The role of
social cues and social reference points
in economic decision-making

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Matthias Praxmarer, M.Sc.
aus Umhausen, Tirol, Österreich

Referent:	Prof. Dr. Matthias Sutter
Korreferentin:	Prof. Dr. Bettina Rockenbach
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To my family.

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CONTENTS

LIST OF FIGURES

LIST OF TABLES

LIST OF ABBREVIATIONS

BFH **B**artling, **F**ehr, and **H**erz
DG **D**ictator **G**ame
DM **D**ecision **M**aker
e.g. **e**xempli **g**ratia (for example)
EUT **E**xpected **U**tility **T**heory
GLS **G**eneralized **L**east **S**quares
i.e. **i**d **e**st
IV **I**ntrinsic **V**alue
M **M**ean
OLS **O**rdinary **L**east **S**quares
PD **P**risoner's **D**ilemma
PST **P**ortfolio **S**election **T**heory
R^2 **R**-squared; Statistical measure (Coefficient of determination)
SD **S**tandard **D**eviation
SVO **S**ocial **V**alue **O**rientation
vs. **v**ersu**s**
y. **y**ears

CHAPTER 1

INTRODUCTION

Actors do not behave or decide as atoms outside a social context, [rather] their actions are embedded in concrete, ongoing systems of social relations.
Granovetter (1985, p. 487)

This thesis aims to shed light on how the social environment affects economic decision-making. Humans are social beings and the surrounding social environment has tremendous influence on individuals (e.g., Salganik et al., 2006). Hence, human decision-making does not take place in a social vacuum, but is surrounded and influenced by social context, such as social information about and (social) interaction with others (e.g., Deutsch and Gerard, 1955; Granovetter, 1985; Larrick, 2016).

In economics, standard economic theory remains silent on social determinants of human decision-making in economically relevant situations. In contrast, the rising field of behavioral economics enriches economic analysis by incorporating findings from other academic fields, such as (social) psychology, evolutionary biology, anthropology, and sociology. The power and strength of this approach is that it incorporates, among others, social determinants of economic decisions and could therefore contribute to the understanding of human decision-making. A significant contribution to the development of behavioral economics comes from data gathered from experimental research (Villeval, 2007), since some experiments are not designed to test standard economic theories, but to capture, for instance, social aspects of economic decisions (Schmidt, 2009). In general, the experimental approach in economics is well-established (e.g., Harrison and List, 2004; Falk and Heckman, 2009; Levitt and List, 2009) and developed efficient tools to measure the impact of certain factors on economic decisions by isolating them in a controlled environment.

This thesis comprises four independent research articles that examine how the social environment affects human decision-making by investigating different dimensions of it. In particular, I will address (i) whether social cues such as age influence cooperative behavior of individual decision makers, (ii) if and how changing the decision environment from individual to group decision-making affects the psychic benefits of holding a decision right, and (iii) whether social reference points in the form of peer income trigger relative income concerns, and thus affect honesty. According to the underlying research questions, this thesis consists of two parts. In the first part, *Chapter 2* and *Chapter 3*, I focus on whether belonging to different generations (i.e., being young, old) affects behavior in social dilemma situations. While previous findings highlight high levels of cooperation among complete strangers, one information about a "stranger" is relatively easy to observe, the age. In terms of standard economic theory, this information should not lead to a different behavior. However, from an evolutionary point of view the intergenerational transmission of cooperative norms is fundamental for establishing large-scale cooperation (Henrich et al., 2015). Hence, age seems to play a crucial role in this context. Along this line, I examine two potential mechanisms of norm transmission from older to younger generations, namely an (i) institutionalized channel via punishment (*Chapter 2*), and an (ii) institution-free channel via social and cultural learning (*Chapter 3*).

The second part of this thesis consists of *Chapter 4* and *Chapter 5*. *Chapter 4* discusses a topic which is at the heart of economic decision-making, namely the value decision makers assign to having the right to decide about outcomes. Standard economic theory would predict that simply the outcomes at stake matter, but recent behavioral findings show that having the right to decide creates a value itself, i.e., non-material benefits of having a decision right (e.g., Bartling et al., 2014). In particular, we are interested in how this value is affected by the decision-making environment. Therefore, we investigate whether non-material values of having a decision right change if individuals decide on their own or within a group. In *Chapter 5*, we discuss the impact of the social environment on moral and ethical decision-making. Given that humans are embedded in a social structure and are surrounded by others, it is inevitable to get information about them or to observe certain characteristics. This information allows them to compare themselves with others, i.e., to engage in social comparison. In this study, we investigate whether social information, i.e., information about the income of a peer, affects honesty in a situation where dishonesty could be used for monetary gains.

Before I summarize all chapters of this thesis, I want to discuss the methodological approaches used in this thesis. I used either laboratory experiments or artefactual field experiments (Harrison and List, 2004) as foundations of my research projects. While both chapters in the second part of the thesis are standard laboratory experiments involving a standard subject pool (university students), the research questions in the first part forced me to enrich this toolbox. In *Chapter 2* I conducted a laboratory experiment where we invited students and non-students to the lab (i.e., a combination of a standard and non-standard subject pool), while in *Chapter 3* I ran the experiment in the field with subjects drawn from the general population and recruited on the spot. In what follows, I give a summary of each research project.

Summary of the research projects

Chapter 2 with the title ***"Cooperation and Third-Party Punishment Within and Across Generations: An Experimental Study with Juniors and Seniors"*** is joint work with *Bettina Rockenbach and Matthias Sutter*.[1] In this paper, we examine whether or not the institution of third-party punishment could help to transmit innergenerational cooperative norms from older to younger generations. Therefore, we invited two generations to our lab, i.e., students in their 20ies and people drawn from the population 50 years and older. Our vehicle of research to test this potential mechanism is a repeated Prisoner's Dilemma game with costly third-party punishment. First, both players simultaniously decide whether they want to cooperate with each other or not. After the decisions of the players, the observer learns the decisions of both players and could punish them at a cost.

We report findings from four treamtents where we vary the interaction between both generations in a systematic way and the generation composition is common knowledge in all sessions, i.e., two homogeneous treatments where players and observers belong to the same generation and two heterogeneous treatments where players belong to the same generation and the observer belongs to the other generation.

We find that members of the older generation achieve significantly higher cooperation rates than members of the younger generation. Moreover, members of the older generation punish norm violations more often than members of the younger generation. Interestingly, the generation of the third-party observer has no impact on the aggregated cooperation rates in both generations.

Chapter 3 called ***"The Secret of Success: Older Generations Teach Cooperation to Younger Generations"*** is joint work with *Stefania Bortolotti, Wilhelm Hoffmann, Angelo Romano and Matthias Sutter*.[2] This study aims to test hypotheses on intergenerational transmission of cooperative norms derived from an evolutionary theory on social learning, namely prestige-biased learning. This theory postulates that younger generations learn from prestigious and experienced members of the society. In particular, followers condition their behavior toward prestigious members of the society, and the prestigious members enforce cooperation with high levels of generosity toward their followers. It

[1]All authors contributed equally to this project. Financial support from the University of Innsbruck is gratefully acknowledged. I presented this paper at the CAS Workshop "Moral Behavior" in Munich (July 2015), the ESA European meeting in Heidelberg (September 2015), and the Symposium "Self-Control and Motivation"-Follow-up in Cologne (December 2015).

[2]All authors contributed equally to this project. Financial support from C-SEB (Center for Social and Economic Behavior, University of Cologne) is gratefully acknowledged. I presented this paper at the "Bonn-Innsbruck-Munich"- workshop at the MPI Bonn (November 2017). I want to thank Simon Albertini, Alexandra Baier, Helena Fornwagner, Martina Rief, Lukas Schilling, Michael Seidl, and Katharina Vieth for great research assistance.

is worth mentioning that within this framework high levels of cooperation among strangers could be achieved without coercive force and punishment institutions. In our study, we use the age of participants as a proxy for prestige and experience.

Therefore, we conducted an artefactual field experiment in Austria where we recruited subjects 18 years or older drawn from the Austrian general population. In order to be able to test the predictions derived from the theory, we divided the subject pool into three age cohorts, i.e., Juniors (18-39 years), Middle (40-59 years), and Seniors (60 years or older). All subjects participated in two experimental games, a Dictator game and a Prisoner's Dilemma game. Our main innovation to test the hypotheses from the prestige-biased learning theory is that participants could condition their decisions in both economic games on the age cohort of the partner.

We find that, indeed, decision-makers use social cues of experience in social dilemma situations. Participants cooperate more with older generations than with younger generations. This pattern is particularly strong in the youngest generation. In addition, we show that age is positively correlated with generosity and that the oldest generation shows higher levels of unconditional cooperation when they are matched with the youngest cohort. These findings suggest that more experienced generations try to transmit cooperative norms to younger generations within their group.

Chapter 4 titled ***"The Intrinsic Value of Decision Rights – Team vs. Individual Decision-making"*** is joint work with *Justin Buffat and Matthias Sutter*.[3] In this paper, we ask the question how individuals and teams value decision rights. Decision makers value decision rights because they carry two values which reflect on the one hand the power to influence the outcome, i.e., the *instrumental value*, and on the other hand the psychic benefits of having the right to decide, i.e., the *intrinsic value*. While a positive *instrumental value* is expected, recent experimental evidence at the individual level report a positive *intrinsic value* too (e.g., Bartling et al., 2014). This means that decision makers also value control, power, liberty, and/or autonomy *per se*. Especially from an organizational point of view, decisions about delegating or keeping decision rights are at the heart of managerial decision-making and potential psychic benefits would constitute a source of inefficiency. Yet, decisions in organizations are often taken in teams (a group of individuals), hence we test whether teams are less affected by the psychic component of having the right to decide than individuals.

[3]All authors contributed equally to this project. Financial support by the University of Cologne is gratefully acknowledged. I also want to thank the student assistants Ann-Christin Heilig and Sabine Herzog for coding the chat history, and Holger Herz for providing the original instructions. I presented this paper at the "Innsbruck Workshop 2016" at the MPI in Munich, the "Seminar in Applied Microeconomics" at the University of Cologne (May 2016), and the "FAIR – The Choice Lab PhD Workshop 2017" at the Lofoten, Norway (August 2017).

Therefore, we used Bartling et al.'s (2014) framework which allows us to separate the *intrinsic* from the *instrumental* value of a decision right. To address our research question, we replicated the individual decision-making treatment from the original paper and extended the design with a team decision-making treatment where two individuals have to reach a common decision by using a free-text chat messenger to communicate with each other.

The results of our experiment are threefold. First, we could replicate the original findings by showing that individual decision makers do value decision rights *intrinsically*. Second, we find that the *intrinsic value* of individuals and teams in general are comparable. Third, our results suggest that the individual acceptence and satisfaction of team members with their team decisions have a huge impact on the *intrinsic valuation* of a decision right. If both team members report no issues with respect to giving up in the decision process or feeling excluded, the *intrinsic value* is half of the value of individual decision makers and partly consistent with the hypothesis of a fully rational decision maker.

Chapter 5 with the title ***"Social Reference Points and (Dis)Honest Behavior"*** is a *single-authored* paper.[4] This paper examines whether and how relative income concerns affect decision makers' honesty. Relative income concerns entail the idea that workers do not only care about their absolute income, but also how this income compares to the income of relevant peers. Hence, peer income constitutes a social reference point which is important for the evaluation of the individual position. To test the impact of peer income information on honesty we let decision makers play a cheating game where they first learn their intermediate income by rolling a dice and could compare this income with the income of a relevant peer. Then, decision makers could engage in dishonest behavior to manipulate, i.e., increase or decrease, their final income by misreporting the outcome of the dice throw.

We implemented two treatments where we manipulated the income level of the peer. The variation of the peer income affects the likelihood that the individual comparison of the intermediate income with the peer income is advantageous (decision makers earn more than the peer) or disadvantageous (decision makers earn less than the peer). Since the underlying decision in both treatments is identical, differences in reporting behavior can only arise if relative income concerns matter.

Our results show that decision makers act dishonestly in both high and low peer income situations. But, dishonest behavior is significanlty more frequent in the high

[4]Financial support from the University of Cologne and the Max-Planck-Institute for Research on Collective Goods is gratefully acknowledged. I presented this paper at the "Inaugural Conference of the Experimental Economics Group" in Bonn (May 2018) and at the ESA World Meeting in Berlin (June 2018).

income peer situation than in the low income peer situation. Consequently, relative income concerns affect the honesty of decision makers, and thus are correlated with ethical and moral perceptions as well.

CHAPTER 2

COOPERATION AND THIRD-PARTY PUNISHMENT WITHIN AND ACROSS GENERATIONS: AN EXPERIMENTAL STUDY WITH JUNIORS AND SENIORS

joint work with Bettina Rockenbach and Matthias Sutter

Abstract

High levels of cooperation among genetically unrelated strangers is a key characteristic in humans and a fundamental pillar for the well-functioning of societies. Evolutionary theories suggest that the inter-generational transmission of cooperation norms is crucial for large-scale cooperation in societies. Hence, understanding the transmission of cooperation norms among generations is at the heart of understanding cooperation in human societies. Here we report a study on cooperation in a repeated Prisoner's Dilemma game with altruistic third-party punishment as a means of norm transmission. In our laboratory experiment two generations coexist: Juniors (university students in their twenties) and seniors (50 years or older). We show that seniors have significantly higher cooperation rates than juniors, and demonstrate more rigid third-party punishment activities than Juniors. However, the generation of the observer has no impact on the aggregated cooperation levels of both generations. Our results suggest that third-party punishment might not be the most efficient channel to transmit cooperation norms among generations.

2.1 INTRODUCTION

Cooperation is a key achievement of humans, and yet it fosters the development and cohesion of societies. To date, research has shown that humans are able to achieve high levels of cooperation (e.g., Richerson and Boyd, 2005; Fehr and Fischbacher, 2004a). A strand of the evolutionary literature suggests that this is closely related to the human ability of cultural and social learning. These theories state that inter-generational transmission of social values like cooperative norms is a fundamental pillar of establishing and maintaining large-scale cooperation in societies (Henrich and Boyd, 1998; Henrich et al. 2015). Hence, understanding age-related norm compliance and interpretation is at the heart of understanding the evolution of large-scale cooperation.

In addition to cultural and social learning, research mentions institutional designs as a tool to establish and maintain cooperation. Thus, it might be important to understand if inter-generational norm transmission could also work via institutional designs like punishment in the form of second-party (e.g., Fehr and Gächter, 2000; Fehr and Gächter, 2002; Gürerk et al., 2006; Nikiforakis and Normann, 2008) or third-party punishment (e.g., Fehr et al., 2002; Boyd et al., 2003; Fehr and Fischbacher, 2003; Fehr and Fischbacher, 2004b; Lergetporer et al., 2014). In the former, norm violators may be punished by direct interaction partners that potentially suffer from norm violations, whereas, in the latter, the punisher of the norm violator is an unrelated observer. Consequently, third-party designs exclude monetary motivations to engage in punishment to benefit from behavioral reactions in the course of the game. Hence, this institution seems to be the most powerful tool to study the individual importance of norm compliance.

Despite the large literature on cooperation, an important part of our societies is underrepresented so far, namely senior citizens (Lim and Yu, 2015). This constitutes a substantial lack in the literature because of the ongoing demographic change (e.g., Lutz et al., 2008), and even more striking, the higher social IQ of older people in solving social dilemmas (Baltes and Staudinger, 1993; Grossmann et al., 2010). Hence, it might be important to learn more about older people's norm compliance and norm interpretation.

A relatively new literature on social dilemmas compares the behavior of student and non-student samples like representative samples of the population, politicians or clerical workers in social dilemmas without (Gächter et al., 2004; Burks et al., 2009; Carpenter and Seki, 2011; Butler and Kousser, 2015; Dragone et al., 2015) and with second-party punishment tools (Bigoni et al., 2013; Bortolotti et al., 2015). The findings suggest that student samples are less cooperative than non-student samples. In addition, findings including a punishment option are ambiguous, but it seems that in non-student samples antisocial punishment is more frequent than in student samples which affects cooperation results negatively. Moreover, Gächter and Hermann (2011) compare the behavior of Russian citizens younger and older than 30 years in a Public Goods game. All participants participated in two one-shot games, one time with and one time without a punishment option. They show that cooperation is positively correlated with age and that students and non-students with the same age do not differ in terms of cooperation. With regard to punishment, they find, similar to the other studies, a non-neglectable share of misdirected punishment (punish cooperators) which could neutralize the positive impact of a punishment option. Empirical findings suggest, according to a study by Bicchiere et al. (2017), that punishment alone is not sufficient to establish a behavior, but a common understanding of the shared norm is crucial for the efficacy of punishment.

More closely related to our study, we are aware of two studies where age is an important feature of the design (Charness and Villeval, 2009; Gutiérrez-Roig et al., 2015). In the first study, the authors recruited juniors (below the age of 30) and seniors (50 years or older) and introduced a repeated Public Goods game. In their within-subjects design, participants played the game with and without being aware of the generation of the other players. In the latter study, the authors recruited participants from 10 to 87 years for a repeated Prisoner's Dilemma game. They designed two treatments, one where people from the same age cohort were placed in a group and one with a random composition. The general finding of both papers is that age is positively correlated with cooperative behavior.

Our paper is also related to studies which investigate inter-generational behavior in common pool resource experiments with student samples (Chermak and Krause, 2002; Sadrieh, 2004; Fischer et al., 2004). In these experiments, students either operate in overlapping generations environments (Chermak and Krause, 2002) or in inter-generational environments (e.g., Sadrieh, 2004; Fischer et al., 2004) where their decisions affect the outcome of succeeding generations. The studies show that decision makers do not take into account the dynamic externalities caused by their actions and thus show relatively low levels of inter-generational concerns.

2.1.1 THE CURRENT RESEARCH

This study investigates a potential channel of inter-generational norm transmission, namely inter-generational punishment. We study if and how inter-generational punishment as means of norm transmission in a social dilemma setting affects cooperation using two age cohorts of the adult population. In particular, the study focuses on the inter-generational transmission of innergenerational norms from the older to the younger age cohort. On the one hand, inter-generational norm transmission could strengthen existing cooperation or could enforce non-existing cooperation in the young cohort. On the other hand, non-executed inter-generational norm transmission could weaken existing cooperation or could destroy already weak cooperation.

Table 2.1: The Prisoner's Dilemma-game (in points)

	Player 2	Send	Keep
Player 1			
Send		200, 200	50, 275
Keep		275, 50	125, 125

We study how the coexistence of different generations, i.e. of younger (students in their twenties) and older subjects (> 50 years), in social groups affects cooperation rates, in particular when costly third-party punishment is possible. Therefore, we introduce a repeated (20 periods) Prisoner's Dilemma game (Table 2.1) with altruistic third-party punishment. The game was computerized using zTree (Fischbacher, 2007) and recruitment of junior participants was organized with the online recruitement software "hroot" (Bock et al., 2014). In our experiment, subjects are either players or observers and stay in their assigned role throughout the whole experiment. Subjects in the role of a player interact with each other, while those in the role of an observer can punish the players at a cost after they learned the outcome of the Prisoner's Dilemma game. An observer receives 200 points in each round and punishment is a binary decision to deduct 50 points of a player at a cost of 20 points. The dominant strategies for players and observers are "Keep" and "no punishment", respectively.

Importantly, players are always interacting with players of the same generation, while observers belong either to the same or to the other generation. We designed our treatments in this way to have a clean measure of the efficiency of third-party punishment as a tool to transmit cooperation norms and exclude other forms of norm transmission, i.e., norm transmission at a player level if young players interact with older players. It is worth mentioning that subjects had full information about the composition of the groups, i.e., the generation(s) involved in the game. Hence, we are in the position to distinguish subjects' behavior in homogeneous and heterogeneous settings.

In total, we recruited 180 subjects, 94 younger and 86 older subjects (see, e.g., Tables 2.7 and 2.8 in the Appendix). We add to the literature how cooperation and norm interpretation differs across two age cohorts in the adult population, and thus show relevant findings for potential inter-generational learning aspects. Our results could help us to understand age-related differences in norm interpretation and contribute to the discussion on the evolution of large-scale cooperation in humans. In addition, our findings provide insights for future research on institutional designs involving different generations.

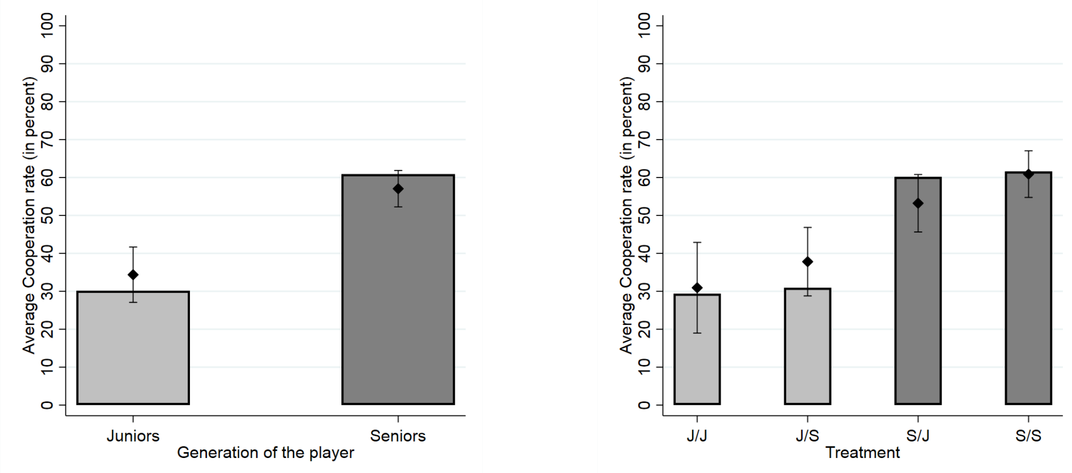
2.2 EXPERIMENTAL RESULTS

In the following section, we present the results of the experiment. First, we present the findings for the players. Second, we analyze the behavior of the observers. Finally, we look at the behavior over time.

2.2.1 COOPERATIVE BEHAVIOR

Our first result, the average cooperation rates, reveals that seniors cooperate twice as much as juniors do, irrespective of the generation of the observer.

Figure 2.1: Average Cooperation rates



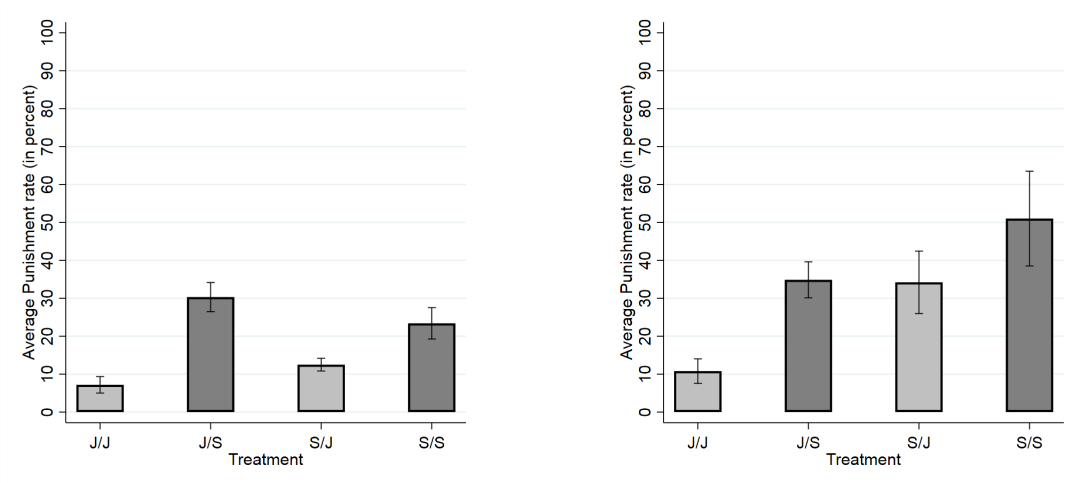
(a) At the Generational level (b) At the Treatment level

Notes: Bars show the average aggregated cooperation rates at the generational (Figure 2.1a) and at the treatment (Figure 2.1b) level. The black markers indicate the average aggregated expectations about the partner's behavior. Error bars of expectations represent standard error at means (average ± SEM). SEM's are based on the average expectations at the matching group level.

Figure 2.1a shows the cooperation rates at the generation level of the players and Figure 2.1b at the treatment level (the first letter indicates the generation of the players and the second the generation of the third-party observer). On average, juniors cooperate in 3 out of 10 cases, whereas seniors do choose cooperation in 6 out of 10 cases. The cooperation rates between both player generations are significantly different at the generation level (p = 0.002, two-tailed Mann-Whitney U-test) and at the treatment level (J/J vs. S/J: p = 0.049; J/J vs. S/S: p = 0.037; J/S vs. S/J: p = 0.028; J/S vs. S/S: p = 0.011, two-tailed Mann-Whitney U-tests). We find no significant differences in cooperation rates between players of the same generation at the treatment level (J/J vs. J/S: p = 0.635; S/J vs. S/S: p = 0.848, two-tailed Mann-Whitney U-tests). This finding still holds if we just account for cooperative choices in the first period where players are not biased by previous actions and experiences (J/J vs. J/S: $p = 0.305$, S/J vs. S/S: $p = 1.000$, two-tailed Fisher's exact tests). Furthermore, a similar pattern can be observed for the average expectations about the partner's behavior. A comparison of behavior and beliefs within both generations yields marginally significant differences for juniors and seniors ($p = 0.070$ and $p = 0.054$, respectively, both Wilcoxon signed-rank matched-pairs tests).[5] While behavior and expectations are not significantly different from each other in homogeneous groups at all conventional levels, they are significantly different in heterogeneous groups (J/S: $p = 0.049$, S/J: $p = 0.022$, Wilcoxon signed-rank matched-pairs tests).

2.2.2 PUNISHMENT BEHAVIOR

Figure 2.2: Average Punishment rates



(a) Overall (b) Social punishment

Notes: Bars show the average aggregated punishment rates at the treatment level. Figure 2.2a shows the overall punishment rates, and Figure 2.2b punishment of defection (social punishment). Error bars of punishment represent standard error at means (average ± SEM).

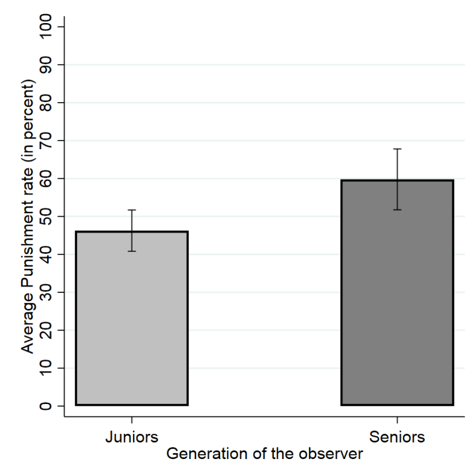
We now analyze the behavior of the third-party observers and present differences in the norm enforcement of both generations. Again, third-party observers could punish none of the players, one of them or both players, hence we result in 40 observations for one observer (2 choices – 1 for each player – in each round). Figure 2.2a shows the average punishment rates over treatments for all periods, irrespective of the corresponding behavior of players. To begin with, we start with the observation that, overall, seniors

[5] We compare behavior and beliefs at the matching group level (see, e.g., Methodology).

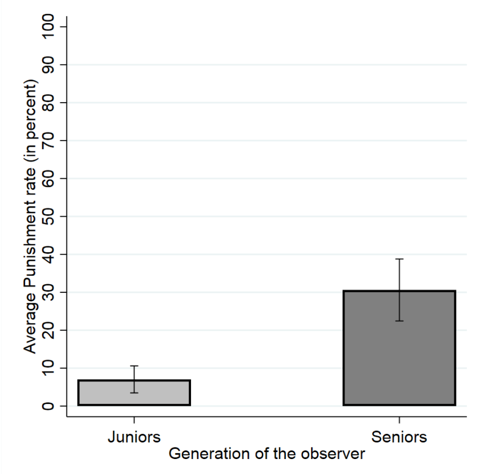
tend to punish in both cases, i.e., observing juniors or seniors, to a higher extent than juniors (J/J vs. J/S: $p = 0.002$ and S/J vs. S/S: $p = 0.025$, two-tailed Mann-Whitney U-tests). While punishment rates for senior observers are not significantly different between both treatments ($p = 0.323$, two-tailed Mann-Whitney U-test), the punishment rates in J/J and S/J are marginally significantly different ($p = 0.073$, two-tailed Mann-Whitney U-test), with juniors punishing seniors more often than their peers.

In terms of norm transmission, social punishment, i.e., punishment of players who chose to defect in the first stage of the game, might yield more insightful results. Figure 2.2b compares average social punishment rates at the treatment level and thus allows us to say more about the norm enforcement of both generations. In general, independent of the outcome of the Prisoner's Dilemma Game, we observe a significant difference between both homogeneous treatments (players and observer belong to the same generation) with seniors punishing defection in 1 out of 2 cases and juniors in 1 out of 10 cases ($p = 0.026$, two-tailed Mann-Whitney U-test). However, a comparison of both heterogeneous treatments yields an informative result: Junior and senior observers have nearly identical punishment rates, between 34 and 35 percent, if a player defects in the first stage of the game ($p = 0.644$; two-tailed Mann-Whitney U-test). Hence, both generations adapt their punishment behavior against defectors in treatments where the players belong to the other generation. While the behavioral change is insignificant for seniors ($p = 0.248$, two-tailed Mann-Whitney U-test), it is significant for juniors ($p = 0.013$, two-tailed Mann-Whitney U-test).

Figure 2.3: Social punishment by the outcome of the Prisoner's Dilemma



(a) Asymmetric outcome



(b) Mutual defection

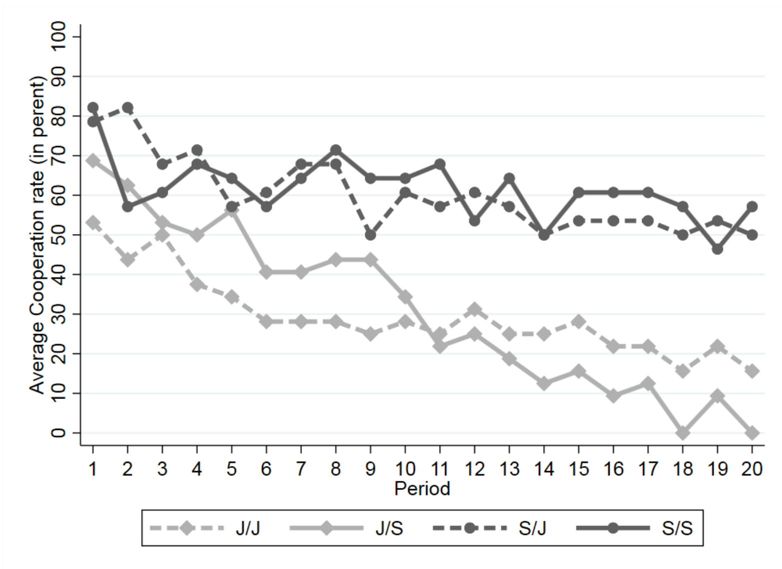
Notes: Bars show the average aggregated punishment rates at the generation level of the observer. Figure 2.3a shows punishment of defection in an asymmetric outcome of the game, and Figure 2.3b shows punishment of defection in a mutual defection outcome. Error bars of punishment represent standard error at means (average ± SEM).

Our design allows us to examine generational differences in norm enforcement when dealing with norm violators in a more sophisticated way. Therefore, we distinguish between two cases of norm violations, namely (i) asymmetric norm violation (one player defects while the other one cooperates, Figure 2.3a) and (ii) mutual norm violation (both players defect, Figure 2.3b). The most obvious norm violation, i.e., asymmetric norm violation, is perceived rather similar by juniors and seniors with punishment rates of 46 percent and 60 percent, respectively ($p = 0.131$, two-tailed Mann-Whitney U-test). On the contrary, mutual norm violation is perceived differently by both generations. Here, seniors punish in 31 percent and juniors only in 7 percent of all cases ($p = 0.008$, two-tailed Mann-Whitney U-test).

2.2.3 COOPERATION OVER TIME

We observe a strong negative impact of time on cooperation for junior players, while the trend is less strong for senior players. In addition, in the second half of the experiment the negative time trend for senior players is obsolete (see, e.g., Table 2.2 in the Appendix). Figure 2.4 shows the average cooperation rates over time for all treatments. The darker grey lines indicate treatments with senior players and the solid lines point out treatments with senior observers. The findings for junior players can be confirmed, but the negative trend for senior players is only driven by the heterogeneous treatment S/J. The homogeneous senior treatment (S/S) has no significant time trend at all (see, e.g., Table 2.3 in the Appendix). Figure 2.4 contains a further finding regarding the junior player treatments. In particular, treatments J/J and J/S are the relevant treatments with respect to the transmission of cooperation norms from the older to the younger generation via third-party punishment. Figure 2.4 shows that cooperation rates are higher in J/S than in J/J in the first half of the experiment, but cooperation rates decrease faster in treatment J/S. It is worth mentioning that we do not observe an increase in punishment behavior neither in treatment J/J nor J/S over time. In contrast, we do observe negative time trends of punishment behavior in both treatments (J/J: $\beta_{Period} = -0.006$, $p = 0.038$; J/S: $\beta_{Period} = -0.008$, $p = 0.034$). Coefficients obtained from panel random-effects probit regressions. The next subsection focuses on this observation and shows why we observe this pattern.

Figure 2.4: Average Cooperation rates over time



Notes: The dark (light) grey lines indicate senior (junior) players and a solid (dashed) line represents senior (junior) third-party observers.

2.2.4 NORM TRANSMISSION AND THE EFFICIENCY OF PUNISHMENT

The aggregated behavioral reactions (see Table 2.4 in the Appendix), i.e., the impact of the chosen action and (received) punishment in the previous period on the action in the actual period, yield insightful results. A short outline of our findings leads to the following explanation. Our data reveal that punishment has a positive impact on cooperation in both junior treatments, and senior observers punish norm violation more frequently than junior observers. However, the more frequent punishment by senior observers relative to junior observers may create short-term benefits, but it affects the stability of cooperation in the long run.

At first glance, punishment seems to work in both junior player treatments. Juniors switch from defection to cooperation in 21 (J/S) to 25 (J/J) percent of the cases if they received punishment in the previous period, but only in 6.6 (J/J) to 11.7 (J/S) percent of the cases if they received no punishment. In addition, senior and junior third-parties observe a similar number of non-cooperative choices, but senior observers punish norm violations three times more often than junior observers (44 our of 425 cases in J/J and 145 out of 410 cases in J/S). Both findings suggest that cooperation rates should be higher in treatment J/S than in treatment J/J.

But, a less stable cooperation outweighs the positive impact of punishment in treatment J/S. Consider the combination where players chose cooperation in the previous period and received no punishment. Overall, around 70 percent of the participants choose cooperation in the acutal period in treatments J/J, S/J, and S/S, but just 57 percent in J/S. This difference arises in the second half of the experiment. The rates are similar for all treatments in the first half, but in J/S it drops to 25 percent in the second half of the experiment.

Hence, the more strict enforcement of the cooperative norm by senior observers has an impact on the long-run stability of cooperation for players which do not share the same strict norm interpretation than their observers. For instance, junior observers tend to wait approximately twice as long as seniors until they use the punishment tool when they observe defection. That is true for J/J vs. J/S as well as S/J vs. S/S (both $p = 0.090$, two-tailed Mann-Whitney U-tests).[6] Thus, cooperative behavior of subjects with a more generous norm interpretation might be more sensitive to interventions. Especially, the more frequent punishment by seniors affects the stability of cooperation negatively. In this case, a careful and more deliberate punishment from peers seems to be the more efficient way.[7]

These findings have an impact on the overall welfare, i.e., the aggregated earnings, as well. Welfare comparisons between treatments do not yield any significant differences for both junior treatments (J/J vs. J/S: $p = 0.529$, two-tailed Mann-Whitney U-test) and both senior treatments (S/J vs. S/S: $p = 0.655$; two-tailed Mann-Whitney U-test) as well as for the homogeneous treatment comparison (J/J vs. S/S: $p = 0.148$, two-tailed Mann-Whitney U-test). But, we find a significant difference if we look at the

[6]We do not find significant differences when we compare juniors or seniors observing the own or the other generation (both $p > 0.100$, two-tailed Mann-Whitney U-tests).

[7]The questionnaire reveals that seniors also have a more positive image of mankind and life than juniors (see Table 2.5). This might also have an impact on the generational differences in punishment behavior.

welfare at the generational level of the players (J/J and J/S vs. S/J and S/S). Here, we find that individual earnings are higher when seniors are in the role of the player ($p = 0.001$, two-tailed Mann-Whitney U-test).

2.2.5 ROBUSTNESS CHECKS AND OUTCOME COORDINATION OVER TIME

In Table 2.6 we show the results of panel random-effects probit regressions which confirm the findings of our non-parametric analysis that cooperation is significantly higher in both senior player treatments. Model 1 confirms the main treatment effect without any control variables. Model 2 is the same as Model 1 but with controls for time trend and other individual control variables. In Model 3, we examine the predictive power of expected cooperation of the matched partner on cooperative choices and include individual controls. Model 4 includes variables which control for the own action and received punishment in the previous period with individual control variables. Model 5 confirms the robustness of the treatment effect controlling for the impact of the action of the partner in the previous period with individual control variables.

Moreover, we observe different patterns with respect to the outcomes of the Prisoner's Dilemma game. Figures 2.5a and 2.5b (see Appendix) show the average symmetric behavior over time at the generation level of the players. Junior players coordinate towards the Nash equilibrium, while this is not true for senior players. On average, junior players reach in 58.59 percent the equilibrium outcome, but senior players only in 17.14 percent of the cases ($p < 0.001$, two-tailed Mann-Whitney U-test). In contrast, seniors reach in 38.93 percent of the cases the socially preferred outcome, i.e. mutual cooperation, but juniors only in 18.91 percent ($p = 0.009$, two-tailed Mann-Whitney U-test).

2.3 DISCUSSION

In this paper we show inter-generational differences in the interpretation of cooperation norms in both players and unrelated third-party observers. We find higher cooperation rates in seniors than in juniors, independent of the generation of the third-party observer. In addition, our data reveal that there exists a generational difference how unrelated third-parties punish norm violation. While both generations react similarly to asymmetric norm violations, seniors punish mutual norm violations more often than juniors. This might be due to the fact that asymmetric norm violation is the more obvious one (e.g., Fehr and Fischbacher, 2004a), while it needs more social skills to identify the severity of a norm violation in a mutual case. This supports previous findings that life experience could play an important role in solving social dilemmas

(Baltes and Staudinger, 1993; Grossmann et al., 2010) and that generational differences might be a key aspect in inter-generational learning. Moreover, we show that (age-related) differences in norm interpretation is a fundamental point for the design and the effectiveness of institutions. We find that third-party interventions do not constitute an efficient channel for the transmission of innergenerational norms from older to younger generations. Finally and based on our findings on age-related differences in the interpretation of the norm, we suggest to conduct further experiments to learn more about the mechanisms of inter-generational transmission of cooperative norms.

