: $z = -13.428$, $p < 0.001$). We observe the same effect for perceived social distance. Subjects who were matched with a dissimilar partner report a significantly higher social distance measured by a lower score on the *IOS* scale ($M_D = 2.57$, $SE = 0.140$) than subjects matched with a similar partner ($M_S = 4.69$, $SE = 0.180$) (*WRST*: $z = -7.799$, $p < 0.001$).

III.5.2.2 Gain Domain

In the overall sample, 8 subjects (similar:1, dissimilar:7) were risk seeking and 102 subjects (similar:55, dissimilar:47) were risk averse. Again, we restrict our analysis to the 102 risk averse subjects and report the results for the risk seeking subjects as well as the entire sample in Appendix A. First of all, we find no significant treatment effect on the decision for oneself (*WRST*: $M_{S,S} = 3.82$, $SE = 0.217$, $M_{D,S} = 4.21$, $SE = 0.227$, $z = -1.237$, $p = 0.216$) and therefore, we are not able to confirm hypothesis *H3*. While this effect is not statistically significant, we find a tendency for more risk taking for oneself, when faced with a dissimilar partner. Regarding hypothesis *H1*, we find that subjects in the dissimilar treatment take significantly more risk for their partner ($M_{D,P} = 4.51$, $SE = 0.181$) than subjects in the similar treatment ($M_{S,P} = 3.87$, $SE = 0.160$) (*WRST*: $z = -2.518$, $p = 0.012$). The target decision gap is significantly positive for a dissimilar partner (*WRST*:

²In addition to the decision tasks from Experiment 1, subjects participated in a group risk task and a dictator game, reported in Schütt (2021).

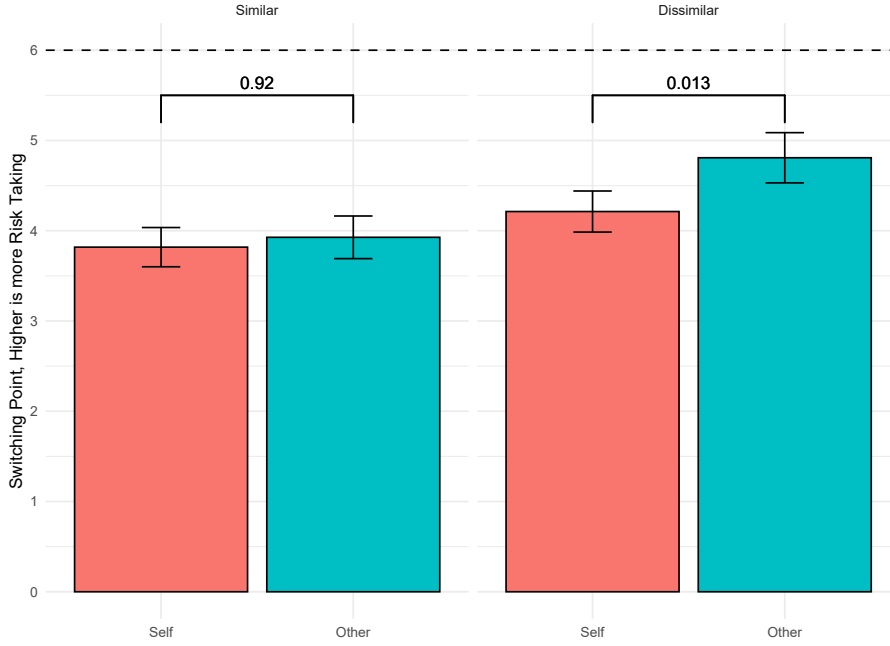


Figure III.6: Switching point in the gain domain

$\Delta_D^G = 0.60, SE = 0.270, z = -2.478, p = 0.013$) and not significantly different from zero for a similar partner ($WRST: \Delta_S^G = 0.11, SE = 0.195, z = -0.107, p = 0.915$). Furthermore, the target decision gaps are significantly different from each other ($WRST: z = -2.006, p = 0.045$). For a similar partner, we measured a negligible effect size of $d = 0.064$. Running a TOST in order to rule out effect sizes of 0.3 (in absolute terms) and larger, we conclude that the means are significantly equivalent ($TOST : t(54) = -1.666, p = 0.051$). These results are in line with hypothesis $H2$ in the gain domain.

Finally, we investigate possible order effects. Neither decisions for oneself ($WRST: z = -1.165, p = 0.244$), decisions for the partner ($WRST: z = -1.198, p = 0.231$) nor the resulting target decision gaps ($WRST: z = -0.312, p = 0.755$) depend significantly on the decision order. However, Figure III.7 shows a tendency for subjects to take more risk when deciding for their partner first. This suggests that the decision order has at most a small effect on risk taking, and that the results of Experiment 1 are not driven by order effects.

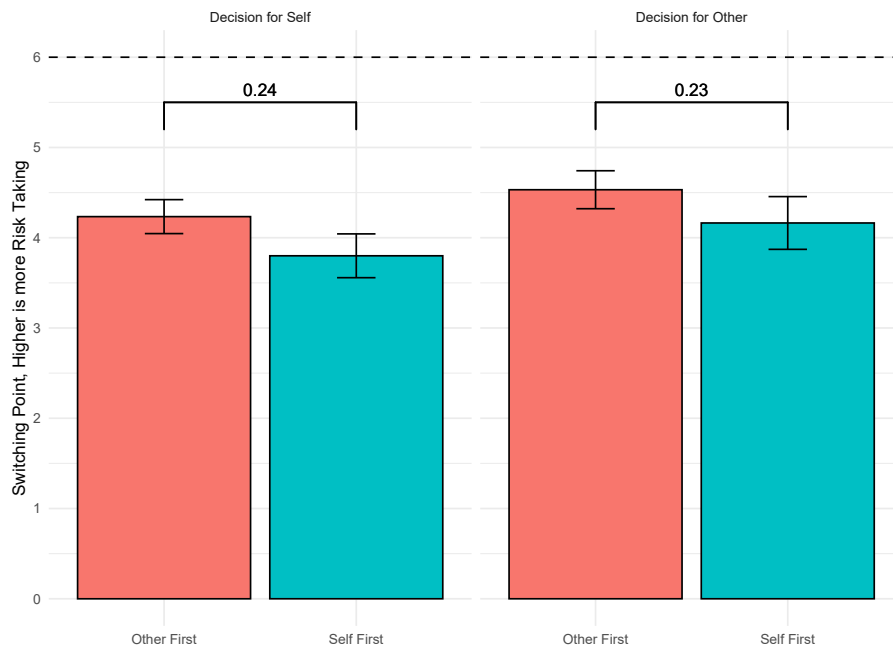


Figure III.7: Switching point in the gain domain by target and decision order

III.5.2.3 Mixed Domain

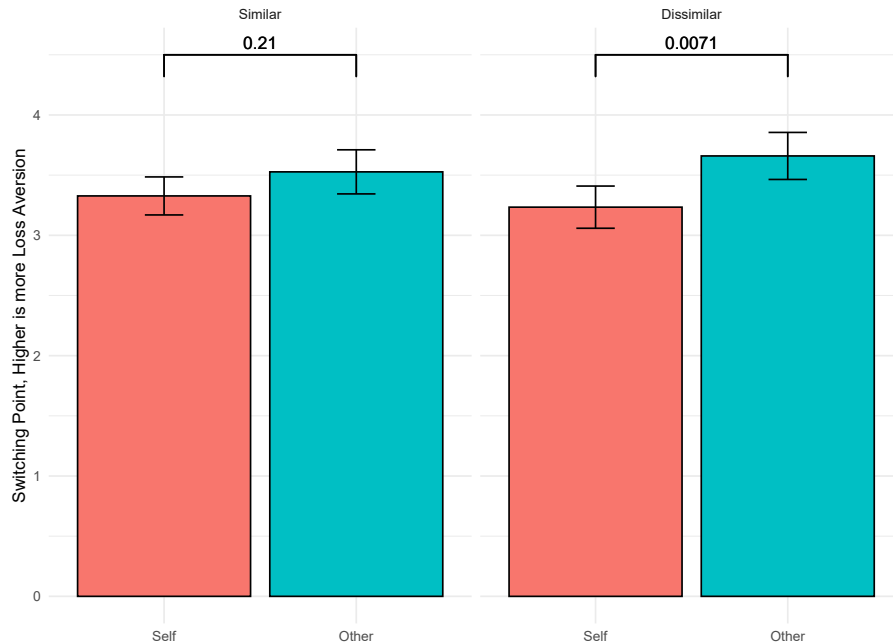


Figure III.8: Switching point in the mixed domain

Similar to Experiment 1, we find some support for CLT in the mixed domain. Figure III.4 shows the average switching points for oneself and the partner in both

