Social Context and Behavior under Uncertainty

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Introduction

This thesis consists of five chapters that investigate decision-making under different kinds of uncertainty and how behavior under uncertainty interacts with the social context. The foundation of each study is either a laboratory or a field experiment. They further have an interdisciplinary approach in common: each chapter makes use of manipulation techniques adopted from social psychology or proposes also psychological explanations to structure observed effects (or both). Specifically, the studies presented here provide various examples on how *cognition research* can be integrated into economic reasoning.

Three kinds of uncertainty are considered in this thesis: natural, strategic and contextual uncertainty. The first two chapters deal with ‘natural uncertainty’. Under natural uncertainty, the uncertainty decision makers face results from random draws of nature with the ex-ante distribution often being known. In particular, in Chapter 1, we make use of an information cascade game to test whether a shared social identity may help experimental participants learn about an uncertain true state of the world from observing others’ behavior. In Chapter 2, we investigate whether and how outcome and procedural fairness affect risk taking in a social context and how this compares to individual decision-making under risk. The next two chapters deal with ‘strategic uncertainty’. Under strategic uncertainty, the uncertainty is not due to a random draw of nature but due to the behavior of other human decision makers. Therefore, under strategic uncertainty, the probability distribution with regard to the actions one might face is not known; instead, one has to form a belief about it. Chapter 3 presents the results of an experiment that investigates anchoring and focality in minimum effort games and tests how the corresponding effects interact with the degree of strategic uncertainty. Chapter 4 makes use of a stag hunt game to observe whether a strategic setting with aligned interests triggers per se more cooperation compared to an otherwise identical setting involving natural risks. The final chapter deals with ‘contextual uncertainty’ which is, e.g., caused by context-specific norms or rules. Specifically, in Chapter 5, we discuss the results of a field experiment where we induce indirect information with regard to the norm in a voluntary payment setting while observing the behavior of customers.

In the following, I provide a short summary of each chapter of the thesis, focusing on the research ideas, the experimental designs and the main findings.

Chapter 1. This chapter is entitled *“Group identity as a lubricant for social learning”* and it is joint work with Sebastian Berger and Axel Ockenfels. All authors contributed equally to the project.[1] In this research, we combine an information cascade game with the recent literature emphasizing the importance of group identity in economic interaction. In the information cascade game, decision makers have to guess an uncertain state of the world and receive a positive payoff if correct. In order to make their guess, they receive a private but noisy signal about the true state and also learn about the guesses of prior decision makers. Importantly, under specific circumstances, it is rational not to choose the private signal but to follow the behavior of others. However, much of the previous literature indicates that experimental participants follow too little and stick to their private signal too often. We aim at testing whether this reluctance to follow others can be mitigated by a shared group identity.

We conduct a laboratory experiment inducing different identities and vary whether participants choose right after someone who has the same identity or after someone who has a different one. We find strong support for the idea that people are more willing to follow others

[1] All authors were equally involved in generating the ideas regarding research question, experimental design, paper design and statistical analyses. The experiment was planned and conducted by Sebastian Berger receiving feedback from Christoph Feldhaus. Statistical analyses were carried out by Christoph Feldhaus with help from Sebastian Berger. All authors were equally involved in writing this draft.

with whom they share an identity. We put forward a cognitive reasoning behind this finding which cannot be explained by theories suggesting different degrees of social preferences towards in- and outgroup members.

Chapter 2. The title of this chapter is *"Social risk taking: Unequal outcomes and fair procedures"*. It is joint work with Gary Bolton and Axel Ockenfels. All authors contributed equally to the project.[2] It investigates the effects of outcome and procedural fairness in social risk taking and tests whether the reluctance to choose social risks that cause unequal outcomes can be mitigated by a fair decision procedure.

We set up a laboratory experiment where participants decide between a sure payoff and a lottery either only for themselves or on behalf of their group. In case of taking the decision on behalf of the group two factors are varied: first, risks are either correlated or uncorrelated and second, the mechanism that aggregates the will of the group is either more or less (procedurally) fair. The first variation alters whether outcomes are equal or unequal ex-post whereas the second variation alters the extent to which all members of a group can equally participate in arriving at a joint decision. The experimental data provide no evidence that a fair procedure may substitute for a fair outcome. In contrast, we observe evidence that both a fair outcome *and* a fair procedure are required to foster social risk taking and to make decision makers choose according to their private risk preferences when deciding for their group. This suggests that outcome and procedural fairness are complements rather than substitutes in social risk taking.

Chapter 3. This chapter is entitled *"Anchoring vs focality in coordination: Evidence from minimum effort games"*. It is single-authored. In this paper, I investigate the extent to which private vs public anchors affect decision-making in social interaction and discuss how this is

[2] All authors were equally involved in generating the ideas regarding research question, experimental design, paper design and statistical analyses. The experiment was planned and conducted by Christoph Feldhaus. Statistical analyses were carried out by Christoph Feldhaus. The current draft was mainly written by Christoph Feldhaus.

related to focality. I further test whether the scale of such anchoring effects interacts with the degree of strategic uncertainty. Thereby, I am able to observe (1) whether also cognition plays a role in the effects of focal points and (2) whether the extent of the respective reactions increases in the degree of strategic uncertainty.

I set up a minimum effort game experiment varying whether subjects are provided with a private or a public anchor, the height of the anchor and the gains from coordination and the losses from miscoordination. I find that decision makers react to both mere private but even more to public anchors. This suggests that cognition as well as strategic reasoning play relevant roles when decision makers are confronted with focal points. I further find that the extent to which subjects react to anchors depends on the gains and losses from (mis)coordination, indicating the importance of the degree of strategic uncertainty for the efficacy of anchoring effects in social interaction.