2.4 METHODOLOGY AND PROCEDURE

RECRUITMENT STRATEGY. Two generations were participating in this experiment: Students (in their twenties) and senior citizens (50 years or older). The recruitment process of junior participants followed the commonly applied procedure. We used the existing subject pool of the Innsbruck Laboratory for Economic Research. In addition, we had to set-up a new subject pool for senior participants because the existing one did not include participants of this generation. Our recruitment strategy was three-pronged. First, we visited the largest fair for elderly people in North Tyrol, SenAktiv, in November 2015 which is void of any religious or political views. Second, we used the contacts of the university sports center which offers courses for active senior citizens. Third, we contacted non-profit adult education organizations which offer courses for senior citizens. We conducted the experiment between March and July 2015.

SUBJECTS. In total, we recruited 180 subjects, 94 juniors and 86 seniors (Table 2.7 and Table 2.8 for further details about the subject pool). The median year of birth in juniors and seniors is 1993 (22 years) and 1948 (67 years), respectively and is similar to the mean year of birth in both subsamples (1992 for juniors and 1949 for seniors).

COMPARABILITY OF BOTH SUBJECT POOLS. An issue when working with a non-standard subject pool, e.g., seniors, could be concerns regarding self-selection and its influence on the outcome. We can not rule out self-selection at a full glance, but we think that most of the concerns can be ruled out by our recruitment process. Senior participants required a lot of information regarding the project and did not spontaneously agreed to participate in the experiment. Another important point for experiments with both a standard and a non-standard subject pool is to assure the comparability of the results. Therefore, we applied two rules during the recruitment process of junior participants. First, we restricted the junior subject pool to subjects who participated at most three times in economic experiments. Second, we made sure junior participants had no previous experiences with similar types of experiments (e.g., Public Goods games, Prisoner's Dilemma games, and games including third-party punishment). The applied restrictions should minimize potential experience effects.

THE EXPERIMENTAL GAME. We introduced a standard Prisoner's Dilemma game with third-party punishment over 20 periods. Hence, we had two roles in the experiment, i.e., players and observers. We organized the interactions between both generations in a systematic way with four treatments. We have two homogeneous treatments where players and observers belong to the same generation and two heterogeneous treatments where players belong to the same generation but the observer belongs to the other generation. The participants stayed in their respective roles throughout the entire experiment. We used a stranger-matching protocol to rule out the possibility to built-up reputation during the experiment. Therefore, participants got assigned to matching groups of six participants including four players and two observers.

First stage (Prisoner's Dilemma game): In the first stage, players played the Prisoner's Dilemma game. Each player received a fixed payment of 50 points. Further, the players received an additional amount of 75 points each. Players could use the additional amount in two ways and had to decide independently and simultaneously about their individual action. One option was to send the 75 points to the partner which would double the amount and the partner received 150 points. The second option was straight forward namely to keep the additional amount of 75 points. In other words, players had to choose between cooperation and defection where defection was the strictly dominant strategy.

Second stage (Punishment and Beliefs): In the second stage, observers learned the outcome of the previous stage. Specifically, observers received information about the decisions of both players and the corresponding interim payoffs of both players. Then, they were asked to decide whether they want to deduct points from the interim payoff of player 1, player 2, both players or none of them. In our design, punishment implied a binary decision to subtract 50 points of the interim payoff of a player at a cost of 20 points per punishment, or not. Therefore, each observer received an endowment of 200 points. Hence, punishment within this framework could be interpreted as "altruistic" punishment. In the meanwhile, players had to answer two questions about their expectations (beliefs). They answered two questions: (i) Which action did your partner choose in this round? Transfer or Keep, and (ii) Do you think that the observer subtracts points from your account in this period? Yes or No?

Third stage (Summary): At the end of each period, players and observers received a summary of the current period, including all actions taken in this stage (own and partner's action), the punishment decision(s), the interim payoffs and the net payoffs (= interim payoffs - punishment). Then, the next period started. The exchange rate was 100 points = 80 Euro cents.

MATCHING PROTOCOL. Players and observers were randomly assigned to a matching group of six participants which included four players and two observers. The assignment to one specific matching group for a player was fixed for the entire 20-period Prisoner's Dilemma game. At the beginning of a period, participants of a matching group were randomly assigned into subgroups. A subgroup consisted of two players and one observer. Hence, a matching group had two subgroups in each period with three players each. In both homogeneous treatments participants were randomly assigned to the role of a player or observer at the beginning of the experiment, while the roles were fixed in both heterogeneous treatments. In total, we have 8 matching groups in both treatments with junior players and 7 matching groups in both treatments with senior players.

PROCEDURES. At the beginning of each session, the experimenter in charge read the instructions aloud in front of the participants. It is worth noticing that we did not use the usual form of payoff matrices to explain the game. Instead, we described the payoff structure verbally. We chose this method because we could not take familiarity with payoff matrices for granted (see Section 2.5.3 for the instructions and sample screens). Before the start of the Prisoner's Dilemma game, each participant had to answer six control questions correctly. Subjects were not allowed to proceed until they had answered all questions correctly. The control questions were shown on two screens, with three questions each. The participants had to select the correct option for all three questions of the first screen to proceed to the second screen. The median number of trials for each part was one trial for both generations. We can observe a difference between both groups when we perform χ^2-tests over the whole distributions with (some) senior participants needing more trials than junior participants ($p = 0.028$ and $p = 0.023$ for the first and second screen, respectively). A potential explanation could be a design choice in our zTree-program. If participants selected an answer of a control question – irrespective of the correctness of the chosen option – it was highlighted in green. This created some confusion among participants and we observed this pattern particularly in seniors. However, in the results section we show that the number of trials needed to answer the control questions have no systematic influence on the decisions of an individual (see, e.g., Table 2.6 in the Appendix). A session lasted on average 75 minutes, but sessions including senior participants lasted longer than homogeneous junior sessions. Participants received on average 26.3 Euros (exclusive 5 Euros show-up fee).

POST-EXPERIMENTAL QUESTIONNAIRE. After the Prisoner's Dilemma game, participants had to answer a questionnaire. We asked questions regarding individual characteristics (e.g., age, education), questions regarding individual experiences with the other generation, and questions from the world value survey targeting trust attitudes, attitudes towards income differences, and fairness (see, e.g., Table 2.5 in the Appendix). In addition, we measured the social value orientation (SVO) using the Triple-Dominance Measure of Social Values (e.g., Van Lange et al., 1997). This measure allows us to classify individuals as individualistic, prosocial or competitive (see, e.g., Table 2.9 in the Appendix). These classifications are a measure for the individual attitudes towards social norms.

2.5 APPENDIX

2.5.1 TABLES

Table 2.2: Time trend at the generational level

	All periods		**Last 10 periods**	
	Coefficient	Marginal effects	Coefficient	Marginal effects
Period x Junior	-0.136***	-0.030***	-0.167***	-0.028***
	(0.020)	(0.004)	(0.024)	(0.004)
Period x Senior	-0.035***	-0.008***	-0.035	-0.006
	(0.012)	(0.003)	(0.025)	(0.004)
Constant	0.667***		0.791**	
	(0.206)		(0.327)	
N.cluster	30		30	
N.obs.	2400	2400	1200	1200
Log-PseudoLL	-1089.861		-485.669	

Notes: Panel random-effects probit regressions with the dependent variable Cooperation (1=cooperation, 0=defection). Period x Junior is the interaction between the variable "Period" and the dummy for junior player. Period x Senior is the interaction between the variable "Period" and the dummy for senior player. Robust standard erros in parentheses are clustered on the matching group level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.3: Time trend at the treatment level

	All periods		Last 10 periods	
	Coefficient	Marginal effects	Coefficient	Marginal effects
J/J x Period	-0.106***	-0.023***	-0.144***	-0.025***
	(0.033)	(0.007)	(0.043)	(0.008)
J/S x Period	-0.160***	-0.035***	-0.188***	-0.032***
	(0.019)	(0.004)	(0.029)	(0.005)
S/J x Period	-0.047***	-0.010***	-0.038	-0.007
	(0.014)	(0.003)	(0.036)	(0.006)
S/S x Period	-0.025	-0.005	-0.031	-0.005
	(0.018)	(0.004)	(0.022)	(0.004)
Constant	0.658***		0.784**	
	(0.198)		(0.325)	
N.cluster	30		30	
N.obs.	2400	2400	1200	1200
Log-PseudoLL	-1084.166		-484.455	

Notes: Panel random-effects probit regressions with the dependent variable Cooperation (1=cooperation, 0=defection). J/J (J/S, S/J, S/S) x Period are interactions between the four treatment dummies and the variable "Period". Robust standard erros in parentheses are clustered on the matching group level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.4: Behavioral reactions of players

		Period t-1			
		Cooperate		**Defect**	
		No punishment	Punishment	No punishment	Punishment
Period t					
Cooperate	J/J	74.0 %	50.0 %	6.6 %	25.0 %
		(134)	(1)	(25)	(11)
	J/S	57.4 %	58.1 %	11.7 %	21.4 %
		(89)	(25)	(31)	(31)
	S/J	71.7 %	80.0 %	40.5 %	37.7 %
		(228)	(4)	(60)	(23)
	S/S	72.6 %	61.5 %	41.7 %	43.0 %
		(220)	(16)	(43)	(43)
Defect	J/J	26.0 %	50.0 %	93.4 %	75.0 %
		(47)	(1)	(356)	(33)
	J/S	42.6 %	41.9 %	88.3 %	78.6 %
		(66)	(18)	(234)	(114)
	S/J	28.3 %	20.0 %	59.5 %	62.3 %
		(90)	(1)	(88)	(38)
	S/S	27.4 %	38.5 %	58.3 %	57.0 %
		(83)	(10)	(60)	(57)

Notes: Behavioral reactions of players from period t-1 to period t per action and received punishment. Total number of the event in parantheses.

Table 2.5: Questionnaire overview

Question 1:	Would you say most people can be trusted?
Answer 1:	Yes or No
Statistics:	Fraction of yes: Seniors = 80.23 % and juniors 58.51 %, p = 0.002 (two-tailed Fisher's Exact test)
Question 2:	Do you think most people would try to take advantage of you if they got a chance (Statement 1) or would they try to be fair (Statement 2)?
Answer 2:	Scale from 1 to 10 (1 = strong agreement with statement 1; 10 = strong agreement with statement 2
Statistics:	Mean: Seniors = 7.49 and juniors = 6.29, p <0.002 (two-tailed Mann-Whitney U-test)
Question 3:	Do you think most people can only get rich at the expense of others (Statement 1) or wealth can grow so there's enough for everyone (Statement 2)?
Answer 3:	Scale from 1 to 10 (1 = strong agreement with statement 1; 10 = strong agreement with statement 2
Statistics:	Mean: seniors = 7.53 and juniors = 6.77, p <0.064 (two-tailed Mann-Whitney U-test)

Table 2.6: Probit regressions – Robustness checks

	Cooperation of a player				
	Model 1	Model 2	Model 3	Model 4	Model 5
J/S	0.014	0.013	-0.005	-0.003	0.007
	(0.141)	(0.135)	(0.089)	(0.129)	(0.120)
S/J	0.293**	0.285*	0.225**	0.270*	0.254*
	(0.139)	(0.147)	(0.104)	(0.138)	(0.132)
S/S	0.293**	0.267*	0.182*	0.254*	0.244*
	(0.133)	(0.146)	(0.102)	(0.139)	(0.128)
Period		-0.017***			
		(0.003)			
Expect Cooperation			0.410***		
			(0.042)		
Cooperate t-1				0.128***	
				(0.047)	
Antisocial punishment t-1				0.022	
				(0.053)	
Social punishment t-1				0.068**	
				(0.030)	
CooperateP2 t-1					0.183***
					(0.033)
Female		0.005	0.009	0.010	0.022
		(0.059)	(0.046)	(0.056)	(0.058)
SVO Prosocial		0.019	-0.004	0.004	-0.003
		(0.082)	(0.060)	(0.074)	(0.072)
SVO Individualist		-0.116*	-0.118**	-0.110*	-0.120*
		(0.071)	(0.059)	(0.064)	(0.063)
Trials questions		0.005	0.006	0.004	0.005
		(0.011)	(0.007)	(0.010)	(0.010)
N.obs.	2400	2400	2400	2280	2280

Notes: Marginal effects of panel random-effects probit regressions. Dependent variable is *Cooperation* (1=cooperation, 0=defection). Robust standard errors in parentheses are clustered at the matching group level. **Independent variables:** *J/S*, *S/J*, and *S/S* are treatment dummies (baseline is treatment J/J), *Period* is a variable from 1-20, *Expect Cooperation* takes the value 1 if an individual expects cooperation and 0 otherwise, *Cooperate t-1* takes the value 1 if an indiviudal cooperated in the previous period and 0 otherwise. *Antisocial punishment t-1* and *Social punishment t-1* take the value 1 if an individual received an antisocial or social punishment in the previous period and 0 otherwise, *CooperateP2 t-1* takes the value 1 if the matched partner cooperated in the previous period and 0 otherwise. *Female* takes the value 1 if the individual is female and 0 otherwise, *SVO Individualist* and *SVO Prosocial* are dummies for the SVO classification, and *Trials questions* indicates the number of trials needed to answer all control questions. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.7: The Subject Pool

Generation	**Gender**		**Total**
	Female	**Male**	
Junior	45 (47.87 %)	49 (52.13 %)	**94**
Senior	48 (55.81 %)	38 (44.29 %)	**86**
Total	**93** (51.67 %)	**87** (48.33 %)	**180**

Table 2.8: Educational background of Senior participants

Education	**Observations**	**Cumulative frequency**
No answer	6	07.50 %
Mandatory school	7	16.25 %
Vocational training	17	37.50 %
High school	29	73.75 %
University degree	27	100.00 %
Total	**86**	

Table 2.9: SVO Triple-Dominance Measure

Situation	Option 1	Option 2	Option 3
1	You get: 480 Other gets: 80	You get: 540 Other gets: 280	You get: 480 Other gets: 480
2	You get: 560 Other gets: 300	You get: 500 Other gets: 500	You get: 500 Other gets: 100
3	You get: 520 Other gets: 520	You get: 520 Other gets: 120	You get: 580 Other gets: 320
4	You get: 500 Other gets: 100	You get: 560 Other gets: 300	You get: 490 Other gets: 490
5	You get: 560 Other gets: 300	You get: 500 Other gets: 500	You get: 490 Other gets: 90
6	You get: 500 Other gets: 500	You get: 500 Other gets: 100	You get: 570 Other gets: 300
7	You get: 510 Other gets: 510	You get: 560 Other gets: 300	You get: 510 Other gets: 110
8	You get: 550 Other gets: 300	You get: 500 Other gets: 100	You get: 500 Other gets: 500
9	You get: 480 Other gets: 100	You get: 490 Other gets: 490	You get: 540 Other gets: 300

Notes: An individual is classified when she/he makes six or more type-consistent choices. Prosocial choices are: 1c, 2b, 3a, 4c, 5b, 6a, 7a, 8c, and 9b. Individualistic choices are: 1b, 2a, 3c, 4b, 5a, 6c, 7b, 8a, and 9c. Competitive choices are: 1a, 2c, 3b, 4a, 5c, 6b, 7c, 8b, and 9a

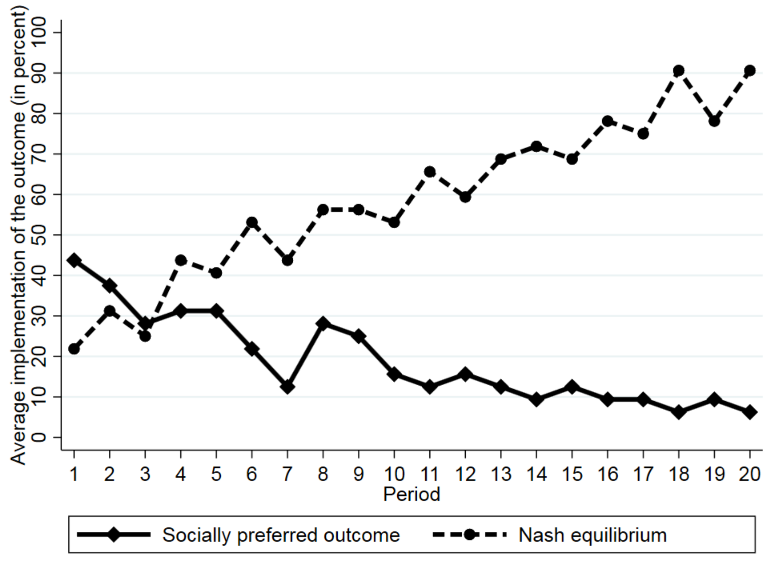
Explanation:
We could classify 129 individuals as prosocial, 35 as individualistic, 1 individual as competitive, and 15 individuals made no type-consistent choices.

Differences between generations:
We find differences between both generations and being classified as prosocial or type-inconsistent. While seniors are more often classified as prosocial than juniors with 80.23 percent and 63.83 percent, respectively ($p = 0.017$, two-tailed Fisher's Exact test), juniors are more often type-inconsistent than seniors (juniors = 12.77 percent and seniors

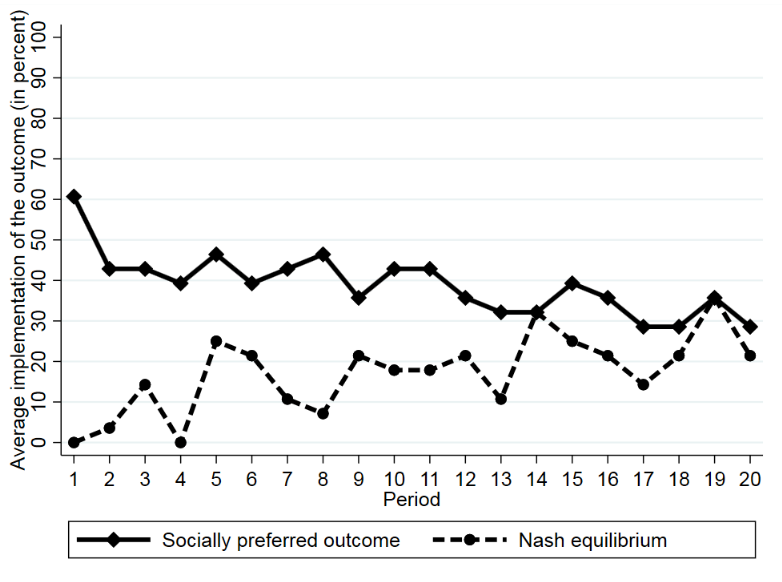
= 3.49 percent, $p = 0.038$, two-tailed Fisher's Exact test). We do not find any significant differences between both generations and the classification as an individualist or competitor at all conventional levels (two-tailed Fisher's Exact tests).

2.5.2 ADDITIONAL FIGURES

Figure 2.5: Symmetric outcomes over time



(a) Junior players



(b) Senior players

Notes: Average rates of symmetric outcomes of the Prisoner's Dilemma game over time. Figure 2.5a summarizes treatments J/J and J/S (junior players) and Figure 2.5b summarizes treatments S/J and S/S (senior players). The solid line indicates mutual cooperation (socially preferred outcome) and the dashed line mutual defection (corresponds to the Nash equilibrium of the game).

Instructions [8]

Welcome to today's experiment. You receive €5 as a show-up fee. During the experiment you will be asked to make decisions and so will the other participants. Your decisions, as well as the decisions of other participants, will determine your monetary payoff. Therefore, it is important that you carefully read the instructions. If you have any questions during the experiment, please raise your hand and one of the experimenters will come to assist you.

Please do not communicate with other participants before the end of the experiment. Please switch off your mobile phone. At the end of the experiment we will pay you in cash €5 as a show-up fee and the money you've earned during the experiment. Your income in the experiment will be in points. At the end of the experiment, points will be converted into Euro at an exchange rate of

100 points = 80 Euro cents

The Game

Generations, Roles, Periods, and Groups

- In principle, two generations are participating in this study: "**senior citizens**" and "**students**". *In this session, only senior citizens (students) are participating.*

- We assign each participant to a specific role at the beginning of the experiment. There are two roles: "**players**" and "**observers**". Each participant belonging to the **senior citizens/students** is a **player** and each participant of the **students/senior citizens** is an **observer**. You stay in your role throughout the whole experiment. *You will be randomly assigned to one of the two roles and you will stay in your role throughout the whole experiment. You learn your role at the beginning of the experiment on the screen.*

- The game will be repeated for 20 periods and each period has the same structure.

[8] Additional comments for homogeneous treatments shown in the instructions in italics.

- Before each period you will be matched into a group of three participants. One group consists of **two players** and **one observer**. This means that you interact in each round within a new group where group members could differ from round to round. But, you stay in your assigned role throughout the whole experiment. You will never learn the identity of another matched participant in a round. This means that all decisions are anonymous.

Structure of the game

Each period consists of **two** stages.

Stage 1: The decision of the players

- In stage 1, only players make decisions. The decisions of both players will determine the interim payoff of a player.
- At the beginning of each round, a player receives an endowment of 50 points.
- In addition, a player receives **75 additional points**. A player can decide individually how she wants to use these additional points.
- A player could either keep the 75 additional points or send them to the other player in the group. Hence, a player has to decide about keeping or transferring the additional points.
- "**Transfer**" means that a player **loses her 75 additional points**, but the **other player** in the group **receives 150 points**. This means that **transferred additional points get doubled**.
- "**Keeping**" means that a player **keeps her 75 additional points**. The additional points do not get doubled in this case. The **other player** in the group **receives nothing**.
- Both players decide simultaneously about keeping or transferring the additional points. Hence, we ed up with four potential outcomes:

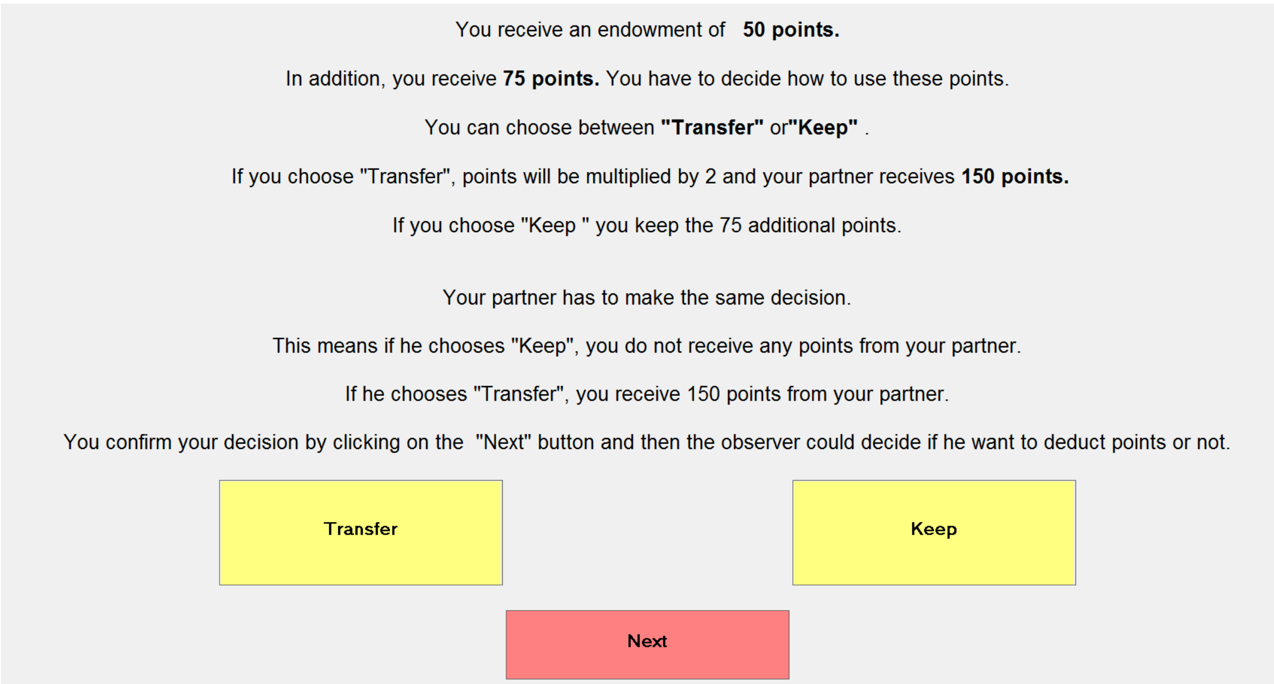
1. You choose "Transfer" and the other player chooses "Transfer" as well. This means that you and the other player receive an interim payoff of 200 points each. Each of you receives an endowment of 50 points and 150 transferred points of the other player.

2. You choose “Transfer” and the other player chosses “Keep”. This means that you receive 50 points and the other player 275 points as interim payoff. You transferred your 75 additional points and end up with the endowment of 50 points. The other player receives an endowment of 50 points, keeps her 75 additional points, and receives your 150 transferred points.

3. You choose “Keep” and the other player chooses “Transfer”. This means that you receive 275 points and the other player 50 points as an interim payoff. You receive an endowment of 50 points, you keep your 75 additional points and recieve 150 transferred points from the other player. The other player ends up with her endowment of 50 points.

4. You choose “Keep” and the other player chooses “Keep” as well. This means that you and the other player receive an interim payoff of 125 points each. Each of you receives an endowment of 50 points and keeps her 75 additional points. Both players do not receive any additional transferred points.

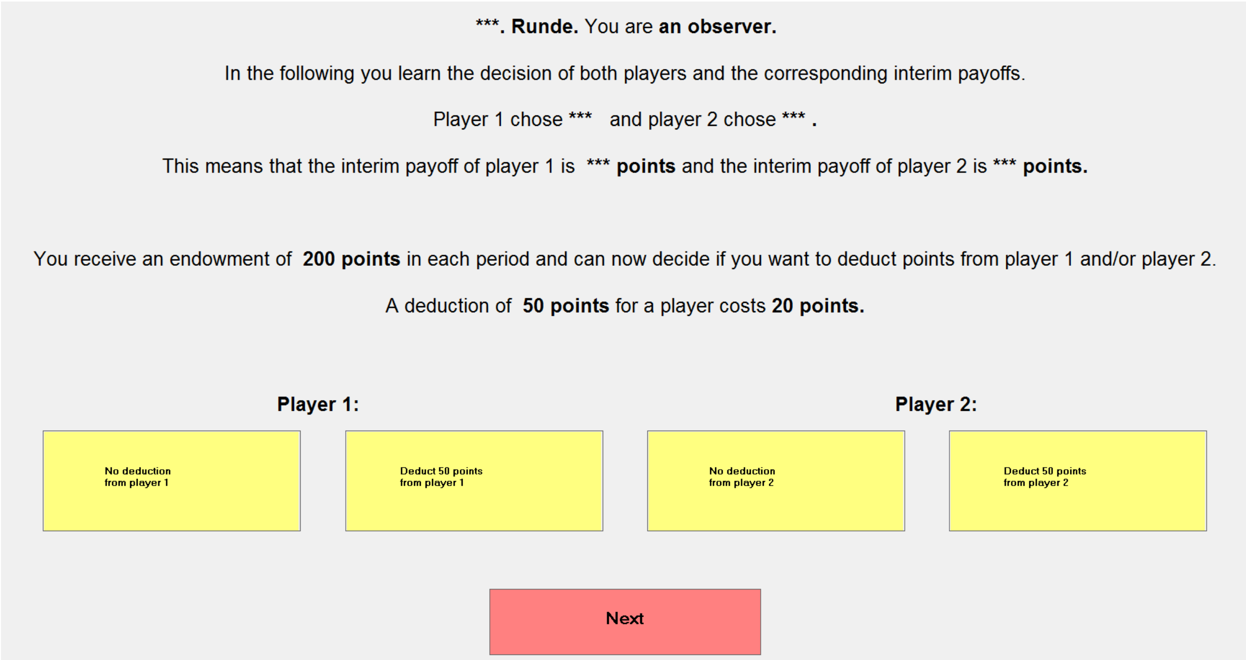
In **each of the 20 periods**, both **players** see the following screen and have to decide individually if they choose “Transfer” or “Keep”. Your decision as well as the decision of the other player determine your interim payoff.



This is the decision screen for players where they have to decide between “Transfer” and “Keep”. Please click on the button of you preferred action. You can confirm your decision if you click on the "Next" button. If you want to change your decision you can simply click on the other button.

Stage 2: The decision of the observer

- In this stage, the observer has to make his decision.
- After both players chose between "Transfer" and "Keep", the observer learns the decisions of both players.
- The observer receives an **endowment of 200 points**. He can use part of his endowment to deduct points from the players' interim payoffs.
- To be more precise, an observer could decide if he wants to deduct 50 points from the interim payoff of a player or not. A deduction of 50 points costs 20 points which are deducted from his own endowment.
- If an observer wants to deduct 50 points from the interim payoff of both players, he has to pay 40 points in total and ends up with a payoff of 160 points in this round.
- A random example:
- Consider the case, in which player 1 chooses "Transfer" and player 2 chooses "Keep". This means that player 1 recieves an interim payoff of 50 points and player 2 receives an interim payoff of 275 points. The observer wants to deduct 50 points from each player's interim payoff. This leads to the following final payoff in this period:
  - Player 1: 50 - 50 = 0 points
  - Player 2: 275 - 50 = 225 points
  - Observer: 200 - 2x20 = 160 points
- In **each of the 20 periods**, an **observer** sees the following screen. The observer receives information about the decisions and interim payoffs of both players in the upper part of the screen. On the lower left part of the screen he has to decide if he wants to deduct points from player 1. On the lower right part of the screen he has to decide if he wants deduct points from player 2.



This is the decision screen of the observers. The text is conditional on the decision of both players and could vary from period to period.

The observer has to make the decisions for player 1 on the lower left part of the screen by clicking on one of the two options for player 1. The observer makes the decisions for player 2 on the lower right part of the screen by clicking on one of the two options for player 2. If you want to change you decision(s) for a player just click on the other option.

Summary screen

- At the end of a period (after both stages), each player and the observer receive a summary of the period. This means that they receive the following information:
  - The decisions of both players
  - The interim payoffs of both players
  - Potential deduction points for both players
  - The final payoff of the period for both players and the observer
- You **total earnings** are the **sum of all your final period payoffs** of all 20 periods. You will learn your total earnings after period 20 and you receive your total earnings in cash at the end of the experiment.

Questionnaire

We ask you to fill out a post-experimental questionnaire before you receive the payment. The answers do not influence your income and are anonymous. We use the answers for the analysis of this experiment only.

Payment

At the end of the experiment we convert your total earnings in Euro and you receive the payment in cash with the 5 Euro show-up fee.

Questions

If you have questions regarding the experiment please raise your hand. If you need help during the experiment please raise your hand. We are happy to help you in any case.

CHAPTER 3

THE SECRET OF SUCCESS: OLDER GENERATIONS TEACH COOPERATION TO YOUNGER GENERATIONS

joint work with Stefania Bortolotti, Wilhelm Hofmann, Angelo Romano, and Matthias Sutter

Abstract

Humans exhibit a remarkable ability to cooperate in large-scale groups. According to a gene-culture coevolution perspective, social learning provides a foundation for the evolution of cooperation in humans. One powerful channel of social learning works via experienced community members being responsible for teaching and enforcing norms of cooperation in their community. Yet empirical support for the effect of social learning and experience on cooperation is scarce at best. We present the first experimental test on the role of older and more experienced people in fostering cooperation. Here we show that older generations exhibit higher levels of generosity than younger generations and display more unconditional cooperation when paired with younger members of the community. Importantly, these results cannot be explained by ingroup favoritism or cognitive decline. Our study sets the ground for a better understanding of the role of experience and social learning in sustaining human cooperation over time and generations.

3.1 INTRODUCTION

Decades of economic, social, and biological research have studied the mechanisms explaining how cooperation is promoted and maintained among past, present and future generations (Richerson et al., 2016). However, this is an extremely challenging endeavor, as sacrifices made by the older generations pay out in the future and it is extremely difficult for future generations to reciprocate costly behaviors made in the past (Hauser et al., 2014). To date, the empirical evidence on inter-generational cooperation among non-kins is scarce and the mechanisms at the root of this phenomena are still unclear.

A gene-culture coevolution perspective proposes a conceptual framework to solve this conundrum (Richerson et al., 2016). Cultural transmission and the ability to learn from other members of the community can enforce extraordinarily high levels of cooperation among humans (Richerson and Boyd, 2001). While imitation can be important per se, for promoting cooperation it must be guided by social cues. This perspective, in fact, posits that individuals tend to copy members who gained prestige or experience and that natural selection favored learners who can evaluate and follow these most successful models (Atkisson et al., 2012; Cartwright et al., 2013; Chudek and Henrich, 2011; Chudek et al., 2012; Rakoczy et al., 2010). Therefore, individuals may have evolved a psychology to condition their cooperation according to specific cues of their social environment related to the perception of experience of their group members. These important members can hence have a key role in promoting and maintaining norms of cooperation through the inter-generational transmission of cooperative norms to their younger and less experienced followers (Henrich et al., 2015; Henrich and Boyd, 1998).

Importantly, experienced members can shape the cooperative norms of their community without the use of coercive force or formal institutions (Henrich, 2004). Hence, this mechanism is fundamentally different from the ones based on peer punishment (Fehr and Gächter, 2002; Rockenbach and Milinski, 2006), or on social contracts aimed at imposing cooperation with future generations (Hauser et al., 2014). Social learning is also different from direct and indirect reciprocity (Nowak and Sigmund, 2005), and it does not require in-group favoritism to explain cooperation among different generations.

Empirical evidence on the relation between inter-generational cooperation and social learning is scattered. Here we present the first experimental test of the so called "big man mechanism" (Henrich et al., 2015) with actual and not just simulated participants. The basic principles of the big man mechanism are derived from mathematical and agent-based models. They suggest that cooperation can evolve when: (i) agents selectively cooperate with role models that can have gained experience in their life time; and (ii) these high-status/experienced group members display high levels of generosity (Henrich et al., 2015). Conditions (i) and (ii) are sufficient to guarantee cooperation across generations even when reciprocity, reputation, punishment, voting, and signaling cannot play a role. Evidence in favor of social learning and the big man mechanism would support the cultural evolution idea that humans enforce norms of cooperation within the group, even when these norms are costly for the self.

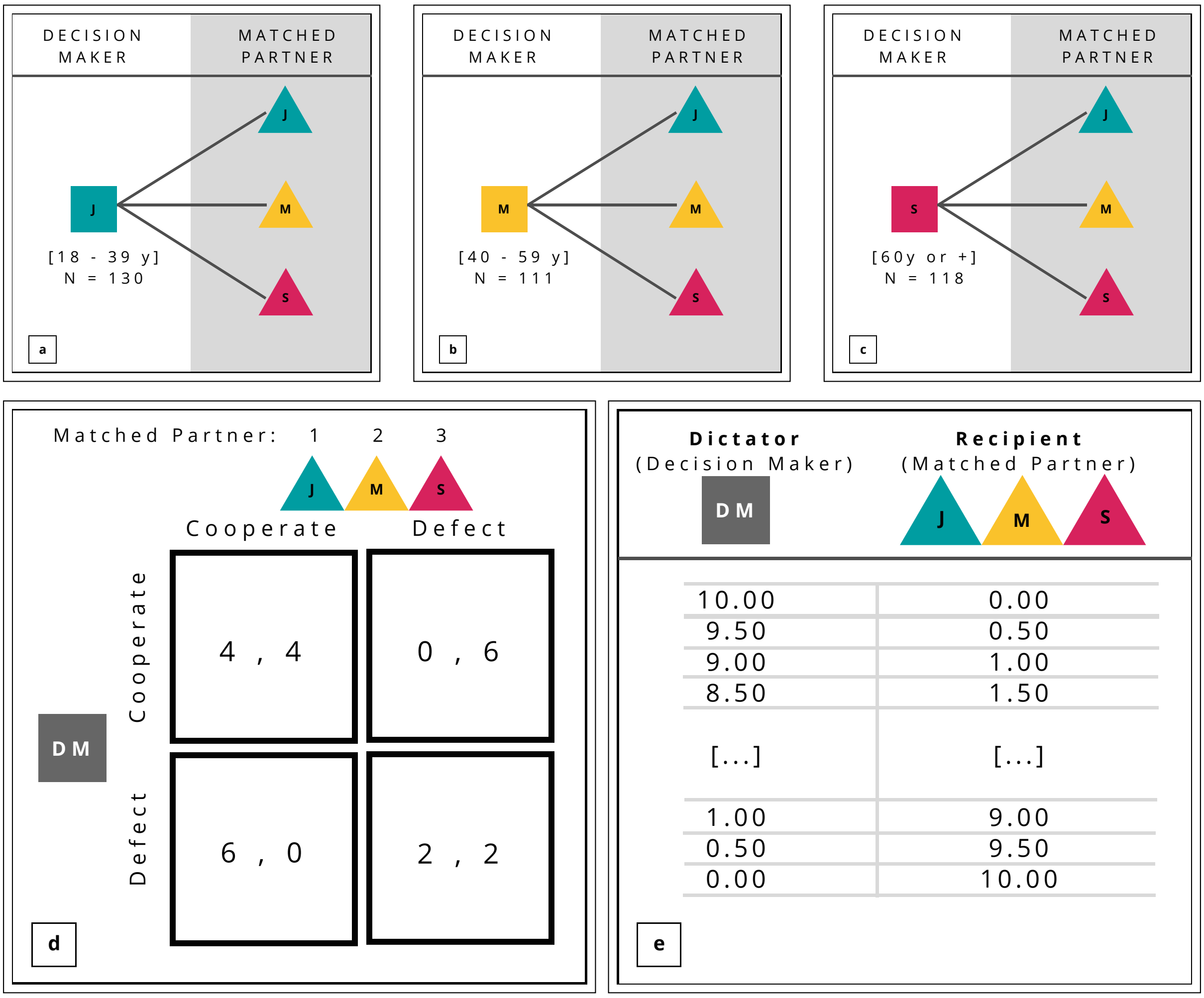
In this study, we test the predictive power of the big man mechanism in interactions among different generations by focusing on one of the possible cues of experience, namely age. Previous research suggests that old people tend to be perceived as more prestigious and experienced than young individuals, since they have likely gained more knowledge in the past (Henrich and Gil-White, 2001). Living longer in itself is a sign of having complex survival skills (Henrich and Gil-White, 2001). Hence, old members of a group may be important models to learn from. Moreover, to learn from specific members of the group is fundamental to show deference and respect (Henrich and Gil-White, 2001). Empirical research testing this hypothesis found that across 34 small-scale societies, only 2 societies did not show explicit evidence of deference toward the seniors (Silverman and Maxwell, 1978). Importantly, forms of respect and deference have been found to be crucial in promoting cooperation in groups and organizations (De Cremer, 2003; Tyler and Blader, 2000; Halevy et al., 2011). Therefore, there seems to exist a universal tendency to invest in relations with older members of a group, and this tendency may translate in higher levels of cooperation toward them.