treatments. We find no difference in the level of loss aversion for oneself (*WRST*: $M_{S,S} = 3.33$, $SE = 0.158$, $M_{D,S} = 3.23$, $SE = 0.175$, $z = -0.296$, $p = 0.767$), nor for the level of loss aversion for the partner (*WRST*: $M_{S,P} = 3.53$, $SE = 0.183$, $M_{D,P} = 3.660$, $SE = 0.195$, $z = -0.678$, $p = 0.498$). The target decision gap for a dissimilar partner is significantly positive (*WRST*: $\Delta_D^M = 0.43$, $SE = 0.145$, $z = -2.691$, $p = 0.007$) and not significantly different from zero for a similar partner (*WRST*: $\Delta_S^M = 0.20$, $SE = 0.141$, $z = -1.258$, $p = 0.209$). The difference between both target decision gaps, though not statistically significant (*WRST*: $z = -1.328$, $p = 0.184$), reveals a tendency for a larger target decision gap in the dissimilar treatment. Although we find that the target decision gap in the similar treatment is not significantly different from zero, a TOST reveals that equivalence can be rejected (*TOST*: $d = 0.156$, $t(54) = -0.803$, $p = 0.213$).

Furthermore, we find a significant order effect on the decision for oneself (*WRST*: $z = -2.299$, $p = 0.021$). Subjects who first decided for themselves are on average more loss averse than subjects who first decided for their partner. The decision for the partner is not affected by the decision order (*WRST*: $z = -0.142$, $p = 0.887$) (see Figure III.9). This effect tends to produce larger target decision gaps (*WRST*: $z = -2.376$, $p = 0.018$) when deciding for oneself first. This difference in overall target decision gaps might have influenced the result in Experiment 1.

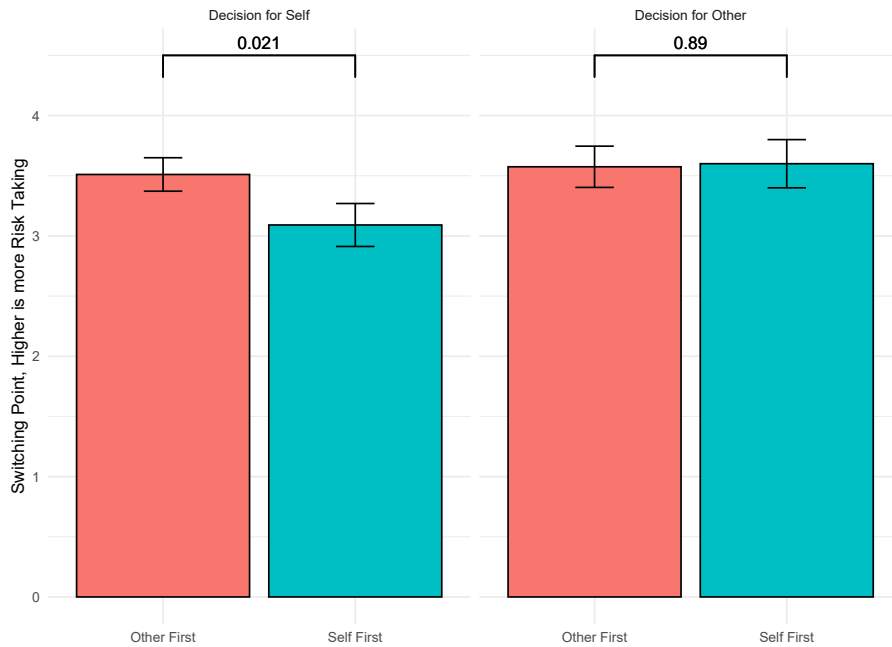


Figure III.9: Switching point in the mixed domain by target and decision order

III.5.3 Summary of the results

In Experiment 2, we were able to replicate our previous findings. First, we were able to manipulate perceived similarity and social distance based on the personality profiles. Second, we find strong support in favor of CLT in the gain domain, that is, a risky shift towards risk neutrality in the dissimilar treatment and no shift in the similar treatment. Third, we find that CLT does not explain the results in the mixed domain entirely. Considering possible order effects, we conclude that the results in the gain domain are robust to a randomization of the decision order, while the decision gap in the mixed domain exhibits some order effects. In the next step, we pool the data of both experiments to check the robustness of the observed effects by controlling for other possible confounds like demographics, order and self-reported risk attitudes. While CLT is able to explain all treatment differences in the gain domain, there seem to be additional mechanisms at work in the mixed domain. We use the pooled sample to investigate whether care for a partner can explain behavior in the mixed domain via an empathy gap.

III.6 Pooled Analysis

In this section, we report the results based on the pooled data of both experiments. The only differences between Experiment 1 and 2 are the decision order (fixed vs. randomized) as well as the point at which the information about the partner was revealed (after the decisions for oneself vs. before any decisions were made). The results of both experiments do not differ qualitatively and we could not find significant order effects in the gain domain. Therefore, we are confident in analyzing the pooled data while including dummy variables for differences between the two experiments. Specifically, we control for the information structure, decision order and potential level effects between the experiments. With the pooled data, we investigate CLT as well as other alternative determinants of the decision target gap. In particular, we investigate whether care for the partner's payoff explains differences in the decision target gap. Care was measured on a seven-point Likert-scale, asking for how much the subjects cared about the partner's payoff, and differs significantly between the treatments ($WRST: z = -3.559, p < 0.001$). As a further control, we include the *Basic Empathy Scale* in a German validated version (Heynen et al., 2016) as a measure of the propensity to feel care towards another person. Again, we present the results for the gain and mixed domain separately.

III.6.1 Gain Domain

In the gain domain we found evidence in favor of CLT in both experiments. That is, a larger target decision gap for subjects in the dissimilar treatment. Table III.1 shows the results of OLS-Regressions of the target decision gap on various controls. In *model (1)-(4)* we explore the robustness of the observed treatment effect and in *model (5)* and *(6)* we investigate treatment specific channels. Overall, we find a significant and robust treatment effect. *Model (1)* confirms our previous results that the target decision gap in the dissimilar treatment is positive and significantly larger than in the similar treatment ($\beta = 0.60$, $SE = 0.210$, $p = 0.005$). Including a dummy for the experiments and the level of the own decision (*model (2)*) reveals that there is no experiment specific effect ($\beta = 0.10$, $SE = 0.196$, $p = 0.605$), but a significant level effect of the own decision on the target decision gap ($\beta = -0.40$, $SE = 0.066$, $p < 0.001$). That is, the closer one's own decision is to risk neutrality, the smaller the target decision gap is. These findings are robust to order effect ($\beta = -0.18$, $SE = 0.294$, $p = 0.538$) and demographic controls such as age, gender (*Female*), the self-reported relative risk attitude to other students of Kiel University (*Relative Attitude*), economics as the field of study (*Econ*) and empathy (*model (3)*). Including the motive care as an alternative explanation reveals that caring about the partner's payoff significantly influences the target gap ($\beta = -0.18$, $SE = 0.063$, $p = 0.008$) (*model (4)*). That is a higher degree of care leads to a smaller target decision gap. Even after including care, the treatment dummy remains significant. Therefore, the mechanism only partially mediates the effect on the target decision gap. Running separate regressions for both treatments reveals that this holds only when subjects are matched with a similar ($\beta = -0.22$, $SE = 0.069$, $p = 0.002$) rather than a dissimilar partner ($\beta = -0.10$, $SE = 0.118$, $p = 0.404$).

To further investigate whether care indeed only partially mediates the difference in the target decision on the subject level, we conduct a mediation analysis based on an SEM approach proposed by Imai et al. (2010). The estimation is conducted with the R package mediation (Tingley et al., 2014). Figure III.10 presents the results of the mediation analysis in a path model. The coefficients and significance levels were computed with OLS regression (see Table III.A2 in Appendix B).