Chapter 4. The title of the fourth chapter is *"Social interaction promotes risk taking in a stag hunt game"*[3] and it is joint work with Gary Bolton and Axel Ockenfels. All authors contributed equally to the project.[4] The aim of this paper is to investigate whether and how risk taking in a stag hunt game differs from an otherwise identical setting where the risk is due to nature. Previous literature suggests that people are more reluctant to choose the risky option in a trust game when the decision to reward trust is taken by a human player rather than by nature, an effect often referred to as "Betrayal aversion". However, importantly, the trust and the stag hunt game differ in how the involved players have conflicting or aligned interests from choosing to cooperate.

In fact, in an experimental stag hunt game, we find just the opposite of what has previously been observed in the trust game, i.e., that subjects are more willing to take a risk in case it is

[3] This chapter is published as Bolton, G., C. Feldhaus, and A. Ockenfels (forthcoming). Social interaction promotes risk taking in a stag hunt game. *German Economic Review*.

[4] All authors were equally involved in generating the ideas regarding research question, experimental design, paper design and statistical analyses. The experiment was planned and conducted by Christoph Feldhaus. Statistical analyses were carried out by Christoph Feldhaus. All authors contributed equally to writing the paper.

caused by another human player rather than by nature. Our joint explanation for these two findings (less risk taking in the trust game and more risk taking in the stag hunt game) is that the stag hunt game triggers a different mode of information processing in the sense that cooperative games make the interpretation of a situation more optimistic compared to games that involve a conflict of interest which make the interpretation more pessimistic. We argue that this is evidence suggesting that the way decision makers process information can be affected by the mere structure of a game.

Chapter 5. This chapter is entitled *"Norm uncertainty and voluntary payments in the field"* and it is joint work with Tassilo Sobotta and Peter Werner. All authors contributed equally to the project.[5] In this research, we conduct a field experiment investigating the effect of indirect information about a norm in an environment where people are likely uncertain about the relevant norm. Specifically, in a voluntary payment setting, we vary the scales shown to customers in a publicly announced survey asking them about their opinion with regard to the appropriate payment for a service – the values in one scale are rather low whereas the values in the other scale are rather high.

We find a strong effect of this variation in the sense that customers pay substantially more for the service when confronted with a scale suggesting high values. In order to test whether this effect is indeed driven by uncertainty about the norm, we set up a second experiment providing customers with an additional piece of explicit norm-relevant information. We find that the effect of the scale is mitigated when the explicit information is provided. In addition, we find that the effect of the scale-variation tends to be amplified in a social environment. Our study hence suggests that uncertainty about norms can make decisions very volatile and easily affected by minor variations in the context.

[5] Christoph Feldhaus and Peter Werner generated the research question. The location was organized and the experiment implemented and conducted by Christoph Feldhaus and Tassilo Sobotta. Statistical analyses were carried out by Christoph Feldhaus and Peter Werner. Christoph Feldhaus and Peter Werner wrote the current draft receiving feedback from Tassilo Sobotta.

In sum, these five studies emphasize the relevance of the social context for behavior under uncertainty. Importantly, while some of the observed effects are not in line with standard economic theory, various examples are provided where cognition research can help structure empirical findings. Thereby, this thesis highlights the role of interdisciplinary approaches for the progress of behavioral economics.

Chapter 1

Group identity as a lubricant for social learning*

Co-authors: Sebastian Berger and Axel Ockenfels

Abstract

We investigate the impact of group identity on social learning in a laboratory information cascade game. We find that subjects are more likely to follow choices made by ingroup decision makers than choices made by outgroup decision makers. This can be explained with the help of recent social cognition research, which suggests that group identity may serve as a lubricant for social learning.

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1. Introduction

Individual decisions rarely take place in a social vacuum: instead they are most often influenced by others' advice and behavior. In many situations decision makers therefore face a trade-off between sticking to their own assessment of the state of the world and following the advice or behavior of others. A well-known vehicle to study this conflict in a laboratory environment is the information cascade game pioneered by Anderson and Holt (1997). In this game players must guess the true state of the world based on a noisy private signal and, additionally, on what they know about others' prior behavior, which in turn may be based on their respective private signals.

In the present research, closely adopting the experimental design by Anderson and Holt (1997; see Fahr and Irlenbusch 2011, for a similar setting), we consider a situation with two possible states of the world, which are either a RED urn or a BLACK urn, both being drawn with equal probability. Each member of a six-player group sequentially receives a private and stochastic signal about the true state of the world. The signal is either a red ball or a black ball, with the probability of RED (BLACK) conditional on drawing a red (black) ball being equal to 2/3. After receiving her private signal, each player bets on one of the two states of the world, which gives her a fixed payoff if correct. Apart from their private signals, players know the history of choices of all prior decision makers.

Standard economic theory assumes that players update their beliefs about the true state of the world according to Bayes' rule, further assuming that this is common knowledge among all players. However, empirical research on information cascade decision-making shows that learning from others' behavior (what might be called "social learning") and from one's private signal often deviates from Bayesian predictions. [1] Furthermore, studying a meta-

[1] See Bannerjee (1992) and Bikchandani et al. (1992) for theory, and Huck and Oechssler (2000), Hung and Plott (2001), Çelen and Kariv (2004), Kübler and Weizsäcker (2004), Drehmann, Oechssler and Roider (2005), Kübler and Weizsäcker (2005), Dominitz and Hung (2009), and Fahr and Irlenbusch (2011) for laboratory evidence.

dataset consisting of the results of 13 similar experiments on information cascade decision-making, Weizsäcker (2010) finds that subjects do not only fail to do Bayesian updating but that they are also too reluctant to discard their private signal empirically. That is, they err in sticking to their private signal when it actually would have been empirically optimal (in terms of expected utility) to follow their predecessor's choice (see also Ziegelmeyer et al. 2013). Similarly, Kübler and Weizsäcker (2004) find that many decision makers purchase private signals even when these are uninformative in Bayesian terms. Such findings are in line with Nöth and Weber's (2003) notion of overconfidence in cascade games, as well as with Goeree et al.'s (2007) quantal response equilibrium analysis, with all these studies implying that people overweight their private signals compared to publicly provided information.