To our knowledge, there is no experimental work showing that people condition their cooperation with strangers on the age of their interaction partners. Hence, we devised a new paradigm that aims at shedding light on the relation among past, present and future generations. Past research on inter-generational cooperation does not allow disentangling between social learning and other mechanisms, such as kin selection and ingroup favoritism (Gutiérrez-Roig et al., 2014). Moreover, research on the relation between generosity and cooperation across age is sparse and inconclusive (Van Lange et al., 1997; Bellemare et al., 2008; Raihani and Bshary, 2012; Matsumoto et al., 2016; Charness and Villeval, 2009; Gutiérrez-Roig et al., 2014; Peters et al., 2004; Molina et al., 2018).

3.1.1 THE CURRENT RESEARCH

We ran a lab-in-the-field experiment with adult subjects (N = 359) drawn from the Austrian general population (see Table 3.1 in the Appendix and *Methodology*). We devised a new experimental paradigm where participants could make decisions in two incentivized games – Prisoner's Dilemma (PD) (Van Lange and Kuhlman, 1994) and Dictator Game (DG) (Forsythe et al., 1994) – and where decisions could be conditioned on the age cohort of the matched partner. We distinguished between three cohorts: Junior (18-39 years), Middle (40-59 years), and Senior (60 years or more, see Figure 3.1a to 3.1c). The PD serves as a measure of cooperation, whereas the DG as a proxy for generosity. Participants were recruited over three consecutive weekends in shopping malls and at a senior fair. Participation to the paper and pencil experiment was voluntary and subjects were paid anonymously and in cash right at the end of the experiment. All participants played both games, and the order was reversed in the last sessions (see Appendix) to control for any possible order effects.

Figure 3.1: Conditional decisions and experimental games



Notes: Panels a to c give an overview of the decision environment for both games and panels 1d and 1e summarize both experimental games. For all panels it holds that the square indicates the decision makers (DM) and the triangle the partners. The letters J, M, and S specify the age cohort and stand for Junior (18-39 years), Middle (40-59 years), and Senior (60 years and older), respectively. In the Instructions (see Appendix), we always used the age brackets and did not use loaded terms (Junior, Middle, and Senior). Panels a to c indicate that decision makers from each age cohort made three choices – for each game – conditional on the potential age cohort of the matched partner. Panel d shows the Prisoner's Dilemma Game. A decision maker could either choose to cooperate or to defect dependent on the age cohort of the partner. The first number in each of the four possible outcomes indicates the decision maker's payoff, the second number the payoff for the matched partner. Panel e summarizes the Dictator game. A decision maker received 10 Euros and could choose how many Euros he/she wants to transfer to a partner dependent on the age cohort of the partner.

Participants played a simultaneous one-shot PD (Figure 3.1d) where mutual cooperation yields the largest overall outcome [€4 each], but each individual has an incentive to deviate from cooperation. The Nash Equilibrium of the game predicts that both players defect, hence ending up with less [€2 each] than what they would have obtained if they both had cooperated. Participants made three decisions, one for each possible age cohort (Junior, Middle, Senior) of the partner. Participants were also asked – in a series of non-incentivized questions – what they expected from each of their three potential matched partners. At the end of the experiment, participants were matched with a partner from one of the three cohorts and paid accordingly.

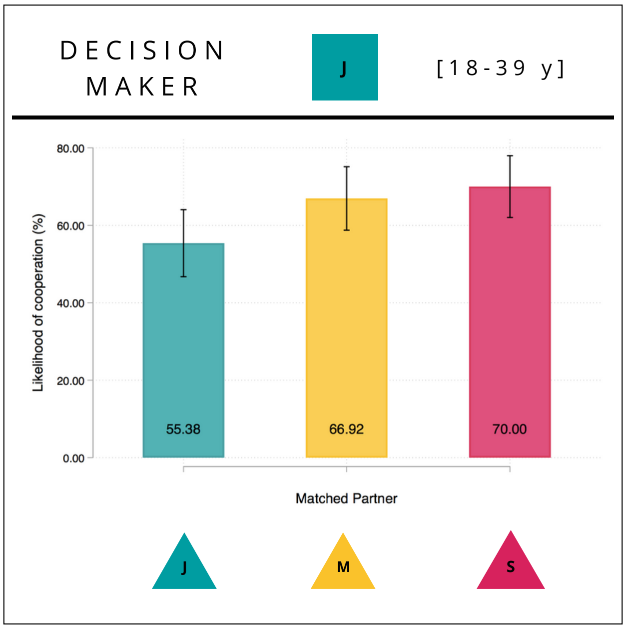
In the DG (Figure 3.1e) all participants had to make a decision in the role of a dictator. As dictators, they were asked how much of a €10 endowment they wanted to keep for themselves and how much they wanted to give to a recipient that belonged to the Junior, Middle, and Senior cohort. As recipients, participants also had to state how much they expected to receive from a dictator from the Junior, Middle, or Senior cohort. At the end of the experiment, participants were assigned to the role of either dictator or recipient and the relevant choices were implemented and paid in cash (expectations were not incentivized). After stating their expectations about dictators' behavior, participants chose the age cohort of the dictator they wished to be matched with in case they were assigned to the role of recipient. This feature of the game is designed to control for possible misunderstanding of the rules of the game (see Table 3.7 in the Appendix).

To summarize, predictions from the big man mechanism are twofold. First, individuals will be more cooperative toward senior people compared to younger generations (*Hypothesis 1*). We will use the likelihood of cooperation as a measure for testing this hypothesis. Second, senior people will show relatively higher levels of generosity compared to younger generations. To test this second hypothesis, we will investigate whether seniors are more generous than younger generations in the DG (*Hypothesis 2a*) and whether they show higher levels of unconditional cooperation in the PD when matched with younger as compared to older generations (*Hypothesis 2b*). In addition, we test for alternative explanations based on indirect reciprocity, by investigating whether our findings are driven by ingroup favoritism (*Hypothesis 3*). Finally, our design allows us to test for an additional alternative explanations, namely cognitive decline.

3.2 EXPERIMENTAL RESULTS

3.2.1 AGE AND COOPERATION

Figure 3.2: Cooperation behavior of Junior decision makers



Notes: The figure presents the cooperation behavior of the Junior cohort. The first bar reports the percentage of cooperation for the case in which a Junior had to make a decision for a partner from the Junior cohort. The middle bar reports the percentage of cooperation for the case in which the partner was from the Middle cohort. Finally, the last bar reports the case in which the partner was from the Senior cohort. Standard errors are reported for each bar.

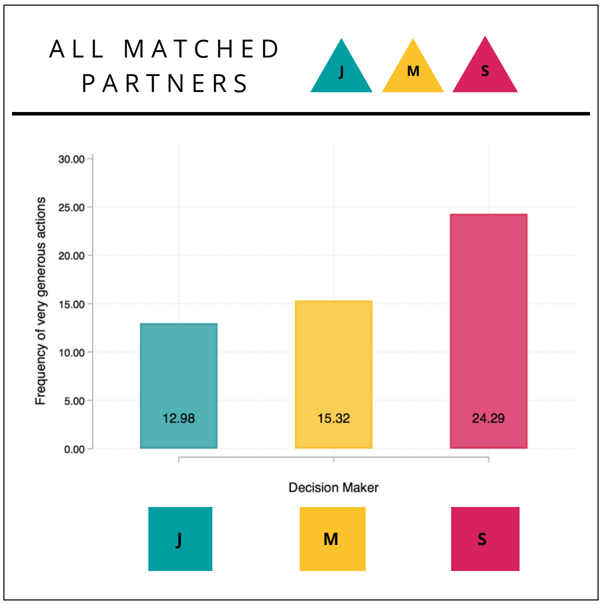
Participants cooperated more when their partner was from the Senior cohort (68.4%) compared to when it was from the Middle (63.1%) or the Junior cohort (62.5%). Random-effects panel logit regressions (see Table 3.3 in the Appendix) test for differences in cooperation based on the age cohort of the partner. We coded two dummies: one for a partner from the Middle cohort (1 = Partner from the Middle cohort, 0 = otherwise) and one for a partner from the Senior cohort (1= Partner from the Senior cohort, 0 = otherwise). A partner from the Junior cohort is the baseline in our regression. We show that there is significantly more cooperation when the matched partner belongs to the Senior cohort, compared to the base case of a Junior cohort ($\beta = 0.433$ and $p = 0.029$, Model 2 with controls). Participants are also more cooperative when the partner belongs to the Senior cohort compared to the case that the partner is from the

Middle cohort (F-test between the two dummies, partner Middle and partner Senior, $p = 0.049$). To further test the idea that younger community members are more cooperative toward seniors (*Hypothesis 1*), we focus our attention on the behavior of the Junior cohort (Figure 3.2). Decision makers from the Junior cohort cooperated significantly more when matched with a participant from the Senior rather than the Junior cohort (70.0% vs. 55.4%, $p = 0.002$, Mc Nemar test). These results support the idea that individuals use cues of experience to condition their cooperation toward strangers. In particular, young individuals cooperate more with older generations, a finding in line with the first hypothesis of the big man mechanism.

3.2.2 AGE, GENEROSITY, AND UNCONDITIONAL COOPERATION

We now test whether seniors are more generous than younger generations. A prominent hypothesis from a gene-cultural evolution perspective is that experienced members enforce the norms of cooperation in their group by showing higher degrees of generosity (*Hypothesis 2a*), even if they incur a personal cost (Henrich et al., 2015). In the DG, dictators from the Senior cohort give away a slightly larger share of their €10 endowment (M = 4.65, SD = 2.50) than dictators from the Middle (M = 4.24, SD = 2.25) or the Junior cohort (M = 4.09, SD = 2.30). We run GLS regressions with random-effects and two dummies: Dictator from Middle cohort (1 = Yes, 0 = No) and Dictator from Senior cohort (1 = Yes, 0 = No). Although the difference between the base category Junior and the dummy for Senior is not significant when we add the controls in the regression (Model 2 with controls $\beta = -0.039$ and $p = 0.928$, see Table 3.4 in the Appendix), we find that age (in years) is positively correlated with generosity (Model 4 with controls $\beta = 0.019$, $p = 0.050$, see Table 3.4 in the Appendix). People also expected more generosity from the Senior (M = 3.89, SD = 3.16), compared to the Middle (M = 3.57, SD = 2.63), or the Junior cohort (M = 2.81, SD = 2.78). Non-parametric analysis confirms this trend (both $p < 0.001$, Wilcoxon signed-rank extension for trend). Interestingly, senior participants are more likely to be extremely generous and give to their matched partner even more than what they keep for themselves (Figure 3.3). This finding provides at least partial support to the idea that more prominent members of a community are more generous (*Hypothesis 2a*).

Figure 3.3: High levels of generosity in the DG

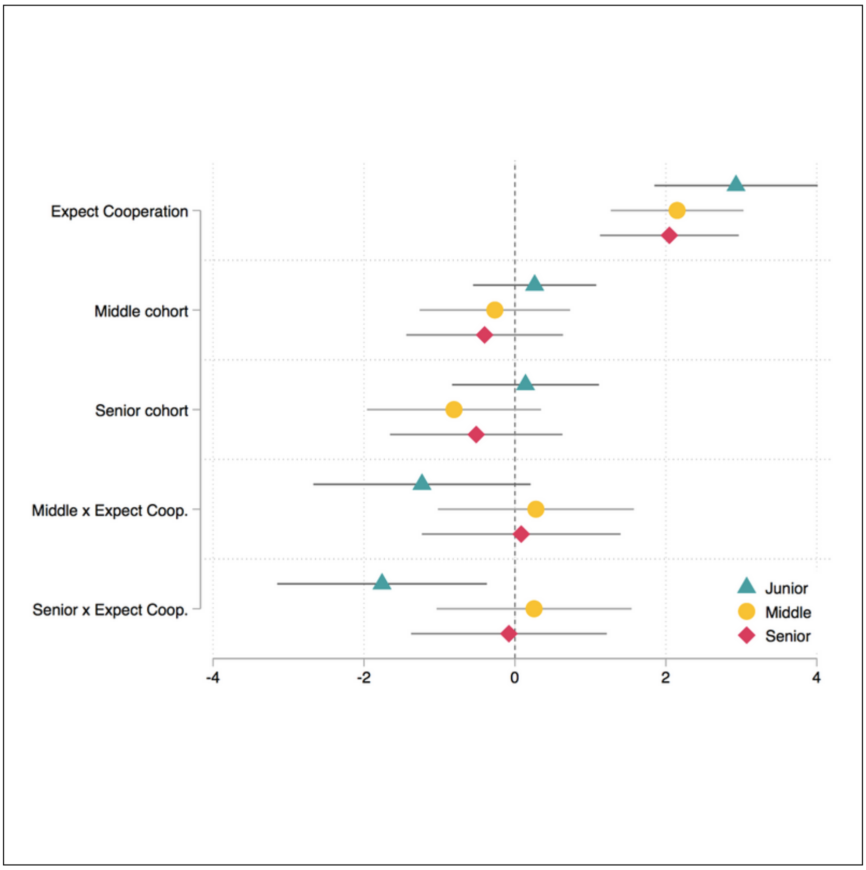


Notes: The figure shows the frequency of very generous actions in the DG, by age cohort of the dictator. We classify as very generous those dictators who gave to the recipient more than they kept for themselves.

We also tested whether participants from older cohorts showed unconditional cooperation toward the Junior cohort (*Hypothesis 2b*). While it might be relatively easy to cooperate if you expect your matched partner to cooperate as well, it is extremely challenging to cooperate if one thinks the partner will defect. However, teaching and sustaining cooperation across generations might crucially hinge on the ability of older generations to bear a personal cost and show the virtue of cooperative behavior even in the face of adversities (i.e., meeting a defector). In fact, teaching is most needed and powerful when one is matched to a defector.

To test *Hypothesis 2b*, we considered a regression model where we regress cooperation on expectation about cooperation, a dummy for the cohort of the decision maker (Middle cohort, and Senior cohort: Junior cohort is the baseline), and an interaction between expectations and cohort of the decision maker. The regression includes a number of controls for socio-demographic characteristics (same as Table 3.3 in the Appendix). We run separate regressions for each age cohort of the matched partner, and Figure 3.4 reports the coefficients of the three regressions (Table 3.5 in the Appendix). Expected cooperation positively predicted the level of cooperation independently from the cohort of the partner (Junior: $\beta = 2.930$, $p < 0.001$, Middle: $\beta = 2.149$, $p < 0.001$, and Senior: $\beta = 2.047$, $p < 0.001$). However, when we consider the interaction between the age cohort of the decision maker and his/her expectations, we uncover an interesting pattern. When a decision maker from the Senior cohort is matched with a partner from the Junior cohort, expectations become much less important (DM Senior x Expect Cooperation: $\beta = -1.761$, $p = 0.013$ for the case in which the partner is Junior). Seniors are more likely to cooperate with a defector from a Junior cohort, than Juniors themselves are. This suggests that seniors tend to be lenient when they have to deal with younger partners and this behavior can be a way to teach cooperation to younger members of the society.

Figure 3.4: Expectations and unconditional cooperation in the PD



Notes: The figure reports the results of three logit regressions, one for each age cohort of the matched partner (Table 3.5 in the Appendix). The dependent variable takes value 1 for cooperation and 0 for defection in the PD. The markers plot the estimated β coefficients and the bars the 95% Confidence Interval. In line with previous studies, expectations are key determinants of cooperation (see Expect Cooperation, all three markers; e.g., Brañas-Garza et at., 2017; Lergetporer et al., 2014). There is however a notable exception: Seniors matched with Junior partners (see triangular marker for the regressor Senior x Expect Coop). Seniors matched with Juniors show a higher propensity to cooperate unconditionally (i.e., even if they expect the Junior partner to defect).

It is important to notice that we do not observe the same when seniors are matched with someone from the Middle or the Senior cohort (Middle: $\beta = 0.254$, $p = 0.700$, and Senior: $\beta = -0.080$, $p = 0.904$). The Middle cohort shows a similar – even though less robust – pattern when matched with younger people (DM Middle x Expect Cooperation: $\beta = -1.232$, $p = 0.094$). A noteworthy addition to this finding is that this result was in part driven by a set of people who are often in the position to educate younger members of the society, such as females (coefficient for being male: $\beta = -0.499$, $p = 0.057$), and people with more children in general ($\beta = 0.224$, $p = 0.071$). Taken all together, our results support the hypothesis that experienced people are willing to incur a cost to enforce and teach cooperative norms to the younger generations.

3.2.3 INGROUP FAVORITISM AND COGNITIVE DECLINE

We also tested alternative predictions from different theories that explain how people would cooperate in inter-generational situations. First, we investigated whether our findings could be driven by ingroup favoritism, i.e., the tendency to cooperate with people of the same group – same age-cohort in our case. In fact, previous research found that individuals tend to cooperate with people belonging to the same group, even in minimal contexts where groups are established through an arbitrary category (Yamagishi and Kiyonari, 2000). To do so, we tested whether people were more generous and more cooperative when they knew that their partner was from the same age cohort. Results from panel random-effects regression analysis with a dummy for ingroup (1 = partner from the own cohort, 0 = partner belongs to a different cohort) show that people did not discriminate in favor of their age cohort, neither in the DG (Model 2 with controls $\beta = 0.117$, $p = 0.415$) nor in the PD (Model 2 with controls $\beta = -0.247$, $p = 0.141$, Table 3.6 in the Appendix). Therefore, we did not find support for our results being predicted by ingroup favoritism.

Then, we checked whether our findings can be explained by another different mechanism related to older people, i.e., the cognitive understanding of the situation. To rule out this possibility we checked for proxies of low understanding of the earning opportunities in the game. In particular, we examined whether participants preferred partners from whom they expected the highest level of generosity if they were selected as recipients. After that, we classified participants in three different types, maximizers, indifferent and non-maximizers. Participants were coded as maximizers when their choice was to pick the partner from whom they expected the highest generosity, we coded them as indifferent when they expected the same level of generosity across different age cohorts and therefore did not have a preference in the partner to pick. Finally, we classified as non-maximizers the ones that picked a different partner than the one from whom they expected the highest generosity. Making a non-maximizing choice can be understood as a sign of poor understanding of the rules of the game. Looking at the fraction of non-maximizers by age cohorts, there was no difference between the Junior, the Middle, and the Senior cohort ($p = 0.388$, χ^2-test, see Table 3.7 in the Appendix). This evidence suggests that older participants could understand the mechanism of the games as well as their younger counterparts. Moreover, all the results we have presented survive if we control for participants that behaved as non-maximizers (Table 3.8 to 3.11 in the Appendix).

3.3 DISCUSSION

Modern societies are characterized by surprisingly high levels of cooperation among strangers. Yet pressing human challenges such as global warming require to maintain and even strengthen the norms of cooperation. Furthering our understanding of the mechanisms that promote and maintain cooperation is hence of paramount importance for the betterment of society. In particular, it is fundamental to gain a better understanding of how norms can be taught and maintained across generations. A gene-culture coevolution perspective proposes that humans evolved a psychology to learn cooperative norms in groups from individuals who have proved to possess crucial experience (Henrich and Gil-White, 2001). This theory predicts that humans condition their cooperation toward members with experience, and that this cooperation is enforced by extreme levels of generosity toward their followers (Henrich et al., 2015). We tested these hypotheses using age as a proxy of experience, and by letting participants make choices in a novel experimental paradigm where they could decide conditional on the age of their partners.

We found that people cooperated more when they knew that their partner was older, and that senior participants were more generous. In particular, senior participants were more likely to cooperate unconditionally when matched with the young generation. Such findings have profound theoretical implications. In fact, our results inform the current intense debate about how humans evolved to cooperate (Delton et al., 2011). Past research found that most of the evidence supporting the role of culture and transmission of norms in promoting cooperation, including altruistic punishment, can be explained by other mechanisms such as indirect reciprocity (Krasnow et al., 2016). Our study provides clean experimental evidence on the predictive role of social learning in promoting cooperation between generations (Richerson et al., 2016). Our results can also inform previous research findings that 18 years old are more cooperative than younger children (Gutiérrez-Roig et al., 2014). Importantly, we ruled out the possibility that our findings can be explained by ingroup favoritism and cognitive decline, a pattern of results that would have supported the role of alternative mechanisms (Yamagishi et al., 1999). Therefore, our study provides a first empirical effort that illuminates the power of teaching cooperative norms in societies.

Our results have relevant practical implications as well. Nowadays humans have to face important and pressing challenges that involve coordination and cooperation among different generations, such as global warming (Milinski et al., 2008; MacKay et al., 2015). Those problems urge us to find solutions to combat selfish behaviors and promote more cooperation in modern societies. This research stresses the crucial role of learning norms from specific people, such as the ones recognized for having gained experience in their group (Henrich et al., 2015). In particular, our findings show the importance of generosity by old people as a tool to transmit cooperative norms in humans. We used age as a proxy of experience and found that elderly people may be a crucial channel for teaching cooperation to young generations. Yet experience and prestige in groups are not exclusively limited to age (Reyes-Garcia et al., 2008). These results may extend to the understanding of political leadership, teachers in schools and institutions who may gain status in specific groups, religions, or nations. This means that future interventions should have a strong focus on empowering categories of people or institutions that may function as role models to solve cooperation problems within and between societies.

Despite the relevance of our findings, future research is necessary to even better understand the development and function of social learning in promoting cooperation. A first limitation of this study is that our findings are related to a very specific proxy of experience/prestige: age. In fact, it is possible that part of our findings can be influenced by confounds related to the use of this specific proxy, such as differences in risk-aversion (Dohmen et al., 2017). We tried to minimize this aspect by ruling out possible alternative explanations, such as ingroup favoritism and cognitive decline. Another possible alternative explanation for our findings could be related to differences in wealth across the different age-cohorts. Although this argument could explain part of our data, it is reasonable to expect that experienced models in the group are the ones who also have more access to resources in their group. Moreover, differences in wealth can only explain parts of our results, since it is not clear why young generations should cooperate more with the rich seniors. Then, an alternative explanation could be that elderly people give more to young generations because they perceive them as needier. Yet, if this stereotype was true, we should also have observed the middle cohort giving more to young people, a pattern that is not supported by our data. Finally, it could be argued that our results are driven by the fact that while part of the data were collected in the capital of the region, some other were collected in relatively small communities. That said, we had a quite heterogeneous sample of people from more than 90 different towns and the results hold when controlling for people from small and big cities (see Tables in the Appendix). Nonetheless, future research will also need to investigate the dynamics of status and prestige across different groups and nations to understand alternative teaching models that next generations may use to learn norms of cooperation. In fact, we live in societies that are getting older and it is possible that in societies with a higher percentage of elderly people, age will become a less reliable proxy for individuals who want to teach norms in their group.

To conclude, our study has crucial implications for the evolutionary understanding of human cooperation. Indeed our results suggest that old generations may drive cooperation with younger generations through high levels of generosity. This result enlightens us on the power of a psychology evolved to learn and transmit cultural norms in groups. Future research should start investigating mechanisms that go beyond reciprocity, signaling and punishment and incorporate culture, teaching, and status in the study of cooperation toward unrelated strangers.

3.4 METHODOLOGY AND PROCEDURE

ETHICS STATEMENT. The study was approved by the Ethical Board of the University of Innsbruck (Certificate of Good Standing, 35/2016). All locations involved in the study granted us permission to collect data in their facilities. In addition, all participants signed an informed consent. All data were treated confidentially. No association was ever made between real names and answers in the study.

RECRUITMENT STRATEGY. The experiment was conducted in North Tyrol, Austria, over three consecutive weekends (12 Nov, 2016 – 26 Nov 2016). Our recruitment strategy was two-pronged: (i) we recruited participants either in one of two shopping malls (first and third weekend) or (ii) at the largest fair for senior citizens in North Tyrol (second weekend). Both shopping malls were located in small towns (10,000 to 15,000 inhabitants) located 30 and 60 kilometers west of Innsbruck, the capital of North Tyrol. The senior fair took place in Innsbruck (130,000 inhabitants, Figure 3.5a in the Appendix). We opted for this strategy to be able to reach the widest possible audience from different walks of life. In particular, older people are difficult to reach and that required to consider an event specifically designed for this age group. The senior fair takes place each year and is void of any political or religious connotation.

RECRUITMENT PROCESS. We set up two teams, including one experienced experimenter and three enumerators each. The enumerators were recruited among students at the University of Innsbruck. They were trained by the experienced experimenters and received a script to approach potential participants. The two teams rotated over the two shopping malls, while one larger team with 5 enumerators and one experienced experimenter was in charge of the data collection at the senior fair. All enumerators wore official badges of the University of Innsbruck and made it clear to potential participants that the study had no commercial purpose, but was a research project approved by the University of Innsbruck. The process followed the same strict rules in all locations. Once a subject agreed to participate in the paper and pencil experiment, he/she was accommodated in a quiet place and was given a booklet with detailed instructions of the games and the decision sheets (see Instructions in the Appendix). The enumerators gave all the necessary privacy to participants, but remained available in case they had any question.

EXPERIMENTAL GAMES, MATCHING, AND PAYMENT. Both games – PD and DG – were presented with a neutral framing. The age cohorts of the matched partners were identified by the corresponding age brackets: 18-39 years; 40-59 years; and 60 years or older. While this classification is somehow arbitrary, it is the same followed by the local statistical office. The partner changed from one game to the other. We reversed the order of the games for the last visit in the shopping malls to control for potential order effects.

Once a subject completed the decision booklet, he/she was escorted to the experimenter desk where role assignment in the DG and matching were performed. First, the enumerator performed a coin toss to determine the role in the dictator game – either dictator or recipient. For subjects assigned to the role of dictator, an additional random draw was performed to determine the age cohort of the recipient. For subjects assigned to the role of recipient, the age cohort chosen in the DG was relevant. Once the role in the DG and the age cohort were determined, the participant was matched with someone from the relevant age cohort and with a different role. For the PD, the decision maker was matched with someone with his/her same role in the DG. While the partner changed from one game to the other, the age cohort of the partner was the same for both games (but the information was not provided in advance to avoid any strategic decision in the DG). Following common practice in these type of lab-in-the-field experiments, the matching of the participants was made on a rolling basis. In order to have matching partners for the first subjects in the first visit to the shopping malls, we collected data from six volunteers – two per age cohort – that were paid on a later date. We applied the same matching procedure as in the field. To ensure comparability, we did not include these six observations in the dataset. The average completion time was 10 minutes and the average payment was €8.70. The payment corresponded to the sum of earnings in the DG and in the PD. For the sake of simplicity and to avoid any hedging, expectations were not incentivized. Subjects were paid in cash and in private right at the end of the experiment.

3.5 APPENDIX

3.5.1 LOCATIONS AND SUBJECT POOL

The lab-in-the-field experiment was conducted in Austria (8.7 million inhabitants), and more precisely in Tyrol (746,000 inhabitants), one of the 9 federal states. The shopping malls are located in smaller towns (10,000 - 15,000 inhabitants), and the senior fair took place in Innsbruck (Figure 3.5a). The first shopping mall has more than 70 stores and 7,000–10,000 customers per day, while the second shopping mall has 40 shops. Even though the two shopping malls are located in relatively small towns, they attract customers from several neighboring towns. This granted us the possibility to recruit a varied sample of people resident in about 90 different towns/villages: hence reducing the chances that participants knew each other. The senior fair in 2016 was organized over three days and had 9,000 visitors (Figure 3.6b). In each location, we had banners to advertise the study (Figure 3.5b) and we set up an experimental desk were the payments were carried out (Figure 3.6a).

Table 3.1: Socio-demographic characteristics of the subject pool

	Our sample			**Austrian Population**		
	Junior 18-39 years	**Middle 40-59 years**	**Senior 60+ years**	**Junior 18-39 years**	**Middle 40-59 years**	**Senior 60+ years**
Share of the adult population$^{(a)}$	36.21 %	30.92 %	32.87 %	34.58 %	35.64 %	29.78 %
Females$^{(a)}$	57.69 %	63.06 %	54.24 %	48.95 %	50.06 %	55.54 %
Average age$^{(a)}$	28 years	51 years	70 years	29 years	50 years	73 years
Education$^{(b)}$						
High school	33.08 %	27.03 %	25.42 %	34.81 %	29.24 %	22.61 %
Bachelor degree or more	24.43 %	25.23 %	22.88 %	17.82 %	14.97 %	7.75 %
Employment status$^{(c)}$						
Employee	74.05 %	75.68 %	9.32 %	77.22 %	70.95 %	5.43 %
Self-employed	3.82 %	6.31 %	5.93 %	4.96 %	10.71 %	1.76 %
Residents in Innsbruck	12.31 %	10.81 %	29.66 %	–	–	–

Notes: All data for Austrian refer to the adult population. Sources: *(a,b)* "Statistics Austria" – the federal statistical office of Austria – and *(c)* "Eurostat". Last data check: 23rd May, 2018. *(a)* Population statistics refer to year 2018. *(b)* Education data refer to year 2014. Due to data availability, the Junior cohort is defined over the interval 20-39 years. *(c)* Employment data refer to the 2011 Census. Due to data availability, the Junior cohort is defined over the interval 20-39 years.

Our enumerators were instructed to approach passersby and invite them to participate in a research study organized by the University of Innsbruck. In the recruitment process, we tried to limit as much as possible the participation of multiple family members by inviting only one of them to the study. We also ensured that nobody took part in the study more than once. Enumerators were asked to try to approach people from different age cohorts. A total of 359 subjects participated in our study (Table 3.1).[9] 344 participants filled in all the items of the experiment, while 15 failed to complete one or more items.[10] In the analysis, we always use all the available data for the specific question at hand. As a result, the number of observations might change from one test to the other.

Table 3.2: Socio-demographic characteristics of the subject pool by location

	Shopping malls			Senior fair		
	Junior	**Middle**	**Senior**	**Junior**	**Middle**	**Senior**
	18-39 years	**40-59 years**	**60 years**	**18-39 years**	**40-59 years**	**60+ years**
Share of the adult population	52.05%	33.34%	14.61%	11.43%	27.14%	61.43%
Females	56.14%	63.01	43.75%	68.75%	63.16%	58.14%
Average age	28 years	50 years	69 years	28 years	53 years	70 years
Education						
High school	30.70%	27.40%	31.25%	50.00%	26.32%	23.26%
Bachelor degree or more	22.81%	28.77%	21.88%	37.50%	18.42%	23.26%
Employment status						
Employee	78.07%	83.56%	6.25%	50.00%	60.53%	10.47%
Self-employed	3.51%	5.48%	6.25%	6.25%	7.89%	5.81%
Residents in Innsbruck	2.63 %	0.00 %	0.00 %	81.25 %	31.58%	40.70 %

[9]Table 3.2 provides the breakdown of socio-demographic characteristics by location type.

[10]The number of participants who failed to complete the experiment in all its pats is similar across age cohort (6 for Junior, 5 for Middle, and 4 for Senior).

Figure 3.5: Experimental locations 1

(a) Map

(b) Banner

Figure 3.6: Experimental locations 2

(a) Experimenter desk

(b) Senior fair

3.5.2 REGRESSION TABLES

Table 3.3: Cooperation by age cohort of the partner

	Cooperation in PD (1=Cooperate, 0=Defect)	
	Model 1	Model 2
Partner from Middle cohort *(1=Yes, 0=No)*	0.041	0.044
	(0.191)	(0.193)
Partner from Senior cohort *(1=Yes, 0=No)*	0.406**	0.433**
	(0.195)	(0.198)
Resident in Innsbruck *(1=Yes, 0=No)*		0.133
		(0.412)
Constant	0.785***	0.165
	(0.172)	(0.967)
Controls	No	Yes
N.obs.	1073	1049
Log. Likelihood	-643.097	-618.577

Notes: Panel random-effects logit regressions. The dependent variable assumes value 1 for cooperation and 0 for defection in the PD. Age cohort of the partner was dummy coded with Junior cohort as base. Controls include the following variables. *Male* that takes value 1 for male and 0 for females. Three dummies for different education levels: *Bachelor or more*, *High school*, *N.A.* in case the information is not available. The baseline category for education is *Vocational training or lower*. Further, we control for the *Number of children*. We included two dummies for the location where the experiment took place: *Shopping mall 1 (d)* and *Senior fair* (baseline category is *Shopping mall 2*). Finally, we included a dummy for each enumerator. As a robustness check, we re-run all the regressions and added a dummy *PD-first* to control for any potential order effect. The dummy takes value 1 for the sessions in which the PD was presented before the DG. All results are qualitatively and quantitatively the same. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.4: Generosity by the age cohort of the Dictator

	Generosity in DG *(amount given)*			
	Model 1	Model 2	Model 3	Model 4
Dictator from Middle cohort *(1=Yes, 0=No)*	0.170	-0.041		
	(0.304)	(0.355)		
Dictator from Senior cohort *(1=Yes, 0=No)*	0.587**	-0.039		
	(0.299)	(0.430)		
Age of the Dictator (in years)			0.023***	0.019**
			(0.007)	(0.010)
Resident in Innsbruck *(1=Yes, 0=No)*		0.287		0.258
		(0.412)		(0.412)
Constant	4.074***	3.685***	3.210***	3.126***
	(0.206)	(0.999)	(0.345)	(1.035)
Controls	No	Yes	No	Yes
N.obs.	1073	1049	1067	1043
R^2 (overall)	0.008	0.032	0.022	0.039

Notes: Panel random-effects GLS regressions. The dependent variable is the amount given to the matched partner in the DG. The baseline in Model 1 and Model 2 is that the dictator (decision maker) belongs to the Junior cohort. For a detailed list of the Controls, refer to the notes to Table 3.3. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

Table 3.5: Cooperation by age cohort of the player

	Cooperation in PD		
	(1=Cooperate, 0=Defect)		
	The partner belongs to the ... cohort		
	Junior	*Middle*	*Senior*
Expect Cooperation *(1=Yes, 0=No)*	2.930***	2.149***	2.047***
	(0.552)	(0.449)	(0.469)
Decision Maker from Middle cohort *(1=Yes, 0=No)*	0.261	-0.266	-0.402
	(0.416)	(0.509)	(0.530)
Decision Maker from Senior cohort *(1=Yes, 0=No)*	0.140	-0.807	-0.513
	(0.497)	(0.589)	(0.583)
DM Middle x Expect Cooperation	-1.232*	0.278	0.083
	(0.734)	(0.663)	(0.672)
DM Senior x Expect Cooperation	-1.761**	0.254	-0.080
	(0.709)	(0.659)	(0.662)
Resident in Innsbruck *(1 = Yes, 0 = No)*	0.088	0.053	-0.160
	(0.426)	(0.452)	(0.444)
Constant	-3.051***	-0.756	-0.795
	(1.146)	(1.068)	(1.063)
Controls	Yes	Yes	Yes
N.obs.	347	348	348
Log. Likelihood	-186.938	-178.125	-173.492

Notes: Logit regressions by age cohort of the matched partner. *Expect cooperation* takes value one if the decision maker stated that he/she expect the partner for the relevant cohort to cooperate and 0 otherwise. For a detailed list of the Controls, refer to the notes to Table 3.3. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3.6: Ingroup favoritism

	Generosity in DG (*amount given*)		Cooperation in PD (*1=Cooperate, 0=Defect*)	
	Model 1	Model 2	Model 3	Model 4
Partner from own cohort *(1=Yes, 0=No)*	0.119	0.117	-0.259	-0.247
	(0.141)	(0.144)	(0.166)	(0.168)
Resident in Innsbruck *(1=Yes, 0=No)*		0.288		0.134
		(0.407)		(0.409)
Constant	4.280***	3.647***	1.016***	0.401
	(0.132)	(0.990)	(0.147)	(0.955)
Controls	No	Yes	No	Yes
N.obs.	1073	1049	1073	1049
R^2 (overall)/Log. Likelihood	0.001	0.032	-644.516	-620.415

Notes: Models 1 and 2 report GLS panel regressions with random-effects at the individual level. The dependent variable is the amount given to the matched partner in the DG. Models 3 and 4 report Logit panel regressions with random-effects at the individual level. The dependent variable assumes value 1 for cooperation and 0 for defection in the PD. The variable *Partner from own cohort* takes the value 1 if the matched partner is from the same age cohort as the decision maker and 0 otherwise. For a detailed list of the Controls, refer to the notes to Table 3.3. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

3.5.3 COGNITIVE DECLINE AND UNDERSTANDING

In the DG, participants had an equal chance to be in the role of the dictator or the recipient. We used the strategy method and ask participants to make decisions in both roles. When playing as recipient, they were asked to state their beliefs and they also had to choose the age cohort of the dictator they wanted to be matched with. The idea is that one should choose what he/she believes to be the most generous cohort, as that would maximize his/her payoffs. For instance, if a participant expects the Middle cohort to be the most generous of all, he/she should choose to be matched with a dictator from the Middle cohort. We interpret departures from this logic as a possible sign of poor understanding of the rules.[11]

To test if cognitive decline among the seniors might have affected the proper understanding of the games, we classify recipients in the DG according to three types:

- *Maximizer:* they maximize their material payoff, according to their expectations. We also include players that stated the same expectations for two cohorts and chose one of the two;
- *Indifferent:* contains all recipient that held the same expectations for all cohorts. This implies that we cannot infer anything about their rationality based on the choice of the dictator cohort;
- *Non-maximizer:* means that these decision makers did not choose the age cohort that would maximize their material payoff, based on their expectations.

Table 3.7: Distribution of non-maximizers

Age cohort	**Maximizer**	**Indifferent**	**Non-maximizers**
Junior	40.63%	42.96%	16.41%
Middle	38.53%	44.04%	17.43%
Senior	38.98%	38.14%	22.88%

[11]Please notice that this is the case also for an altruistic decision maker, as dictators are entirely free to choose the distribution they prefer. There is also no strategic incentive to choose a cohort only to make sure they could get the €10 endowment, as every dictator was matched with a partner.

Table 3.7 reports the distribution of types by age cohort: we fail to find any statistically significant difference in the distribution of types between age cohorts (p = 0.752, χ^2-test). A χ^2-test also reveals that the fraction of *Non-maximizer* types is not significantly different across age cohorts (p = 0.388).[12] To further control that our results are not driven by cognitive decline or poor understanding of the instructions, we re-run all the analysis excluding or controlling for non-maximizers (Table 3.8 to 3.11). All the results are qualitatively and quantitatively the same.