The analysis reveals that there is a significant treatment effect when ignoring care as an potential mediator (see Table III.A2 *model (1)*). This is still the case when including care as a mediator into the regression (see Table III.A2 *model (3)*). Furthermore, it can be shown that the treatment significantly influences care towards a partner (see Table III.A2 *model (2)*). That is, subjects in the similar treatment care more about their partners' payoff. Estimating the model with the SEM approach,

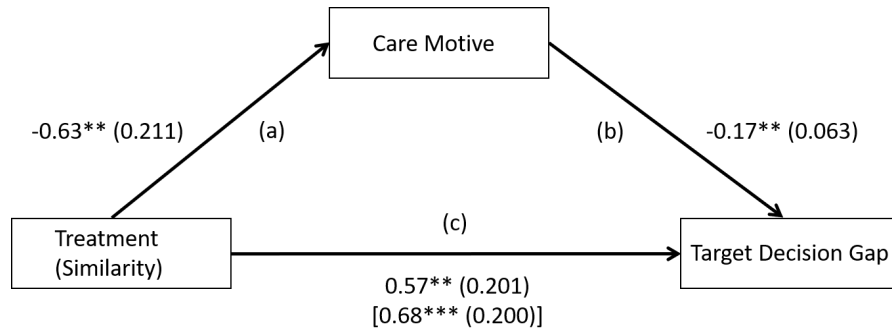


Figure III.10: Path diagram of mediation path in the gain domain. The coefficients are estimated with OLS (see Table III.A2 in Appendix B). Standard errors are in parenthesis. The estimate in the square brackets corresponds to the treatment effect ignoring care as a potential mediator. The significant levels are: + $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

we find that both the indirect effect ($p = 0.008$) (illustrated by the pathway (a)-(b)) and the direct effect are significant ($p = 0.004$) (see Table III.A1 in Appendix B). The proportion of the variance mediated by care amounts to approximately 16% ($p = 0.008$). Hence, we find that the treatment difference in the target decision gap is only partially mediated by the motive care in the gain domain.

III.6.2 Mixed Domain

In the mixed domain, we did not find that CLT was the main and only driver of risk taking behavior for others in either experiment. That is, while there was a tendency for a treatment effect, we did not find a significant difference in the target decision gaps.

Analogously to the gain domain, Table III.2 shows the results of OLS-Regressions of the target decision gap on various controls in the mixed domain. In the pooled data, we find a significant treatment effect in *model (1)* ($\beta = 0.29$, $SE = 0.150$, $p = 0.055$), which is robust to the respective experiment ($\beta = 0.05$, $SE = 0.142$, $p = 0.749$), the level of the own decision ($\beta = -0.30$, $SE = 0.055$, $p < 0.001$) (*model (2)*) and possible order effects ($\beta = 0.29$, $SE = 0.212$, $p = 0.168$) as well as demographics (*model (3)*). However, the treatment effect vanishes when including care for the partner as an alternative explanation. That is, a higher degree of care leads to a smaller target decision gap ($\beta = -0.16$, $SE = 0.045$, $p = 0.001$). Running separate regressions for both treatments reveals that this holds for both, a similar ($\beta = -0.16$, $SE = 0.050$, $p = 0.002$) and a dissimilar partner ($\beta = -0.14$, $SE = 0.084$, $p = 0.093$).

To investigate whether care mediates the difference in the target decision on the

Table III.1: Regression analysis for Target Decision Gap in the Gain Domain

	<i>Dependent variable:</i>					
	Δ^G					
	(1)	(2)	(3)	(4)	(5)	(6)
Dissimilar	0.60** (0.21)	0.69*** (0.20)	0.68*** (0.20)	0.57** (0.20)		
Experiment 1		0.10 (0.20)	0.19 (0.24)	0.24 (0.24)	0.35 (0.27)	-0.003 (0.42)
Level Effect		-0.40*** (0.07)	-0.37*** (0.07)	-0.40*** (0.07)	-0.35*** (0.07)	-0.39** (0.13)
Self First			-0.13 (0.30)	-0.09 (0.29)	-0.25 (0.31)	0.22 (0.54)
Care				-0.17** (0.06)	-0.22** (0.07)	-0.10 (0.12)
Empathy Score			-0.02 (0.02)	-0.01 (0.02)	-0.03+ (0.02)	0.01 (0.03)
Econ			0.13 (0.23)	0.10 (0.22)	0.10 (0.25)	0.06 (0.39)
Relative Attitude			-0.47 (0.66)	-0.49 (0.65)	0.60 (0.67)	-1.66 (1.22)
Female			0.34 (0.21)	0.31 (0.21)	0.11 (0.22)	0.58 (0.36)
Age			0.01 (0.02)	0.01 (0.02)	-0.01 (0.02)	0.02 (0.03)
Constant	0.11 (0.14)	1.61*** (0.31)	2.03* (0.87)	2.68** (0.89)	3.48*** (0.94)	2.12 (1.57)
Data	All	All	All	All	Similar	Dissimilar
Nr. of observations	226	226	226	226	120	106
R ²	0.04	0.17	0.19	0.21	0.28	0.19

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

subject level, we conduct a mediation analysis. Figure III.11 presents the results of the mediation analysis in a path model. The coefficients and significance levels were computed with OLS regression (see Table III.A4 in Appendix B).

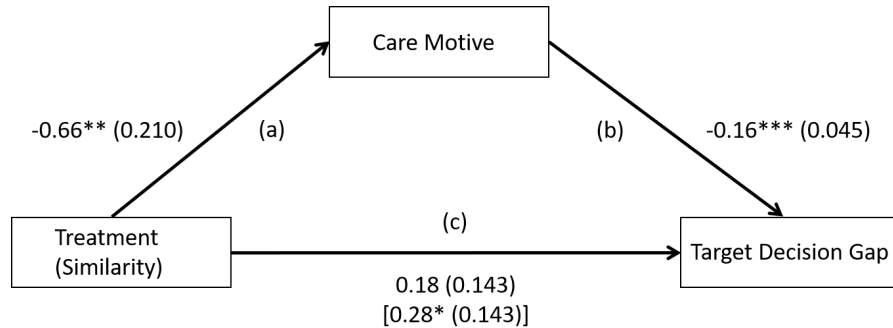


Figure III.11: Path diagram of mediation path in the mixed domain. The coefficients are estimated with OLS (see Table III.A4 in Appendix B). Standard errors are in parenthesis. The estimate in the square brackets corresponds to the treatment effect ignoring care as a potential mediator. The significant levels are: + $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

The analysis reveals that there is a significant treatment effect when ignoring care as a potential mediator (see Table III.A4 *model (1)*). Including care as a mediator into the regression, the treatment effect becomes non-significant (see Table III.A4 *model (3)*). Furthermore, it can be shown that the treatment significantly influences care towards a partner (see Table III.A4 *model (2)*). That is, subjects in the similar treatment care more about their partners' payoff. Estimating the model with the SEM approach, we find that the indirect effect is significant ($p < 0.001$) (illustrated by the pathway (a)-(b)). The direct effect is not significant ($p = 0.224$) (see Table III.A3 in Appendix B). The proportion of the variance mediated by care amounts to approximately 38% ($p = 0.044$). Hence, we find that the treatment difference in the target decision gap is fully mediated by the motive care in the mixed domain.

Table III.2: Regression analysis for Target Decision Gap in the Mixed Domain

	<i>Dependent variable:</i>					
	Δ^M					
	(1)	(2)	(3)	(4)	(5)	(6)
Dissimilar	0.29 ⁺ (0.15)	0.28* (0.14)	0.28* (0.14)	0.18 (0.14)		
Experiment 1		0.05 (0.14)	-0.09 (0.18)	-0.04 (0.17)	-0.03 (0.19)	0.01 (0.30)
Level Effect		-0.30*** (0.06)	-0.30*** (0.06)	-0.32*** (0.06)	-0.23*** (0.06)	-0.42*** (0.10)
Self First			0.29 (0.21)	0.33 (0.21)	0.18 (0.22)	0.43 (0.39)
Care				-0.16*** (0.05)	-0.16** (0.05)	-0.14 ⁺ (0.08)
Empathy Score			-0.002 (0.01)	0.0005 (0.01)	-0.01 (0.01)	0.01 (0.02)
Econ			-0.03 (0.16)	-0.06 (0.16)	0.01 (0.18)	-0.14 (0.28)
Relative Attitude			-0.25 (0.46)	-0.30 (0.45)	-0.50 (0.48)	0.02 (0.84)
Female			0.15 (0.15)	0.13 (0.15)	0.24 (0.16)	0.01 (0.26)
Age			0.02 (0.01)	0.02 (0.01)	0.03 ⁺ (0.02)	0.01 (0.02)
Constant	0.19 ⁺ (0.10)	1.17*** (0.22)	0.61 (0.63)	1.20 ⁺ (0.63)	1.28 ⁺ (0.69)	1.22 (1.12)
Data	All	All	All	All	Similar	Dissimilar
Nr. of observations	226	226	226	226	120	106
R ²	0.02	0.13	0.15	0.20	0.22	0.20