This kind of evidence raises the question which factors facilitate social learning: that is, which factors increase people's inclination to rely more on information provided by others? In this paper, we hypothesize that the social context in which a decision is taken influences the effectiveness of social learning. In particular, recent economic and psychological research suggests that group identity can play a crucial role for actual behavior in social contexts (Tajfel and Turner 1979; Akerlof and Kranton 2000; Chen and Li 2009; Mussweiler and Ockenfels 2013).

In our information cascade game, there are two different channels through which group identity can work, resulting in two different hypotheses. The first is based on Chen and Li's (2009) observation that group identity affects people's preferences and in particular promotes altruism towards ingroup members (see also Brewer 1979; McLeish and Oxoby 2007; Chen and Chen 2011; Ockenfels and Werner 2014). In the information cascade game, the only way by which a player can be altruistic is by revealing her private information, which is valuable to other group members, even when it may be profit-maximizing to do otherwise. This suggests that uniquely dealing with ingroup members in this game leads to more decisions in line with one's private signal than when also dealing with outgroup members *(Hypothesis 1).*

Our second hypothesis is based on the idea that group identity affects *information processing*. Experiments in social cognition research have shown that information processing depends on the degree to which others are perceived to be similar, which in turn translates into systematic differences of judgment and behavior (Mussweiler and Ockenfels 2013, and the references therein). One reason is that similarity induces less judgmental uncertainty and a more 'trusting mindset' (e.g., Gino et al. 2009; Mussweiler and Posten 2012; Bolton et al. forthcoming). Because a common group identity is an important source of interpersonal similarity (Brewer 1979), this line of research suggests that information generated by ingroup members is more strongly relied upon than information provided by outgroup members in our context of an information cascade game. That is, subjects are expected to be more likely to follow an ingroup member's choice than an outgroup member's choice *(Hypothesis 2)*.

Our null hypothesis is that group identity does not affect the weights put on private and others' information, and thus suggests no difference between treatments that vary only in the degree of shared identity. In fact, neither standard theory, nor quantal response equilibrium or overconfidence would predict a difference with regard to identity, although the latter models would in principle allow for different degrees of overweighting one's private signal in different social contexts.

Our data show that subjects are significantly more likely to follow an ingroup member's choice than an outgroup member's choice. This is consistent with *Hypothesis 2* but inconsistent with our other hypotheses. We conclude that group identity may thus serve as a lubricant for social learning as decision makers generally tend to follow too little (as, e.g., suggested by Weizsäcker 2010).

2. Experimental design and procedure

144 subjects (6 sessions of 24) participated in a two-treatment between-subjects experiment on the effect of group identity on social learning. The experiment consisted of three stages: a

group-formation task, an identity-enhancement task, and an information cascade game. The group-formation and identity-enhancement tasks were adopted from previous research on group identity (Tajfel et al. 1971; Chen and Li 2009; Chen and Chen 2011). The information cascade game was adopted from Anderson and Holt (1997).

Group formation and identity enhancement. The group formation task involved the review of five pairs of paintings by the artists Klee and Kandinsky, with one piece in each pair by Klee and the other by Kandinsky. Under uncertainty about the painter of the pictures, subjects were asked to indicate their preferred painting in each pair. Based on this preference, they were assigned to one of two groups: the Klee group or the Kandinsky group, with each group consisting of the 12 subjects who had a relatively stronger preference for the respective painter.

Subsequently, all 12 members from the same group engaged in a ten-minute chat to increase identity concerns (as in Chen and Li 2009). Their task was to discuss an additional painting and to assess who the painter of this artwork might be. Subjects could exchange information via a computerized chat-program, but in the end, had to make a private guess. If correct, they received another 1.00€ as additional compensation but were only notified about the correctness of their guess after the experiment.

Information cascade game. In the third stage, subjects were re-assigned to groups of six. Groups were either formed by members of the same identity group only (i.e., all sharing the preference for the same artist and having been in the same chat), called *same-identity* groups, or by three members from either group, called *mixed-identity* groups. In mixed-identity groups, a member of the Klee group was *always* followed by a member of the Kandinsky group in the information cascade game, and vice versa. Thus, each decision maker was placed right after an outgroup decision maker. In contrast, in same-identity groups, all decisions in a group were taken by ingroup members, sharing the same induced identity. Subjects were informed about the group composition and the positioning of ingroup and outgroup members in the base game.

Our base game closely replicated the laboratory information cascade game presented in Anderson and Holt (1997). At the start of the base game, the experimenters secretly rolled a die to determine which of two urns would be used for the respective period. Urn RED contained two red balls and one black ball, urn BLACK contained two black balls and one red ball. Urns and balls were uniform in size, color, and weight. Urn RED was used if the role of the die displayed an even number, urn BLACK if it displayed an odd number. Subjects in each group were approached individually, one after the other, to observe one private draw from the urn, with replacement. After drawing, subjects could privately record what they saw on sheets of paper and had to make a decision on which urn to bet. Unobserved by the experimenter, subjects entered their decisions into a computer. This information was shared with the other five subjects in the group. Thus, each subject knew her own private signal and everyone knew the decisions of all prior decision makers. However, the private signals of prior subjects were not disclosed.

As in Anderson and Holt (1997), the base game was repeated for 15 periods. The order of positions was counterbalanced across periods so that subjects made decisions in all positions. In each period, after all six subjects had made their decision, the true urn was publicly revealed and subjects received 1.50€ into their account if they had guessed the true state of the world in the corresponding period.

Procedure. The experiment was conducted in May of 2012 in the Cologne Laboratory for Economic Research (CLER). Participants were invited via ORSEE (Greiner 2015) from the student body of the University of Cologne and the experiment was programmed with zTree (Fischbacher 2007). The experiment took approximately 90 minutes; the average payoff from the game was 14.63€ plus a show-up fee of 2.50€ and an additional 1.00€ if the picture after the chat was attributed to correct artist. Participants (33% males, average age 23) were recruited from all fields of study.