Table 3.8: Cooperation by age cohort of the partner – Robustness check

	Cooperation in DG	
	(1=Cooperate, 0=Defect)	
	Model 1	Model 2
Partner from Middle cohort *(1=Yes, 0=No)*	0.009	0.044
	(0.221)	(0.193)
Partner from Senior cohort *(1=Yes, 0=No)*	0.524**	0.433**
	(0.229)	(0.198)
Resident in Innsbruck *(1=Yes, 0=No)*	0.151	0.140
	(0.498)	(0.412)
Non-maximizer *(1=Yes, 0=No)*		-0.200
		(0.313)
Constant	-0.544	0.183
	(1.118)	(0.966)
Controls	Yes	Yes
N.obs.	848	1049
Log. Likelihood	-484.423	-618.374

Notes: Panel random-effects logit regressions. In Model 1, we restrict the analysis to participants who were classified as maximizers or indifferent. In Model 2, we consider the full sample but we include a dummy taking value 1 if a decision maker was classified as non-maximizer. See Table 3.3 for a detailed description of the variables. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

[12]Participants classified as *Indifferent* tend to choose their own cohort. A potential explanation for this pattern is that participants have more accurate information about peers from their own cohort and thus their estimates might be more reliable.

Table 3.9: Generosity by the age cohort of the Dictator – Robustness check

	Generosity in DG *(amount given)*			
	Model 1	Model 2	Model 3	Model 4
Dictator from Middle cohort *(1=Yes, 0=No)*	-0.075	-0.048		
	(0.391)	(0.356)		
Dictator from Senior cohort *(1=Yes, 0=No)*	0.017	-0.063		
	(0.483)	(0.433)		
Dictator Age (in years)			0.022**	0.019*
			(0.011)	(0.010)
Resident in Innsbruck *(1=Yes, 0=No)*	0.208	0.282	0.209	0.254
	(0.464)	(0.412)	(0.465)	(0.412)
Non-maximizer *(1=Yes, 0=No)*		0.182		0.113
		(0.324)		(0.323)
Constant	3.373***	3.662***	2.756**	3.123***
	(1.092)	(1.001)	(1.129)	(1.036)
Controls	Yes	Yes	Yes	Yes
N.obs.	848	1049	842	1043
R^2 (overall)	0.047	0.032	0.057	0.039

Notes: Panel random-effects GLS regressions. In Models 1 and 3, we restrict the analysis to participants who were classified as maximizers or indifferent. In Models 2 and 4, we consider the full sample but we include a dummy taking value 1 if a decision maker was classified as non-maximizer. See Table 3.4 for a detailed description of the variables. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

Table 3.10: Cooperation by age cohort of the player – Robustness check

	Cooperation in PD *(1=Cooperate, 0=Defect)*					
	The partner belongs to the ... cohort					
	Junior		*Middle*		*Senior*	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Expect Cooperation *(1=Yes, 0=No)*	2.819***	2.927***	2.461***	2.147***	2.470***	2.080***
	(0.578)	(0.552)	(0.518)	(0.449)	(0.554)	(0.472)
DM from Middle cohort *(1=Yes, 0=No)*	0.394	0.261	-0.033	-0.274	-0.055	-0.382
	(0.462)	(0.416)	(0.560)	(0.510)	(0.584)	(0.531)
DM from Senior cohort *(1=Yes, 0=No)*	0.597	0.144	-1.263*	-0.819	-0.320	-0.430
	(0.575)	(0.498)	(0.694)	(0.590)	(0.678)	(0.591)
DM Middle x Expect Coop.	-0.765	-1.228*	-0.214	0.284	-0.250	0.074
	(0.811)	(0.735)	(0.752)	(0.663)	(0.772)	(0.673)
DM Senior x Expect Coop.	-1.510*	-1.761**	0.109	0.254	-0.361	-0.143
	(0.784)	(0.709)	(0.762)	(0.659)	(0.777)	(0.666)
Resident in Innsbruck *(1=Yes, 0=No)*	0.109	0.089	0.304	0.051	-0.059	-0.153
	(0.497)	(0.426)	(0.525)	(0.453)	(0.520)	(0.443)
Non-maximizer *(1=Yes, 0=No)*		-0.030		0.097		-0.318
		(0.317)		(0.336)		(0.339)
Constant	-3.950***	-3.043***	-1.104	-0.762	-1.883	-0.749
	(1.388)	(1.149)	(1.194)	(1.070)	(1.205)	(1.084)
N.obs.	280	347	281	348	281	348
Log. Likelihood	-144.986	-186.933	-137.667	-178.083	-131.547	-173.055

Notes: Panel random-effects GLS regressions. In Models 1, 3, and 5, we restrict the analysis to participants who were classified as maximizers or indifferent. In Models 2, 4, and 6, we consider the full sample but we include a dummy taking value 1 if a decision maker was classified as non-maximizer. See Table 3.5 for a detailed description of the variables. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

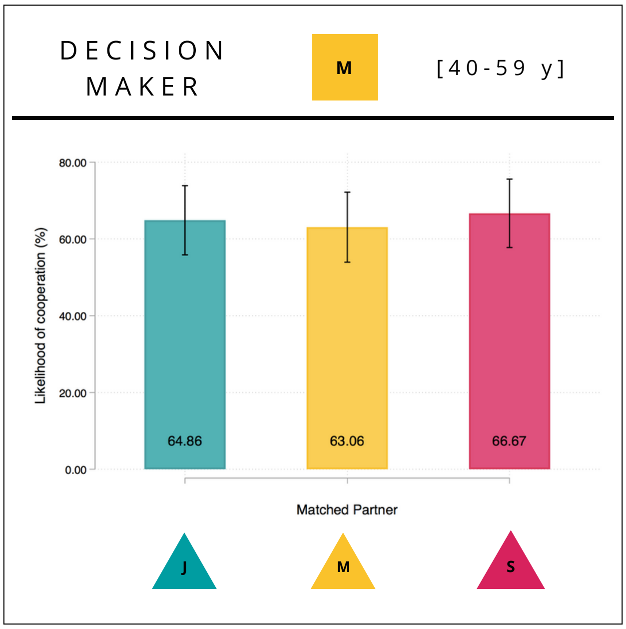
Table 3.11: Ingroup favoritism – Robustness check

	Generosity in DG (amount given)		**Cooperation in PD** (1=Cooperate, 0=Defect)	
	Model 1	Model 2	Model 3	Model 4
Partner from own cohort *(1=Yes, 0=No)*	0.168	0.117	-0.238	-0.247
	(0.157)	(0.144)	(0.192)	(0.168)
Resident in Innsbruck *(1=Yes, 0=No)*	0.213	0.282	0.151	0.141
	(0.458)	(0.407)	(0.491)	(0.409)
Non-maximizer *(1=Yes, 0=No)*		0.178		-0.200
		(0.319)		(0.310)
Constant	3.315***	3.626***	-0.286	0.420
	(1.080)	(0.992)	(1.096)	(0.954)
Controls	Yes	Yes	Yes	Yes
N.obs.	848	1049	848	1049
R^2 (overall)/Log. Likelihood	0.048	0.033	-487.145	-620.208

Notes: Models 1 and 2 report GLS panel regressions with random-effects at the individual level. Models 3 and 4 report Logit panel regressions with random-effects at the individual level. In Models 1 and 3, we restrict the analysis to participants that were classified as maximizers or indifferent. In Models 2 and 4, we consider the full sample but we include a dummy taking value 1 if a decision maker was classified as non-maximizer. See Table 3.6 for a detailed description of the variables. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

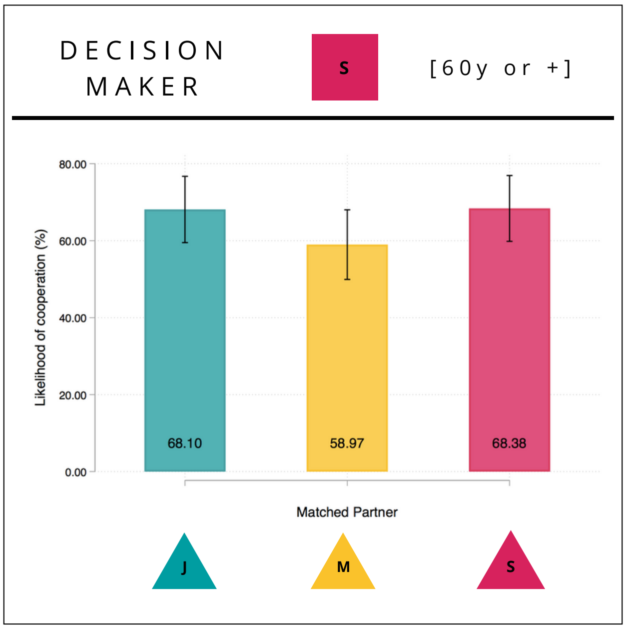
3.5.4 ADDITIONAL FIGURES

Figure 3.7: Cooperation behavior of Middle cohort decision makers



Notes: The figure presents the cooperation behavior of the Middle cohort. The first bar reports the percentage of cooperation for the case in which a Middle had to make a decision for a partner from the Junior cohort. The middle bar reports the percentage of cooperation for the case in which the partner was from the Middle cohort. Finally, the last bar reports the case in which the partner was from the Senior cohort. Standard errors are reported for each bar.

Figure 3.8: Cooperation behavior of Senior cohort decision makers



Notes: The figure presents the cooperation behavior of the Senior cohort. The first bar reports the percentage of cooperation for the case in which a Senior had to make a decision for a partner from the Junior cohort. The middle bar reports the percentage of cooperation for the case in which the partner was from the Middle cohort. Finally, the last bar reports the case in which the partner was from the Senior cohort. Standard errors are reported for each bar.

3.5.5 DESCRIPTIVE STATISTICS

Table 3.12: Summary statistics of the Dictator Game

Age cohort of the DM	**Age cohort of the recipient**		
	Junior	Middle	Senior
Junior	4.16	3.74	4.28
	(2.69)	(2.46)	(2.89)
Middle	4.54	3.89	4.30
	(2.92)	(2.58)	(2.90)
Senior	5.03	3.84	5.17
	(3.23)	(3.01)	(3.43)

Notes: The table shows the mean amount given (standard deviation in parentheses) for each age cohort of dictators conditional on the age cohort of the recipient.

Table 3.13: Summary statistics of the Prisoner's Dilemma Game

Age cohort of the DM	**Age cohort of the partner**		
	Junior	Middle	Senior
Junior	0.55	0.67	0.70
	(0.50)	(0.47)	(0.46)
Middle	0.65	0.63	0.67
	(0.48)	(0.48)	(0.47)
Senior	0.68	0.59	0.68
	(0.47)	(0.49)	(0.47)

Notes: The table shows the fraction of cooperators (standard deviation in parentheses) for each age cohort conditional on the age cohort of the partner.

3.5.6 EXPERIMENTAL INSTRUCTIONS

Page 1[13]

General information

Thank you very much for participating in this short study. You can earn money during the study and we will pay in cash and in private at the end of the study. Please, read the instructions carefully because your decisions can influence your earnings and the earnings of other people. You will be able to participate in two games.

Instructions Game 1

We will form pairs and one member of the pair will be randomly assigned €10 and the other €0. At the end, we will determine if you get the €10 or the other member of the pair by a coin flip.

If you are ready you can turn the page. You have to answer to Part A first and can then move to Part B.

If you have any question during the experiment, please ask a member of the staff.

Page 2

Decisions Game 1: Part A

Consider the case in which you
win the coin flip and get €10.

You have to decide if and how much you want to give to the other person. You can condition the amount on the age group of the other person. You will know the actual age only at the end. If you win the coin flip, your earnings are equal to €10 minus what you give to the other person.

How much would you like to give to the other person?

You can give any amount between 0 Euro and 10 Euro, in increments of 50 cents.

[13] The design was presented in a A5 format booklet. The original instructions are in German and are available upon request from the authors.

Age range of the other person	Give to the other person
18-39 years	.... Euro
40-59 years	.... Euro
60 years +	.... Euro

Page 3

Decisions Game 1: Part B

Consider the case in which you
lose the coin flip and get €0.

You have to state how much you expect to receive from the other member of the pair. You can condition your answer on the age group of the partner. You also have to choose the age group of the person you want to be paired with.

How much do you think the other person would give you?
With whom do you want to be paired?

Remember that the sum of the money the other person keeps and gives to you must be 10 Euro. Please round the amounts to the nearest 50 cents.

Age range of the other person	Receiving from another person	Choose with whom (age range) you want to be matched with [ONLY ONE ANSWER]
18-39 years	.... Euro	O
40-59 years	.... Euro	O
60 years +	.... Euro	O

Page 4

Instructions Game 2

In this part of the experiment, you **receive 2 Euro** and will be matched with another participant (different from the one in Game 1).

You have to decide between two actions: **Keep** or **Transfer**.

- If you **Keep**, you **keep the 2 Euro and the other participant** receives nothing from you.
- If you **Transfer**, you **send the 2 Euro to the other participant** and he/she receives **4 Euro**.

The other participant is playing the same game as you, and can either Keep or Transfer. There are four possible cases:

1. **Both you and the match Keep:** both earn 2 Euro.
2. **You Keep and your match Transfer:** you earn 6 Euro and your match earns 0 Euro
3. **Both you and your match Transfer:** both earn 4 Euro
4. **You Transfer and your match Keep:** you earn 0 Euro and your match earns 6 Euro

If you have any question during the experiment, please ask a member of the staff.

Page 5

Decisions Game 2

Please make your decision.

Now you have to decide if you choose to **Keep** or **Transfer**. You can condition your answer on the age group of the partner. You also have to state what do you expect from your match. Do you think the person matched with you will Keep or Transfer? (you can condition on the age of the other person)

Please mark your choices with an X.

Age range of the other person	Your decision	What do you expect from the person matched with you?
18-39 years	O Transfer O Keep	O Transfer O Keep
40-59 years	O Transfer O Keep	O Transfer O Keep
60 years +	O Transfer O Keep	O Transfer O Keep

Page 6

Questionnaire

Age (in years):	....
Gender:	O Male O Female
Place of residence (Zip code):	....
Highest education level attained:	....
Number of children:	....
Employment:	(Please choose one): O Employee O Self-employed O Unemployed O Student O Housewife O Retired O N/A

CHAPTER 4

THE INTRINSIC VALUE OF DECISION RIGHTS – TEAM VS. INDIVIDUAL DECISION-MAKING

joint work with Justin Buffat and Matthias Sutter

Abstract

Power and decision rights are valued for their instrumental role – the ability to affect the consequence of a decision – but are also valued *per se*. While recent experimental and empirical work has shown that individual decision makers value decision rights intrinsically, evidence for teams and group decision-making is scarce. In this paper, we build on Bartling et al.'s (2014) framework to quantify and determine if groups of decision makers value decision rights intrinsically. Specifically, we compare team decision-making with individual decision-making. While our results suggest that teams do value power intrinsically, our data reveal that the value placed on decision rights depends on the decision-making process prevailing in the team. When none of the teams members reported any issue regarding the decision process, teams assign lower values than individuals. In line with the literature on team decision-making, this result suggests that teams displays a behavior that is more consistent with the rational decision maker than individuals. However, for teams reporting an issue, the weight placed on keeping the decision rights dramatically increases, even above the value of individual decision makers. The findings suggest that groups of individuals assign lower importance to being in control than individuals only when no issues arise in the decision process.

4.1 INTRODUCTION

Decision makers value power and decision rights. Having the right to decide gives control over the outcome that is preferred. This value, referred to as the instrumental value, is determinant in shaping behavior and has long been considered as the only driver of behavior (see, e.g., Grossman and Hart, 1986; Hart and Moore, 1990). Yet, decision rights carry an additional value, an *intrinsic value*, because decision makers enjoy decisions and power *per se*. Recent experimental work confirms this idea and shows that decision makers do value decision rights beyond their instrumental value (Bohnet and Zeckhauser, 2004; Bohnet et al., 2008; Fehr et al., 2013; Bartling et al., 2014;

Owens et al., 2014; Neri and Rommeswinkel, 2017) and that this value is sizable. The gap between the *intrinsic* and the instrumental value reflects the psychological value of being in control. Hence, decision makers do not only value the outcome dimension of a decision, but also the procedure that generates the outcome (Frey et al., 2004; Chlaß et al., 2014; Dold and Khadjavi, 2017).[14]

While many studies support the hypothesis that decision rights are valued *intrinsically* by individual decision makers, decisions in organizations are mostly made by a group of individuals rather than individually. Examples include boards of CEOs, hiring committees, central bank committees, and executive boards in political parties. In these situations, the power is shared by individuals and decisions are taken at the group level. Since most decisions in organizations are made in teams, it is relevant to understand whether teams value decision rights *intrinsically*. While the literature on team decision-making provides no definite answer how teams behave compared to individuals, a fair amount of studies show that teams behave in a way which is more consistent with the hypothesis of a rational decision maker than individuals (see, e.g., Charness and Sutter, 2012; Kugler et al., 2012, for an overview). Because a rational decision maker would only care about the consequences of a decision – and not about the process leading to the outcome – she would assign zero importance to the decision right *per se*. Teams behaving as such should hence value decision rights much less than individuals.

In this paper we address these questions. Specifically, we test whether teams value decision rights *intrinsically* and, if so, how this value compares to that of individuals. To do so, we build on Bartling, Fehr and Herz's (2014) framework to quantify the *intrinsic value* of decision rights in a team decision-making context. The main advantage of their design lies in the ability to discriminate between the *instrumental value* and the *intrinsic value* of a decision rights. The design comprises two parts: The goal of the first part is to elicit the principal's preferences over keeping or delegating the decision right to an agent regarding the implementation of a risky project. The second part present the principal with two lotteries that capture first part's preferences but take place outside of the delegation context: A "control" lottery and a "delegation" lottery. The lotteries reflect the probabilities and payoff consequences of the first part, but are *exogenously given* to the principal. The certainty equivalent of both lotteries is elicited. Since the *intrinsic value* component of the decision right is absent in the second part, a principal

[14]Both the economics and psychology literature provide studies showing the existence of an intrinsic value of decision rights (see, e.g., Deci and Ryan, 1985, 2000; Sen, 1985; McClelland, 1975). For instance, Neri and Rommeswinkel (2017) study the reasons for individuals valuing decision rights intrinsically and show that they do so because they want to avoid others to interfere in their outcome, rather than for the freedom of choice, nor to have control over someone else.

being indifferent between keeping and delegating the decision right should assign equal certainty equivalents. Only if the certainty equivalent of the delegation lottery is larger than the certainty equivalent of the control lottery, does the principal assign a larger value to being in control of a decision right than delegating it, i.e., a non-zero intrinsic value. The results of Bartling et al. (2014) clearly show that individuals do value decision rights *per se* and that being in control of the decision is valued about 17% more than delegating the decision.

In our study, we design two treatments: an individual decision-making treatment (*SINGLE*) – a replication of Bartling, Fehr and Herz's (2014) original condition – and a team decision-making treatment (*TEAM*). In the latter treatment, a team of two subjects plays the role of the principal, while the agent remains a single subject. Team members make decisions separately, but can freely exchange messages to coordinate. Importantly, members of a team must agree on a common decision. In this framework, team members have an incentive to communicate to efficiently coordinate on a common decision.

Following the literature on team-decision making, teams are expected to be less affected by psychological factors than individuals. They are hence expected to be less willing to keep control over decision rights than individuals. If true, the *intrinsic value* of teams should be lower than the *intrinsic value* of individuals. As for whether teams value decision rights *intrinsically*, the literature provides no definite answer. On the one hand, studies show that under some circumstances groups of individuals behave more consistent with the hypothesis of a rational decision maker than individuals. For instance, in standard ultimatum games teams have been shown to send less money to recipients and display a very low rejection rate when in the shoes of the recipient (Bornstein and Yaniv, 1998). Also, teams systematically choose lower numbers than individuals in the Beauty Contest game, learn faster than individuals and outperform individuals in terms of payoffs (Kocher and Sutter, 2005), and the larger the size of the team the smaller the chosen numbers (Sutter, 2005). Moreover, teams transfer less money in a dictator game (Luhan et al., 2009) and are less likely to create price bubbles in financial markets than individuals (Cheung and Palan, 2012).[15] Extending these findings to our design, teams in our experiment should hence be almost indifferent between keeping and delegating a decision right, which would imply a zero or fairly low *intrinsic value*.

On the other hand, some studies report that team decision-making does not lead to a different behavior than individual decision-making. For instance, Bone et al. (1999) find

[15]In addition, it has been shown that teams coordinate better than individuals (Feri et al., 2010), perform better in the so-called Monty-Hall problem (Slembeck and Tyran, 2004; Maciejovsky et al., 2013) and play more strategically in signaling games (Cooper and Kagel, 2005).

that group discussion does not lead to more consistent choices than individual decision-making in so-called common ratio problems. Rockenbach et al. (2007) compare team and individual decision-making under risk. In particular, the authors test behavior against two benchmarks, namely standard expected utility theory (EUT) and portfolio selection theory (PST). They find no difference between individuals and teams with respect to the EUT, but teams are more consistent with the PST than individuals.[16]

Our data yield insightful results. First, our treatment replicating Bartling et al.'s (2014) individual decision-making condition yields very similar *intrinsic values*. Remarkably, the average *intrinsic value* (in percent) in our *SINGLE* treatment is 17.0% and differs very little from the value reported in the original paper (16.7%), and the two samples are statistically indistinguishable from each other. The data in our sample also very strongly support the hypothesis of a positive and economically important *intrinsic value*. In addition, we replicated all effects observed in their study regarding (i) stake sizes and (ii) various degrees of conflict between the principal and the agent. Also, for 9 out of 10 different delegation games (i.e., with different payoff consequences) the value in our data is statistically indistinguishable from the value in their data.

Second, our data suggest that overall groups assign a positive *intrinsic value*, as do individuals, and that the value they assign is not different from the value assigned by individuals. This is true for all stake sizes and degrees of conflict of interest between the principal and the agent. While this result is at first surprising, it can be expected due to the rules governing team decision-making. In our design, requiring team members to agree on a common decision potentially creates situations in which team members hardly find an agreement unless one of them gives up. Anticipating this possibility, we asked each team member about any issues encountered during the experiment. In particular, we asked team members to report whether they "felt excluded" or "gave up" in the decision making process. Based on this information, we divide the sample of teams in two groups: Teams reporting no issues in the decision-making process (*smooth teams*) and teams in which at least one team member reported an issue (*conflict teams*).

When controlling for the issues that arose in the decision process, the data show that *smooth teams* assign a very low *intrinsic value* to decision rights, about 8%. This value is less than half of the value individuals display (treatment *SINGLE*), 17%, and this difference is statistically significant. This finding is in line with studies reporting that groups of individuals are less affected by psychological factors than individuals.

At contrary, *conflict teams* assign a greater importance to being in control, with an *intrinsic value* of about 40%. While these teams annihilate the overall difference between

[16]Other studies do not report systematic differences in behavior between teams and individuals (see, e.g., Davis, 1992; Kerr et al., 1996; Bateman and Munro, 2005; Sutter et al., 2009; Harrison et al., 2012; Baker et al., 2008).

teams and individuals, very little can be said on the reasons for such a high value as too few teams reported an issue.

While the self-reported variables used to split the sample of teams into two allows to document an heterogeneous effect, a more objective measure is needed to back up issues in the decision-making process. Hence, we provide further evidence that teams reporting an issue use communication less efficiently than teams reporting no issues. Teams without issues were also able to coordinate on a common decision in the very first proposal more often than teams reporting issues, but also much sooner in the experiment. Rather than relying on communication to coordinate, the data suggest that teams with issues use strategies in which one member adjusts his or her decision to the decision of the other. Moreover, we coded the text messages that were exchanged between team members into various categories to learn more about the quality of the decision-making process. Interestingly, the more often team members seek the opinion of each other and the more they express their preferences regarding the decisions, the lower the value they assign to being in control of the decision. Finally, we compare the selected effort of both team types and individual decision makers with the theoretical optimal effort level.

Our results are novel in several ways. To begin with and to the best of our knowledge, we are the first to study whether groups of individuals value decision rights beyond their instrumental role and how this valuation compares to the valuation of individual decision makers. Crucially, we show that without conditioning on the decision-making process, teams do not behave differently from individuals. It is only when teams have no issues in the decision-making process that teams are less reluctant to give up control over decision rights than individuals. Since many decisions are made at the team level, this result is relevant from an organizational as well as a managerial point of view and has never been documented before.

The paper is organized as follows. Section 4.2 outlines the original design and our experimental treatments. In Section 4.3, we present our findings and discuss their implications. In Section 4.4 we provide some insights on heterogeneity in teams. Finally, Section 4.5 concludes.

4.2 EXPERIMENTAL DESIGN

We build on the design developed by Bartling, Fehr and Herz (2014, hereafter BFH) to elicit and measure the *intrinsic value* of decision rights of subjects. As noted by BFH (2014), the main difficulty in obtaining a value is to separate the *intrinsic value* from the instrumental value. To achieve this goal, BFH (2014) designed an experiment in two parts – a delegation part and a lottery part – based on subjects' revealed preferences. Their design provides a measure that expresses (in percentage) how much subjects value being in control of a decision rather than delegating the decision. The intuition for a two-part design is the following. The first part serves to elicit subjects' preferences over the delegation of a decision right. The second part uses the data from the first part to present subjects with lotteries that have identical payoff consequences but are given outside of the delegation context so that the component responsible for the *intrinsic valuation* is absent.

In what follows, we describe the setup used for the elicitation method developed by BFH (2014) and then expose our treatments. We finally describe additional measures we collected. We start by describing the first part: the delegation part.

4.2.1 THE SETUP

The Delegation part

The first part consists of a principal-agent delegation game in which the principal initially owns the decision right and can delegate it to the agent. Both the principal and the agent make two decisions in case they own the decision right: (i) They choose one of the two project alternatives ($\mathcal{P}$ or $\mathcal{A}$) and (ii) they select the probability of success of the chosen project. Choosing the probability of success is framed as a costly effort provision, with $e^i \in [0, 100]$ the effort level that determines the probability of success of the selected project, where $i = \{P, A\}$ denotes the principal and the agent. The party which finally holds the decision right has to pay the cost for the chosen effort level, $c(e) = ke^2$ with $k > 0$ as a parameter. The payoffs to the principal and to the agent, which depend on the chosen alternative and on the success of the project, are displayed in Table 4.1 for the 10 different delegation games.

The principal, who initially holds the decision right, has to make a third decision: She selects a minimum requirement for the agent's effort, denoted $\underline{e}$. This is the main innovation of BFH's (2014) design and this is what determines whether the decision is delegated or not. Specifically, delegation takes place if and only if the agent's effort is at least as high as the minimum required by the principal, i.e., $e^A \geq \underline{e}$. This mechanism ensures that the principal keeps the decision right whenever the agent's chosen effort would make her worse off and delegates the right otherwise. In the words of BFH (2014), it is in the principal's best interest to set the minimum requirement in a way that if the chosen effort of the agent is equal to the minimum requirement, the principal is indifferent between keeping and delegating the decision right.

Note that by design, holding the decision right induces a trade-off: On the one hand, the party with the decision right has her decisions implemented, and this is important because the principal weakly prefers project $\mathcal{P}$ over $\mathcal{A}$, and the agent $\mathcal{A}$ over $\mathcal{P}$. On the other hand, the party with the decision right has to bear the cost of the effort. The delegation game is played for 10 different situations that reflect different stakes and different payoff allocations between the principal and the agent. The following example summarizes the decisions and payoff consequences of the delegation part:

NUMERICAL EXAMPLE. Suppose that in Game 3 the principal chooses an effort $e^P = 60$ (effort cost of 36), a minimum effort requirement $\underline{e} = 40$ (cost of 16) and projective alternative $\theta^P = \mathcal{P}$.

If $e^A < 40$: The decision right remains with the principal and project alternative $\mathcal{P}$ will be implemented. Thus, the principal and the agent receive $180 - 36 = 144$ and 140 with probability 0.6 (because $e^P = 60$), respectively, or $100 - 36 = 64$ and 100 with probability 0.4, respectively.

If $e^A = 60 \geq 40$: The decision right is delegated to the agent. Hence, the principal and the agent receive 140 and $180 - 36 = 144$ with probability 0.6 (because $e^A = 60$), respectively, or 100 to the principal and $100 - 36 = 64$ to the agent with probability 0.4, respectively.

Table 4.1: Parameters of the delegation games

	Project Successful				**Project Unsuccessful**				
	Alternative $\mathcal{P}$		Alternative $\mathcal{A}$		Outside option		Stake size	Conflict of interest	Cost parameter (k)
	Principal	Agent	Principal	Agent	Principal	Agent	HS - high,	HC - high, LC - low,	$k = 0.01$ - low,
	$P_{\mathcal{P}}$	$A_{\mathcal{P}}$	$P_{\mathcal{A}}$	$A_{\mathcal{A}}$	P_0	A_0	LS - low	NC - no	$k = 0.02$ - high
Game 1	220	190	190	220	100	100	LS	LC	0.01
Game 2	280	235	235	280	100	100	LS	LC	0.01
Game 3	180	140	140	180	100	100	LS	HC	0.01
Game 4	220	160	160	220	100	100	LS	HC	0.01
Game 5	260	260	260	260	100	100	LS	NC	0.01
Game 6	440	380	380	440	200	200	HS	LC	0.02
Game 7	560	470	470	560	200	200	HS	LC	0.02
Game 8	360	280	280	360	200	200	HS	HC	0.02
Game 9	440	320	320	440	200	200	HS	HC	0.02
Game 10	520	520	520	520	200	200	HS	NC	0.02

Notes: The table shows the payoffs for the principal and the agent in 10 different games depending on whether the project is successful or unsuccessful. The table also indicates "LS" for low-stake games (Games 1-5) and "HS" for high-stake games (Games 6-10) and displays a measure of the degree of conflict between the principal's and the agent's payoffs, with "NC" standing for no conflict, "LC" for low conflict and "HC" for high conflict.

The Lottery Part

The goal of the lottery part is to present the principals with payoff consequences that are equivalent to the delegation part but that take place outside of the delegation context, i.e., the *intrinsic* component is absent.

Specifically, in this part the principal is presented with two lotteries: a "control" lottery and a "delegation" lottery. Although these lotteries reflect decisions from the first part, they are *exogenously* given to the principal in this part. The task of the decision maker is to state her certainty equivalents for each of the lotteries. As will be shown later, the difference in certainty equivalents between the control lottery and the delegation lottery will be used as a measure of the *intrinsic value* of a decision right.

Control lotteries represent payoffs and probabilities when the principal keeps the decision rights. They are hence solely determined by the principal's choices regarding the project alternative and the probability of success. Let $L^c(e^P, \theta^P)$ denote the control lottery, with θ^P the project alternative chosen by the principal. The delegation lottery represents payoffs and probabilities when the principal delegates the decision. Let $L^d(e^A, \theta^A)$ denote any delegation lottery when the agent choses effort $e^A \geq \underline{e}$ and project alternative θ^A. The worst delegation lottery to the principal is achieved when the agent chooses her preferred alternative $\mathcal{A}$ and selects the lowest minimum effort so that delegation takes place, i.e., $e^A = \underline{e}$. This worst delegation lottery is denoted $L^{\underline{d}}(\underline{e}, \mathcal{A})$.

A NUMERICAL EXAMPLE. Based on the previous numerical example, the *control lottery* yields payoffs 144 to the principal and 140 to the agent with probability 0.6, and payoffs 64 to the principal and 100 to the agent with probability 0.4. The *delegation lottery* yields 140 to the principal and 164 to the agent with probability 0.4, and 100 to the principal and 84 to the agent with probability 0.6. Subjects report their certainty equivalent for each lottery, $CE(L^c)$ for the control and $CE(L^{\underline{d}})$ for the delegation lottery.

MEASURING THE INTRINSIC VALUE. In this paragraph, we show how the *intrinsic value* of the decision right can be obtained from comparing the utilities of both lotteries. Let's start with the case of a principal not valuing decision rights *per se*. If the principal plays the lottery, her expected utility is given by the consequences of the lottery she faces, $U(L^c)$ for the control lottery and $U(L^{\underline{d}})$ for the delegation lottery. If the principal does not play the lottery she receives her stated certainty equivalent $CE(L)$ and the agent receives the outside option $\mathcal{A}_0$, so that the principal's utility is $u(CE(L), \mathcal{A}_0)$.[17]

It is optimal for a principal to choose a minimum effort requirement $\underline{e}^*$ such that the utilities are equal, i.e.,

$$U(L^c) = U(L^{\underline{d}}) \Leftrightarrow u(CE(L^c), \mathcal{A}_0) = u(CE(L^{\underline{d}}), \mathcal{A}_0). \tag{4.2.1}$$

For the latter equation to hold, the certainty equivalents must be equal, $CE(L^c) = CE(L^{\underline{d}})$. Hence, without any *intrinsic valuation* of the decision right, the gap in certainty equivalents is zero.

Now suppose that the principal *intrinsically values* a decision right. This means that her utility directly depends on the allocation of the decision right. The expected utility of a lottery is then described as $U(L^w, w)$ with $w = \{c, d\}$ indicating either keeping control over the decision ($w = c$) or delegating it ($w = d$). Expression 4.2.1 is modified accordingly:

$$U(L^c, c) = U(L^{\underline{d}}, d) \Leftrightarrow u(CE(L^c) + V_c, \mathcal{A}_0) = u(CE(L^{\underline{d}}) + V_{\underline{d}}, \mathcal{A}_0), \tag{4.2.2}$$

where V_c and V_d capture the potential *intrinsic values* of being in control and of delegating, respectively. For equation (4.2.2) to hold, it must be that

$$V_c - V_{\underline{d}} = CE(L^{\underline{d}}) - CE(L^c) \equiv IV, \tag{4.2.3}$$

where we defined IV, the *intrinsic value*, as the difference in certainty equivalents. If $V_c > V_{\underline{d}}$, the value placed on being in control outweighs the value of delegating and $IV = CE(L^{\underline{d}}) - CE(L^c) > 0$. Hence, a difference in certainty equivalents may indicate either a positive or negative *intrinsic value*.

4.2.2 PROCEDURE

In the instructions, principals and agents were neutrally referred to as Role A and Role B, respectively. In the first part, principals (Role A) and agents (Role B) played each of the 10 situations of the delegation game (see Table 4.1) in a random order.[18] We implemented a perfect-stranger matching such that in each round a principal is randomly matched with an agent but only once.

In the lottery part, both roles faced a control lottery and a delegation lottery for each of the 10 situations from the first part. In total, they faced 20 decisions in a random order. Role A was randomly matched with Role B in any new round. The certainty

[17]The agent receives the outside option to match what the agent would receive in the delegation game when the project is not successful.

[18]The order was random at the session level but the same for all principals within a given session.

equivalent for each lottery was elicited using the incentive compatible mechanism proposed by Becker et al. (1964). In particular, the principals had to specify the smallest certain payoff she would accept – the certainty equivalent – instead of facing the offered lottery. Then, the computer randomly drew a number from an uniform distribution and the lottery was played if the randomly selected number was smaller than the principal's certainty equivalent.[19] If the certainty equivalent was smaller or equal than the randomly drawn number, the principal received the randomly drawn number. In addition, the principal's decision affects the payment of a randomly matched participant, whose payoff is determined by the outcome of the lottery should the lottery be played or by a fixed payment if the lottery is not played (outside option of either 100 or 200, see Table 4.1).

In total, each subject played 10 rounds of the delegation stage (10 different situations) and 20 rounds of the lottery stage (10 delegation lotteries and 10 control lotteries).

4.2.3 TREATMENTS

We implemented two treatments, labeled *SINGLE* and *TEAM*. Treatment *SINGLE* is a replication of the original condition in BFH (2014). The treatment follows closely the steps described above and the subjects were provided with the original instructions. The only difference between our *SINGLE* treatment and the original treatment in BFH (2014) is the wording we use to describe the task to the participants. While BFH (2014) refer to the principal and to the agent as Participant A and Participant B, respectively, we refer to them as Role A and Role B. We used this wording to facilitate the comparison between both treatments, since in treatment *Team* a principal consisted of two individuals.

TEAM DECISION-MAKING. In treatment *TEAM*, a team of two persons plays in the role of a principal (Role A), while the agent is a single subject (Role B). The pair of principals remains fixed for the entire duration of the experiment. The principals in *TEAM* faced the same set of decisions as the single principal in *SINGLE*, but they must reach a common decision before moving to the next round. At any time during a round, team members could exchange messages through a free-text chat box. The chat box is erased and cleared at the end of each round. At the end of each situation/lottery, a summary screen displays the own as well as the partner's choices. They are then given the possibility to adjust their choices. Only when all of their decisions coincide, could they proceed to the next round.

[19]Note that the boundaries of the distribution was determined by the low and the high payment of the given lottery.

Importantly, we did not impose any limit on the time for the principals to reach a common decision. We did so to avoid time pressure and let them as much time as needed to reach a common decision. Finally, both roles were aware of the composition of the other role as well as the rules governing decisions within each role.

4.2.4 ADDITIONAL VARIABLES

At the end of the experiment, we collected additional measures that potentially affect the *intrinsic value*. As BFH (2014), we collected a measure of loss aversion. Both roles made six lottery decisions that can yield an additional gain of €3 per person or a loss of €X, with both outcomes being equally likely. We varied the size of the loss X in steps of €0.50 from €1 to €3.50 (see, e.g., Abeler et al., 2011, for a similar methodology). Although BFH (2014) showed that loss aversion did not affect the *intrinsic value* of decision rights in an individual decision-making context, we decided to measure it to account for potential differences between teams and individuals.[20] Since we do not observe any significant differences between the degree of loss aversion of individuals and teams ($p = 0.572$, two-tailed Mann-Whitney U-test), and we do not find any correlation between loss aversion and the average *intrinsic value* of decision rights as defined in equation (4.2.3) (Pearson's correlation coefficient: $\rho = -0.036, p = 0.784$ for individuals, and $\rho = 0.237, p = 0.208$ for teams), we do not report on this measure in the analysis.

We also obtained a measure of illusion of control. This variable measures the willingness of subjects to give up money in exchange for rolling themselves the dices that determine whether the project is successful or not.[21] Only a few individuals and teams decided to spend money to be able to roll the dices themselves (20 % in each treatment, $p = 1.000$ two-tailed Mann-Whitney U-test). Moreover, the willingness to pay did not vary significantly between treatments ($p = 0.990$, two-tailed Mann-Whitney U-test). Also, as in BFH (2014), this measure is not correlated with the average *intrinsic value* of decision rights (Pearson's correlation coefficient: $\rho = -0.168$, $p = 0.201$ for individuals and $\rho = -0.275$, $p = 0.141$ for teams). Hence, we do not report on this measure in what follows.