Note:

+ p<.1; * p<.05; ** p<.01; *** p<.001

III.7 Discussion and Conclusion

In this paper, we present a novel approach to study risk taking for others. By varying the objective and perceived similarity to a partner, we are able to manipulate the social distance towards said partner. This allows us to compare target decision gaps for various strangers, while keeping information about the partners' risk preferences constant. Other approaches compare risk taking between two different targets, be it for oneself, friends or strangers, whereby knowledge about risk preferences varies between the targets. Subjects have full information about their own preferences, partial information about their friends' preferences and none about strangers preferences. Any differences in risk taking might therefore be driven by differences in knowledge about risk preferences or anonymity. By comparing target decision gaps, i.e., within differences in risk taking for oneself and a partner, we are able to control for the information gap and investigate how differences in similarity and social distance influence behavior. Our approach relies on the induction of perceived similarity in meaningful, objective personal characteristics which allow no inferences on risk preferences, namely personality traits. In two between subjects designs, we matched each subject with either a similar or a dissimilar partner based on pre-elicited Big-Five personality traits and measure risk preferences for oneself and the partner in the gain and in the mixed domain. We analyse differences in the target decision gaps between the similar and dissimilar treatment groups and investigate mechanisms for the observed effects. Specifically, we explore Construal Level Theory (CLT), which predicts increased risk taking for dissimilar partners and negligible differences for similar partners, as the main driver of the target decision gap. As other studies have found a consistent effect of social distance on social preferences and a care motive, we investigate care towards a partner as an alternative mechanism for the decision process. In the gain domain, we find strong empirical evidence in favor of CLT, whereas in the mixed domain we find that some evidence for CLT, but also stronger evidence for emotional decision making based on the motive care.

The results in the gain domain offer strong support for CLT as a relevant pathway for a risky shift in decision making for others. Not only do we find the predicted large positive target decision gaps for dissimilar partners, but we also find truly negligible differences when deciding for a similar partner. Finding evidence for this highly specific hypothesis on the sign and size of the effect for similar partners makes a strong case in favor of CLT. Our results are robust to the alternative explanations of care towards the partner and order effects. Specifically, decisions for oneself do not seem to be influenced by the knowledge of a dissimilar partner, as suggested by

Li et al. (2017).

In the mixed domain, CLT does not seem to be the main driver of our results. Contrary to the predictions of CLT, we find positive target decision gaps for both similar and dissimilar partners. While there is a tendency for larger target decision gaps for dissimilar partners, this difference is not robust when controlling for alternative explanations. In contrast to the gain domain, the prosocial motive of care, which was also manipulated by the similarity induction, seems to be an important determinant for the decision process. Specifically, we find significantly less loss aversion for both partners in the mixed domain, with loss aversion being attenuated with the stated care towards the partner. This is also confirmed by a mediation analysis, which leads to the conclusion that care fully mediates the treatment effect.

We conclude that target decision gaps are strongly influenced by social distance, which triggers different mechanisms in the gain and mixed domain. While increased construal seems to be the main mechanism in the gain domain, care seems to be the main driver in the mixed domain. In the presence of losses, lower stated care towards the partner leads to an empathy gap, which decreases loss aversion. This is in line with previous literature, which finds less loss aversion when deciding for strangers (Andersson et al., 2016; Vieider et al., 2016; Pahlke et al., 2012; Füllbrunn and Luhan, 2017) and decreased loss aversion when deciding for strangers rather than friends (Faro and Rottenstreich, 2006; Montinari and Rancan, 2018; Ziegler and Tunney, 2012). Trautmann and van de Kuilen (2012) have compared the preciseness of prospect theory and CLT in predicting risky decisions in the context of decision making for others. They found evidence that psychological effects modeled by prospect theory, such as loss aversion, are stronger than the impact of psychological distance. Lastly, just as Montinari and Rancan (2018), we find significant order effects in the mixed domain, which influences the target decision gap between treatments.

Our findings suggest three important conclusions. First, on a methodological level, it is important to study target decision gaps, i.e. within differences of the decision for oneself and others. In particular, our approach lends itself to the variation of other aspects of the partner, by disclosing belief relevant information about age, gender or other aspects that are associated with risk preferences. Hence, it is possible to disentangle the effect of similarity and stereotypes about the decision target. For example, one could study the effects of gender for decision targets with different degrees of similarity. In this way, our approach could be promising to gain a more comprehensive understanding on the effect of the identity of the decision target in decision making for others. Second, on an empirical level we provide further evidence

for a risky shift in the gain and mixed domain. In particular, we identify social distance as an important determinant of risk taking for others in a setting which allows to manipulate the degree of similarity, without varying the information or beliefs about the other's risk preferences. Third, we suggest and test two mechanisms that might explain the target decision gap, the level of abstraction in the decision process and care towards the partner. We find that the level of abstraction, which increases with higher social distance (according to CLT), is the main driver in the gain domain, while care towards the partner, which increases with higher social distance, is the main driver in the mixed domain.

Some limitations of our study should be addressed in the future. We only consider one decision in the gain and mixed domain respectively, in which probabilities and outcomes are varied simultaneously. It is well known that both probabilities and outcomes influence the decision in a non-linear fashion, and that preferences exhibit a fourfold pattern of risk attitudes. To address this issue, one should study more extensive risk tasks that study the variation of probabilities and outcomes separately, such as Pahlke et al. (2012). In such a framework, it would be interesting to investigate how similarity and social distance effect the value function and probability weighting. At the same time it would be interesting to further investigate CLT more precisely as a relevant pathway. Therefore, it would be important to construct and validate a standardized questionnaire on CLT which can be used to conduct mediation analyses.

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Appendix A: Additional Results

In this section, for completeness, we present the analysis for the full sample and for risk seeking individuals separately. As mentioned above, we formulated our main hypothesis regarding CLT for risk averse subjects. While risk aversion is considered as a bias, the motivation for risk seeking preferences is not clear and we are not able to make a meaningful prediction due to the few observations for risk seeking subjects. According to CLT, risk seeking subjects should take less of a risk for their partner if a debiasing takes place, hence, they should shift towards risk neutrality when deciding for a dissimilar partner. Figure III.A1 and Figure III.A2 show the switching points in the gain domain by treatment for the full sample and risk seeking subjects in Experiment 1 respectively. In the full sample, the effect of CLT is still present. We find no treatment difference in the decision for oneself ($z = -1.170$, $p = 0.242$) and subjects in the dissimilar treatment take more of a risk for their partner, compared to subjects in the similar treatment ($z = -2.439$, $p = 0.014$). The target decision gap is positive for a dissimilar partner ($z = -2.439$, $p = 0.014$) and not significantly different from zero for a similar partner ($z = -0.536$, $p = 0.592$). Running a TOST reveals that the two decisions for oneself and the partner are equivalent in the similar treatment ($d = 0.042$, $t(74) = 2.098$, $p = 0.020$).

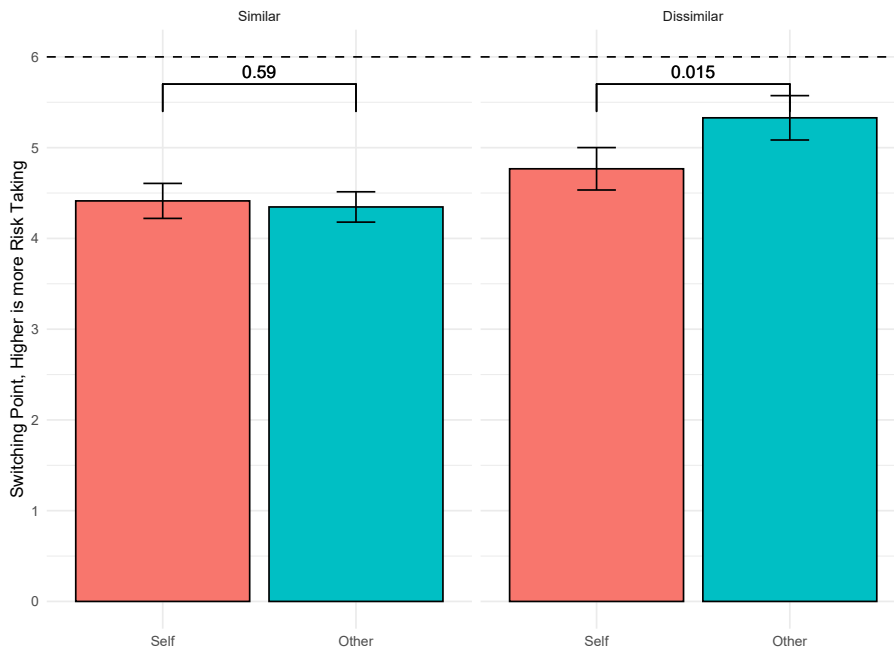


Figure III.A1: Switching point in the gain domain for the full sample in Experiment 1