3. Results

For our analyses, we closely follow Anderson and Holt's (1997) and Fahr and Irlenbusch's (2011) definitions and assumptions regarding rational behavior. In the information cascade game, a cascade can occur when it is optimal for a decision maker to discard her own signal and to follow the decision of her predecessor. Therefore, observations caused by rationally starting or continuing information cascades are not included into Bayesian updating. Importantly, we follow Anderson and Holt (1997) in assuming that (irrationally) breaking a cascade, however, yields an informative signal. That is, we assume that subjects expect cascade breakers to report their true signal. It is finally assumed that decision makers follow their own signal if they are indifferent in Bayesian terms.

A first look at the data. In total, we conducted 3 sessions per treatment with 4 independent groups each, which makes 12 independent groups per treatment. This yields a total of 2,160 bets on either the RED (R) or the BLACK (B) urn, 1,877 (86.9%) of which are in line with Bayesian updating. While bets in line with Bayesian updating are very common when it is rational to stick to one's own signal (89.9%), rationality is substantially lower when it is optimal to discard it (70.2%).[2] This difference is highly significant using a Wilcoxon signed ranks test (WSR) on the level of independent groups ($p_{WSR} < 0.01$, two-tailed). It further supports the observation that subjects are more likely to behave rationally when the rational decision coincides with their privately observed signal, as emphasized by Weizsäcker (2010), Nöth and Weber (2003) and Goeree et al. (2007).

[2] These values are close to those found in previous experiments on information cascade decision-making, e.g., in Anderson and Holt (1997), Alevy et al. (2007), and Fahr and Irlenbusch (2011).

According to *Hypothesis 1* subjects should be more likely to stick to their own signal when being in a same-identity group, as this altruistically provides useful information to their group. Overall, we observe that in 80.7% of all 2,160 decisions subjects stick to their private signal.[3] This share is slightly and insignificantly smaller in the same-identity groups than in mixed-identity groups (80.1% vs. 81.4%; p_{MWU} = 0.58, two-tailed, based on independent groups). Moreover, altruistic motives should be most relevant for behavior of those decision makers in the second position, who observe a different signal compared to the first decision. The reason is that here a purely selfish decision maker would be indifferent, so that altruism has its best chance to emerge. However, the share of these choices that is consistent with one's own signal is with 84.3% slightly and insignificantly smaller in same-identity groups than the 88.6% that we observe in mixed identity groups (p_{MWU} = 0.67, two-tailed, based on independent groups). We conclude that there is no support for *Hypothesis 1* in our data.

According to *Hypothesis 2*, the overweighting of one's own private signal is moderated by the social context and elevated in mixed-identity groups. Overall, participants in same-identity groups follow their predecessors' decisions significantly more often (74.1%) than participants in mixed-identity groups (64.7%; p_{MWU} = 0.02, two-tailed, based on independent groups).[4] These findings are in line with *Hypothesis 2,* supporting the view that subjects in our two treatments process the information about the behavior of prior decision makers in different ways.

Parametric analyses. Parametric statistics confirm these findings. As suggested by Weizsäcker (2010), we make use of linear probability models (LPM) that provide an easier

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[3] When testing for *Hypothesis 1* only the first five decision makers have an incentive to stick to their signal as the last decision maker cannot provide useful information to subsequent ones. Similarly, when it comes to *Hypothesis 2,* only decision makers from the second position onwards should be affected by the treatment variation. For simplicity, however, we will conduct our analyses with the whole sample. Our results do not change when restricting the sample to the relevant decision makers.

[4] The difference can be separated into two observations. First, decision makers show a lower propensity to choose private signals in same-identity groups in those cases in which their signal differs from the decision of the predecessor (53.9% vs. 66.8%; p_{MWU} = 0.01, two-tailed). Second, they tend to be more likely to choose their signal if it is in line with the decision of the predecessor, though this effect is only weakly significant (92.8% vs. 89.6%, p_{MWU} = 0.08, two-tailed).

interpretation of interactions. Table 1 shows the results of two models estimating the probability that decision makers choose the same color as their direct predecessor. In Model 1, we find that subjects in same-identity groups are more likely to choose the same color as their predecessor. Model 2 controls linearly for the period a decision is taken in as subjects might learn to follow, includes dummies for the position of the respective decision maker within the group in the sequential game and a variable indicating whether a decision maker's private signal is identical to the choice of the direct predecessor (Signal-choice congruence). We find that the controls do not change the main result that the social context affects people's propensity to follow. The estimated probability to follow is 7.2 percentage points higher in same-identity groups compared to mixed-identity groups.

TABLE 1. LPM MODELS PREDICTING THE PROBABILITY THAT DECISION MAKERS CHOOSE THE SAME COLOR AS THEIR PREDECESSOR.

Dependent var.: **Follow**	(1)	(2)
Same identity	0.094***	0.072**
	(0.036)	(0.030)
Signal-choice congruence	--	0.519***
		(0.025)
Position dummies	--	Yes
Period control	--	Yes
Subjects	144	144
Observations	1800	1800
R-squared overall	0.011	0.034

Notes: Results obtained from random effects LPM models, Same choice as predecessor (Follow, 1 = yes, 0 = no) serves as the dependent variable, Same identity (1 = yes, 0 = no), Signal-choice congruence (1 = Signal is equal to choice of predecessor, 0 = Signal is different from choice of predecessor), Position dummies, and Period (from 1 to 15) serve as independent variables; standard errors clustered on group level are presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level.