Lastly, to account for the fact that agreement within a team may vary between teams, we also collected information on how team members perceived the overall decision-making process. In particular, we asked each team member whether (i) she felt excluded from the decision-making process and (ii) she gave up in the decision-

[20]Note that team members made again a common decision. They used a free-text chat to coordinate on the lotteries to accept and reject.

[21]As in BFH (2014), both roles received a few more tokens and could decide how much to spend to be able to roll the dices themselves. Team members had to agree on whether to spend any tokens and, if so, on how much to spend.

making process at some point. Both questions could be answered by "Yes" or "No". These two questions were motivated by previous research pointing out that besides group consensus, individual acceptance and member satisfaction are important for effective team decisions (see, e.g., Priem et al., 1995).

4.2.5 PARTICIPANTS AND PAYMENTS

The sessions took place at the laboratory of the University of Cologne (C-LER) in June and July 2016. A total of 210 participants were recruited with ORSEE (Greiner, 2015). We conducted 7 sessions, with exactly 30 participants in each, accordingly: 4 sessions for treatment *SINGLE* (60 Role A, 60 Role B) and 3 sessions for treatment *TEAM* (60 Role A, 30 Role B). The experiment was computerized using Z-Tree (Fischbacher, 2007). Subjects were students of the University of Cologne with a self-reported fluency in German. At the end, one decision out of the ten delegation situations and two decisions out of the twenty lottery decisions were randomly chosen to be payoff relevant.

The experiment lasted on average 105 minutes for subjects in treatment *SINGLE* and 125 minutes for subjects in treatment *TEAM*.[22] The participants earned on average €25 (including a €4 show-up fee).

At the beginning of each part, new instructions were handed out individually together with control questions that tested their understanding of the experiment.[23] Subjects typed in their answers to the control questions and could start the experiment when all questions were correctly answered.

4.3 EXPERIMENTAL RESULTS

We report the results as follows. We first compare our estimates of the intrinsic value *IV* from our *SINGLE* treatment with the estimated values obtained from BFH (2014). As already stated, our treatment *SINGLE* followed a strict replication procedure of the original condition in BFH (2014).[24] We then present the main results for the team decision-making treatment *TEAM* and compare it with the individual decision-making treatment *SINGLE*.

[22]While the duration of this experiment was on average longer than a standard economic experiment at the University of Cologne, our experiment was shorter than the original experiment by BFH (2014). Rather than having student assistants verifying control questions of the participants one person at a time, answers were typed in the computer and automatically verified.

[23]A translated version of the instructions for *SINGLE* and *TEAM* is available in the Appendix.

[24]The only difference between our treatment SINGLE and BFH's (2014) original condition lies in the wording used to describe the principals and agents. While they use "Participant A" and "Participant B", we labeled them as "Role A" and "Role B" to account for the fact that a Principal in the TEAM treatment comprises more than one participant.

In what follows we focus on the intrinsic value *IV*, defined in equation (4.2.3), and expressed in percentage of the certainty equivalent of the control lottery, i.e.,

$$IV\% = \frac{CE(L^{\underline{d}}) - CE(L^c)}{CE(L^c)}.$$

This measure indicates the percentage to which the principal values the delegation lottery over the control lottery. In addition, this measure ensures that values of games with different stakes, hence different certainty equivalents, can be compared with each other.

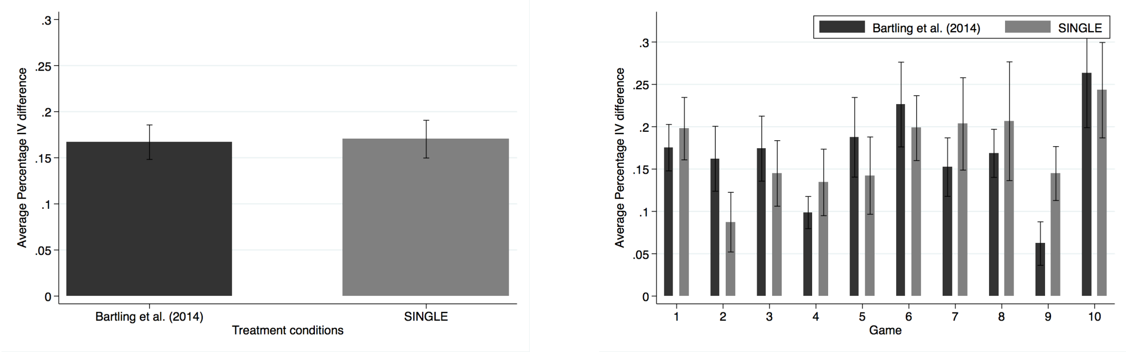
Figure 4.1a compares the $IV\%$ averaged over all delegation games of our treatment *SINGLE* with the data of BFH (2014). Both datasets show that individual decision makers value decision rights beyond the *instrumental value* of the decision rights such that the measured $IV\%$ is strictly positive. The data clearly suggests that there is no difference between the two samples. Remarkably, the $IV\%$ is 16.7% in BFH (2014) and 17.0% in our sample. Consequently, both samples are statistically indistinguishable from each other ($p = 0.745$, two-tailed Mann-Whitney U-test).

Figure 4.1b shows the average *intrinsic value* of treatment *SINGLE* and BFH(2014) at the game level. In every delegation game, each measured *IV%* in our data for individual decision makers is strictly positive, and is significantly different from zero for all ten delegation games (Wilcoxon-signed-rank tests, all $p < 0.010$, except for game 2, $p = 0.021$). Except for game 9, there is no significant difference between the *IV%* in BFH (2014) and our data.[25]

Overall, the data show that our treatment *SINGLE* is a strong replication of BFH (2014) and the slightly different wording and different subject pool that we used had no effect on decision-making.

[25]The $IV\%$ in game 9 is 6.2% in BFH (2014) and 14.5% in our data, and this difference is statistically significant ($p = 0.030$, two-tailed Mann-Whitney U-test).

Figure 4.1: Comparison of BFH (2014) and treatment *SINGLE*



(a) Delegation games (b) Aggregated

Notes: Percentage *IV* difference for the individual condition in BFH (2014) and treatment *SINGLE* at the aggregated level (panel a) and at the game level (panel b). Error bars represent standard errors at means (*average* ± *SEM*) clustered at the decision maker-level.

4.3.1 TEAM VS. INDIVIDUAL DECISION-MAKING

Since *IV*s (in percentage) do not differ between the original treatment and our *SINGLE* treatment, we proceed with the analysis of the treatment of interest: team decision-making. We present the results at the aggregate level, i.e., averaging over the ten delegation games (Figure 4.2a), as well as for each of the ten delegation games separately (Figure 4.2b).

Figure 4.2a shows the *IV*% at the aggregated level. On average, the *IV*% measured for teams accounts for 17.6%, compared to 17.0% for individuals. This difference is statistically not significant ($p = 0.504$, two-tailed Mann-Whitney U-test).

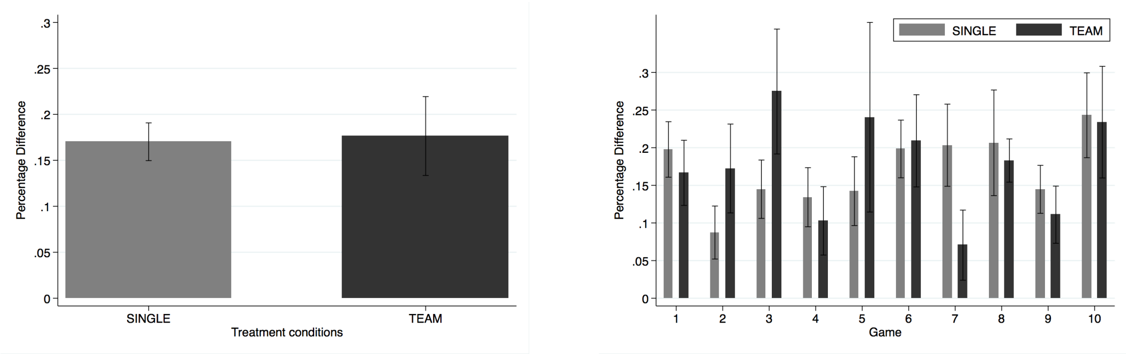
At the disaggregated level, Figure 4.2b provides additional evidence that teams did not assign different values to decision rights than individuals. Except for game 7, individuals and teams did not value decision rights differently.[26] Furthermore, the *IVs*, standardized by the certainty equivalent of the control lottery, of the team decision-making treatment are significantly positive at conventional levels for all delegation games ($p < 0.010$, except for game 4, $p = 0.047$, game 5, $p = 0.038$, and game 7, $p = 0.055$, Wilcoxon signed-rank tests).

The previous analysis suggests that, overall, teams do value decision rights intrinsically, in a way comparable to individual decision makers. We summarize the first result accordingly.

Result 1. *Individuals and teams in general both value decision rights intrinsically and do not assign different values.*

[26]Table 4.5 in the Appendix shows the OLS regressions of the IV% on the dummy *TEAM* for each of the ten games.

Figure 4.2: Comparison of treatment *SINGLE* and *TEAM*



(a) Aggregated (b) Delegation games

Notes: Percentage *IV* difference for treatments *SINGLE* and *TEAM* at the treatment level (panel a) and game level (panel b). Error bars represent standard errors at means (*average* ± *SEM*) clustered at the decision maker-level.

While it first comes as a surprise that teams and individuals in our setup behave very similarly given the extensive literature showing that behavior differs, many studies also reported no differences in behavior.[27]

In our design, team members had to agree on a common decision and could use a free-text chat to better coordinate. While unanimity is a natural practice, it is likely to create complicated situations when team members have opposite preferences. For instance, one member could feel excluded from the decision-making process or even give up expressing her own preferences. Anticipating this possibility, we included two questions in a questionnaire at the end of the experiment: (i) Did you feel excluded from the team decision-making process? and (ii) did you give up in the decision-making process? Participants could answer "Yes" or "No".

This measure, while self-reported, provides some insights on the difficulties in the decision-making process a team might encounter that would otherwise be hard to observe using a more objective measure. Out of 60 team members, 50 subjects of the *TEAM* treatment answered in the negative to both questions. Out of the 10 answering in the positive to one of the questions, 3 subjects "felt exculded", 3 subjects "gave up", and 4 subjects answered both questions positively. In total, for 21 teams both team members reported no issues while 9 teams comprised one member or more reporting at least one issue. Throughout the rest of the section, we distinguish between the two types of teams: *Smooth teams*, teams reporting no issues, and *conflict teams*, teams with at least one member reporting an issue. We now condition the results on the type of teams and put more focus on teams reporting no issues (*smooth teams*).

[27]See, e.g., Davis (1992), Kerr et al. (1996), Bone et al. (1999), Rockenbach et al. (2007), Baker et al. (2008), and Harrison et al. (2012).

Figure 4.3a depicts the *IV*% for treatment *SINGLE*, teams reporting no issues (*smooth teams*) and teams reporting issues (*conflict teams*) in the decision-making process. *Smooth teams* value decision rights much less than individuals (8.6% vs. 17.0%). This difference in values represents half of the baseline value in the *SINGLE* treatment and is highly significant ($p = 0.021$, two-tailed Mann-Whitney U-test). This result indicates that teams reporting no issues are less attached to keeping decision rights than individual decision makers and speaks in favor of the literature developed above. Interestingly, the graph shows that the *intrinsic value* for *conflict teams* is relatively high, 38.7%, and significantly larger than in *SINGLE* ($p = 0.015$, two-tailed Mann-Whitney U-test). Although the difference in values is substantial (about 100% of the baseline in *SINGLE*), we do not make strong claims due to the very limited number of *conflict teams*.

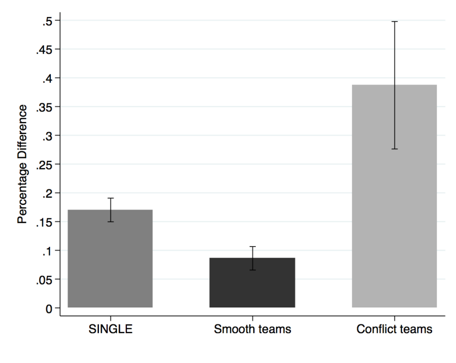
Figure 4.3b plots the *IV*% for each of the 10 delegation games for individual decision-making (*SINGLE*) and *smooth teams*. These teams display mostly lower *intrinsic values* than individuals and the values are not significantly different from zero in 40% of the delegation games (Games 2, 4, 5, and 7, $p > 0.100$, Wilcoxon signed-rank tests). For delegation game 10, the *IV*% is significantly different from zero ($p = 0.073$), as well as for games 1, 6, and 9 ($p < 0.050$), and games 3 and 8 ($p < 0.010$, all Wilcoxon signed-rank tests). Interestingly, the data show significant differences between individuals and *smooth teams* in six out of ten delegation games.[28]

Figure 4.3c plots the *IV*% for each of the 10 delegation games but compares individuals in *SINGLE* with *conflict teams*. The graph shows that the IV is systematically larger for these teams than for individuals, but due to limited power (i.e., very few teams) significant differences are detected in only 30% of the situations (games 2, 5 and 6, see Table 4.7 in the Appendix). We again do not make any statement regarding the behavior of *conflict teams*. These findings lead us to our second result.

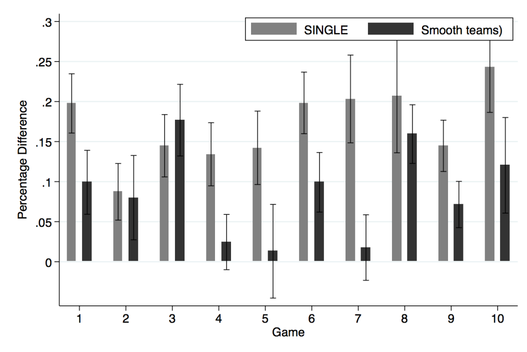
Result 2. *Smooth teams have a significantly lower intrinsic value (*8.6%*) than individuals (*17.0%*). Moreover, the intrinsic value in 4 out of 10 delegation situations is not statistically different from zero.*

[28]For games 1, 4, 5, 6, 7, and 9 (see Table 4.6 in the Appendix).

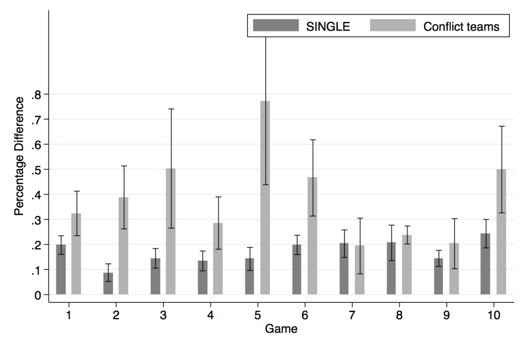
Figure 4.3: Comparison of treatment *SINGLE* and both team types



(a) Aggregated



(b) Delegation games *smooth teams*



(c) Delegation games *conflict teams*

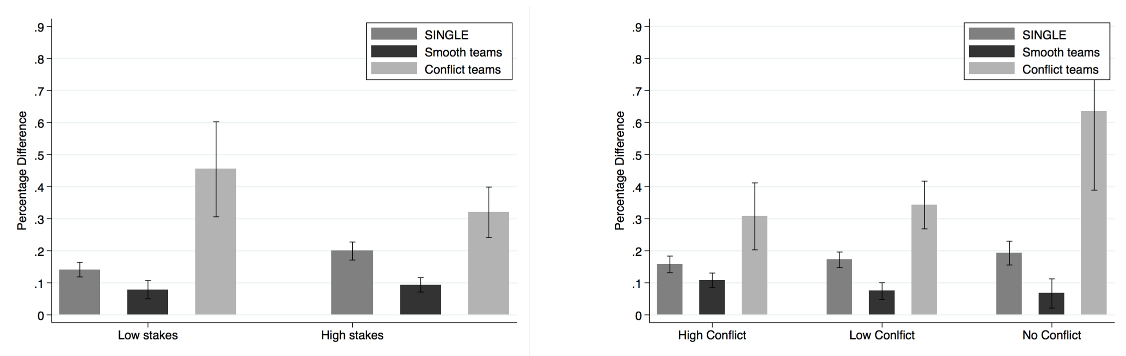
Notes: Percentage *IV* difference for treatment *SINGLE*, *smooth teams* and *conflict teams* at the aggregated level (panel a), at the game level for individuals and *smooth teams* (panel b) and individuals and *conflict teams* (panel c). Error bars represent standard errors at means (*average* ± *SEM*) clustered at the decision maker-level.

In the analysis we used the measure *IV%* to compare behavior of teams in treatment *TEAM* with individual decision makers in *SINGLE*. While the method to obtain the measure allows to separate the *intrinsic* from the *instrumental value*, BFH (2014) acknowledge that it is time-consuming and that a good proxy is available (pp. 2025-2026). They suggest to use the difference in expected values of the delegation and control lotteries generated in the delegation stage and they show that it is indeed a good proxy (it is highly correlated with the measured *intrinsic value*). In Section 4.6.2 in the Appendix, we also perform this analysis and show that the decisions in the delegation stage are indeed a good proxy, but especially for individual decision makers in SINGLE and for *smooths teams*, but are less reliable for *conflict teams*. We consider this as more evidence that a seperation in *smooth teams* and *conflict teams* is necessary to understand how teams value decision rights.

4.3.2 STAKES AND CONFLICT OF INTEREST

The experimental design allows us to control for the effect of the stake size and for the conflict of interest between the principal and the agent. The stakes in games 6 to 10 are twofold the stakes in games 1 to 5. As for the conflict of interest, the situations generate different tensions between the principal and the agent regarding their relative outcome in the projects. BFH (2014) measured the degree of conflict between the principal and the agent as the principal's relative payoff difference between project alternatives $\mathcal{A}$ and $\mathcal{P}$, denoted as $\alpha = (P_{\mathcal{A}} - P_0)/(P_{\mathcal{P}} - P_0)$. The less attractive the outcome of the agent's favorite alternative $\mathcal{A}$, compared to the principal's preferred alternative $\mathcal{P}$, the higher the conflict of interest, hence the lower α. For instance, in games 5 and 10 the difference in outcomes between the project alternative and the outside option is the same so that there is no conflict ($\alpha = 1$). For games 1, 2, 6 and 7, the conflict is "low" ($\alpha = 0.75$) and in games 3, 4, 8 and 9 the conflict is "high" ($\alpha = 0.5$).

Figure 4.4: The role of stake size and conflict of interest



(a) Stakes (b) Conflict of interest

Notes: Percentage *IV* difference for treatment *SINGLE*, *smooth teams*, and *conflict teams* for different stake sizes (panel a) and various degrees of conflict of interest (panel b). Error bars represent standard errors at means (*average* ± *SEM*) clustered at the decision maker-level.

Figure 4.4a displays the *intrinsic value* for *SINGLE*, *smooth teams*, and *conflict teams* over low and high stakes. As in BFH (2014), the larger the stakes the higher the *intrinsic value* of individual decision makers (from 14.1% to 19.9%, paired t-test, $p = 0.064$). For *smooth teams*, however, stakes do not significantly affect their *intrinsic value* (7.9% for low stakes, 9.4% for high stakes, paired t-test, $p = 0.642$). The difference in the *intrinsic value* between treatment *SINGLE* and *smooth teams* is not significant for low stakes ($p = 0.111$, two-tailed Mann-Whitney U-test), but significant for high stakes ($p = 0.044$, two-tailed Mann-Whitney U-test). Further, we can observe that *conflict teams* report a higher *intrinsic value* than individuals. This difference is significant for low stakes situations (p = 0.002), but fails to be significant for high stakes situations (p = 0.135, both two-tailed Mann-Whitney U-tests).

Figure 4.4b plots the *intrinsic value* for the three degrees of conflict of interest. As depicted in BFH (2014), individual decision makers assign higher *IVs* the lower the degree of conflict. Although the positive trend depicted in the graph is not significant for all specifications, the numbers are very similar to BFH (2014). However, *smooth teams* are not affected by the degree of conflict and the trend from high to no conflict is statistically insignificant.[29] More interestingly, these teams display lower *IVs* than individuals for low conflict games (p = 0.022) and no conflict games (p = 0.053), but not for high conflict games (p = 0.546, all two-tailed Mann-Whitney U-tests). The results for *conflict teams* are similar to the previous results on the stake size. These teams report a higher *intrinsic value* for all degrees of conflict compared to individuals. The differences are significant at the 10%-level for no-conflict games, and at the 5%-level for high-conflict games (all two-tailed Mann-Whitney U-tests). Our last result can be summarized accordingly:

Result 3. *Smooth teams value decision rights intrinsically less than individuals, regardless of the stake size and the degree of conflict.*

4.4 HETEROGENEOUS TEAM BEHAVIOR

In the previous section, we showed that teams reporting no issues in the decision-making process value the pure right to decide over outcomes much less than individuals, while teams with at least one member reporting an issue place a higher value on the decision right *per se*. While theses results are insightful, they rely on self-reported values.

In this section, we go one step further and provide evidence that the two types of teams behave differently with respect to more objective measures. As a first measure, we use the ability of team members to agree on a common decision early in the decision process. In particular, we compare agreement rates regarding the very first proposal in the delegation stage. We then have a look at the sequence of events during the decision process and investigate how teams use the communication tool to better coordinate on a common decision. The second measure is based on the plain messages that are exchanged between team partners, grouped into various categories. In particular, we study how the frequency of messages intended to seek the opinion of the team member and the expression of a preference differs between the types of teams and how these

[29]This is true for both the *IV*% and the absolute *IV* at all conventional levels (paired t-tests).

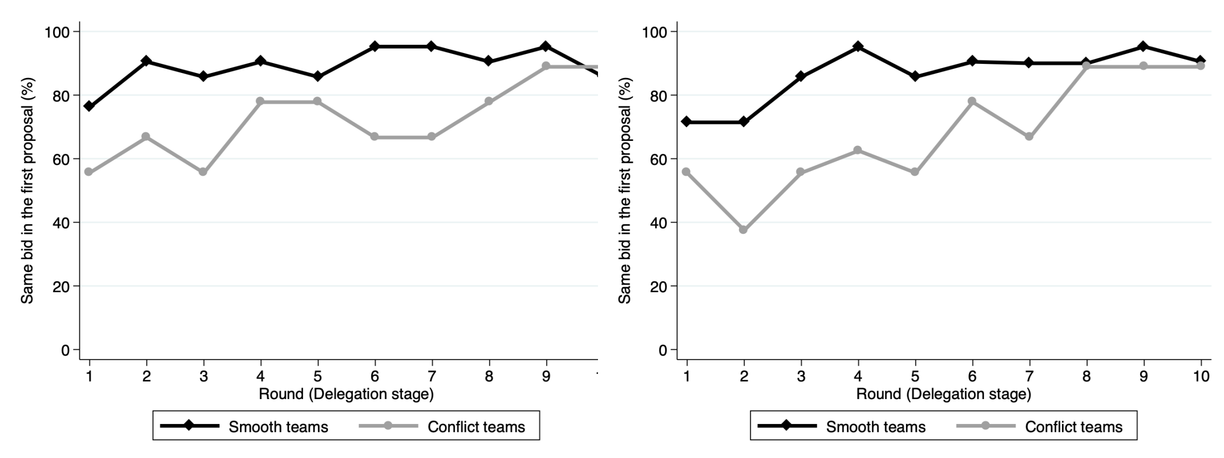
categories affect the average *IV%*. Finally, we compare effort choices of individuals, *smooth teams*, and *conflict teams* with respect to the theoretical optimal effort choice.

4.4.1 COORDINATION ON DECISIONS

We first study how teams differ in their ability to coordinate on decisions in the delegation stage.[30] In particular, we focus on the agreement rate between team members on their very first proposal. Figure 4.5 shows the fraction of initial agreement between team members regarding (a) the chosen minimum effort requirement and (b) the chosen effort. Both graphs suggest that *smooth teams* agreed in the first proposal more often than *conflict teams*. As for the minimum effort requirement, *smooth teams* display an overall agreement rate of 89%, while the rate of *conflict teams* is 72% ($p = 0.059$, two-tailed Mann-Whitney U-test).

The same pattern emerges for the chosen effort. The overall agreement rate is lower for *conflict teams*, 68%, and drops to less than 40% in the second round. The agreement rate in these teams is significantly different from the agreement rate in *smooth teams* with 86.6% ($p = 0.085$, two-tailed Mann-Whitney U-test).

Figure 4.5: Agreement rate in both effort choices



(a) Minimum effort requirement (b) Effort

Notes: The graphs display the agreement rates between team members for the first proposal regarding (a) the minimum effort requirement and (b) the chosen effort for both types of teams for the 10 rounds in the delegation stage.

To better understand why the agreement rate is lower for *conflict teams*, we have a closer look at the sequence of events taking place in the delegation game. In particular,

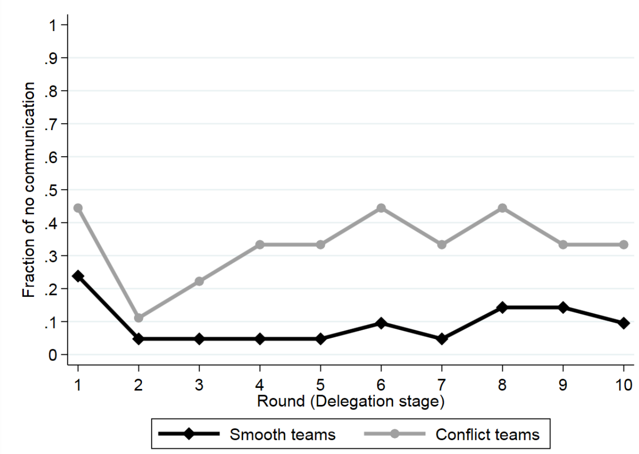
[30] In any round, team members entered their decisions (minimum effort requirement and effort level) before seeing the summary screen displaying both team members' choices. Team members could communicate through free-text chat at any time during a round.

we investigate whether team members use the free-text chat tool to agree on a common decision before making it. We investigate this pattern, because not using the free-text chat from the beginning could create a conflict when team members have to adjust their individual decisions to come up with common decisions in the end. Recall that in the delegation stage, team members make three sequential decisions: the minimum effort requirement, the project alternatives and the effort level in case they retain the decision right. At any time in a given round, they can freely exchange chat messages. Once each member has individually selected her choices, a summary displays the decisions of both members with the possibility to revise their decisions and to agree on common decisions.

Figure 4.6 displays the fraction of team members making an entry on the minimum effort requirement *before* chatting with the other team member over rounds. We use the data for the mimimum effort requirement as the first event within each round. This allows us to have a clean order of events because for both other decisions we are not in the position to clearly seperate the order of events (communication and decisions). Over all rounds, members of *conflict teams* are more likely to make a decision individually before chatting with the other team member than members of *smooth teams* (33.3% vs. 9.52%, $p = 0.027$, two-tailed Mann-Whitney U-test).

We interpret this result as evidence that members of *conflict teams* are less cooperative than members of *smooth teams* in finding an agreement. They tend to agree ex-post, while members of *smooth teams* are more likely to agree on a common decision ex-ante.

Figure 4.6: Fraction of no communication before a decision



Notes: Fraction of team members not chatting with the other member before making the first decision on the minimum effort requirement in the delegation stage.

4.4.2 CHAT ANALYSIS

In total, 4,701 text messages were coded into 11 categories. Table 4.2 provides a description of each category, the agreement rate between the coders for each category, Cohen's Kappa statistics and the frequency of messages in each category.[31] Coding was performed by two student helpers independently. The agreement rate varies from 67.37% for the category "Preference statement" to 99.81% for the category "Time stress". The level of agreement within each category can be tested against chance agreement and tests show that agreement was substantial in each category (Landis and Koch, 1977; all κ's statistics $p < 0.010$). Finally, one researcher of our team read through all coding inconsistencies and solved the disagreements.

Table 4.2: Description of coding categories

#	Category	Definition	Agreement rate	Cohen's Kappa	Total frequency
1	Preference statement	Statements which contain a preference statement	67.37 %	0.39	44.46 %
2	No preference	Statements that a participant has no preference at all or just follows the team member	98.14 %	0.45	0.70 %
3	Asking partner	Statements/Questions which indicate that a team member wants to get some knowledge about the other team member's preferences/opinions	95.38 %	0.88	27.46 %
4	Compare outside option	Statements where participants refer to the payment if the project is not successful (delegation part) or make statements w.r.t. the certainty equivalent (lottery stage)	96.87 %	0.17	0.91 %
5	Comparison with role B	Statements where participants compare the team members' payoff with the payoff of Role B	98.32 %	0.43	0.77 %
6	Time stress	Communication where one team member wants to put some time pressure on the team partner	99.81 %	0.47	0.15 %
7	Mentioning risk	Communication where team members explicitly talk about risk	99.49 %	0.77	1.06 %
8	Group thinking	Messages which explicitly refer to team decision (compromise, we as a team, ...)	99.60 %	0.34	0.30 %
9	Problem with rules	Messages which indicate that rules were not 100 % clear or people ask the partner for clarification of a point	98.49 %	0.20	0.49 %
10	Communication not optimal	Messages which directly indicate that the communication between both team members is not optimal	99.79 %	0.44	0.11 %
11	Residual category	Statements which does not relate to any of the other ten categories	-	-	47.10 %

Notes: Message frequencies and Cohen's Kappa (separately evaluated for each category) as a measure of reliability.

[31]Cohen's κ is a statistical test that measures agreement between two independent raters for categorical items.

In what follows, we focus on the dummy categories “Asking partner”(category 3) and “Preference statement” (category 1). The former takes the value one whenever one member seeks the opinion of the partner regarding the decision at hand and zero otherwise, while the latter takes the value one whenever one of the members expresses her preferences regarding the decision and zero otherwise.

We focus on these two variables because they respectively account for 27.5% and 44.5% of all exchanged messages. These categories are also good proxies for the quality of the decision-making process within a team as they measure how often team members seek the opinion of the teammate and how often a team member expresses her preferences.

Columns (1)-(3) in Table 4.3 provide insights on the frequency to which members seek the opinion of the other team member. While members in *smooth teams* seem to be more likely to seek the opinion of their teammate, none of the differences is significant. Columns (4)-(6) in Table 4.3 provide OLS regressions of the average *IV%* on the variable “Asking partner”. The table provides some evidence that seeking the opinion of the other team member is associated with lower *IVs* in the delegation stage (column 5, $p = 0.027$), but not in the lottery stage (column 6), nor overall (column 4).

The category “Preference statement” is also associated with lower *IVs* but only in the delegation stage (Table 4.4, column 5, $p = 0.032$), not in the lottery stage (column 6) nor overall (column 4).

Table 4.3: OLS regressions for the category "Asking Partner"

	Asking partner			Average *IV*%		
	(1)	(2)	(3)	(4)	(5)	(6)
Smooth team	0.092	0.467	-0.014			
	(0.278)	(0.548)	(0.177)			
Asking partner				-0.009	-0.014**	-0.013
				(0.006)	(0.006)	(0.014)
Constant	1.540***	2.275***	1.050***	0.180***	0.214***	0.173***
	(0.236)	(0.468)	(0.146)	(0.039)	(0.049)	(0.039)
Stage	Pooled	Delegation	Lottery	Pooled	Delegation	Lottery
R^2	0.001	0.009	0.000	0.005	0.017	0.004
Clusters	30	30	30	30	30	30
Observations	798	289	509	798	289	509

Notes: The table reports OLS regressions for the category "Asking partner". The dependent variable in columns (1)-(3) and independent variable in columns (4)-(6), "Asking partner", indicates how often team members made a statement in line with the category "Asking partner". *Smooth team* is a dummy variable and takes the value one if no member within a team reports any issues and zero otherwise. Average *IV*% is the average *IV*% per group. Levels of significance: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table 4.4: OLS regressions for the category "Preference statement"

	Preference statement			Average *IV*%		
	(1)	(2)	(3)	(4)	(5)	(6)
Smooth team	0.129	0.625	0.008			
	(0.358)	(0.737)	(0.166)			
Preference statement				-0.005	-0.011**	-0.001
				(0.004)	(0.005)	(0.012)
Constant	2.475***	3.600***	1.725***	0.178***	0.222***	0.160***
	(0.306)	(0.629)	(0.129)	(0.036)	(0.054)	(0.030)
Stage	Pooled	Delegation	Lottery	Pooled	Delegation	Lottery
R^2	0.001	0.009	0.000	0.003	0.018	0.000
Clusters	30	30	30	30	30	30
Observations	798	289	509	798	289	509

Notes: The table reports OLS regressions for the category "Preference statement". The dependent variable in columns (1)-(3) and independent variable in columns (4)-(6), "Preference statement", indicates how often team members made a statement in line with the category "Preference statement". *Smooth team* is a dummy variable and takes the value one if no member within a team reports any issues and zero otherwise. Average *IV*% is the average *IV*% per group. Levels of significance: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

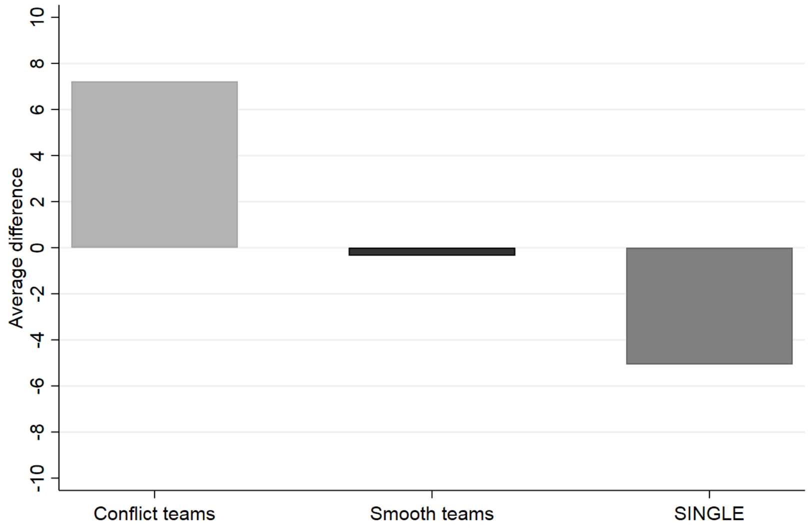
4.4.3 OPTIMAL EFFORT CHOICES

In this section, we investigate effort choices of teams and individuals and compare them with optimal levels. Assuming risk-neutral principals over the payments described in Table 4.1 and quadratic effort costs ke^2, there exists an unique level of effort, e^*, that maximizes the principal's expected utility in each situation.

Figure 4.7 displays the average differences between the principals' chosen effort and the optimal effort for individual decision makers in *SINGLE* and both team types. A positive (negative) difference hence means exerting too much (little) effort. The graph shows that *conflict teams* over-provide effort (+7.22, p = 0.110, Wilcoxon signed-rank test), *smooth teams* are close to efficiency (−0.35, p = 0.945; Wilcoxon signed-rank test), and *single* individuals under-provide effort (−5.07, p = 0.051, Wilcoxon signed-rank test).[32]

[32] Single decision makers in BFH (2014) also under-provide effort and to a similar extent (−5.98, $p < 0.001$, Wilcoxon signed-rank test).

Figure 4.7: Optimal versus actual effort choice by decision makers



Notes: Average differences between the principals chosen effort (e^P) and optimal effort (e^*) for *conflict teams*, *smooth teams* and individual decision makers.

The mismatch between the chosen effort and optimal effort of *conflict teams* is significantly different from the mismatch of individuals in *SINGLE* ($p = 0.014$) and *smooth teams* ($p = 0.046$). Interestingly, *smooth teams* and *single* individuals do not differ ($p = 0.303$, all two-tailed Mann-Whitney U-tests).

4.5 CONCLUSION

Based on previous research showing that individuals do value decision rights *per se* and not solely according to the fundamental value of the underlying decision (see, e.g., Fehr et al., 2013; Bartling et al., 2014; Neri and Rommeswinkel, 2017), this paper addresses the questions whether teams value decision rights *intrinsically* and how their valuation compares to the valuation of individuals. Extending BFH's (2014) design, we replicate their individual decision-making treatment (*SINGLE*) and introduce a team decision-making treatment (*TEAM*) in which teams of two subjects take decisions unanimously.

Our experiment yields insightful results. To begin with, our individual decision-making treatment *SINGLE* is a fairly accurate replication of BFH's (2014) individual decision-making condition. In particular, we also find that principals display a strictly positive *intrinsic value*, regardless of the stake size and the degree of conflict between the principal and the agent. Remarkably, the average *intrinsic value* of decision rights is 17.0% in our *SINGLE* treatment and thus very similar to the value reported in BFH (2014), namely 16.7%. In addition, our data provide two new results.

First, an overall comparison of team and indvidual decision-making yields no difference in the *intrinsic valuation* of decision rights. In both treatments, principals value being in control about 17% more than delegating the decision right to an agent. This holds regardless of the size of the payoffs at stake or the degree of conflict between the principal and the agent.

Second, our data reveal that the decision-making process within a team matters. Remember that teammates have to coordinate on a common decision which might lead to some issues. For instance, teammates could either use the free-text chat to coordinate on a common decision before they individually enter their decisions or they could first enter their decisions individually and then adjust their choices to come up with a common decision. To account for potential issues within teams, we asked team members at the end of the experiment whether they had issues with the decision-making process or not. This self-reported impressions give us the opportunity to account for heterogeneity in treatment *TEAM*, and thus we divide teams into teams where none of the team members reported an issue (*smooth teams*) and *conflict teams* where at least one member reported an issue. In addition, we provide more objective measures, e.g., how they use communication, to show differences between both team types. This categorization leads to insightful results. *Smooth teams* value decision rights much less *intrinsically* than individuals with 8.6% and 17.0%, respectively. Further, *smooth teams* do not display a significant positive *intrinsic value* in 4 out of 10 situations, and thus behave partly in line with the benchmark of a fully rational decision maker. Moreover, in 6 out of 10 games *smooth teams* have a significantly lower *IV*% than individuals, and are less affected by stake sizes and conflicts of interest. In contrast, *conflict teams* seem to have a very high *intrinsic valuation* of decision rights.

Our results are novel in several ways. To begin with, we are the first to study how groups of individuals value decision rights and power *per se*, and how this value compares to the value of individuals. This is rather surprising given that many decisions in organizations are made at the group level. Also, to the best of knowledge, we are the first to compare teams and individuals in the context of delegation. This is relevant since most applications of theoretical considerations (see, e.g., Grossman and Hart, 1986; Hart and Moore, 1990) focus on the *instrumental value* of decision rights, not on the *intrinsic value*. To conclude, our findings partly provide another rationale for team decision-making in organizations, since well-functioning teams are less affected by psychic benefits of decision rights than individuals. In general, our results provide some insights for further research on team and individual decision-making.