For risk seeking individuals, it seems that, in both treatments, subjects shift their

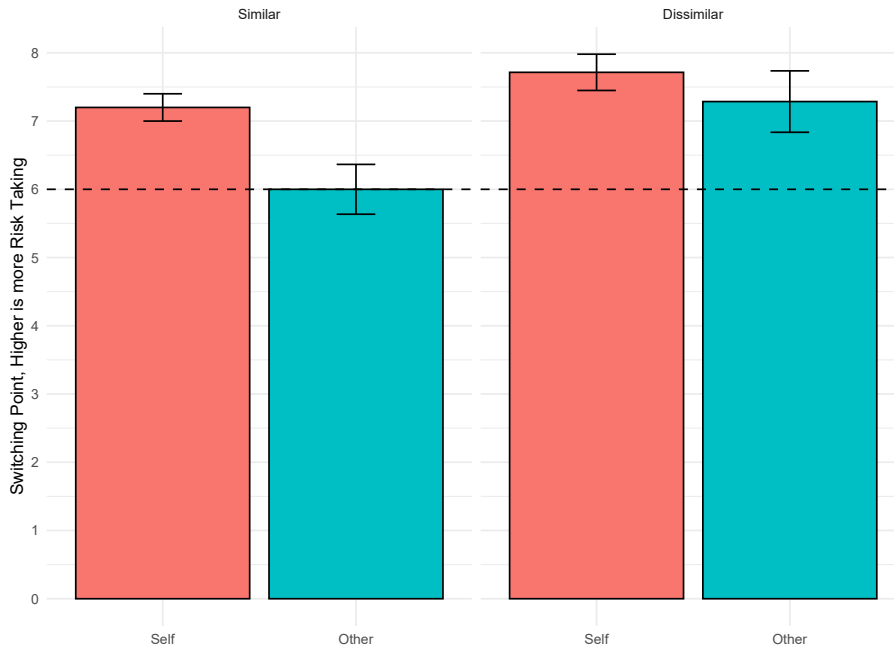


Figure III.A2: Switching point in the gain domain for risk seeking subjects in Experiment 1

decision for the partner towards risk neutrality, while the effect is stronger in the similar treatment. Conversely, risk seeking subjects also take more risk for their partner in the dissimilar treatment ($z = -2.092$, $p = 0.036$) compared to subjects in the similar treatment ($z = -2.700$, $p = 0.007$). As a result, both target decision gaps are significantly negative, whereas the target decision gap for a similar partner is larger ($z = -1.894$, $p = 0.058$). This indicates that CLT does not explain the behavior of risk seeking individuals. Again, these results have to be interpreted with caution, since we have very few observations. To examine a clear direction of the effect further research of risk seeking subjects has to be conducted.

Analogously, in the mixed domain we find a significantly positive decision gap in both treatments in the full sample. Subjects are less loss averse in the decision for their partner irrespectively of whether they are matched with a similar ($Z = -1.992$, $p = 0.046$) or a dissimilar partner ($z = -2.355$, $p = 0.019$) and the gaps are not significantly different from each other ($z = -1.057$, $p = 0.291$). Considering only risk seeking subjects, we do not find a significant difference between the treatments and the decisions for oneself and the partner. On average risk seeking subjects are less loss averse for themselves, and take a similar decision for their partner in both the similar ($z = -0.289$, $p = 0.772$) and the dissimilar treatment ($z = -0.257$, $p = 0.797$). Again, the result for the risk seeking individuals has to be interpreted with caution, due to the small sample size.

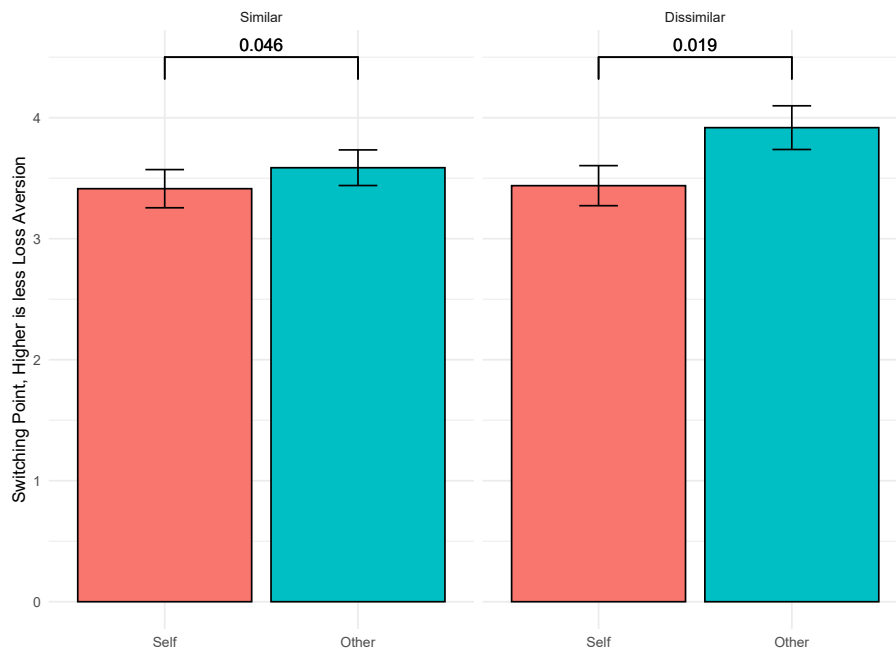


Figure III.A3: Switching point in the mixed domain for the full sample in Experiment 1

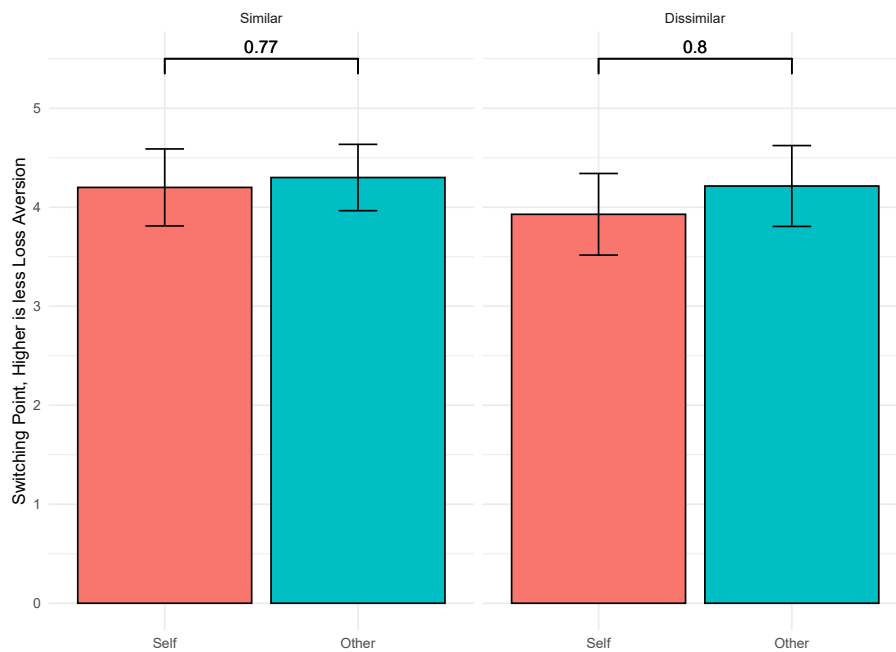


Figure III.A4: Switching point in the mixed domain for risk seeking subjects in Experiment 1

In Experiment 2, we find the same pattern. In the full sample, subjects matched with a dissimilar partner take significantly more of a risk for their partner compared to subjects matched with a similar partner ($z = -2.747$, $p = 0.006$). The target

decision gap for a dissimilar partner is significantly positive ($z = -1.737, p = 0.082$), while the target decision gap for a similar partner is not significantly different from zero ($z = -0.399, p = 0.680$). However, the target decision gaps are not significantly different from each other ($z = -1.485, p = 0.138$). This is mainly driven by a significant difference in the decision for oneself. A visual inspection of Figure ?? suggests that subjects in the dissimilar treatment take more risk for themselves, compared to subjects in the similar treatment. This is statistically confirmed by a Wilcoxon-Rank-Sum test ($z = -2.133, p = 0.033$). We attribute this effect to a sampling error, since we have only one risk seeking subject in the similar treatment and 7 subjects in the dissimilar treatment.