Table 2 shows the results of further LPM models estimating the probability that the BLACK urn is chosen. In both models, a subject's private signal, the choice of the direct predecessor, the group-composition (dummy 'Same-identity') and the interaction between the choice of the direct predecessor and the group-composition are included as explanatory variables. In Model 1, we additionally control for the rational posterior belief up until the direct predecessor (as this choice is added to the model as the 'predecessor's decision'), assuming that subjects applied Bayesian updating as defined above. Model 2 instead controls for the color chosen in the first position, as this decision might play an important role for a simpler decision heuristic. In both models, our main variables of interest are the group-composition (Same identity) and the interaction of the predecessor's choice and the group-composition. If subjects differed in their propensity to use the publicly provided information by their predecessor conditional on a shared group identity (*Hypothesis 2*), we should expect a significant effect of both variables. While we expect a negative effect of 'Same identity' on the probability to choose BLACK (as this implies cases where the predecessor has the same identity and chose RED), we expect a positive effect of 'Predecessor decision BLACK x Same identity' (as this implies cases where the predecessor has the same identity and chose BLACK). In addition, the joint effect of both is expected to be significant.

TABLE 2. LPM MODELS PREDICTING THE PROBABILITY THAT BLACK IS CHOSEN.

Dependent var.: **BLACK**	(1)	(2)
Signal black	0.495***	0.508***
	(0.026)	(0.029)
Predecessor decision BLACK	0.123***	0.161***
	(0.033)	(0.046)
Same identity	-0.084***	-0.100***
	(0.028)	(0.035)
Predecessor decision BLACK x Same identity	0.098**	0.123**
	(0.045)	(0.057)
Posterior T-2	0.803***	--
	(0.038)	
First choice BLACK	--	0.177***
		(0.026)
Joint p-value	0.008	0.016
Subjects	144	144
Observations	1800	1800
R-squared overall	0.506	0.443

Notes: Results obtained from random effects LPM models, choice (1 = black, 0 = red) serves as the dependent variable, private signal (1 = black, 0 = red), the choice of the predecessor (1 = black, 0 = red), Same identity (1 = yes, 0 = no), the interaction between the prior decision and the same identity variable (1 = yes, 0 = no) and first choice (1 = black, 0 = red) as independent variables. Posterior T-2 denotes the information set available for the predecessors' decisions. Joint p-value is Wald test measuring the joint significance of 'Same identity' and 'Predecessor decision BLACK x Same identity'; standard errors clustered on group level are presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level.

Model 1 shows, not surprisingly, that a subject who receives the black signal is more likely to choose the BLACK urn and vice versa. The posterior regarding BLACK, which summarizes the public information until the second to the last position, also positively affects the likelihood to choose the BLACK urn ($p < 0.01$, two-tailed).

Moreover, the predecessor choosing BLACK ($p < 0.01$, two-tailed) also increases the respective probability. Importantly, both the variable ‘Same identity’ ($p < 0.01$, two-tailed) and the variable ‘Predecessor decision BLACK x Same identity’ ($p < 0.05$, two-tailed) show that the effect of the predecessor’s choice differs conditional on the group-composition. We find the same results when replacing the posterior by the choice of the first decision maker in Model 2.[5] This implies that a decision maker in the same-identity treatment is less likely to choose BLACK when her predecessor chose RED and more likely to choose BLACK when her predecessor chose BLACK, both compared to the mixed-identity treatment. Similar to our initial findings on the propensity to follow, the difference in the likelihood to choose a respective color conditional on the identity of the predecessor is between 8.4 and 12.3 percentage points. Overall, as predicted by *Hypothesis 2*, the influence of others’ decisions depends crucially on the social context in terms of a shared group identity.

Does a same-identity context promote smarter decisions? The previous analyses show that sharing the same identity facilitates following other’s choices in the information cascade game. Because earlier studies can be summarized as collectively showing that subjects tend to overly stick to their private signal compared to what would have been empirically optimal (e.g., Weizsäcker 2010), our study hence suggests that a shared group identity may improve decision-making.

That said, we caution that we did not find evidence in our data that a shared group identity makes decision makers more rational in the sense that the increase in the propensity to follow is stronger in those cases where it is actually *ex ante* or *ex post* beneficial to follow. Taking all 2,160 decisions into account, and following Anderson and Holt’s (1997) prescription for

[5] The result generally translates into models where information revealed not only by one’s direct predecessor but also by earlier predecessors is considered (see Table A.1 in Appendix A for details).

rational behavior, discarding one's private signal to follow a rational cascade is recommended in 329 situations, 178 times in same-identity groups and 151 times in mixed-identity groups. If we restrict our analysis to these situations, we replicate the previous finding that decision makers discard their signal (and thus follow their predecessor) more often in same-identity groups (74.2%) than in mixed-identity groups (65.6%). However, decision makers in same-identity groups also more often discard their signal and follow their predecessor when they should rationally stick to their own signal (18.7% of 182 cases in same-identity groups vs. 13.4% of 247 cases in mixed identity groups). Similarly, in situations in which it is optimal to follow the predecessor's choice based on the *ex post* actual state of the world (as opposed to *ex ante* Bayesian decision-making), we find that decision makers are more likely to follow in same-identity groups (79.5% of 596) than in mixed-identity groups (72.7% of 575). While the difference seems larger when it is *ex post* optimal to not follow (63.5% of 304 in same-identity groups vs. 50.5% of 325 in mixed-identity groups), LPM models investigating the propensity to follow do not reveal a significant interaction of group identity and the *ex ante* or *ex post* rationality of following (see Appendix A Table A.2). This suggests that, while group identity generally promotes following others, it does not necessarily help decision makers to better identify situations where following is actually a good choice.