4.6 APPENDIX

4.6.1 ADDITIONAL TABLES

Table 4.5: IV%: *TEAM* vs. *SINGLE* for each of the 10 games

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TEAM	-0.031	0.085	0.130	-0.031	0.098	0.011	-0.133*	-0.024	-0.034	-0.009
	(0.057)	(0.069)	(0.091)	(0.060)	(0.133)	(0.072)	(0.072)	(0.076)	(0.049)	(0.093)
Constant	0.198***	0.087**	0.145***	0.134***	0.142***	0.198***	0.203***	0.206***	0.145***	0.243***
	(0.037)	(0.035)	(0.039)	(0.039)	(0.046)	(0.038)	(0.055)	(0.070)	(0.032)	(0.057)
R^2	0.003	0.019	0.029	0.003	0.009	0.000	0.028	0.001	0.005	0.000
Observations	90	90	90	90	90	90	90	90	90	90

Notes: OLS regressions. The columns (1)-(10) represent the games (1-10) shown in Table 4.1. The dependent variable is IV%. TEAM is a dummy which takes the value 1 if the principal is from the team treatment and 0 otherwise. The reference group is *SINGLE*. Levels of significance: $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 4.6: IV%: *Smooth teams* vs. *SINGLE* for each of the 10 games

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Smooth team	-0.099*	-0.007	0.032	-0.110**	-0.129*	-0.099*	-0.186***	-0.047	-0.073*	-0.123
	(0.054)	(0.063)	(0.059)	(0.052)	(0.074)	(0.054)	(0.068)	(0.079)	(0.043)	(0.082)
Constant	0.198***	0.087**	0.145***	0.134***	0.142***	0.198***	0.203***	0.206***	0.145***	0.243***
	(0.037)	(0.035)	(0.039)	(0.039)	(0.046)	(0.038)	(0.055)	(0.070)	(0.032)	(0.057)
R^2	0.027	0.000	0.003	0.030	0.028	0.026	0.045	0.002	0.021	0.018
Observations	81	81	81	81	81	81	81	81	81	81

Notes: OLS regressions. The columns (1)-(10) represent the games (1-10) shown in Table 4.1. The dependent variable is IV%. *Smooth team* is a dummy for teams without issues. The reference group is *SINGLE*. Levels of significance: $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 4.7: IV%: *Conflict teams* vs. *SINGLE* for each of the 10 games

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Conflict team	0.126	0.300**	0.358	0.151	0.629*	0.267*	-0.009	0.031	0.059	0.256
	(0.096)	(0.131)	(0.241)	(0.112)	(0.336)	(0.157)	(0.124)	(0.079)	(0.105)	(0.182)
Constant	0.198***	0.087**	0.145***	0.134***	0.142***	0.198***	0.203***	0.206***	0.145***	0.243***
	(0.037)	(0.035)	(0.039)	(0.040)	(0.046)	(0.039)	(0.055)	(0.071)	(0.032)	(0.057)
R^2	0.022	0.111	0.093	0.028	0.161	0.074	0.000	0.000	0.006	0.036
Observations	69	69	69	69	69	69	69	69	69	69

Notes: OLS regressions. The columns (1)-(10) represent the games (1-10) shown in Table 4.1. The dependent variable is IV%. *Conflict team* is a dummy for teams with issues. The reference group is *SINGLE*. Levels of significance: $*p < 0.1, **p < 0.05, ***p < 0.01$.

4.6.2 ROBUSTNESS CHECK: PROXY FOR THE IV

Throughout the paper, the *intrinsic value* of a decision right is defined by equation (4.2.3). Although this is a neat measure, it is obtained in the second part (lottery part) and might reflect potential issues in teams that arose in the first part (delegation part). To check for potential issues between the first and the second part of the experiment, we can use a proxy for the *IV*, as suggested by BFH (2014), which is generated in the delegation part. The authors show that the expected monetary payoffs of the control and delegation lotteries from the first part could be used to obtain a good and valid proxy for the *IV*. Therefore it is important that the expected monetary payoffs of the lotteries from the delegation part are highly correlated with the certainty equivalents from the lottery part. In the orginal paper, Bartling and co-authors show that the expected monetary payoffs of the lotteries are indeed highly correlated with the certainty equivalents (Pearson's coefficient of correlation $\rho = 0.887$, $p < 0.001$).

Our data reveal that the expected monetary payoffs of the lotteries are also highly correlated with the certainty equivalents (*SINGLE*: $\rho = 0.878$, $p < 0.001$ and *TEAM*: $\rho = 0.930$, $p < 0.001$). This result still holds when we account for frictions in the decision-making prosess ($\rho = 0.965$ in *smooth teams* and $\rho = 0.865$ in *conflict teams*; both $p < 0.001$). In a second step, we investigate if the difference in expected monetary values (delegation minus control) is positively correlated with the *IV* of a principal (in BFH (2014): $\rho = 0.58$, $p < 0.001$). In our sample, the correlation coefficients for *SINGLE*, *smooth teams* and *conflict teams* are $\rho = 0.578$, $\rho = 0.779$, and $\rho = 0.351$, respectively (all correlations: $p < 0.001$), with the coefficient of correlation for *conflict teams* being of lower magnitude. Finally, we can measure the correlation between the *IV%* and the difference in expected payoffs normalized by the expected payoff of the control lottery (i.e., the percentage difference). The data show that the correlation coefficients for *SINGLE* and *smooth teams* are $\rho = 0.410$ and $\rho = 0.729$ (both $p < 0.001$). However, the coefficient of correlation for *conflict teams* is $\rho = -0.132$ and is not significant ($p = 0.216$). This analysis suggests that the difference in expected payoffs in the first part is a good proxy for the *IV* in treatment *SINGLE* and for teams without issues (*smooth teams*), but is not a good proxy for teams reporting an issue (*conflict teams*).

This is additional evidence that teams reporting an issue behave differently from teams reporting no issues.

Instructions - Part 1

Instructions for role A[33]

Welcome to today's experiment. You receive 4 Euros as a show-up fee. During the experiment you will be asked to make decisions and so will the other participants. Your decisions, as well as the decisions of the other participants, will determine your monetary payoff. Therefore, it is important that you **carefully** read the instructions. If you have any questions during the experiment, please raise your hand and one of the experimenters will come to assist you.

Please do not communicate with other participants before the end of the experiment. Please switch off your mobile phone. In addition, we want to point out that you are only allowed to use the computer functions that are intended for the course of the study. If you **violate these rules**, you will be immediately **excluded from the experiment and all payments**. At the end of the experiment we will pay you in cash €4 as a show-up fee and the money you've earned during the experiment. Your income in the experiment will be in "points". At the end of the experiment, points will be converted into euro at an exchange rate of

100 Points = 2 Euros

This study consists of three parts:

1. The first part of the study lasts 10 rounds. (In each of the 10 rounds you are paired with another participant (always the same person). Together you represent **role A**. You will be called **team members A1 and A2**. **You learn at the beginning of the experiment if you are A1 or A2**.) You will be paired with a different role B in each of the 10 rounds of the study. You can complete a project with the role B who is paired with you in each round. You will find detailed explanations about this first part of the study on the following pages.

2. You will receive exact instructions about the second part as soon as the first part is completed.

3. The third part of the study is very short, and you will receive instructions on screen as soon as the second part is completed.

[33]Modification for treatment TEAM in red and in parenthesis.

General information about the first part of the study

There are two types of roles in the first part of the study: role A and role B. **You are (part of) role A.** (Role A consists of two fixed matched participants. Role B consists of a single person. Role B knows that your role consists of two team members.)

There are ten rounds. You (Role A (you and your team partner)) will be paired with a different role B in each round. A **project** can be completed in each round. A successful completion of the project will lead to a positive payment for roles A and B.

The decision right

In each period, either you (and your team partner) (role A) or role B has the *decision right*. The role with the decision right can make two decisions:

1. **Which alternative of the project - A or B - will be completed?**
   Role A receives the larger share of the project income in alternative A, and role B receives the larger share of the project income in alternative B. (It is possible that roles A and B will receive the same amounts in some rounds.)

2. **What is the probability that the project will be successful?**
   The determination of the probability of success is associated with costs for the role with the decision right. The higher the probability of success, the higher the costs.

Payment of the project

The payments that result from completion of the project vary from round to round. You will be informed of the payments at the beginning of each round.

Example: The payments from the project in a round: In case of success, you (role A (your team)) will receive 200 points (per person) in alternative A and role B will receive 150 points. Role B will reiceve 200 points in case of success in alternative B and you (role A (your team)) will recieve 150 points (per person). If the project is unsuccessful, role A (your team) receives 100 points (per person) and role B receives 100 points.

		Payment to role A (per person)	Payment to role B
Successful completion	**Alternative A**	200	150
	Alternative B	150	200
If unsuccessful		100	100

The probability of success

If you, role A, have the decision right, then you can determine the probability with which the chosen project alternative – A or B – will be successful.

How is the probability of success determined?

The prbability of success is a number between 0 and 100 that can be chosen freely.

$$\boxed{\mathbf{0 \leq probability\ of\ success \leq 100}}$$

A probability of success of 0 means that the project will never be successful. A probability of success of 100 means that the project will always be successful. For all values in between, a project might be successful or it might not be so. A value of 50 means that a project has a 50% chance of being successful.

If the project is successful, roles A and B will be paid out in accordance to the chosen alternative (in the example above, 150 or 200 points). If the project is not successful, both participants will receive a lower payment independent of the chosen alternative (in the example above, 100 points each).

The costs of the choice of the probability of success

The higher the probability of success you choose, the higher are your costs. Two information sheets (yellow and pink) are at your desk; they show you both in a table and in a graph how high the costs are for the various possible probabilities of success. You will be informed in each round whether the costs on the yellow or on the pink sheet apply. You can also always have the computer show you the costs on the monitor while choosing the probability of success.

A roll of the dice determines whether the project is successful

The role with the decision right can roll two dice at his or her desk – they are red and white (and show the numbers from 0 to 9). The red die determines the first digit and the white the second digit. This results in a number between 1 and 100 (two zeros are valued as 100). If the number rolled is **smaller than or equal to** the chosen probability of success, the project is successful. If the number rolled is larger, the project is not successful. **The greater the probability of success that you choose, the greater the possibility that the number rolled is smaller than the chosen number, i.e., that the project is successful.**

Examples:

1. **Example:** You choose **a probability of success of 15**, i.e., 15%

   This means the following:

   - If – through rolling the red and the white dice – a smaller or equal number results, i.e. a number between 1 and 15 (= 15 of 100 possibilities), the project is successful.
   - If the number is greater than 15 (= 16 to 100, or 85 possibilities) results, then the project is not successful.

2. **Example:** You choose **80 as the probability of success**, i.e., a probability of success of 80%.

   This means the following:

   - If – through rolling the red and the white dice – a number between 1 and 80 (= 80 of 100 possibilities), the project is successful.
   - If the number is greater than 80 (= 81 to 100, or 20 possibilities) results, then the project is not successful.

- **Assume that you roll the number 9 with the red die and a 3 with the white one. This results in the number 93.**

  In this case, neither example would have been successful (the number rolled is, in both cases, larger than the chosen probability of success).

- **Assume that you roll the number 5 with the red die and a 4 with the white one. This results in the number 54.**

  In this case, the project in the first example would not have been successful (the number rolled is larger than 15), but the project in the second example would have been successful (the number rolled is less than 80).

- **Assume that you roll the number 0 with the red die and a 3 with the white one. This results in the number 03.**

  In this case, the project would have been successful in both examples (the number rolled is less than the probability of success chosen in each example).

The income

The incomes of roles A and B consist of the following two parts:

- Payment from the chosen project alternative if the project is successful. If the project is not successful, both roles receive a lower payment that is independent of the project alternative.
- The costs for the probability of success will be deducted from the corresponding payment for the role with the decision right.

The following four possibilites thus result for you (as a part of role A):

1. You (Your team) have (has) the **decision right** and the project is **successful**:

   Income = payment from the project alternative you (your team) chose (per person) - costs for the choice of the probability of success (costs are per person)

2. You (Your team) have (has) the **decision right** and the project is **not successful**:

   Income = payment in case of lack of success (per person) - costs for the choice of the probability of success (costs are per person)

3. You (Your team) **do (does) not have the decision right** and the project is **successful**:

   Income = payment from the project alternative (per person) role B chose

4. You (Your team) **do (does) not have the decision right** and the project is **not successful**:

   Income = payment in case of lack of success (per person)

Detailed procedure for a round on the computer

1st stage: role B's decision

In each round, you as role A first have the decision right. You can also delegate the decision right to role B. Before you decide whether you would like to delegate the decision right to role B, role B determines in a binding manner – for the case that the decision right is delegated to her – which project alternative and which probability of success that she would like to select. If you actually delegate the decision right to role B, then the decisions role B makes in the first stage will be realized.

You (and your team partner (role A)) will not yet learn which decisions participant B makes in the first stage.

The team decision (just for TEAM treatment)

In each of the 10 round you and your team partner (together you represent role A) could communicate with each other. That means that **team members A1 and A2** could talk within the rounds about their decisions and could exchange preferences. At the end of each round, role A needs to take decisions. **A decision is valid, if and only if the decisions of A1 and A2 coincide.** So you have to agree on a common decision. Therefore, you can use the *chat which is only visible to role A (A1 and A2).* The chat is visible and available in all subsections of a round in the lower part of the screen. You can send a message if you write a text in the blue line and press the "Enter" button. You and your partner can see the message immediately afterwards. **Both team members A1 and A2 see the same screen but have to enter the decisions individually.** At the end of each round both team members see a summary screen with all their decisions. You can still adapt/change your decisions at this stage. **It is important that both team members entered the same decisions.**

2nd stage: Who has the decision right?

You (and your team partner) can decide in each round – after role B has made her decisions – whether you would like to delegate the decision right to role B or if you would like to retain this for yourself. In this case, you (and your team partner) do not make the decision directly, but by **determining a minimum requirement:**

In each round, you (and your team partner) can determine the minimum probability of success that role B must have chosen for you to be willing to delegate the decision right to her. You can choose any minimum requirement between 1 and 100.

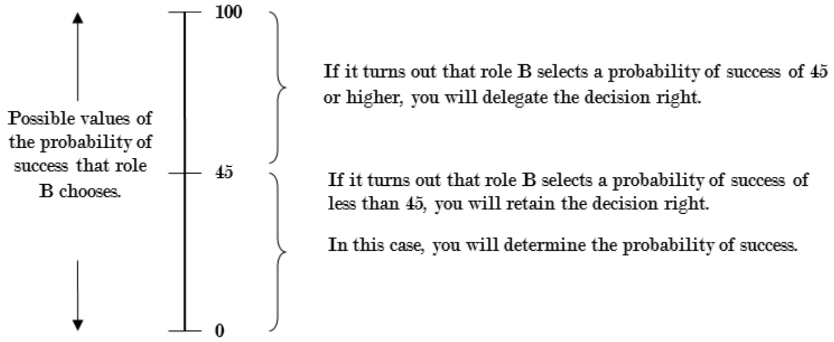
Role B has already chosen her probability of success at the time you (and your team partner) determine a minimum requirement. You thus have no opportunity at all to influence the decision that participant B has already made.

Please take note that you (and your team partner) do not know the probability of success that role B chose when you determine your minimum requirement.

If the probability of success that role B chooses is greater than the minimum requirement that you (your team) determine (determines), you will delegate the decision right. If the probability of success that role B determines is less than your minimum requirement, you (and your team partner) will retain the decision right.

The graph below clarifies the connection between the minimum requirement you (and your team partner) determine, the probability of success that role B chooses, and the question of who has the decision right?

If, for example, you (and your team partner) decide on a minimum requirement of 45, this means that you would then like to delegate the decision right to role B if she selects a probability of success of 45 or greater.



When you (and your team partner) are considering your minimum requirement, you should ask the following:

- Would I (we) want to delegate the decision right if role B selected a probability of success of 1? If not, you should then ask:
- Would I (we) want to delegate the decision right if role B selected a probability of success of 2? If not, you should then ask:
- Would I (we) want to delegate the decision right if participant B selected a probability of success of 3? And so on.

  Do this until you (your team) reach (reaches) role B's probability of success level, above which you would delegate the decision right. You should determine this as your minimum requirement.

  – In the example above, this is the value of 45. This means that you would just be willing to delegate the decision right if role B selects a probability of success of 45, but that you would prefer retaining this right at all values of 44 or less.

Further examples:

1. You (Your team) select (selects) a **minimum requirement of 78.**

   This means the following:

   - If role B selects a probability of success in stage 1 between 0 and 77, you will not delegate the decision right.
   - If role B selects a probability of success in stage 1 between 78 and 100, you will delegate the decision right to her.

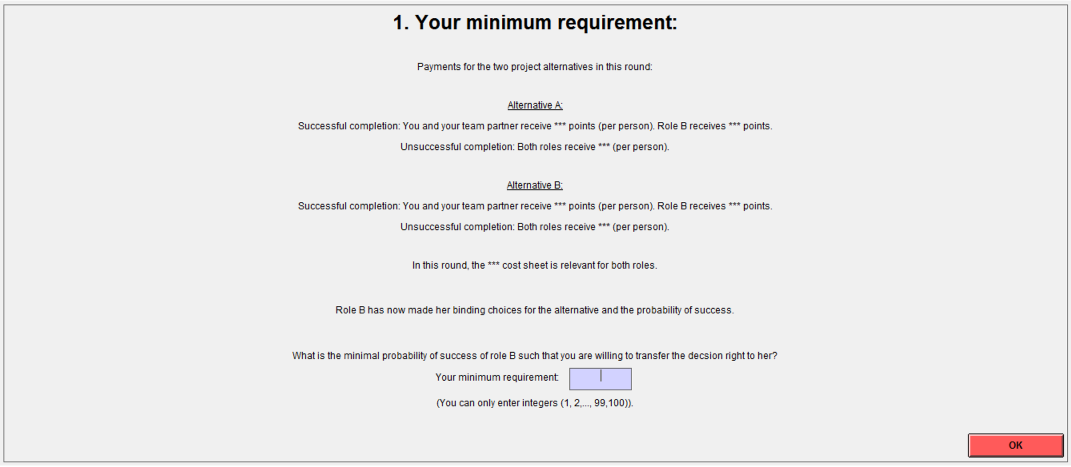
2. You (Your team) select (selects) a **minimum requirement of 4.**

   This means the following:

   - If role B selects a probability of success in stage 1 between 0 and 3, you will not delegate the decision right.
   - If role B selects a probability of success in stage 1 between 4 and 100, you will delegate the decision right to her.

You (and your team partner) make your decision on the minimum requirement for role B on the screen shown below.

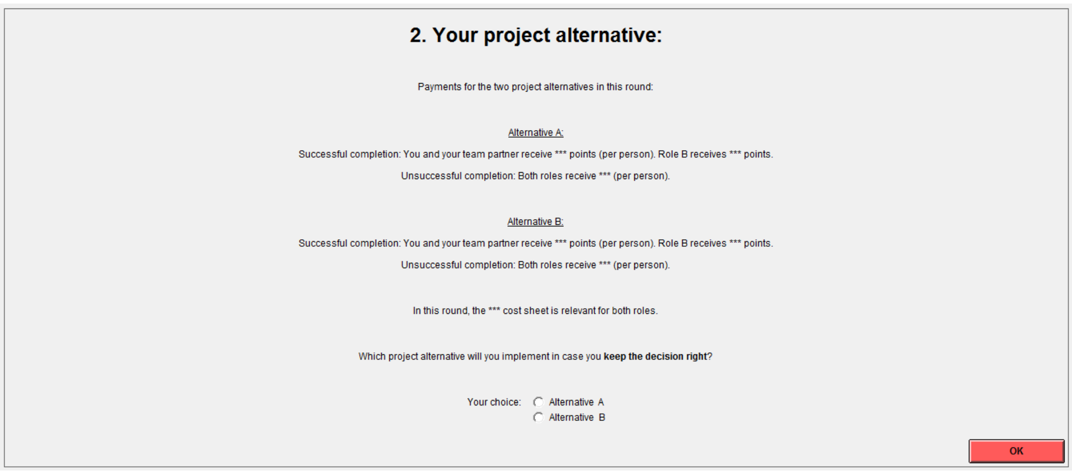
The upper part of the screen always informs you of the payments in the two project alternatives as well as the payment in case of lack of success in the round in question. Furthermore, you will be informed whether the cost schedule on the yellow or the pink information sheet applies. In the middle part of the screen you can enter your minimum requirement. The lower part of the screen contains the chat. Here you can chat with your team partner about your decisions. Here is an example (screenshots for TEAM treatment without chat part):



After you have entered your minimum requirement, please click on the OK button to move to the next stage.

3rd stage: Determination of the project alternative

At the time of the selection of the project alternatives, you (and your team partner) do not yet know whether the probability of success that role B selects is at least as high as your minimum requirement or not. You (and your team partner) therefore do not know whether you delegate the decision right or not. For this reason, you (and your team partner) must select the project alternative that you (and your team partner) would like to realize in case you (your team) retain (retains) the decision right. The selection of the project alternative is made on the following screen:

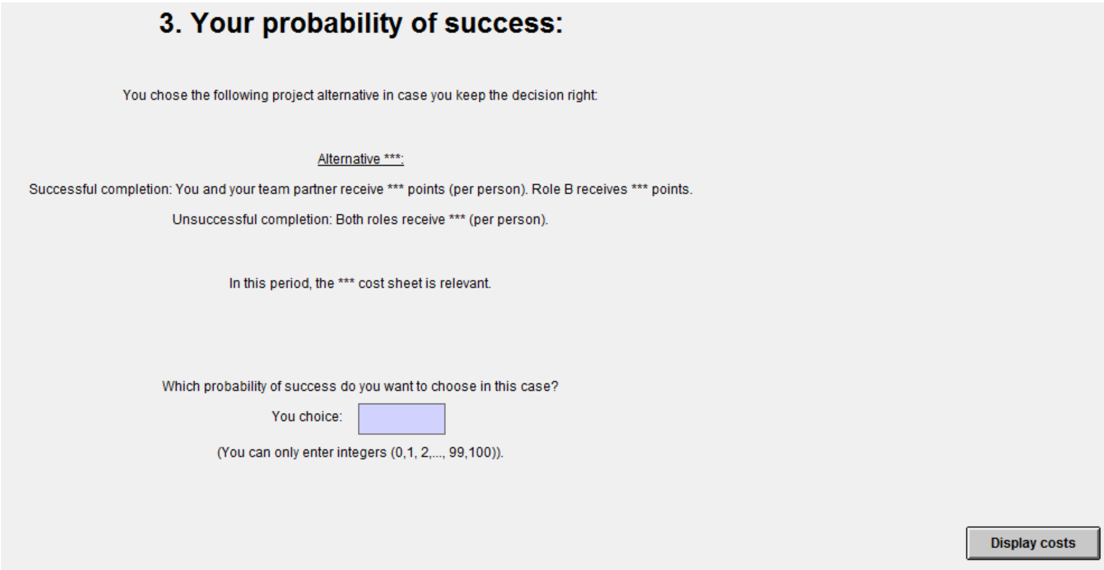


Again, you can use the chat in the lower part of the screen to make common decisions.

After you have chosen the project alternative, please click on the OK button.

4^{th} stage: Selection of the probability of success

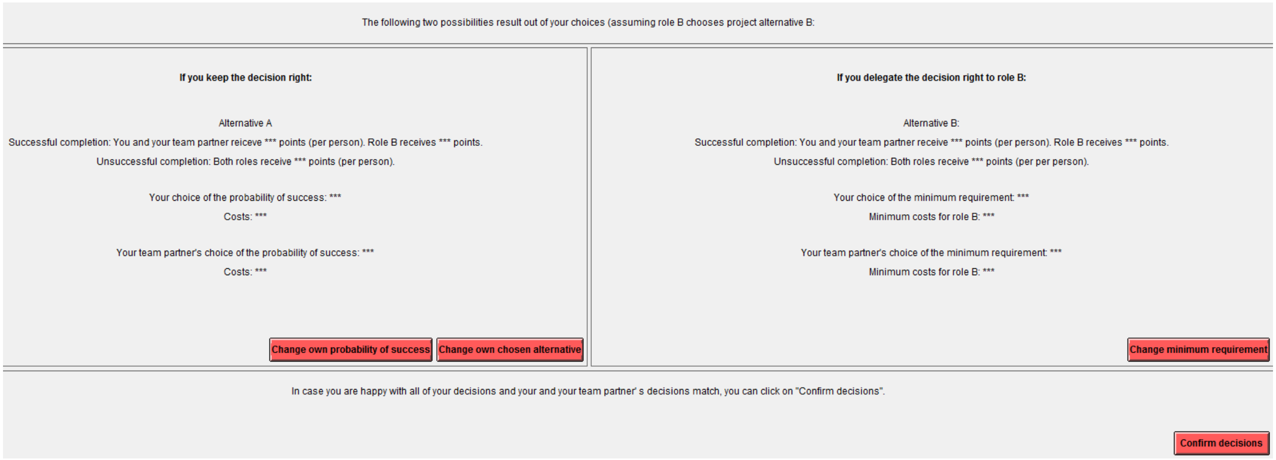
Furthermore, you (and your team partner) do not know at the time of your selection of the probability of success whether the decision right will be delegated or if you will retain it. You (and your team partner) must therefore select the probability of success in case you (your team) retain (retains) the decision right. The costs of the probability of success will only be incurred, however, if you (your team) ultimately retain (retains) the decision right. You make your choice of the probability of success on the screen shown on the next page:



After you have chosen the probability of success, click on the “display costs” button. This will then show the exact costs of the probability of success that you chose. You can then change your probability of success if you wish. You make your final decision with “confirm”.

5^{th} stage: Round summary

At the end of each round you receive a summary screen of your decision.



On the left side, you can see your and your team partner's decisions in case you keep the decision right. On the right side, you can see the values if you delegate the decision right to role B. At this stage, you can make changes in your project alternatives, the probability of success and the minimum requirement.

As soon as one of the members of role A (you or your team partner) clicks on "confirm decisions" then it is the final decision and you are not allowed to change anything. **If your decisions don't match the decisions of your team partner it is not possible to confirm your decisions. This means that you and your team partner have to enter the same decisions everywhere to be able to start with the next round.**

In case you confirmed your decisions, you get to the next round.

6^{th} stage: Determination of the project success At the end of the study, the computer will randomly determine one of the ten rounds, and the payment that determines your income from this part of the study will be decided for this round based on your (and your partner's) decision and that of the role B assigned to you in this period. As you do not know which round the computer will randomly determine, you (and your team partner) should consider your decisions in each round very carefully.

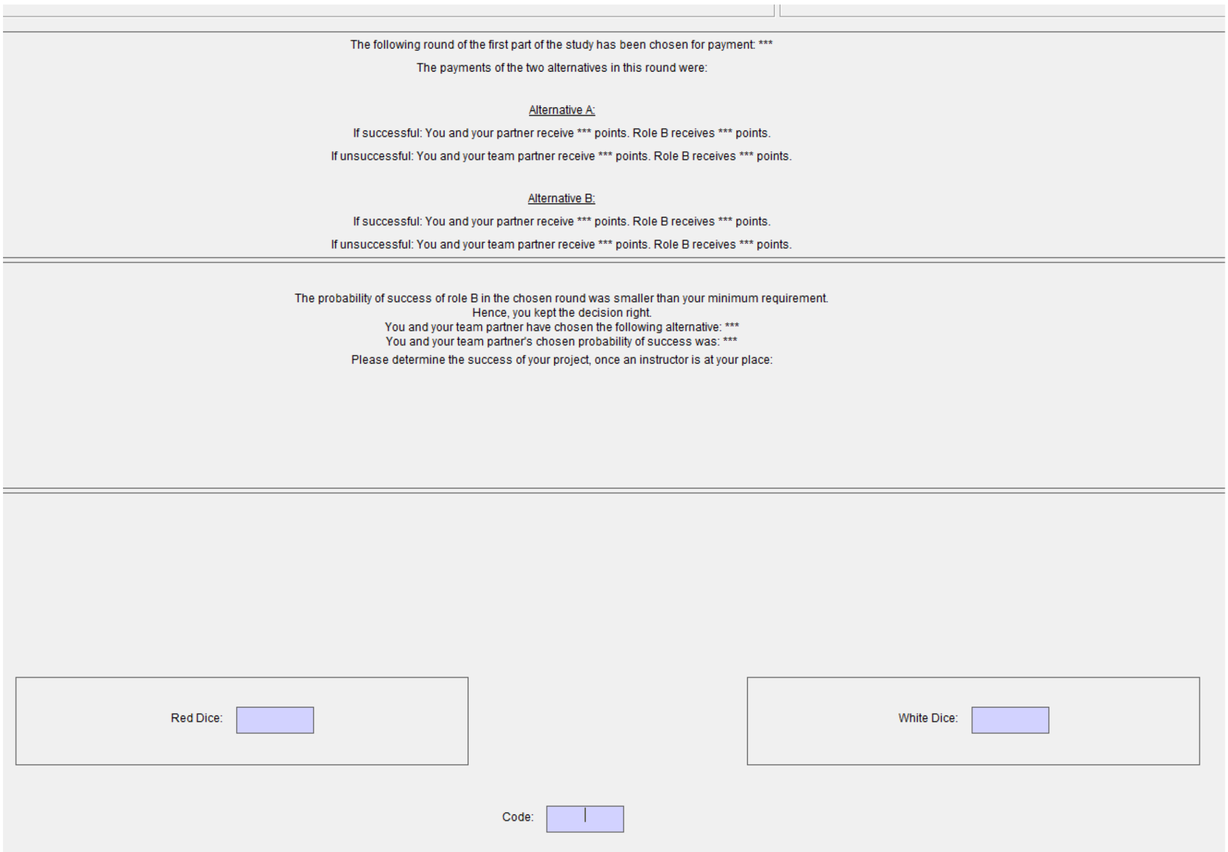
a) The computer will first randomly determine which round will be selected for payment.

b) It will then examine whether the role B randomly assigned to you in this round chose a probability of success that is at least as large as your minimum requirement.

- If the minimum requirement is fulfilled, you (your team) will delegate the decision right.

- If the minimum requirement is not fulfilled, you (your team) will retain the decision right.

If you retain (your team retains) the decision right, you (or your team partner) can determine the project success yourself by rolling the dice. You will do this at your desk, under supervision of the head of the study. You can decide within your team who should roll the dice. The result is entered on the following screen:



You can roll the dice yourself, but the entry of the result and the code (necessary in order to press the "continue" button) must be done by the head of the study.

Do you have questions about the first part of the study? Please raise your hand. We will come to your desk. If you do not have any questions, **please answer the control questions on the next pages. You have to enter the correct answers on the screen later.**

Test questions
Role A and Part 1

Please answer the following test questions. Please contact the head of the study if you have any questions.

1. Assume you (your team) determined a minimum requirement of 85.
   (a) If role B selects a probability of success of 80, who has the decision right in this round? Role ....
   (b) If role B selects a probability of success of 90, who has the decision right in this round? Role ....
2. Assume you (your team) determined a minimum requirement of 55.
   (a) If role B selects a probability of success of 50, who has the decision right in this round? Role ....
   (b) If role B selects a probability of success of 60, who has the decision right in this round? Role ....
3. Assume role B chose a probability of success of 3.
   (a) If you (your team) specify (specifies) a minimum requirement of 1, who has the decision right in this round? Role ....
   (b) What is the probability that the project will then be successful? ....
   (c) If, however, you (your team) specify (specifies) a minimum requirement of 4, who has the decision right in this round? Role ....
4. Assume role B chose a probability of success of 90.
   (a) If you (your team) specify (specifies) a minimum requirement of 85, who has the decision right in this round? Role ....
   (b) What is the probability that the project will then be successful? ....
   (c) If, however, you (your team) specify (specifies) a minimum requirement of 95, who has the decision right in this round? Role ....

5. Assume that you (your team) retained the decision right and chose a probability of success of 54. The cost schedule on yellow information sheet applies in this round. Assume further that you roll an 8 with the red die and a 2 with the white one.

   (a) How high are your costs (per person)? ....

   (b) Would the project have been successful? ....

   The following payments apply for the project:

		Your payment (per person)	Payment to role B
Successful completion	**Alternative A**	200	150
	Alternative B	150	200
If unsuccessful		100	100

   Assume you (your team) chose (chose) project alternative A.

   (a) How high would your income (per person) be? ....

   (b) How high would role B's income be? ....

   Now assume that you (your team) chose (chose) a probability of success of 24. Assume further that you roll a 1 with the red die and a 5 with the white one. The cost schedule on yellow information sheet applies in this round. You (your team) again chose (chose) project alternative A.

   (a) How high are your costs (per person)? ....

   (b) Would the project have been successful? ....

   (c) How high would your income (per person) be? ....

   (d) How high would role B's income be? ....

6. Assume that you (your team) delegated the decision right. Role B selected project alternative B and chose a probability of success of 48. The cost schedule on pink information sheet applies in this round.

The following payments apply for the project:

		Your payment (per person)	Payment to role B
Successful completion	**Alternative A**	200	150
	Alternative B	150	200
If unsuccessful		100	100

Assume further that role B rolls a 5 with the red die and a 7 with the white one.

(a) Would the project have been successful? ....

(b) How high would your income (per person) be? ....

(c) How high would role B's income be? ....

Now assume role B rolls a 3 with the red die and a 9 with the white one.

(a) Would the project have been successful? ....

(b) How high would your income (per person) be? ....

(c) How high would role B's income be? ....

Instructions for role B

Welcome to today's experiment. You receive €4 as a show-up fee. During the experiment you will be asked to make decisions and so will the other participants. Your decisions, as well as the decisions of the other participants, will determine your monetary payoff. Therefore, it is important that you **carefully** read the instructions. If you have any questions during the experiment, please raise your hand and one of the experimenters will come to assist you.

Please do not communicate with other participants before the end of the experiment. Please switch off your mobile phone. In addition, we want to point out that you are only allowed to use the computer functions that are intended for the course of the study. If you **violate these rules**, you will be immediately **excluded from the experiment and all payments**. At the end of the experiment we will pay you in cash €4 as a show-up fee and the money you've earned during the experiment. Your income in the experiment will be in "points". At the end of the experiment, tokens will be converted into euro at an exchange rate of

100 Points = 2 Euros

This study consists of three parts:

1. The first part of the study lasts 10 rounds. You will be paired with a different role A in each of the 10 rounds of the study. You can complete a project with the role A who is paired with you in each round. You will find detailed explanations about this first part of the study on the following pages.

2. You will receive exact instructions about the second part as soon as the first part is completed.

3. The third part of the study is very short, and you will receive instructions on screen as soon as the second part is completed.

General information about the first part of the study

There are two types of roles in the first part of the study: role A (two persons) and role B. **You are role B**.

There are ten rounds. You will be paird with a different role A in each round. A **project** can be completed in each round. A successful competion of the project will lead to a positive payment for roles A and B.

The decision right

In each period, either you or role A has the *decision right*. The role with the decision right can make two decisions:

1. **Which alternative of the project – A or B – will be completed?**
   Role A receives the larger share of the project income in alternative A, and role B receives the larger share of the project income in alternative B. (It is possible that roles A and B will receive the same amounts in some rounds.)

2. **What is the probability that the project will be successful?**
   The determination of the probability of success is associated with costs for the role with the decision right. The higher the probability of success, the higher the costs.

Payment of the project

The payments that result from completion of the project vary from round to round. You will be infromed of the payments at the beginning of each round.

Example: The payments from the project in a round: In case of success, you will receive 200 points in alternative B and role A will receive 150 points. Role A will reiceve 200 points in case of success in alternative A and you will recieve 150 points. If the project is unsuccessful, both role A and role B receive 100 points.

		Payment to role A (per person)	Your payment
Successful completion	**Alternative A**	200	150
	Alternative B	150	200
If unsuccessful		100	100

<u>The probability of success</u>

If you have the decision right, then you can determine the probability with which the chosen project alternative – A or B – will be successful.

How is the probability of success determined?

The prbability of success is a number between 0 and 100 that can be chosen freely.

$$\boxed{\mathbf{0 \leq probability\ of\ success \leq 100}}$$

A probability of success of 0 means that the project will never be successful. A probability of success of 100 means that the project will always be successful. For all values in between, a project might be successful or it might not be so. A value of 50 means that a project has a 50% chance of being successful.

If the project is successful, roles A and B will be paid out in accordance to the chosen alternative (in the example above, 150 or 200 points). If the project is not successful, both participants will receive a lower payment independent of the chosen alternative (in the example above, 100 points each).

The costs of the choice of the probability of success

The higher the probability of success you choose, the higher are your costs. Two information sheets (yellow and pink) are at your desk; they show you both in a table and in a graph how high the costs are for the various possible probabilities of success. You will be informed in each round whether the costs on the yellow or on the pink sheet apply. You can also always have the computer show you the costs on the monitor while choosing the probability of success.

A roll of the dice determines whether the project is successful

The role with the decision right can roll two dice at his or her desk – they are red and white (and show the numbers from 0 to 9). The red die determines the first digit and the white the second digit. This results in a number between 1 and 100 (two zeros are valued as 100). If the number rolled is **smaller than or equal to** the chosen probability of success, the project is successful. If the number rolled is larger, the project is not successful. **The greater the probability of success that you choose, the greater the possibility that the number rolled is smaller than the chosen number, i.e., that the project is successful.**

Examples:

1. **Example:** You choose **a probability of success of 15**, i.e., 15%

   This means the following:

   - If – through rolling the red and the white dice – a smaller or equal number results, i.e. a number between 1 and 15 (= 15 of 100 possibilities), the project is successful.
   - If the number is greater than 15 (= 16 to 100, or 85 possibilities) results, then the project is not successful.

2. **Example:** You choose **80 as the probability of success**, i.e., a probability of success of 80%.

   This means the following:

   - If – through rolling the red and the white dice – a number between 1 and 80 (= 80 of 100 possibilities), the project is successful.
   - If the number is greater than 80 (= 81 to 100, or 20 possibilities) results, then the project is not successful.

- **Assume that you roll the number 9 with the red die and a 3 with the white one. This results in the number 93.**

  In this case, neither example would have been successful (the number rolled is, in both cases, larger than the chosen probability of success).

- **Assume that you roll the number 5 with the red die and a 4 with the white one. This results in the number 54.**

  In this case, the project in the first example would not have been successful (the number rolled is larger than 15), but the project in the second example would have been successful (the number rolled is less than 80).

- **Assume that you roll the number 0 with the red die and a 3 with the white one. This results in the number 03.**

  In this case, the project would have been successful in both examples (the number rolled is less than the probability of success chosen in each example).

The income

The incomes of roles A and B consist of the following two parts:

- Payment from the chosen project alternative if the project is successful. If the project is not successful, both roles receive a lower payment that is independent of the project alternative.
- The costs for the probability of success will be deducted from the corresponding payment for the role with the decision right.