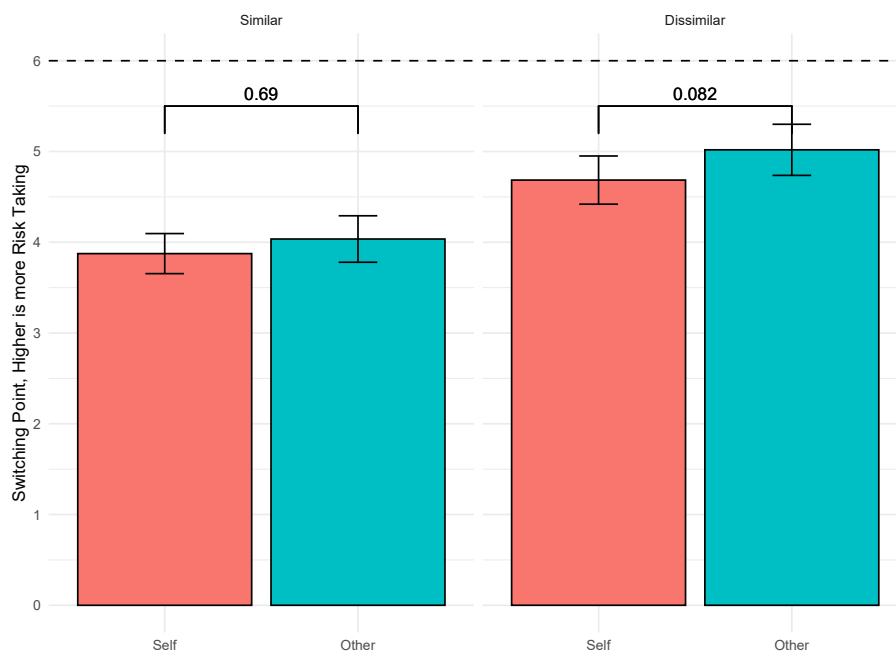


Figure III.A5: Switching point in the gain domain for risk seeking subjects in Experiment 2

For risk seeking subjects, we abstain from a statistical analysis, due to the small sample size. Nevertheless, it seems that subjects in the dissimilar treatment shift the decision for their partner towards risk neutrality. Unfortunately, we cannot make a meaningful comparison to the similar treatment.

In the mixed domain, we find no effect of CLT in the full sample. In the dissimilar treatment, there are subjects are less loss averse in the decision for the partner ($z = -2.327, p = 0.020$) and marginally no statistically significant differences between the decision for oneself and the partner in the similar treatment ($z = -1.533, p = 0.125$), however, equivalence is rejected by a TOST ($d = -0.203, t(55) = -0.505, p = 0.308$). Additionally, the target decision gaps are not statistically different from

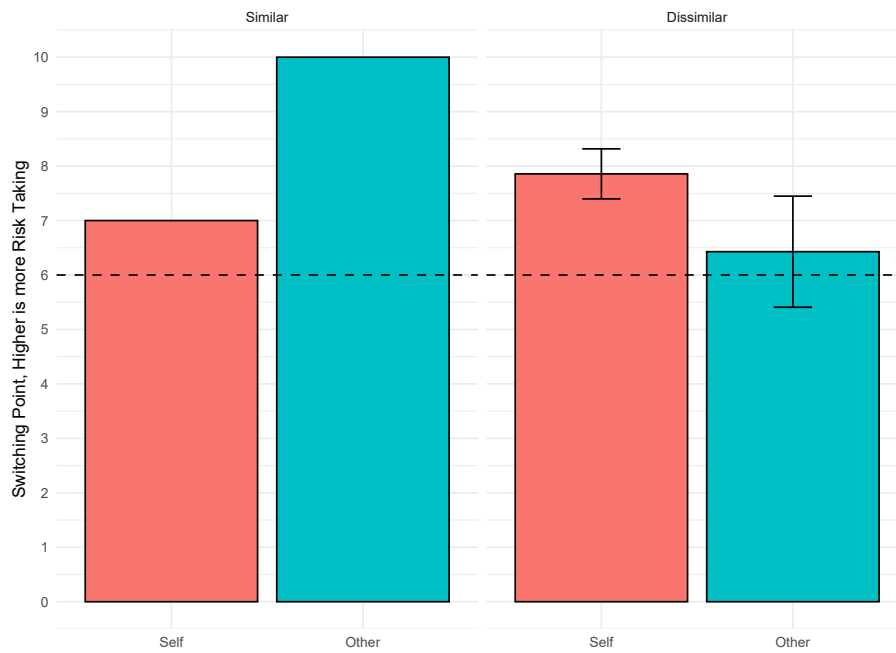


Figure III.A6: Switching point in the gain domain for risk seeking subjects in Experiment 2

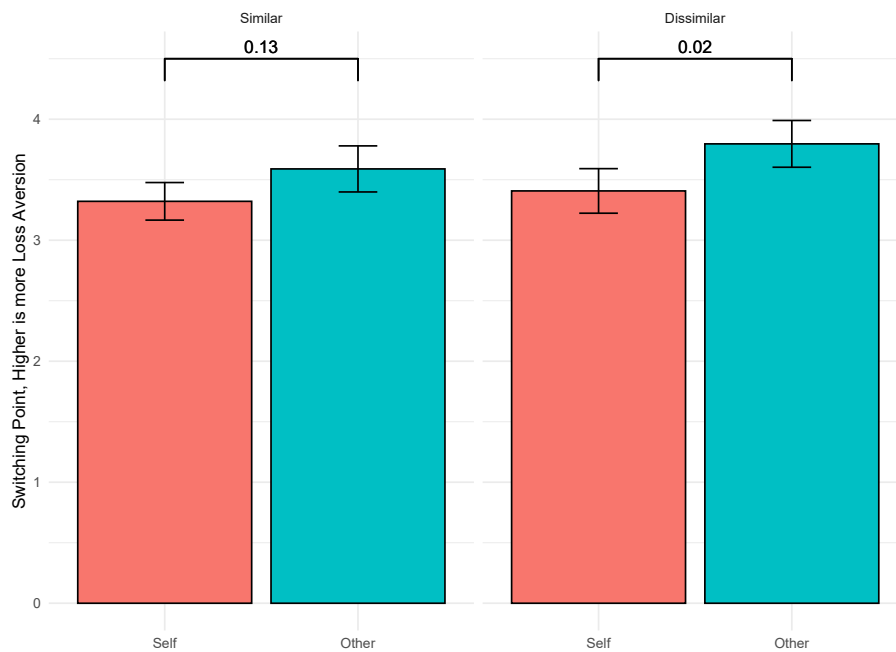


Figure III.A7: Switching point in the mixed domain for risk seeking subjects in Experiment 2

each other ($z = -0.924$, $p = 0.356$).

Again, we cannot make a statistically meaningful statement about the subsample of risk seeking individuals due to the few observations. However, it seems in the

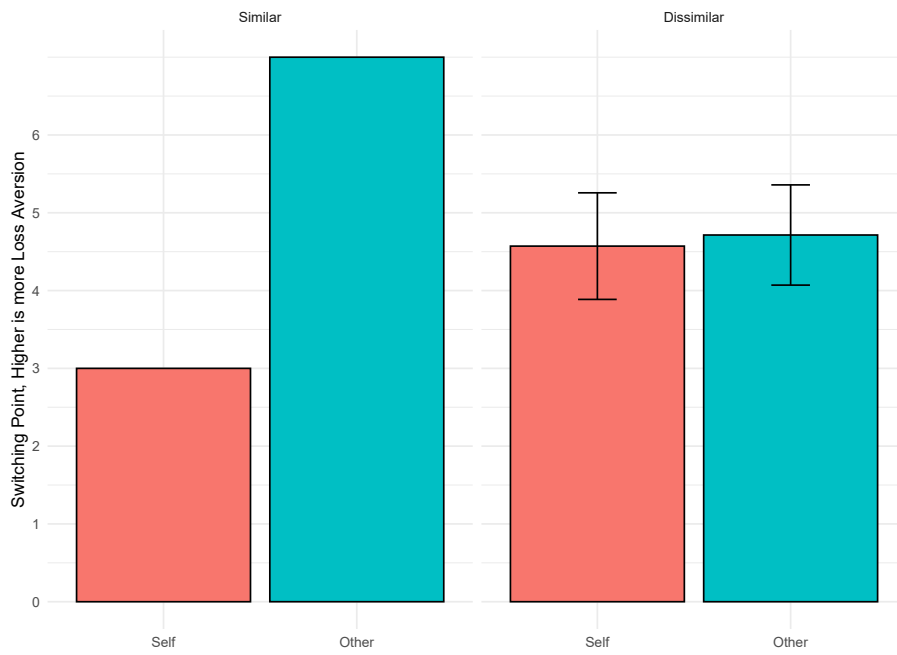


Figure III.A8: Switching point in the mixed domain for risk seeking subjects in Experiment 2

dissimilar treatment, that there is no difference between the decision for oneself and the partner, which supports the finding in Experiment 1.