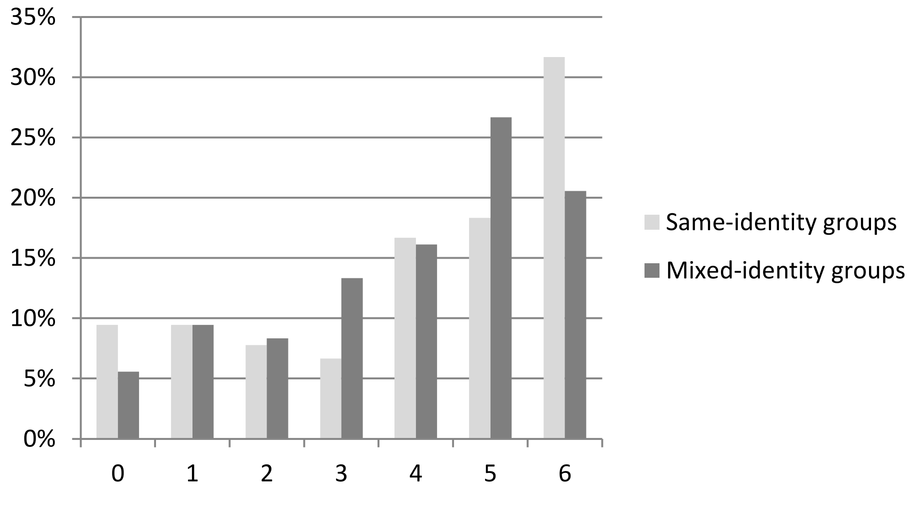

FIGURE 1. SHARE OF PERIODS WHERE 0, 1, … , 6 DECISION MAKERS CHOOSE THE RIGHT URN (IN PERCENT).

There is also no evidence that same-identity groups collectively reveal more private information and in this sense are more efficient (see our tests regarding *Hypothesis 1* that show no general difference in the frequency of private information revelation). The different degrees of following that we observe rather translate into treatment-specific variations of herding. Figure 1 illustrates this. The numbers on the horizontal axis denote the share of periods where 0, 1…, 6 decision makers choose the *ex post* right urn. We find that same-identity groups herd substantially more as indicated by the high share of periods in which all group members choose the same color (41.1% in same-identity groups vs. 26.1% in mixed-identity group; $p_{MWU} = 0.05$, two-tailed). Yet, this substantial increase of herding is observed for both the right *and* the wrong color (as suggested by a non-significant interaction between choosing the right urn and the treatment condition in Model 4 in Table A.2; $p = 0.12$, two-tailed).[6]

4. Conclusion

Motivated by recent social cognition research and the previously observed failure of subjects to sufficiently rely on others, we investigate the hypothesis that group identity promotes social learning. We find strong support for our hypothesis. The propensity to rely on one's own information compared to information provided by others is significantly reduced among ingroup members. In that sense, in the context of information cascade games, a shared group identity facilitates social learning and might thus help to de-bias individual decision-making in contexts where excessive reliance on one's own signal is often observed.

Our findings also extend the social identity literature by emphasizing that salient identity concerns do not only affect subjects' preferences, but may also influence their information

[6] Accordingly, we find that the variance of choices is much higher in the mixed-identity treatment, where variance is measured by a variable with four values: 0 = all chose the same color, 1 = one chooses the one and five the other color, 2 = two choose the one and four the other color, 3 = each color is chosen by three decision makers ($p_{MWU} = 0.03$, two-tailed).

processing. The latter seems particularly important for decision-making under uncertainty where the credibility of social information is a key determinant of subsequent behavior. This holds not only for financial decision-making among ingroup or outgroup members, as captured by our information cascade experiment, but also for other economic contexts such as online market places and social networks with recommender or feedback systems.[7] This observation connects the economic group identity literature to the recent research that highlights the relevance of social cognition research for economic decision-making (e.g., Mussweiler and Ockenfels 2013; Bolton et al. forthcoming).

[7] EBay, for instance, attempts to emphasize the "community" aspect of their marketplace, which may not only increase trust and trustworthiness as suggested by the work of Chen and Li (2009), but also promote stronger reliance on the feedback provided by others.

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Appendix

A. Additional analyses

More public information. In the main body of the manuscript, we focused on the impact of the direct predecessor on a decision maker's behavior as this represents the most salient identity-relevant decision. However, public information in the information cascade game does not only come from the last but from all prior decisions. In Table A.1, we present LPM models that include more public information.

TABLE A.1. LPM MODELS PREDICTING THE PROBABILITY THAT BLACK IS CHOSEN.

Dependent var.: **BLACK**	(1)	(2)
Signal black	0.502***	0.555***
	(0.029)	(0.025)
Share of BLACK choices	0.395***	--
	(0.043)	
Posterior	--	0.749***
		(0.079)
Same identity	-0.108***	-0.134**
	(0.033)	(0.052)
Share of BLACK choices x Same identity	0.137**	--
	(0.057)	
Posterior x Same identity	--	0.211**
		(0.103)
Subjects	144	144
Observations	1800	1800
R-squared overall	0.460	0.513

Notes: Results obtained from random effects LPM models, choice (1 = black, 0 = red), Share of BLACK choices and Posterior are continuous variables, Same identity (1 = yes, 0 = no); standard errors clustered on group level are presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level.

In Model 1 the share of prior choices for the black urn as well as the group composition and the interaction of both are included. Again, a positive effect of the interaction is found. This suggests that the positive effect of a higher share of choices for the black urn on subjects' propensity to choose BLACK is stronger in the same-identities treatment. This, in turn, supports the idea that public information is used less when it is (also) due to outgroup members. We find the same when we replace the share of choices by the expected probability, applying Bayesian updating. This is shown in Model 2.

Better decisions. Model 1 below takes Model 2 from Table 1 and adds a variable that indicates whether it is rational in Bayesian terms (*ex ante*) to follow the predecessor in a specific situation – with the effect being highly significant. However, we find no significant effect of the interaction with our treatment manipulation, indicating that the higher willingness to follow does not interact with the rationality of following. Similarly, looking at cases where it is rational to follow *ex post*, we neither find an effect on the likelihood of following conditional on this being optimal *ex post* or not.