The following four possibilites thus result for you:

1. You have the **decision right** and the project is **successful**:

   Income = payment from the project alternative you chose - costs for the choice of the probability of success

2. You have the **decision right** and the project is **not successful**:

   Income = payment in case of lack of success - costs for the choice of the probability of success

3. You **do not have the decision right** and the project is **successful**:

   Income = payment from the project alternative role A chose

4. You **do not have the decision right** and the project is **not successful**:

   Income = payment in case of lack of success

Detailed procedure for a round on the computer

1st stage: Your decision as role B

In each round, role A first has the decision right. Role A can also delegate the decision right to you as role B. Before role A decides whether he would like to delegate the decision right to you, you determine in a binding manner – for the case that the decision right is delegated to you – which project alternative and which probability of success that you would like to select.

If role A actually delegates the decision right to you, then the decisions you make in the first stage will be realized.

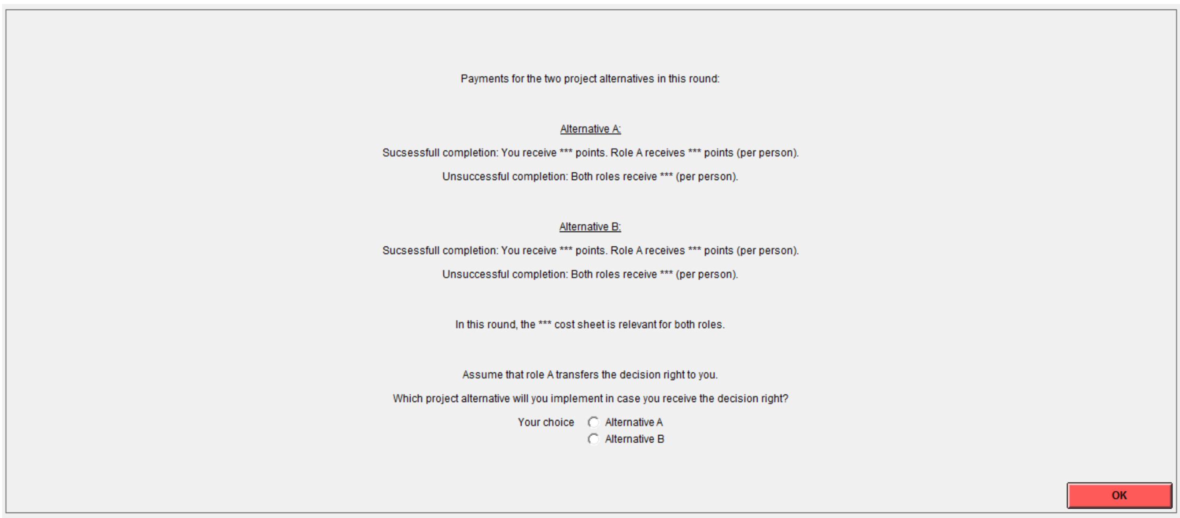
You should therefore carefully consider which probability of success and which project alternative you would like to choose, even though you do not yet know whether your will have the decision right.

Role A will not yet learn which decisions you as role B make in the first stage.

You will make your choices of project alternative on the following screen:

The upper part of the screen informs you of the payments for the two project alternatives as well as the payment in case of lack of success in the round in question. You will also be informed whether the cost schedule on the yellow or the pink information sheet applies. You can choose the project alternative in the lower part of the screen.

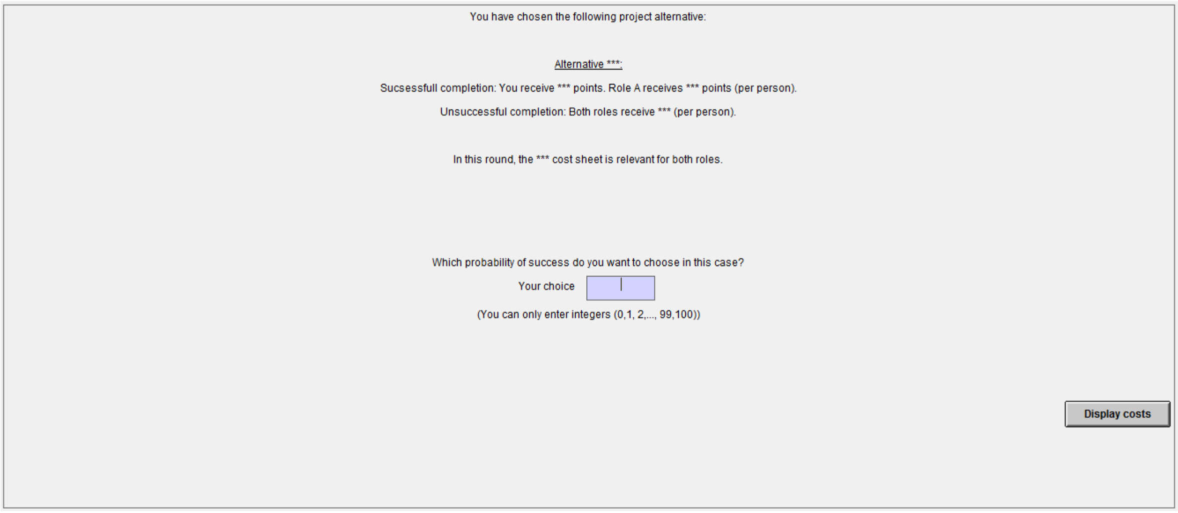
Here is an example:



After you have chosen the project alternative, please click on the OK button.

You make your selection of the probability of success on the screen shown below:

Here is an example:



After you have chosen the probability of success, click on the “display costs” button. This will then show the exact costs of the probability of success that you chose. You can then change your probability of success if you wish. You make your final decision with “confirm”.

Please take note that the costs for the choice of probability of success only apply if the decision right is actually delegated to you.

2nd stage: Who has the decision right?

Role A can decide in each round – after you as role B have made your decisions – whether she would like to delegate the decision right to you or if she would like to retain this for herself.

In this case, role A does not make the decision directly, but **by determining a minimum requirement**:

In each round, role A can determine the minimum probability of success that role B must have chosen for her to be willing to delegate the decision right to role B.

Please take note that role A does not know the probability of success that you chose when she determines her minimum requirement.

If the probability of success that you choose is greater than the minimum requirement that role A determines, role A will delegate the decision right. If the probability of success that you determine is less than the minimum requirement, role A will retain the decision right.

3^{rd} stage: Selection of project and determination of the probability of success by role A

If role A retains the decision right, she selects a project and determines a probability of success.

4^{th} stage: Determination of the project success

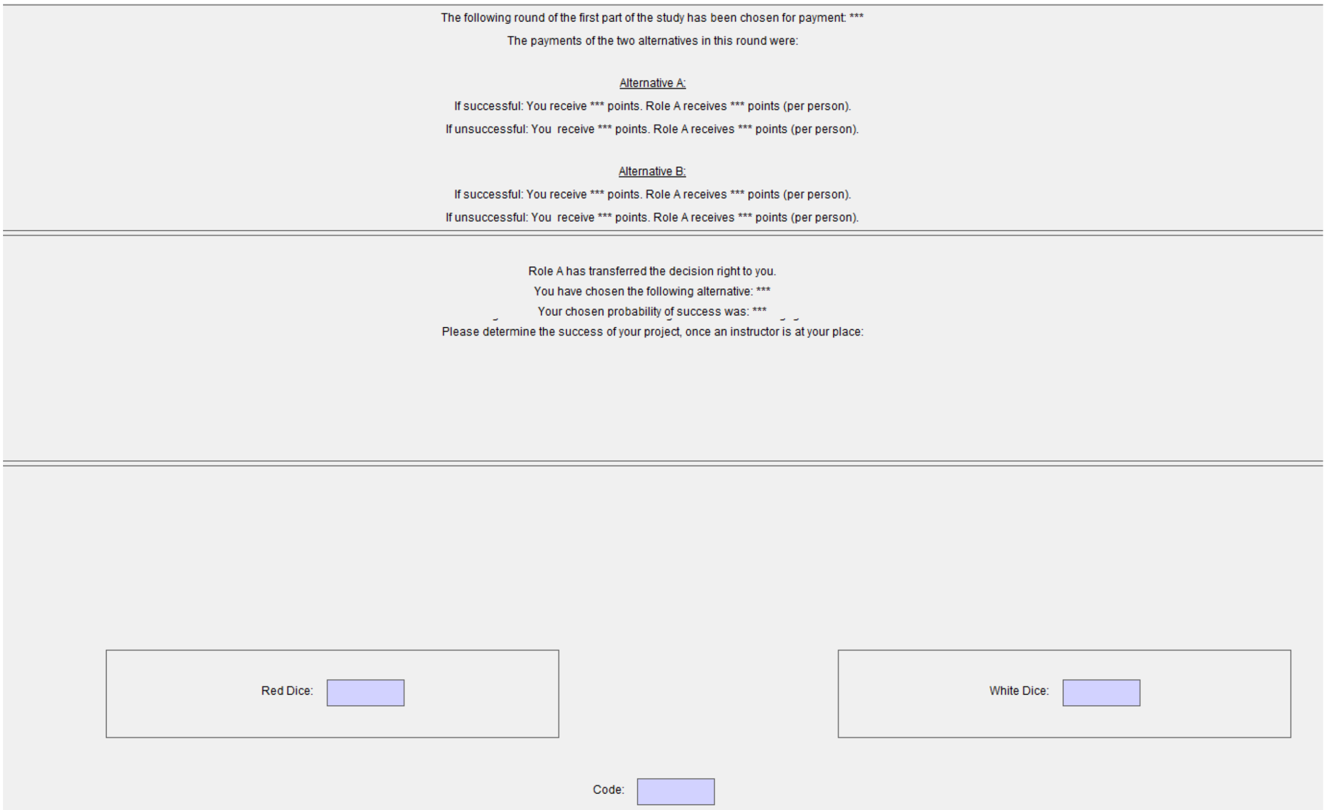
At the end of the study, the computer will randomly determine one of the ten rounds, and the payment that determines your income from this part of the study will be decided for this round based on your decision and that of the role A assigned to you in this period. As you do not know which round the computer will randomly determine, you should consider your decisions in each round very carefully.

a) The computer will first randomly determine which round will be selected for payment.

b) It will then examine whether you chose a probability of success that is at least as large as the minimum requirement of the role A who was randomly assigned to you in this round.

- If the minimum requirement is fulfilled, role A will delegate the decision right to you.
- If the minimum requirement is not fulfilled, role A will retain the decision right.

If you were delegated the decision right, you can determine the project success yourself by rolling the dice. You will do this at your desk, under supervision of the head of the study. The result is entered on the following screen:



You can roll the dice yourself, but the entry of the result and the code (necessary in order to press the "continue" button) must be done by the head of the study.

Do you have questions about the first part of the study? Please raise your hand. We will come to your desk. If you do not have any questions, **please answer the control questions on the next pages. You have to enter the correct answers on the screen later.**

Test questions
Role B and Part 1

Please answer the following test questions. Please contact the head of the study if you have any questions.

1. Assume you chose a probability of success of 3.
   (a) If role A specifies a minimum requirement of 1, who has the decision right in this round? Role ....
   (b) What is the probability that the project will then be successful? ....
   (c) If, however, role A specifies a minimum requirement of 4, who has the decision right in this round? ....

2. Assume you chose a probability of success of 90.
   (a) If role A specifies a minimum requirement of 85, who has the decision right in this round? Role ....
   (b) What is the probability that the project will then be successful? ....
   (c) If, however, role A specifies a minimum requirement of 95, who has the decision right in this round? ....

3. Assume that you received the decision right and chose a probability of success of 54. The cost schedule on yellow information sheet applies in this round. Assume further that you roll an 8 with the red die and a 2 with the white one.
   (a) How high are your costs? ....
   (b) Would the project have been successful? ....

   The following payments apply for the project:

		Payment to role A (per person)	Your payment
Successful completion	**Alternative A**	200	150
	Alternative B	150	200
If unsuccessful		100	100

Assume you chose project alternative B.

(a) How high would your income be? ....

(b) How high would role A's income (per person) be? ....

Now assume that you received the decision right and chose a probability of success of 24. Assume further that you roll a 1 with the red die and a 5 with the white one. The cost schedule on yellow information sheet applies in this round. You again chose project alternative B.

(a) How high are your costs? ....

(b) Would the project have been successful? ....

(c) How high would your income be? ....

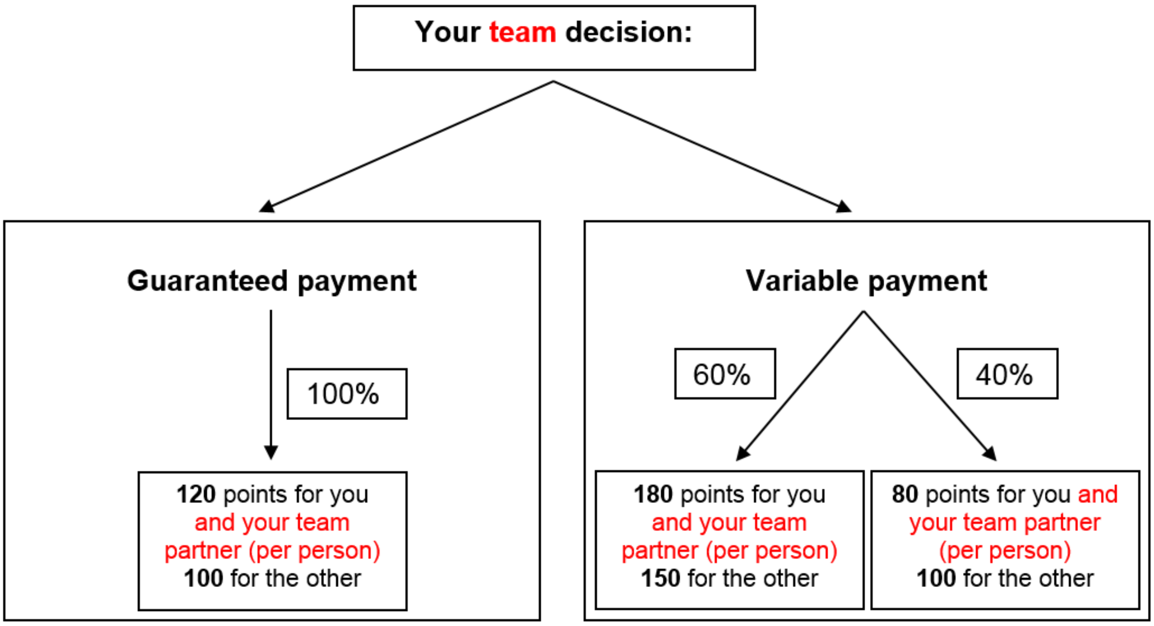
(d) How high would role A's income (per person) be? ....

Second part of the experiment – Instructions[34]

There are 20 rounds in this part of the study. You are randomly paired with another participant in the study in each round. (You and your permanent partner from the previous part (role A) are randomly paired with another participant in the study in each round.) The exchange rate of **100 points = 2 Euro** still applies.

In each round, you (your team) must decide between a guaranteed payment and a variable payment. Your decision also determines the payment of the other participant randomly assigned to you.

An example:



If, in the example above, you decide for the **guaranteed payment**, you will receive 120 points (per person) and the other, randomly assigned participant will receive 100 points.

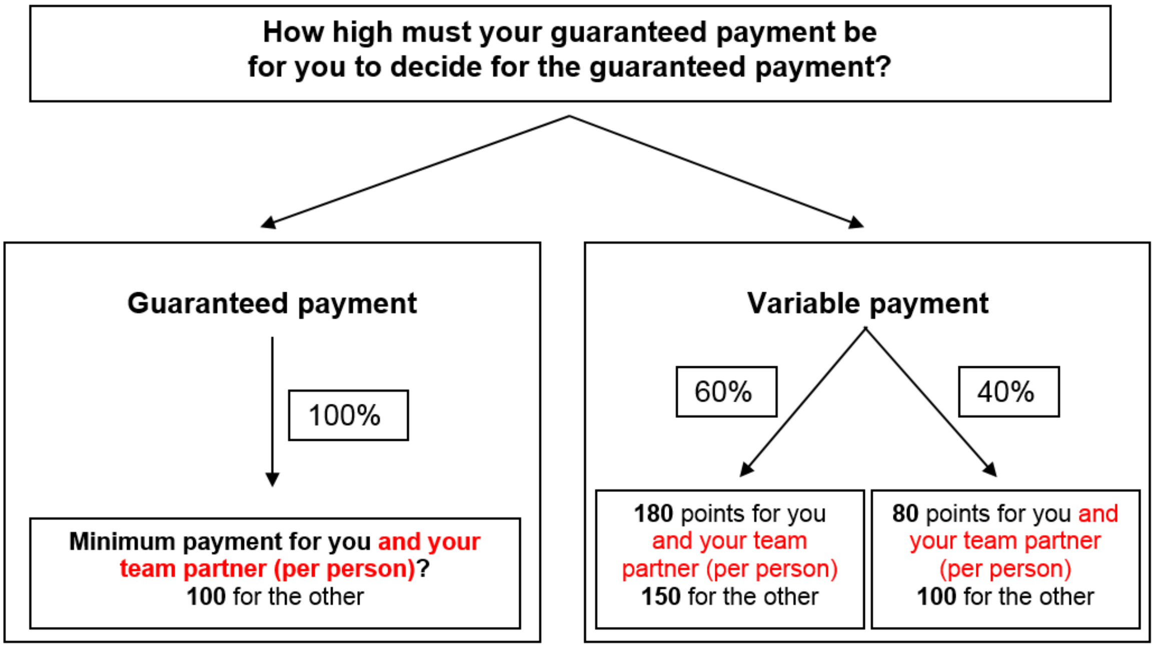
If you (your team) opt for the variable payment, there is a 60% probability that you will receive a payment of 180 points (per person) and the other participant will receive 150 points. There is a 40% probability that you will receive 80 points (per person) and the other participant will receive 100 points.

You (and your team partner) make a decision in each of the 20 rounds between a guaranteed payment and a variable payment. The payments and the probabilities in case of the variable payment differ in each round.

[34]Same instructions for role A and role B, but again modifications for teams in red.

How can you (and your team partner) make your decision between the guaranteed and the variable payment in each round?

When you (and your team partner) make your decision in a round between the guaranteed and the variable payment, you (and your team partner) do not yet know the amount of your guaranteed payment (per person). You (and your team partner) cannot therefore make a direct decision between the guaranteed and the variable payment, rather, you (and your team partner) must indicate how high your guaranteed payment (per person) must be for you to opt for the guaranteed payment instead of the variable payment.



You will be informed of the guaranteed payment for the other participant, the variable payments for you (your team (per person)) and the other participant, and the probabilities in case of the variable payments in each round.

After you (and your team partner) have indicated the **minimum payment** that would make you (your team) decide for the guaranteed payment in a round, your **actual guaranteed payment** in this round will be notified to you. The decision between the guaranteed payment and the variable payment is then realized as follows:

- If the actual guaranteed payment is less than the minimum payment you (your team) indicate (indicates), the variable payment determines your income (your team income) and that of the other participant.

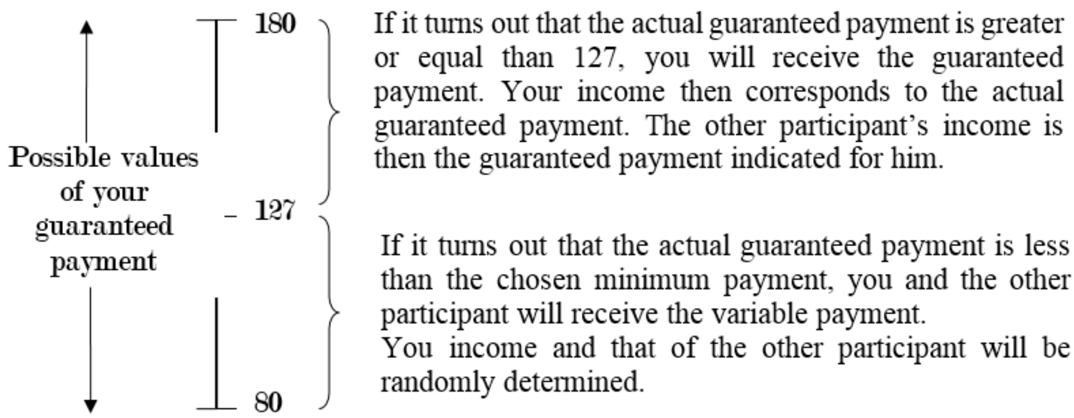
- If the actual guaranteed payment is greater than or equal to the minimum payment you (your team) indicate (indicates), you (and your team partner) will receive the actual guaranteed payment and the other participant will also receive the secure payment shown on the screen (100 points in the example above).

The possible values of your guaranteed (team) payment lie between both of your variable (team) payments (per person) (in the example above, between 80 and 180 points). Any integer value in this interval (80, 81, 82, 83, ..., 180) is equally probable. The minimum payment you (your team) indicate (indicates) can also be any integer value between both of your variable payments.

The graph below again clarifies the connection between the minimum payment you (your team) determine (determines), the amount of the actual guaranteed payment, and your (team) decision between the guaranteed and the variable payment:

If, for example, you (and your team partner) indicate a **minimum payment of 127**, this means that you prefer any guaranteed payment between 127 and 180 points to the variable payment.

You will be informed of the **exact amount of your actual guaranteed payment (per person)** after you determine your minimum payment.



When you (and your team partner) consider your minimum (team) payment (per person), then you should (assuming the numbers from the example above) ask the following questions:

- Would I (we) prefer a guaranteed payment amounting to 180 (per person) points to the variable payment? If yes, then you should ask:

- Would I (we) prefer a guaranteed payment amounting to 179 (per person) points to the variable payment? If yes, then you should ask:

- Would I (we) prefer a guaranteed payment amounting to 178 (per person) points to the variable payment? If yes, then you should ask:

Continue this way until you (your team) reach (reaches) a point amount for the guaranteed payment where you (and your team partner) would just prefer the guaranteed payment. You (Your team) should then enter this point amount as the minimum payment.

The value is 127 in the example above. This means that you (your team) just prefer (prefers) the guaranteed payment instead of the variable payment in case of a guaranteed payment of 127 (per person), but at a lower point amount of 126 (per person) (and at all values below this), you would prefer the variable payment. **The income:**

If the actual guaranteed payment is at least as high as the minimum payment you (your team) indicate (indicates):
You (and your team partner) will receive the actual guaranteed payment. The other participant will receive the guaranteed payment indicated for him.

If the actual guaranteed payment is less than the minimum payment you (your team) indicate (indicates):
The choice between the indicated variable payments for you (your team (per person)) and the other participant will be made randomly with the probabilities that are given.

The computer will randomly determine 2 of the 20 rounds at the end of the study.

In each of the randomly chosen rounds, the minimum payment (per person) you indicate will be compared with the actual guaranteed payment (per person). If the actual guaranteed payment is greater than or equal to the minimum payment you indicate, you will receive the guaranteed payment. If the actual guaranteed payment is less than the minimum payment you (your team) indicate (indicates), a cast of the dice will determine which of the variable payments you and the other participant each receive.

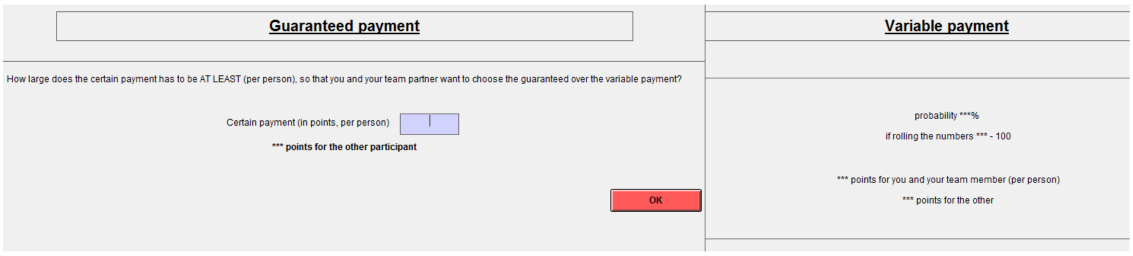
As you (your team) do (does) not know which 2 of the 20 rounds the computer will randomly determine, you should consider your (team) decisions in each round very carefully.

Procedure on the computer

The team decision follows the same rules as in part 1 of this experiment. Both team members A1 and A2 have to make the same decisions. You can communicate via the free text chat again.

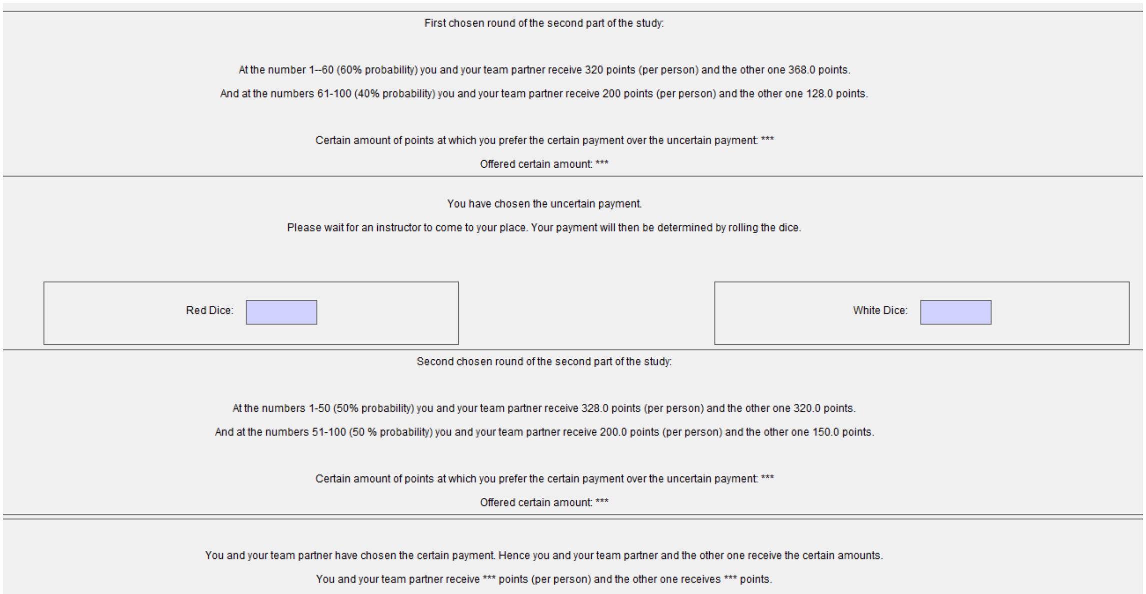
1. You enter **your decision about the guaranteed payment that you must receive as a minimum in order to make you prefer the guaranteed payment over the variable payment** in each round on the computer screen below.

   Here is an example:

   You see the variable payments for you (your team) and for the other randomly chosen participant on the right side of the screen. You will also see the probability with which the payments will occur. This information varies in each of the 20 rounds. You enter your **minimum payment (per person)** on the left side of the screen. The minimum payment indicates which guaranteed payment you (your team (per person)) must receive in minimum to make it so that you prefer the guaranteed payment to the variable payment. When you have made your entry, please click on the OK button. You can change your entry until you click on the OK button.
   A confirmation of your entry is only possible, if the entries of A1 and A2 are identical.

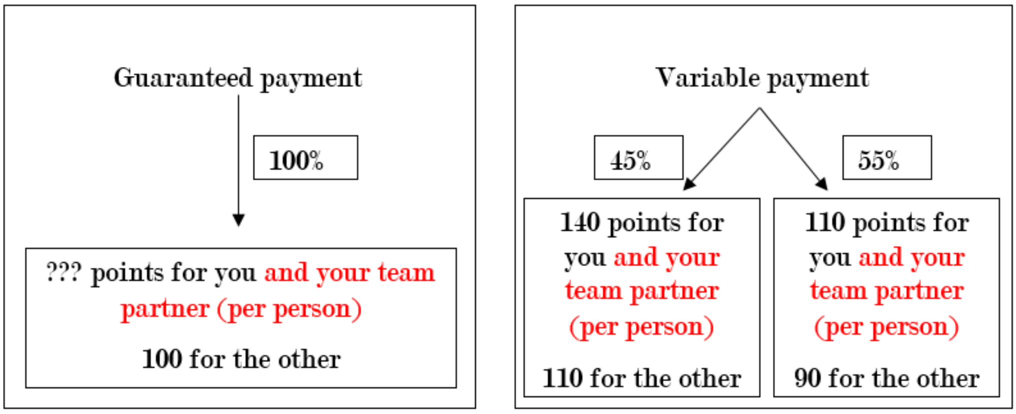
2. If the minimum payment you (your team) indicate (indicates) lies under the actual guaranteed payment in one of the chosen rounds, a cast of the dice will determine which of the variable payments you (your team) and the other participant will receive. Casting the dice works in exactly the same way as in the first part of the study. The head of the study will enter the numbers that are cast in the screen below.



Do you have questions about the second part of the study? Please raise your hand. We will come to your desk. If you do not have any questions, **please answer the control questions on the next pages. You have to enter the correct answers on the screen later.**

Test questions
Part 2

Assume that the following payments and probabilities apply for the case of the variable payment.



1. **Assume you (your team) specify (specifies) a minimum payment of 120.**
   (a) Assume the actual guaranteed payment is 128.
   How high is your payment in this round (per person)? ....
   How high is the other participant's payment in this round? ....
   (b) Assume the actual guaranteed payment is 117.
   How high is your payment in this round (per person)? ....
   How high is the other participant's payment in this round? ....
2. **Assume you (your team) specify (specifies) a minimum payment of 135.**
   (a) Assume the actual guaranteed payment is 128.
   How high is your payment in this round (per person)? ....
   How high is the other participant's payment in this round? ....
   (b) Assume the actual guaranteed payment is 113.
   How high is your payment in this round (per person)? ....
   How high is the other participant's payment in this round? ....
3. **Assume you (your team) specify (specifies) a minimum payment of 115.**
   (a) Assume the actual guaranteed payment is 128.
   How high is your payment in this round (per person)? ....
   How high is the other participant's payment in this round? ....

(b) Assume the actual guaranteed payment is 135.
How high is your payment in this round (per person)? ....
How high is the other participant's payment in this round? ....

Please raise your hand when you have answered the questions. We will come to you at your desk.

Instructions - Part 3

Additional information[35]

The computer will now randomly determine the round that is relevant for your (team) payments from the first part of the study. If you (and your team partner) have the decision right in the chosen round, you can determine the project success by rolling the dice.

We would like to know from you (as a team) whether it is worth points to roll the dice yourself and not to let another person roll the dice. (This only involves rolling the dice and not the selection of the probability of success or the project alternative.)

You (Your team) will now receive another 30 points. You can use some or all of these 30 points to purchase the right "to roll the dice yourself". If you do not purchase this right, the head of the study will roll the dice for you (your team). The head of the study will roll the dice fairly using a cup for dice, i.e., exactly as you would have done it. If you purchase the right to roll the dice, then you do so yourself.

We will pose the following question on the screen:

Are you willing to pay to be able to roll the dice yourself? Yes or No?

Team decision:
You are again matched with your fixed partner. The team decision works as in the previous rounds. You can communicate via the free text chat and have to reach a common decision.

If you (your team) click (clicks) on "yes" on the computer, we will then ask the maximum number of points you are willing to pay to be able to roll the dice yourself (for the case that you retained the decision right).

When answering this question, please take the following procedure into account: you (your team) can purchase the "right to roll the dice yourself" by stating your maximum willingness to pay for this right – this must lie between 1 and 30. A random decision will then determine a price between 1 and 30 for this right. If the price is less than or equal to your willingness to pay, you will pay the price and roll the dice yourself. If the price is higher, you retain the entire 30 points (per person) and the head of the study will roll the dice for you. **This procedure insures that it is best for you to state how many points the value of rolling the dice yourself if worth.**

[35] The same instruction for both roles with team treatment modifications

Example 1: You (Your team) are (is) willing to pay a maximum of 5 points (per person) in order to be able to roll the dice yourself (your willingness to pay is 5 points). The random device determines that the price for rolling the dice yourself is 18 points. As your willingness to pay (per person) is less than the price, you do not pay the price. You retain all 30 points and the head of studies rolls the dice.

Example 2: You (Your team) are (is) willing to pay a maximum of 25 points (per person) in order to be able to roll the dice yourself (your willingness to pay is 25 points). The random device determines that the price for rolling the dice yourself is 7 points. As your willingness to pay (per person) is greater than the price, you pay the price of 7 points. You retain 23 of the 30 points and roll the dice yourself.

If you are willing to pay something to be able to roll the dice yourself, we ask you to enter your exact willingness to pay. If you (your team) delegated or did not receive the decision right in the first part of the study, you will receive the additional 30 points automatically.

If you have questions about these instructions, please raise your hand. We will then come to your desk. Otherwise click on the “continue” button.

Loss aversion lotteries[36]

You now have the possibility to participate in a series of lotteries. Potential earnings will be added to your overall income, potential losses will be subtracted from your overall income.

You will soon see a series of lottery decisions. The team decision works as in the previous rounds. Please decide for each lottery whether you want to "accept" or "reject" the lottery. At the end, one lottery will be randomly chosen.

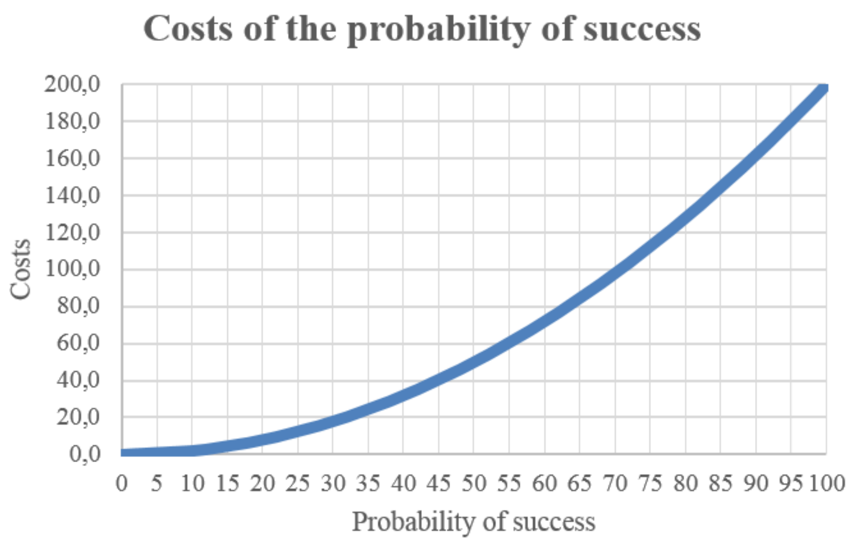
If you accepted that lottery, a random process will determine whether you have won or lost the lottery. If you rejected the lottery nothing happens and your income remains unchanged.

Please decide for each of the following lotteries whether you want to accept or reject the lottery:

1. With 50% probability you win 3 Euro (per person), with 50% probability you lose 1 Euro (per person).
2. With 50% probability you win 3 Euro (per person), with 50% probability you lose 1.50 Euro (per person).
3. With 50% probability you win 3 Euro (per person), with 50% probability you lose 2 Euro (per person).
4. With 50% probability you win 3 Euro (per person), with 50% probability you lose 2.50 Euro (per person).
5. With 50% probability you win 3 Euro (per person), with 50% probability you lose 3 Euro (per person).
6. With 50% probability you win 3 Euro (per person), with 50% probability you lose 3.50 Euro (per person).

[36]The same instruction for both roles with team treatment modifications

Cost sheet (here: high costs)



Probability of success	Costs	Probability of success	Costs	Probability of success	Costs	Probability of success	Costs
0	0,0						
1	0,2	26	13,6	51	52,0	76	115,6
2	0,4	27	14,6	52	54,0	77	118,6
3	0,6	28	15,6	53	56,2	78	121,6
4	0,8	29	16,8	54	58,4	79	124,8
5	1,0	30	18,0	55	60,6	80	128,0
6	1,2	31	19,2	56	62,8	81	131,2
7	1,4	32	20,4	57	65,0	82	134,4
8	1,6	33	21,8	58	67,2	83	137,8
9	1,8	34	23,2	59	69,6	84	141,2
10	2,0	35	24,6	60	72,0	85	144,6
11	2,4	36	26,0	61	74,4	86	148,0
12	2,8	37	27,4	62	76,8	87	151,4
13	3,4	38	28,8	63	79,4	88	154,8
14	4,0	39	30,4	64	82,0	89	158,4
15	4,6	40	32,0	65	84,6	90	162,0
16	5,2	41	33,6	66	87,2	91	165,6
17	5,8	42	35,2	67	89,8	92	169,2
18	6,4	43	37,0	68	92,4	93	173,0
19	7,2	44	38,8	69	95,2	94	176,8
20	8,0	45	40,6	70	98,0	95	180,6
21	8,8	46	42,4	71	100,8	96	184,4
22	9,6	47	44,2	72	103,6	97	188,2
23	10,6	48	46,0	73	106,6	98	192,0
24	11,6	49	48,0	74	109,6	99	196,0
25	12,6	50	50,0	75	112,6	100	200,0

CHAPTER 5

SOCIAL REFERENCE POINTS AND (DIS)HONEST BEHAVIOR

Abstract

This paper experimentally examines how social reference points in the form of peer income affect honesty. Decision makers know the income of a relevant peer and learn their intermediate income for a subsequent real effort task by rolling a virtual dice. By exogenously varying the level of the peer income across treatments, we affect the likelihood that the social income comparison based on the intermediate income is advantageous or disadvantageous from a decision maker's perspective. Then, decision makers have to determine their final income by reporting the outcome of the dice throw. We find that decision makers engage more frequently in dishonest behavior, i.e., misreport the outcome of the dice throw, to increase their income in the treatment where the likelihood of a disadvantageous social comparison is higher. This finding is consistent with the concept of social loss averison. We interpret our result as a shift in the moral and ethical perception due to relative income concerns. Hence, this finding might be relevant for frequently discussed and partly implemented income transparency policies.

5.1 INTRODUCTION

Employee fraud has huge consequences for economies with an estimated annual damage of five percent of the revenues for organizations. More than three quarters of the cases originate from asset misappropriation such as cash larcency, check tampering or issuing payments by making false claims for compensation (Association of Certified Fraud Examiners, 2016). Yet, it is important to know what affects dishonest behavior in this environment. One potential trigger might be relative income concerns. Relative income concerns entail the idea that workers do not only care about the absolute level of their income, but also how their income compares to the income of others (see, for instance, Veblen, 1899; Duesenberry, 1949 for early contributions). More recent studies show that income comparisons are correlated with subjective well-being (e.g., Luttmer, 2005; Ferrer-i Carbonell, 2005; Perez-Truglia, 2019), health (e.g., Marmot, 2004), perceived fairness (Austin et al., 1980), and job and pay satisfaction (e.g., Clark and Oswald, 1996; Card et al., 2012; Cohn et al., 2014). From an organizational point of view relative income concerns also affect relevant behavior such as effort provision (e.g., Gächter and Thöni, 2010; Cohn et al., 2014), work quality (Cohn et al., 2014) and risk-taking attitudes (Schwerter, 2016).