Appendix B: Additional Tables and Figures

Table III.A1: Mediation analysis of care in the gain domain.

	Estimate	95%-CI Lower	95%-CI Upper	p-value
Average indirect effect	0.11	0.024	0.24	0.008**
Average direct effect	0.56	0.173	0.94	0.004**
Average total effect	0.67	0.294	1.05	< 0.001***
Proportion mediated	0.16	0.035	0.43	0.008**
Nr. of observations				226
Nr. Simulations				500

Note: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001

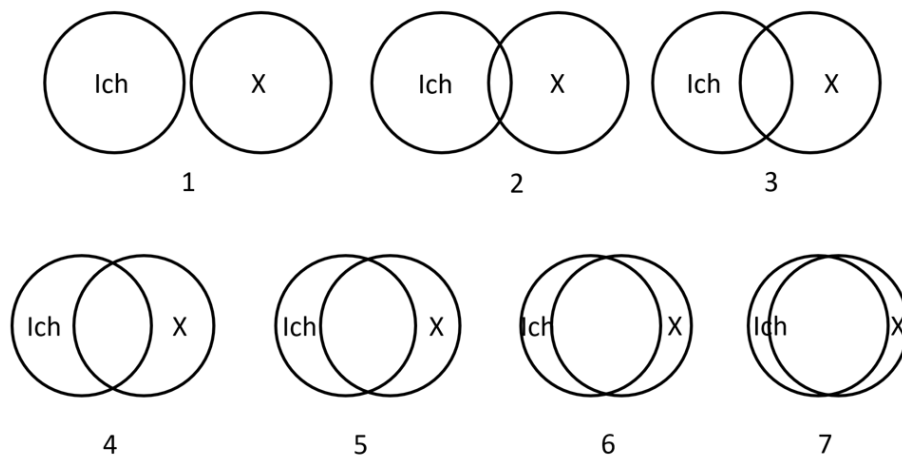


Figure III.A9: Incision of the Other in the Self (IOS) Scale

Table III.A2: Mediation Analysis with OLS in the gain domain

	<i>Dependent variable:</i>		
	Δ_D^G	Care	Δ_D^G
	(1)	(2)	(3)
Dissimilar	0.68*** (0.20)	-0.63** (0.21)	0.57** (0.20)
Care			-0.17** (0.06)
Experiment 1	0.19 (0.24)	0.31 (0.26)	0.24 (0.24)
Level Effect	-0.37*** (0.07)	-0.12+ (0.07)	-0.40*** (0.07)
Self First	-0.13 (0.30)	0.24 (0.31)	-0.09 (0.29)
Empathy Score	-0.02 (0.02)	0.02 (0.02)	-0.01 (0.02)
Econ	0.13 (0.23)	-0.18 (0.24)	0.10 (0.22)
Relative Attitude	-0.47 (0.66)	-0.13 (0.70)	-0.49 (0.65)
Female	0.34 (0.21)	-0.13 (0.22)	0.31 (0.21)
Age	0.01 (0.02)	0.01 (0.02)	0.01 (0.02)
Constant	2.03* (0.87)	3.77*** (0.92)	2.68** (0.89)
Nr. of observations	226	226	226
R ²	0.19	0.09	0.21

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

Table III.A3: Mediation analysis of care in the mixed domain.

	Estimate	95%-CI Lower	95%-CI Upper	p-value
Average indirect effect	0.11	0.030	0.21	< 0.001***
Average direct effect	0.17	-0.104	0.44	0.224
Average total effect	0.28	0.009	0.55	0.044*
Proportion mediated	0.38	0.062	2.72	0.044*
Nr. of observations				226
Nr. Simulations				500

Note: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001

Table III.A4: Mediation Analysis with OLS in the mixed domain

	<i>Dependent variable:</i>		
	Δ_D^M	Care	Δ_D^M
	(1)	(2)	(3)
Dissimilar	0.28* (0.14)	-0.66** (0.21)	0.18 (0.14)
Care			-0.16*** (0.05)
Experiment 1	-0.09 (0.18)	0.31 (0.26)	-0.04 (0.17)
Level Effect	-0.30*** (0.06)	-0.13 (0.08)	-0.32*** (0.06)
Self First	0.29 (0.21)	0.24 (0.31)	0.33 (0.21)
Empathy Score	-0.002 (0.01)	0.02 (0.02)	0.0005 (0.01)
Econ	-0.03 (0.16)	-0.19 (0.24)	-0.06 (0.16)
Relative Attitude	-0.25 (0.46)	-0.33 (0.68)	-0.30 (0.45)
Female	0.15 (0.15)	-0.12 (0.22)	0.13 (0.15)
Age	0.02 (0.01)	0.01 (0.02)	0.02 (0.01)
Constant	0.61 (0.63)	3.77*** (0.92)	1.20+ (0.63)
Nr. of observations	226	226	226
R ²	0.15	0.09	0.20

Note: + p<.1; * p<.05; ** p<.01; *** p<.001

	Lottery A	Expected Payoff	Lottery B	Expected Payoff
1	€6, 10%; €4.80, 90%	€4.92	€11.55, 10%; €0.30, 90%	€1.43
2	€6, 20%; €4.80, 80%	€5.04	€11.55, 20%; €0.30, 80%	€2.55
3	€6, 30%; €4.80, 70%	€5.16	€11.55, 30%; €0.30, 70%	€3.68
4	€6, 40%; €4.80, 60%	€5.28	€11.55, 40%; €0.30, 60%	€4.80
5	€6, 50%; €4.80, 50%	€5.40	€11.55, 50%; €0.30, 50%	€5.93
6	€6, 60%; €4.80, 40%	€5.52	€11.55, 60%; €0.30, 40%	€7.05
7	€6, 70%; €4.80, 30%	€5.64	€11.55, 70%; €0.30, 30%	€8.18
8	€6, 80%; €4.80, 20%	€5.76	€11.55, 80%; €0.30, 20%	€9.30
9	€6, 90%; €4.80, 10%	€5.88	€11.55, 90%; €0.30, 10%	€10.43
10	€6, 100%; €4.80, 0%	€6.00	€11.55, 100%; €0.30, 0%	€11.55

Table III.A5: Lottery choices in the gain domain

	Lottery	Expected Payoff
1	€6, 50%; -€2, 50%	€2.00
2	€6, 50%; -€3, 50%	€1.50
3	€6, 50%; -€4, 50%	€1.00
4	€6, 50%; -€5, 50%	€0.50
5	€6, 50%; -€6, 50%	€0.00
6	€6, 50%; -€7, 50%	-€0.50

Table III.A6: Lottery choices in the mixed domain

Appendix C: Instructions

Experiment 1

You are participating in a study on economic decision making, and in the following you will be asked to make a number of decisions. Please read the following instructions carefully. You have the opportunity to earn money that will be paid out in private and cash at the end of the study. During the study, you are not allowed to talk to the other participants. If you have questions, please raise your hand, and one of the experimenters will approach you, and answer your question.

The study consists of two parts. Only the first part is payoff relevant for you.

The first part of the study consists of two tasks. In each of the two tasks, you are asked to decide between a series of lotteries. In the following, the tasks are explained in detail. During the task the explanations are displayed in the lower part of your decision screen, in case you want to read them again.

Part I

Task I

In this task, you are asked to decide repeatedly between two lotteries. The payoff of the lotteries do not change, however the probabilities of the higher payoff increase from 10% to 100% Figure 1 shows an example. To better understand the respective probabilities, please imagine a ten-sided dice. If you choose option A, you receive €6, if a 1 or 2 was rolled. (20%). If a 3, 4, 5, 6, 7, 8, 9 or 10 (80%) was rolled, you receive €4,80.