TABLE A.2. LPM MODELS PREDICTING THE PROBABILITY THAT DECISION MAKERS CHOOSE THE SAME COLOR AS THEIR PREDECESSOR.

Dependent var.: **Follow**	(1)	(2)	(3)	(4)
Same identity	0.047* (0.025)	0.062* (0.033)	0.071** (0.028)	0.114*** (0.042)
Rational to follow	0.533*** (0.025)	0.541*** (0.051)	--	--
Same identity x Rational to follow	--	-0.020 (0.051)	--	--
Ex-post rational	--	--	0.069*** (0.023)	0.101*** (0.040)
Same identity x Rational to follow	--	--	--	-0.065 (0.042)
Signal-choice congruence	0.218*** (0.034)	0.218*** (0.034)	0.504*** (0.026)	0.504*** (0.026)
Position dummies	Yes	Yes	Yes	Yes
Period control	Yes	Yes	Yes	Yes
Subjects	144	144	144	144
Observations	1800	1800	1800	1800
R-squared overall	0.461	0.461	0.343	0.344

Notes: Results obtained from random effects LPM models, Same choice as predecessor (Follow, 1 = yes, 0 = no) serves as the dependent variable, Same identity (1 = yes, 0 = no), Rational to follow (1 = yes, 0 = no), Ex-post rational (1 = yes, 0 = no), Signal-choice congruence (1 = Signal is equal to choice of predecessor, 0 = Signal is different from choice of predecessor), Position dummies (2 to 6), and Period (from 1 to 15) serve as independent variables; standard errors clustered on group level are presented in parentheses.*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

B. Instructions given to subjects in the experiment

A: Group identity task[8]

Welcome and thank you very much for participating in our experiment. Please do not communicate with any of the other participants from now on until the end of the experiment. Please keep your cubicle free of any materials that have nothing to do with current experiment and switch of your mobile phones. If you do not stick to these rules, we have to exclude you from the experiment and any payoffs.

Please read the instructions carefully. If you have questions after reading the instructions or during the experiment, please raise your hand. One of the experimenters will then come to you in order to answer your question in private. You can earn money in this experiment. How much you earn depends on your decisions and on chance. Additionally, you receive a show-up fee of 2.50€. You do not have to tell anyone how much you have earned in the experiment. All participants received identical instructions.

In the first part of the experiment you are shown 5 pairs of paintings. Each time, one of the two paintings is by Wassily Kandinsky while the other is by Paul Klee. For each pair you are asked which of the two paintings you prefer. Based on your preferences for the paintings compared to the other participants of the experiment you will become a member of one of two groups.

Subsequently, you are asked whether another painting is either by Wassily Kandinsky or by Paul Klee. If you attribute the painting to the right painter you receive an additional payment of 1€. You will learn after the end of the experiment whether you attributed the painting to the right painter. While making your decision, you can discuss the issue with the members of your group with whom you will be matched based on your picture preference. You will only chat with members of your group.

Please obey the following rules while participating in the chat:

1. Do not disclose your identity (Name, age, gender, studies etc.).

2. Please abstain from abusive and aggressive language.

[8] Instructions for the group identity task were the same for all subjects.

B: Information Cascade Game[9]

These are the instructions for the second part of the experiment. Please read the instructions carefully. In the following, you interact with five participants in a group of six. All members of your group receive the same instructions. You interact only with participants who belong to the same group as you do, which is either Klee or Kandinsky. The second part of the experiment consists of 15 periods in total. In each period you are asked to guess from which of two randomly drawn urns a ball stems.

Content of urn RED	**Content of urn BLACK**
two red balls	**one** red ball
one black ball	**two** black balls

Course of each of the 15 periods:

At the beginning of each period a die is thrown which is not seen by the participants. When the result of the throw is 1, 2 or 3, the RED urn is chosen. If the result if 4, 5 or 6, the BLACK urn is chosen. Hence, the probability with which each of the two urns is chosen is identical. Your task is to bet on one of two urns you believe to be the chosen one.

After the roll of the die, the content of the urn is decanted to another container. This container is used for both urns. Thus, you will draw from the same container no matter which urn is actually chosen. Subsequently, we will come to each participant subsequently. Each of you has the chance to draw one ball from the container and to look at it. Then the ball is replaced in the urn. The information about the color you have drawn should not be shared with other members of your group of six.

Your decision (not your private information) for one of the two urns however, is shown to all other members of your group of six. This information is distributed via the computer. When all members of a group have drawn a ball and taken their decision, a period is over. Those participants, who have bet on the right urn, receive a payment of 15 ECU for the respective period. An ECU is worth 0.10€.

[9] Instructions differed between treatments; these instructions are from the INGROUP treatment, the OUTGROUP treatment was analogous but differed in the group-formation as explained.

Chapter 2

Social risk taking: Unequal outcomes and fair procedures*

Co-authors: Gary Bolton and Axel Ockenfels

Abstract

Many collective decisions entail risks. We conduct a set of experiments where subjects choose between a sure payoff and a lottery either for self or on behalf of their group. We vary first the extent to which the social risk potentially creates (in-)equality and second the fairness of the choice aggregation mechanism to elicit the will of the group to take the risk or not. We hypothesize that a fair procedure may substitute for fair outcomes in collective risk taking. This is not supported in our experiment. In contrast, we observe that outcome and procedural fairness are complements rather than substitutes: while social does not differ from individual decision-making when outcomes and procedures are fair, social risk taking gets more conservative when either of them is unfair.

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1. Introduction

Many collective decisions entail risks, e.g., circumstances where taking a chance might have positive or negative implications for the welfare of the society as a whole. This, in turn, might also have important implications for the distribution of the welfare within a society ex-post. Examples include whether or not to build nuclear power plants, to risk economic or military conflicts, to engage in prevention politics, to avoid natural disasters, crime or diseases, etc. While some of these risks affect all members of a society in the same way, others affect them differently conditional on their individual realization of the respective risk.