An important observation from social psychology highlights that people frequently exert social comparison, in particular with relevant others (e.g., Festinger, 1954). The working environment, as Falk and Knell (2004) and Clark and Senik (2010) argue, serves as a particular channel to make peers and peer income salient (i.e., relevant) for social comparison. For instance, Austin et al. (1980) show that the comparison of the own income with a peer income (as a social reference point) has a stronger impact on pay satisfaction than the comparison with an individual (private) reference point like previous earnings. These findings raise the question how individuals react to advantageous or disadvantageous income comparison with relevant peers if they could change their position within the income distribution by engaging in dishonest behavior.[37] In the shadow of recent wage transparency initiatives, this question is indisputably relevant as it may point out potential negative externalities of full wage transparency.

In this paper, we study how social reference points in the form of peer income affect whether or not subjects act honestly when monetary incentives stay constant. In our laboratory experiment, we observe worker pairs where one member (the peer) receives a fixed income for a subsequent real effort task, and the other member (the decision maker) is asked to report the outcome of a computerized dice throw which determines her final income. In detail, in a first step decision makers learn their intermediate income by observing the outcome of a dice throw, and in a second step they determine their finale income by reporting the outcome of the dice throw. This comes with the possiblity to increase (or decrease) their final income relative to the intermediate income.

We implement two treatments in a between-subjects design where we exogenously manipulate the level of the peer income. In one treatment, decision makers are paired with a peer who earns a low income, while in the other treatment they are paired with a peer who earns a high income. Consequently, the variation of the peer income first of all affects the outcome of a decision maker's social comparison process after they learn the intermediate income. If she is paired with a low income-peer, her intermediate income is most likely greater than the peer income. In constrast, if she is paired with a high income-peer, her intermediate income is most likely smaller than the peer income. We examine whether the variation of the peer income affects her decision which determines her final income.

[37] Pettit et al. (2016) show that status as means of a social comparison outcome affects dishonesty. The authors show that people cheat more to avoid falling behind than to go ahead.

To this end, we compare the fraction of dishonest decision makers between the high income-peer and the low income-peer treatment. Differences can only arise from relative income concerns based on the intermediate income. We derive this prediction by applying the concept of loss aversion (e.g., Kahneman and Tversky, 1979; Tversky and Kahneman, 1991) in the social domain. In detail, we assume that people perceive a disadvantageous comparison after the first step as a social loss and an advantageous comparison as a social gain. For instance, the outcome-based social preference model by Fehr and Schmidt (1999) assumes, in line with loss aversion in the social domain, that people experience more disutility from inequality in the envy domain (disadvantageous outcome comparison) than in the compassion domain (advantageous outcome comparison). In our context, social loss aversion implies that decision makers are more sensitive to losses than to gains and thus are more likely to engage in dishonest behavior to avoid a social loss.

Our main finding of this paper is that social reference points affect honesty. A significant fraction of decision makers misreport the outcome in both high and low social reference point situations. But, decision makers engage more frequently in dishonest behavior when they face a high income-peer relative to the situation when decision makers face a low income-peer. We interpret this finding as a shift in the moral and ethical perception of individuals, i.e., the moral and ethical perceptions of individuals are correlated with relative income concerns.

Our result is in line with previous research investigating honesty and reference dependence in the private domain. For instance, Schweitzer et al. (2004) show that participants want to avoid perceived losses in form of unmet goals by using dishonest behavior. Cameron et al. (2010) and Grolleau et al. (2016) report that loss frames lead to more dishonest behavior than gain frames. Shalvi (2012) shows that people tend to act dishonestly to convert a loss into a gain, and Schindler and Pfattheicher (2017) document that people tend to cheat more to avoid a loss than to realize an equal-sized gain.

Our paper complements and extends the existing experimental research investigating honesty, fairness and social comparison processes by Houser et al. (2012) and John et al. (2014).[38] Houser et al. (2012) investigate decision makers' honesty after either participating in a dictator game or randomly receiving payments which represent the most frequent dictator transfers. The authors conclude that perceived unfair treatment (receiving zero in the dictator game) is the main motivation to engage in dishonest

[38] Outcome-based social preference models (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) also deal with fairness concerns when subjects compare their outcome with the outcome of others in allocation games. In these games decision makers affect not only the own outcome but also the outcome of others.

behavior for monetary gains. John et al. (2014) examine honesty in a real effort game where participants either receive a low or a high piece rate in a real-effort task. The authors show that receiving a low piece rate is not enough to act more dishonest per se. But, aversive social comparison does lead to more dishonest behavior in low earners. However, they do not find a higher fraction of dishonest participants, but the magnitude of dishonest actions increases. The difference between our paper and these two papers is that we introduce reference dependence in our experiment to investigate the impact of relative income concerns on honesty and thus exclude other peer effects which might affect dishonesty as well.

In the vein of the literature on reference dependence, both studies have limitations as they do not implement a proper social reference point. First, all participants could engage in dishonest behavior and thus could improve individual earnings. Second, in John et al. (2014) subjects are aware of the different piece rates (and might infer that those earning 25 cents perform better) but they do not know, at the moment they report their score nor afterwards, the absolute income of their peers. The absence of a proper social reference point might rule out important motivations to engage in dishonest behavior and could therefore measure also other peer effects than relative income concerns. For instance, the individual relative position within the income distribution is not salient and might be strongly correlated with the beliefs about others' honesty (including those who should represent the peers). Moreover, the absence of a proper social reference point makes it impossible to know whether a decision maker could avoid a comparison in the form of social loss comparison by acting dishonestly. This might discourage people ex-ante to engage in dishonest behavior and might result in non-observable differences in dishonesty even in the presence of (huge) income inequality. To account for these points, we implement a unique social reference point to fix decision makers' expectations about the income and behavior of the peer and thus make the relative position of an individual salient.

The remainder of the paper is organized as follows. Section 5.2 introduces the experimental procedures, the experimental design, and our hypothesis. Section 5.3 presents the experimental results and Section 5.4 concludes the paper with a discussion of the results.

5.2 EXPERIMENTAL DESIGN

In this section we first describe the procedure and design of the experiment. Then we present our hypothesis. The experiment was conducted between June and December 2017 at the Cologne Laboratory for Economic Research with a total of 272 participants (136 decision makers). The experiment was fully computerized with zTree (Fischbacher, 2007) and recruitment was organized using ORSEE (Greiner, 2015). The subject pool consisted of only male participants because we wanted to keep the subject pool homogeneous.

We implemented a computerized lab experiment with two between-subjects treatments. For the rest of this paper we will refer to them as treatments *High* and *Low*. In both treatments, there are two roles and two stages, and all experimental sessions followed the same structure.

Upon arrival participants were seated and thus randomly assigned to one of the roles. Participants could be assigned to the role of a *peer* or to the role of a *decision maker*. In both treatments, we formed groups of two participants which consisted of one peer and one decision maker. We did not mention the terms peer or decision maker in the instructions, but used neutral expressions for both roles, i.e., Participant A (peer) and Participant B (descision maker). Before the participants learned their respective roles, they received the instructions for the whole experiment. After the experimenter read aloud the instructions in front of the participants, they had time to go through the instructions once again and to ask questions if necessary. Then, the experimenter started the experiment and participants learned their role on the computer screen. The experiment consisted of two stages.

5.2.1 THE FIRST STAGE – TWO STEPS CHEATING GAME

The first stage was the treatment stage and differed across roles. Subjects in the role of a peer received a predetermined income for the subsequent real effort task. The peers received either 5 Euros in treatment *High* or 2 Euros in treatment *Low*. Subjects in the role of decision makers determined their income in a two steps cheating game. Decision makers were aware of the peer income and they knew that both group members had to work on an identical real effort task in the next stage. Each decision maker participated in a two steps cheating game to determine her income.[39] We seperated both steps of the cheating game by displaying both steps on two consecutive screens. In the first step, a decision maker selects among six boxes with equal probability. To make sure that participants understood this mechanism, we mentioned that the outcome is, in principle, the same as the outcome of a dice roll. In our computerized version, one box contained exactly one number between 1 and 6 whereas each number occured only once and was associated with a fixed payoff (see, Section 5.5.3 for a screen shot). The associated payoffs were calculated as follows. The payoffs in Euros equalled 7

[39]Our cheating game is comparable to the "observed" game in Gneezy et al. (2018). They used a paper and pen-based reporting mechanism, while we used a computerized version.

minus the respective number.[40] We constructed the payoffs in this way to avoid any kind of anchoring (e.g., the number six is associated with a payoff of six Euros) and that participants had to think about their report. Importantly, decision makers knew their intermediate income (based on the dice throw outcome) after the selection of the box. Hence, they were also aware of their relative income position (based on the intermediate income). In the second step, we asked the decision makers to report the selected number. We told them that their final income equals the associated payoff of the reported number. It is worth mentioning that we did not explicitly point out the possibility to misreport, neither in the instructions nor on the screen.

5.2.2 THE SECOND STAGE - REAL EFFORT TASK

This stage was identical for both roles and should create a situation with a strong relationship between both roles and thus emphasize the salience of the peer income. The task was counting the number of zeros in a table of zeros and ones with 70 digits (10 rows and 7 columns) (e.g., similar to Abeler et al., 2011 and Balafoutas et al., 2016). Each group member had to solve in total eight tables correctly without a time constraint. In detail, participants had three tries to report the correct amount of zeros per table, otherwise they received a new table.

After participants completed the real effort task, we asked them to answer a questionnaire. We asked them regarding socio-economic characteristics, perceived fairness of the payment mechanism, and their overall perception regarding norm compliance (see Table 5.4). In addition, we included an unincentivized task to elicit the individual social value orientation (SVO) developted by Van Lange et al. (1997). This measure classifies individuals as prosocial, individualistic, competitive, or as a non-consistent type. SVOs usually indicate social preferences of an individual towards social norms like cooperation. Grosch and Rau (2017) show that SVOs also correlate with honesty where a decision does not affect payments of other participants, with prosocial types being more honest than individualistic types. They argue that prosocials might experience higher costs from acting dishonestly (e.g., violate a social norm) than individualistic types.

5.2.3 HYPOTHESIS

Before we turn to the results of the experiment, we present our hypothesis. To do so, we first summarize the most relevant part of the design. Both treatments were

[40] We explained the relationship between the numbers and associated payoffs in a table in the instructions as well as on the decision screen.

constructed in the following way: In both treatments it holds that the peer income is strictly greater than the minimal and strictly smaller than the maximal intermediate income of a decision maker after the first step of the cheating game. A decision maker's intermediate income is either 1, 2, 3, 4, 5 or 6 Euros with equal probability. Hence, in treatment *Low* a decision maker is (theoretically) better off than the peer in 66.67 percent, since the peer earns 2 Euros. In constrast, in treatment *High*, the probability that a decision maker is worse off than the peer is 66.67 percent, since the peer earns 5 Euros. These restrictions are very important when studying the impact of relative income concerns on honesty. Decision makers could always avoid a social loss or turn a social loss into a social gain by misreporting the selected number in the second step, and thus increase the final income relative to the intermediate income.

We expect a higher fraction of dishonest participants, i.e., more decision makers misreport the selected number in the second step, in treatment *High*. In our setting, decision makers are worse off than the peer in 66.67 percent of the cases in treatment *High*, whereas this is true just in 16.67 percent of the cases in treatment *Low* after the first step of the cheating game. This is, to speak in terms of social loss aversion, identical to the likelihood of a social loss comparsion after the first step of the cheating game. Hence, our prediction is based on the assumption that people are loss averse in the social domain and engage in dishonest behavior to avoid a social loss comparison in the end.

5.3 RESULTS

This section is organized as follows. First, we show descriptive statistics of our subject pool. Second, we present a non-parametric analysis of the experiment. Third, we perform a regression analysis.

5.3.1 DESCRIPTIVE STATISTICS OF THE SUBJECT POOL

Table 5.1 shows descriptive statistics of the subjects for each treatment. We compare both treatments at the overall (decision makers and peers) and at the decision maker level. We report the values for the following variables: *Age* indicates the average age of the subjects in years. *Econ students (in %)* shows the percentage of subjects studying economics or business.[41] The two variables *SVO Indivdualist (in %) and SVO Prosocial (in %)* (see Table 5.3) report the fraction of subjects classified as individualistic and prosocial. The last variable, *Norm compliance*, reports the mean value of the individual

[41]For instance, the survey of Rosenbaum et al. (2014) reports that economics majors do engage in dishonest behavior for monetary gains more often than non-economists.

perception of norm compliance within the society in general (see Table 5.4). We infer from these comparisons that randomization worked.

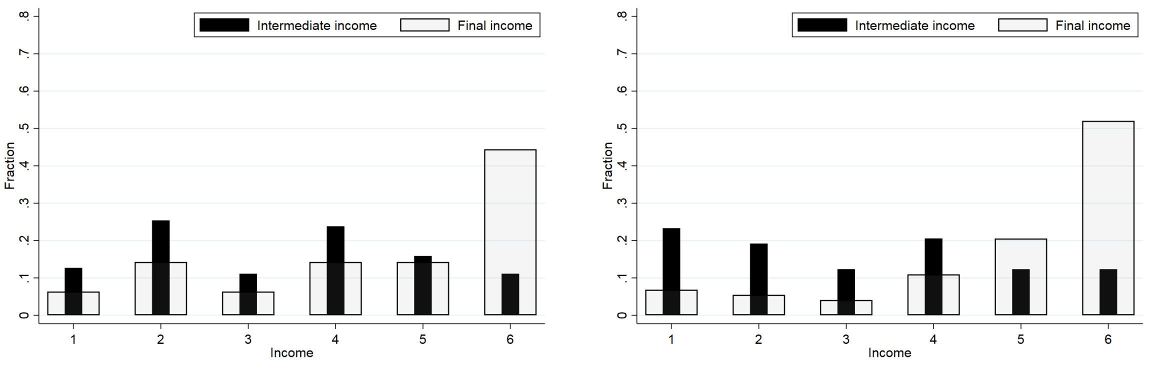
Table 5.1: Descriptive statistics of the subject pool

	Treatment Low n = 126 63 pairs	**Treatment High** n = 146 73 pairs	Low vs. High (p-values)
Age – All subjects	24.67 (5.87)	24.92 (5.89)	0.370
– *Decision makers*	*24.90 (6.08)*	*25.22 (7.09)*	*0.863*
Econ students (in %) – All subjects	0.51 (0.50)	0.47 (0.50)	0.543*
– *Decision makers*	*0.46 (0.50)*	*0.37 (0.49)*	*0.300**
SVO Individualistic (in %) – All subjects	0.43 (0.50)	0.38 (0.49)	0.389*
– *Decision makers*	*0.41 (0.50)*	*0.37 (0.49)*	*0.725**
SVO Prosocial (in %) – All subjects	0.48 (0.50)	0.58 (0.50)	0.145*
– *Decision makers*	*0.51 (0.50)*	*0.59 (0.50)*	*0.389**
Norm compliance – All subjects	4.75 (2.37)	4.67 (2.41)	0.801
– *Decision makers*	*4.60 (2.22)*	*4.75 (2.48)*	*0.819*

Notes: Standard deviations in parentheses. In the fourth column (Low vs. High) we report p-values of either two-tailed Mann-Whitney U-tests for non-binary variables or Fisher's Exact tests for binary variables (indicated with *).

5.3.2 NON-PARAMETRIC ANALYSIS

Figure 5.1: Intermediate vs. final incomes in the cheating game



(a) Treatment *Low* (b) Treatment *High*

Notes: Distribution of intermediate and final incomes for both treatments. The black bars represent the distribution of the intermediate income. The grey bars represent the distribution of the final income.

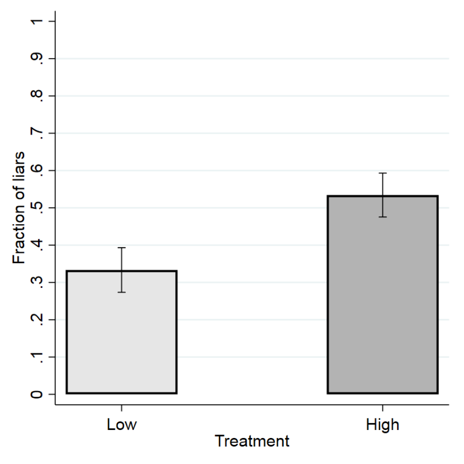
In this subsection, we present the main results of our experiment. Therefore, we compare intermediate and final incomes. By intermediate income we mean the associated payoff if a decision maker would report the selected number truthfully.

Figures 5.1a and 5.1b show the distributions of the intermediate and the final incomes for treatments *Low* and *High*, respectively. The black bars refer to the intermediate income, i.e., payments if all decision makers would have reported truthfully. To be able to compare both treatments, we need to show that the distributions are comparable between both treatments.[42] Indeed, we are in the position to argue that the distributions in both treatments are not significantly different from each other ($p > 0.668$, χ^2-test).[43] The grey bars show the income according to the reported number, i.e., the final income, of the decision makers. Both sides of Figure 5.1 indicate that a substantial fraction of participants reported a number which is associated with a higher payoff than the selected number. The graphical analysis is supported by non-parametric tests. Therefore, we compare the intermediate with the final incomes within both treatments. Both tests reveal a highly significant difference between the intermediate and the final income ($p < 0.001$, two-tailed Wilcoxon matched-pairs signed-ranks tests for both treatments). Hence, we observe dishonesty in both high and low income-peer settings.

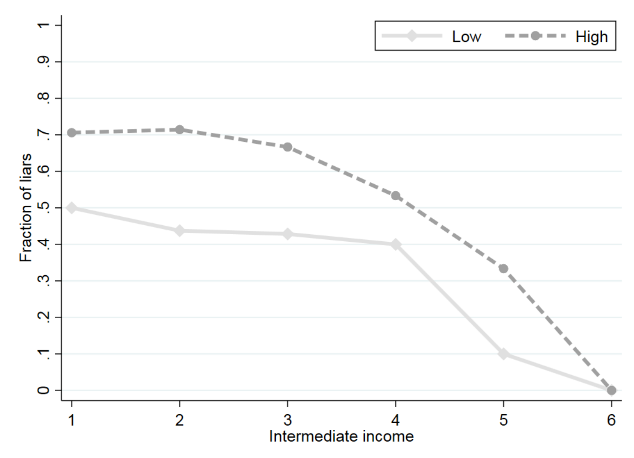
[42]Both distributions are not significantly different from a uniform distribution (both tests: $p > 0.100$, χ^2-tests). We compare the observed frequencies of the numbers with expected frequencies following a uniform distribution.

[43]We come to the same conclusion if we use a two-tailed Mann-Whitney U-test ($p = 0.404$) instead of a χ^2-test.

Figure 5.2: Fraction of dishonest subjects per treatment



(a) Aggregated level

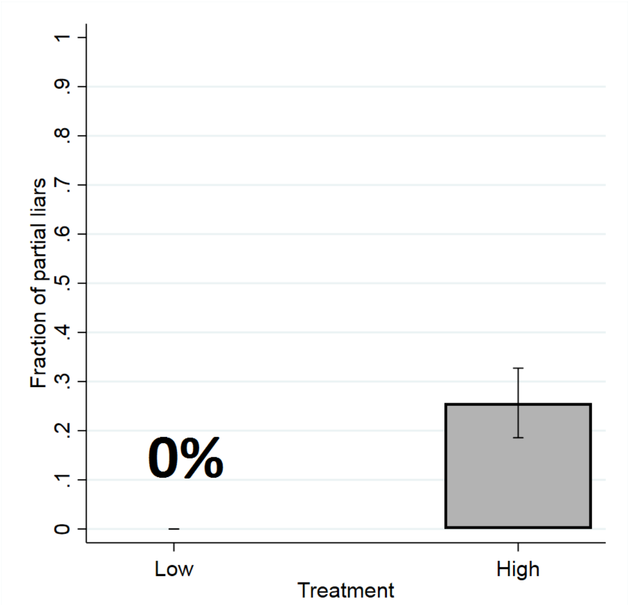


(b) Per intermediate income

Notes: Figure 5.2a shows the aggregated fraction of dishonsts subjects, i.e., liars, per treatment. Dishonesty is a binary variable which takes the value 1 if the intermediate income is different from the final income and 0 otherwise. The left bar represents treatment *Low* and the right bar refers to treatment *High*. Error bars represent standard errors at means (*average* ± *SEM*) at the treatment level. Figure 5.2b shows the fraction of dishonest subjects by the intermediate income. The dashed line represents treatment *High* and the solid line represents treatment *Low*.

For our main treatment analysis, we compare the fraction of dishonest decision makers in each treatment. We measure dishonest behavior by comparing the intermediate and the final income. Hence, dishonest behavior is a binary variable which takes the value zero if a participant reports the selected number and one otherwise. Figure 5.2a shows that the fractions of dishonest decision makers are 33.33 percent and 53.42 percent in treatments *Low* and *High*, respectively. It follows that the level of the social reference income has a significant impact on the honesty of decision makers, with people engaging more often in dishonest behavior if they have relative income concerns ($p = 0.024$, Fisher's Exact test). Furthermore, this pattern holds when we account for the payments associated with a truthful report. Figure 5.2b shows the fraction of dishonest subjects per intermediate income. We see that in the relevant space, i.e., all intermediate payments which are smaller than the maximum payment, dishonest behavior is more frequent in treatment *High*. A logistic regression with the dependent variable "Dishonest behavior", and a dummy for treatment *High* and dummies for all intermediate incomes (baseline: intermediate income = 1) as independent variables shows that subjects in the high income-peer treatment are more likely to engage in dishonest behavior (N = 120, $\beta_{High} = 0.940$ and $p = 0.017$). These findings confirm our hypothesis that dishonest behavior is higher in treatment *High*. Later, we show in a regression framework that this finding is a robust result by controlling for several variables including variations of the intermediate income.

Figure 5.3: Fraction of partial liars



Notes: The graph shows the fraction of partial liars at the treatment level. Error bars represent standard errors at means ($average \pm SEM$) at the treatment level.

Next, we examine the difference between intermediate and final income in each treatment and look at income-based motivations to engage in dishonest behavior. We compare the difference between intermediate and final income between both treatments in two steps. First, we examine the average magnitude of the difference between both incomes. Therefore, we calculate the difference between the final income and the intermediate income for each decision maker. This means that the difference could range from 0 (truthful report) to 5 (intermediate income would be 1 and the final income is 6). A two-tailed Mann-Whitney U-test reveals that the average magnitude of the differences in both incomes is significantly higher in treatment *High* than in treatment *Low* (1.73 vs. 1.11, $p = 0.031$). Second, we compare the average magnitude of misreports of only dishonest participants. This comparison yields no significant difference between both treatments ($p = 0.811$).

Moreover, our design allows us to look at income-based motivations of dishonest behavior. In particular, we are interested in one case of dishonest behavior, namely partial dishonest behavior. Partial dishonest behavior is defined as all cases of dishonest behavior which do not maximize the income. Figure 5.3 shows the fraction of partial dishonest decision makers among all dishonest decision makers for both treatments. While we observe a non-neglectable share of partial dishonest decision makers in treatment *High* ($p = 0.002$, two-tailed Wilcoxcon matched-pairs signed-rank test), this motivation is absent in treatment *Low* since all dishonest decision makers went straight to the maximum income by reporting a "1".[44] In line with the previous test, a comparison between both treatments in the domain of partial dishonest behavior shows that it is significantly higher in treatment *High* ($p = 0.011$, Fisher's Exact test). To be more precise, all except one partial dishonest participants reported the number 2 which results in an income of 5 Euros, i.e., they used dishonesty up to the level of the social reference point.[45] This result is in line with the findings by Abeler et al. (*forthcomming*) and Gneezy et al. (2018) who report that partial dishonesty exists in observed games.[46] However, they report that participants' awareness of the observability of dishonest behavior at the individual level has an impact on dishonest behavior overall, but especially on partial dishonest behavior. In particular, partial dishonesty is more frequent in games where individual behavior is not observable than in games where dishonesty could be detected at the individual level. This pattern seems to originate from social identity concerns, i.e., people want to be perceived as honest by others (Gneezy et al., 2018; Result 4). In contrast, Mazar et al. (2008) argue that partial dishonesty is used to keep a positive self-image. The interpretation of our result, although being cautious in the interpretation due to the points mentioned, is the following. For some decision makers, acting dishonestly to increase the income above the social reference point, i.e., implementing an advantageous income inequality, is not beneficial because the additional intrinsic costs exceed the marginal benefit. The reference income in treatment *High* might be viewed as a fair income, thus using dishonesty to earn 5 Euros could be viewed (from an external or internal perspective) as legitimate.

5.3.3 PARAMETRIC ANALYSIS - A PROBIT REGRESSION APPROACH

The result of the non-parametric analysis of the main treatment effect is confirmed by a probit regression analysis. Table 5.2 encompasses six different specifications of probit regressions. The dependent variable of all models is the dummy variable "dishonest behavior" as defined previously. Model 1 reports the main treatment effect of the high peer-income treatment and confirms the findings from the non-parametic tests. Model 2 adds demographic variables as controls. In model 3 and 4, we show that the treatment effect is still significant when we control for the intermediate income

[44] Figure 5.4 displays the fraction of honest participants, partial liars, and full liars in treatment *High*.

[45] In one case, a participant reported a three. This led to a payment of 4 Euros for the decision maker.

[46] Please note, that the extreme result of no partial dishonesty in treatment *Low* could be due to the already low number of dishonest participants in this treatment.

without and with demographic variables as controls. Model 5 is the same as model 3 and confirms the robustness of the treatment effect when we relax the linearity assumption for the intermediate income variable (see Figure 5.2b). The demographic variables are all orthogonal to the treatment effect, so they should not change the main effect by design. They are not orthogonal against each other, so we conclude that the best model is the full model (Model 6).

Table 5.2: Probit regressions – Robustness checks

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment *High*	0.197**	0.203***	0.169**	0.191***	0.188***	0.208***
	(0.078)	(0.075)	(0.073)	(0.070)	(0.071)	(0.068)
Intermediate income			-0.108***	-0.098***	0.210*	0.203*
			(0.019)	(0.020)	(0.119)	(0.117)
Intermediate income^2					-0.049***	-0.047***
					(0.018)	(0.018)
Age		-0.012		-0.011		-0.011
		(0.008)		(0.008)		(0.008)
Study Econ/Business		0.196**		0.186**		0.181**
		(0.078)		(0.073)		(0.072)
SVO Prosocial		0.202		0.086		0.078
		(0.182)		(0.177)		(0.181)
SVO Indiviudalistic		0.156		0.094		0.085
		(0.184)		(0.176)		(0.181)
Norm compliance		0.037**		0.025		0.025
		(0.016)		(0.016)		(0.015)
N.obs.	136	136	136	136	136	136

Notes: The table reports marginal effects of probit regressions with the dependent variable "Dishonest behavior". Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

5.4 DISCUSSION AND CONCLUSIONS

In this paper, we test whether and how social reference points in the form of peer income affect honesty. Thereby, we contribute to the scarce literature on social reference points, and complement and extend the literature on social comprison processes and dishonest behavior (Houser et al., 2012; John et al., 2014). We do so by presenting results of a laboratory experiment with a homogeneous subject pool of male subjects. In our experiment, decision makers either face a peer which earns a high income or a low income. We find that participants engage more often in dishonest behavior in the treatment where the likelihood of the ex-ante income comparison based on an intermediate income is more likely to be in the form of a social loss comparison. We conclude from this finding that there exists a correlation between moral and ethical perception of decision makers and relative income concerns. In addition, we find in the high income-peer treatment that a substantial fraction of decision makers use dishonesty to increase the own income up to the level of the peer income which might be viewed as a fair payment, while this motivation is absent in the treatment with the low income-peer. Hence, decision makers use dishonest behavior to avert disadvantageous income inequality, but do not cheat to the full extent to maximize the income and implement an advantageous income inequality. This is in line with the general conclusion of the outcome-based social preference model by Fehr and Schmidt (1999), where people are inequality averse and suffer from every form of outcome inequality.

Our results are relevant for income transparency policies which are debated (and partly implemented) in many countries. For instance, the German income transparency law came into force at the time we conducted the first sessions. Income transparency could create positive externalities such as compressing the wage distribution (Mas, 2017), increasing tax reports (e.g., Bø et al., 2015), and closing the gender wage gap. Yet, our work suggests that it might result in negative externalities in the form of counterproductive workplace behavior (such as employee fraud) which are currently ignored. Hence, our findings should be considered by policy makers and practitioners when they decide about potential transparency policies.

5.5 APPENDIX

5.5.1 ADDITIONAL TABLE

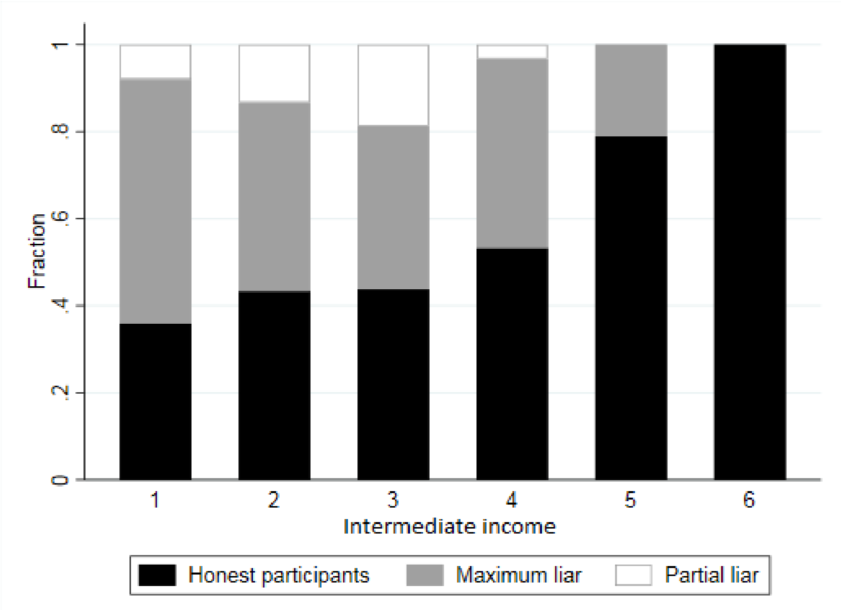
Table 5.3: SVO Triple-Dominance Measure

Situation	Option 1	Option 2	Option 3
1	You get: 480 Other gets: 80	You get: 540 Other gets: 280	You get: 480 Other gets: 480
2	You get: 560 Other gets: 300	You get: 500 Other gets: 500	You get: 500 Other gets: 100
3	You get: 520 Other gets: 520	You get: 520 Other gets: 120	You get: 580 Other gets: 320
4	You get: 500 Other gets: 100	You get: 560 Other gets: 300	You get: 490 Other gets: 490
5	You get: 560 Other gets: 300	You get: 500 Other gets: 500	You get: 490 Other gets: 90
6	You get: 500 Other gets: 500	You get: 500 Other gets: 100	You get: 570 Other gets: 300
7	You get: 510 Other gets: 510	You get: 560 Other gets: 300	You get: 510 Other gets: 110
8	You get: 550 Other gets: 300	You get: 500 Other gets: 100	You get: 500 Other gets: 500
9	You get: 480 Other gets: 100	You get: 490 Other gets: 490	You get: 540 Other gets: 300

Notes: An individual is classified when she/he makes six or more type-consistent choices. Prosocial choices are: 1c, 2b, 3a, 4c, 5b, 6a, 7a, 8c, and 9b. Individualistic choices are: 1b, 2a, 3c, 4b, 5a, 6c, 7b, 8a, and 9c. Competitive choices are: 1a, 2c, 3b, 4a, 5c, 6b, 7c, 8b, and 9a.

5.5.2 ADDITIONAL FIGURE

Figure 5.4: Types of liars in treatment *High*



Notes: The graph shows the fraction of type of liars in treatment *High* by the intermediate income. Black represent honest participants, grey refers to maximum liars, and white indicates partial liars.

Instructions

Welcome to today's experiment. You receive €4 as a show-up fee. In addition, you can earn money during the experiment. Therfore, it is important that you **carefully** read the instructions. If you have any questions during the experiment, please raise your hand and one of the experimenters will come to assist you. Please do not communicate with other participants before the end of the experiment. Please switch off your mobile phone. In addition, we want to point out that you are only allowed to use the computer functions that are intended for the course of the study. If you **violate these rules**, you will be immediately **excluded from the experiment and all payments**. At the end of the experiment we will pay you in cash €4 as a show-up fee and the money you've earned during the experiment.

The experiment

At the beginning of the experiment, you will form pairs. A pair consists of two participants: **Participant A** and **Participant B**. At the beginning of the study, you will be informed if your are Participant A or Participant B. Both participants have the same task. Your task is to count the number of 0s in a table of 1s and 0s and report this number correctly. A table consists of 10 rows and 7 columngs (i.e., 70 digits). The number of 1s and 0s in a table are generated randomly.

Both participants have to solve multiple tables correctly, but the **total amount of tables** are the same for each member of a pair. **You will learn the total amount of tables to solve at the beginning of the task.**

The difference between both roles of participants is in the payment for the task.

You will start with you task as soon as both incomes are determined.

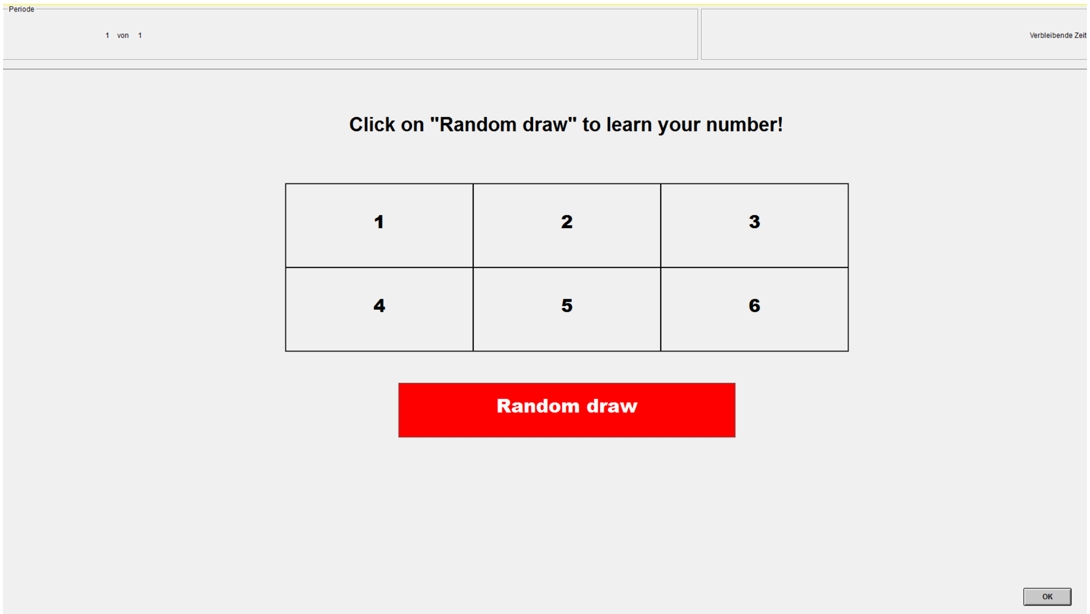
Income of Participant A:
Participant A receives €5 for the task.

Payment Participant A for this experiment = €4 + €5 (€2 in *Low*) = **€9 (€6 in *Low*)**

Income of Participant B:
The income of Participant B will be determined as follows:

Participant B sees at the beginning a **screen** as illustrated on the next page.

- The screen shows the numbers 1 to 6.
- Participant B has to click on "Random draw". The computer picks one of the numbers 1 to 6 randomly. Each number has the same likelihood to appear (You can imagine the outcome of a dice throw). The box of the drawn number will be highlighted in green and we show the drawn number below the box.



- We ask Participant B to remember the number and report it later on the screen.
- The number you report on screen determines your income. You will be paid the equivalent in Euros to the number you report. The link between reported number and income is summarized in the following table.

The conversion table (also shown on screen again):

Reported number	1	2	3	4	5	6
Income in Euros	6	5	4	3	2	1

Payment Participant B for this experiment = €4 + **Income according to the conversion table for the reported number**

Participant A does not lear the income of Paricipant B for this task. All decision are anonymous. Both players start with the respective task as soon as all incomes are determined.

All decision will be treated confidentially and anonymously.

Thank you very much for your participation!

5.5.4 POST-EXPERIMENTAL QUESTIONNAIRE

Table 5.4: Post-experimental questionnaire

Question	Answer
Please enter your age!	…………
What is your major?	0 Business/Management 0 Economics 0 Psychology 0 Maths 0 Physics 0 Other
This is your ... semester at the university?	…………
Do you have a study-accompanying job?	0 Yes, student job 0 Yes, during the holidays 0 Yes, part-time (whole year) 0 Yes, full-time (whole year) 0 No
Please, state your opinion to the following two statements on a scale from 1 to 10. **Statement A:** The mechanism which determined my income for the job was unfair given the income of the other participant! **Statement B:** The mechanism which determined my income for the job was fair given the income of the other participant!	Scale from 1 - 10 with 1 strong agreement with statement A and 10 strong agreement with statement B
Please, state your opinion to the following two statements on a scale from 1 to 10. **Statement A:** People comply with legal norms! **Statement B:** People do not comply with legal norms!	Scale from 1 - 10 with 1 strong agreement with statement A and 10 strong agreement with statement B

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CURRICULUM VITAE

PERSONAL DETAILS

Name:	Matthias Josef Praxmarer
Date of birth:	August 7, 1987
Place of birth:	Innsbruck, Tyrol, Austria

EDUCATION

2015 – 2018	Ph.D. student, University of Cologne
2015	M.Sc in Applied Economics, University of Innsbruck, Austria
2013	B.Sc in Management and Economics, University of Innsbruk, Austria
2007	Matura (Abitur equiv.), Bundeshandelsakademie, Imst (Austria)

ACADEMIC POSITIONS

2017 – present	Research Fellow, Experimental Economics Group, Max Planck Institute for Research on Collective Goods, Bonn
2015 – 2017	Research Assistant, Economics: Design and Behavior, University of Cologne
2014 – 2015	Research Assistant, Chair of Prof. Dr. Loukas Balafoutas, University of Innsbruck

NON-ACADEMIC POSITIONS

2008 – 2011	Finance Department and Controller, MS Design GmbH
2008 – 2008	Skiing instructor, Ski school Obergugl
2007 – 2008	Military Service (Austrian Armed Forces)

REFEREEING

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Erklärung

nach §8 der Promotionsordnung vom 17. Februar 2015

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