If you choose Option B, you receive €11,55, if a 1 or 2 was rolled (20%). Accordingly, you receive €0.30, if a 3, 4, 5, 6, 7, 8, 9 or 10 (80%) was rolled.

Option A		Option B
With a probability of 20 % you receive 6 €	<input type="radio"/>	With a probability of 20 % you receive 11.55 €
With a probability of 80 % you receive 4.80 €	<input type="radio"/>	With a probability of 80 % you receive 0.30 €

Figure 1

At the end of the task, you will have the possibility to review your decision and revise it if you want to.

If this task is paid out at the end of the study, one of the lotteries is chosen randomly and paid out according to your decision.

[Task I starts here.]

Task II

In this task, you receive an initial endowment of €7.

In the following, you decide if you want to accept or reject the presented lotteries.

In these lotteries, you can either win or lose a certain amount of money.

The amount of money and the probability of the amount of money you can win is the same across the lotteries. Only the amount you can lose decreases. Figure 2 shows an example. If you accept the lottery, it will be randomly determined if you win or lose.

If you accept the lottery in the example (Figure 2), you win €6 or lose €2 with a probability of 100%.

Lottery	Accept	Reject
With a probability of 50 % you lose 2 €	<input type="radio"/>	<input type="radio"/>
With a probability of 50 % you win 6 €	<input type="radio"/>	<input type="radio"/>

Figure 2

At the end of the task, you will have the possibility to review your decision and revise it if you want to.

If this task is paid out at the end of the study, one of the lotteries is chosen randomly and paid out according to your decision.

Part II

In this Part of the study, you are matched with a partner.

Before the study, 656 participants completed the "Big Five" personality questionnaire as you did. Now you will be matched with a partner out of these 656 participants, who is the most similar/dissimilar to you based on the "Big Five" personality questionnaire.

[Figure III.2 enters here.]

Please note that that highest score for each dimension is 48 and the lowest is 0.

[Here the participants were invited to reflect on their partner. The page was displayed for 1 minute and could not be skipped.] As mentioned before, you are matched with one of the 656 participants who is the most similar/dissimilar to you based on the "Big Five" questionnaire. In the following graph, your personality profile and the personality profile of your partner are presented.

Please think about your partner.

Which characteristics does your partner have?

[Task I and Task II are presented again in the same order and subjects decide for their partner.]

Experiment 2

You are participating in a study on economic decision making, and in the following you will be asked to make a number of decisions. Please read the following instructions carefully. You have the opportunity to earn money that will be paid out in private and cash at the end of the study. During the study, you are not allowed to talk to the other participants. If you have questions, please raise your hand and one of the experimenters will approach you and answer your question.

Before the study, 656 other participants completed the "Big Five" personality questionnaire as you did. The specific questions are associated with one of the five dimensions namely, Neuroticism, Openness to experience, Extraversion, Conscientiousness and Agreeableness. This classification has been evaluated as meaningful in numerous studies, and is used among others in professional coaching and assessment centers. Hence, your personality profile matches your personality very well. At the end of the study, you receive a personalized analysis of your personality profile. At the beginning of the study, you are matched with the person out of the pool of 656 participants who is the the most similar/dissimilar to you based on the "Big Five" personality questionnaire.

This study consists of two parts. In both parts, you are asked to take decisions for you and for your partner you are matched with. Part I consists of two subparts. To determine your finale payment at the end of the study, either part I or part II are randomly determined and you and your partner are paid out according to your decisions in the respective part. Please note that only you can take decisions, while your partner is not able to influence his/her own payment.

Part I consists of three rounds. In each round you are ask to make a decision once for yourself, once for your partner and once for you and your partner. The rounds are labeled by "me", "my partner" and "both of us".As the number of rounds is randomly drawn, you will be informed at the beginning of each round for whom you will make a decision. Since you do not know which decision will be payoff relevant, you should always make the decision as if it were the one that is paid out.

Each of the three rounds consists of two subparts. The subparts are identical in each round. The rounds only differ in that you have to decide once for yourself, once for your partner and once for yourself and your partner. In each subpart, you are asked to choose between lotteries. In the following the lottery tasks are described in detail. If you want to revisit the instructions at any point, you will find them on the bottom of your screen.

If subpart 1 or subpart 2 of part I is determined as payoff relevant, either round "me" or "both of us" is randomly drawn. If round "me" is drawn, then your partner will be paid according to your decision in round "my partner". If round "both of us" is drawn, your partner will be paid according to your decision you made for both of you. You will not be informed about the payoff for your partner and the outcome of each lottery will be determined separately.

As mentioned before, you are matched with one of the 656 participants who is the most similar to you based on the "Big Five" questionnaire. In the following graph, your personality profile and the personality profile of your partner are presented.

[Figure III.2 enters here.]

Please note that that highest score for each dimension is 48 and the lowest is 0.

[Here the participants are invited to reflect on their partner. The page was displayed only before the subjects decided for their partner for 1 minute, and could not be skipped.] As mentioned before, you are matched with one of the 656 participants who is the most similar/dissimilar to you based on the "Big Five" questionnaire. In the following graph, your personality profile and the personality profile of your partner are presented.

Please think about your partner.

Which characteristics does your partner have?

Part I of the study

[The decision for oneself and the partner were taken in randomized order. If the subject decided first for his/her partner and then for him- or herself, the subject complete Task I and Task II for his/her partner in subpart I, and for him- or herself in subpart II and vice versa.]

Task I

In this task, you are asked to decide repeatedly between two lotteries. The payoff of the lotteries does not change, but the probabilities of the higher payoff increase from 10% to 100%. Figure 1 shows an example. To better understand the respective probabilities, please imagine a ten-sided dice. If you choose option A, you receive €6, if a 1 or 2 was rolled. (20%). If a 3, 4, 5, 6, 7, 8, 9 or 10 (80%) was rolled, you receive €4,80.

If you choose Option B, you receive €11,55, if a 1 or 2 was rolled (20%). Accordingly, you receive €0.30, if a 3, 4, 5, 6, 7, 8, 9 or 10 (80%) was rolled.

Option A		Option B
With a probability of 20 % you receive 6 €	<input type="radio"/>	With a probability of 20 % you receive 11.55 €
With a probability of 80 % you receive 4.80 €	<input type="radio"/>	With a probability of 80 % you receive 0.30 €

Figure 1

At the end of the task, you will have the possibility to review your decision and revise it if you want to.

If this task is paid out at the end of the study, one of the lotteries is chosen randomly and paid out according to your decision.

[Task I starts here.]

Task II

In this task, you receive an initial endowment of €7.

In the following, you decide if you want to accept or reject the presented lotteries.

In these lotteries, you can either win or lose a certain amount of money.

The amount of money and the probability of the amount of money you can win is the same across the lotteries. Only the amount you can lose decreases. Figure 2 shows an example. If you accept the lottery, it will be randomly determined if you win or lose.

If you accept the lottery in the example (Figure 2), you win €6 or lose €2 with a probability of 100%.

Lottery	Accept	Reject
With a probability of 50 % you lose 2 €	<input type="radio"/>	<input type="radio"/>
With a probability of 50 % you win 6 €		

Figure 2

At the end of the task, you will have the possibility to review your decision and revise it if you want to.

If this task is paid out at the end of the study, one of the lotteries is chosen randomly and paid out according to your decision.

Part II of the study

In this part of the study, you receive €10, which you can distribute between you and your partner. Your partner receives nothing. If this part is paid out, your partner receives the amount you transferred to him/her. You get the remaining amount. If this part is randomly chosen to be paid out, you and your partner only receive the amounts you chose in this task.

Appendix

Eidesstattliche Erklärung

Erklärung zum selbständigen Verfassen der Arbeit:

Ich erkläre hiermit, dass ich meine Doktorarbeit „The Role of Care in Economic Decision Making“ selbstständig und ohne fremde Hilfe angefertigt habe und dass ich als Koautor maßgeblich zu den weiteren Fachartikeln beigetragen habe. Alle von anderen Autoren wörtlich übernommenen Stellen, wie auch die sich an die Gedanken anderer Autoren eng anlehnenden Ausführungen der aufgeführten Beiträge wurden besonders gekennzeichnet und die Quellen nach den mir angegebenen Richtlinien zitiert.

Kiel, den 1. Februar 2022

Christoph Schütt