In this paper, we examine the influence of outcome and procedural fairness on social risk taking in controlled experiments. This is compared to individual decision-making under risk. We expect that social is similar to individual risk taking when all are equally affected by the risk whereas group members should get more conservative when the risk causes unequal outcomes. We hypothesize that this reluctance to choose social risks that cause inequality can be mitigated by a fair decision procedure as a fair procedure may substitute for fair outcomes. However, this is not supported in our experiment. In contrast, we will see that there is less risk taking if either the group members can be differently affected by the risk *or* when the choice aggregation mechanism is unfair. In turn, social is found to be similar to individual risk taking when outcomes *and* procedures are fair. These results suggest that the two types of fairness are complements rather than substitutes in social risk taking.

The baseline of our study is a standard individual problem in choice under uncertainty. Here, each individual faces a choice between a sure thing and a lottery that pays either higher or lower than the sure thing. Thereby, we investigate how people decide only for themselves, in absence of any social context.

We then examine the baseline game in a group choice setting. Models of inequality aversion (such as Fehr and Schmidt 1999 and Bolton and Ockenfels 2000) imply that relative payoffs can work their way into an individual's decision to take the chance or not depending on the expected inequality the risk may cause. While we have some treatments where risks are correlated across the group, others resolve the risk individually even though the decision to take the risk or not is taken on behalf of the group as a whole. This implies no potential for inequality in the former cases while inequality is likely to be caused in the latter ones. Inequality aversion therefore predicts that risks that are correlated across risk takers should be more attractive than risks that are resolved independently.[1] To implement this, in some of the treatments in our experiment, decision makers express their option preference knowing that a separate, independent draw resolves the risk for each decision maker. Hence, risks are *socially uncorrelated*. In the other treatments, one common draw resolves the risk for all. Hence, risks are *socially correlated*.

The corresponding hypotheses, which follow from the mentioned models of inequality aversion, are that there will be less risk taking of group members in the uncorrelated treatments compared to the individual decisions (**Hypothesis 1**) and compared to the correlated treatments (**Hypothesis 2**). Furthermore, we expect that risk taking in groups under correlated risks does not differ from individual decision-making which is also in line with these models.

Besides the correlation of risk, the second factor we manipulate is the social choice mechanism that determines whether all members of a group take the risk or all do not. We aim at varying the (procedural) fairness of the mechanism. The first mechanism is a *majority vote* where each player casts her vote either for the safe or the risky option. In this case, the majority determines the relevant option for all. The second mechanism is a *dictator mechanism* where each player decides for the whole group conditional on being the dictator. While the former aggregation mechanism gives each group member "voice" and should hence be perceived as fair (e.g., Tyler et al. 1985), the latter determines one player who alone takes

[1] While the expected payoff of the lottery is the same in both treatments, expected inequality differs conditional on the resolution of the risk. Hence, assuming inequality averse preferences, expected utility differs only due to differences in expected inequality, making risks less attractive when they are resolved individually.

the decision for the whole group, neglecting the choices of all others who hence have no voice in the decision. This might be perceived as less fair. However, we observe no difference in social risk taking between these two mechanisms in our initial experiments. One potential reason for the lack thereof is that this **random** *dictator mechanism* might also be perceived as fair as all players are treated equally in having the same low chance of being chosen as the dictator. We therefore introduce a third mechanism that treats group members differently. In the *unfair dictator mechanism* players also decide conditional on being the dictator. However, in the respective treatments, the probabilities of being chosen as the dictator are different across group members. In the corresponding experiment, we observe that social risk taking indeed differs between the *majority vote* and the *unfair dictator mechanism* which seems to imply that the latter is actually perceived as less fair.

The corresponding hypothesis is that the fairness of a decision procedure matters in particular when a decision can cause unfair outcomes. We hence expect that decision makers are more willing to choose a risky option and to accept future inequality when the decision for the group is implemented by a fair procedure. The conjecture is that they are less likely to shy away from outcome inequality when individually preferring the risk in case the aggregation mechanism is equal and fair. Another way of putting this is that a fair procedure may substitute for a fair outcome. E.g., Bolton et al. (2005) show in allocation decisions that a fair lottery can overcome the objections of those who are, in relative terms, hurt by an outcome. This suggests for our setting that, when the risk causes unfair outcomes, decision makers might be more willing to choose in line with their private risk preferences, and thus more risk seeking, in case the aggregation mechanism is fair (**Hypothesis 3**).

On the other hand, if decision makers are not affected by the social context, as suggested by standard economic theory, neither the correlation of risks nor the aggregation mechanism should influence decision-making in this setting. Thus, we would not expect any difference across treatments which will serve as our null hypothesis in the following.

Our results indicate that individual and social risk taking are similar when social risks are correlated and a fair aggregation mechanism is applied. However, as soon as group members are differently affected by the risk or when the mechanism is unfair, choices get more conservative. Our research does hence not imply that a fair decision procedure mitigates the effect of outcome inequality. Instead, the results show that both fair outcomes *and* fair procedures are needed to foster social risk taking and to make individuals choose according to their private risk preferences when taking a risky decision for their group. This suggests that outcome and procedural fairness are complements rather substitutes in collective risk taking.

To our best knowledge, this study is the first to investigate (1) how risk taking in large groups is affected by possibly unequal outcomes within the group, (2) how this compares to individual decision-making and (3) how fair outcomes and fair procedures interact in social risk taking. Our experiments show that both outcome fairness and procedural fairness are important parameters in collective decision-making under risk: if either outcomes or procedures are not fair, private risk preferences seem distorted in social contexts as choices get more conservative. This potentially reduces the efficiency of public decision-making under risk as people might be less willing to choose a risky but efficient option when outcomes differ among group members ex-post or when the aggregation mechanism is unfair.

The remainder of the paper is structured as follows. In the second section, we discuss the related literature from economics, social psychology and political science. In the third section the experimental design of our first experiment is presented and the respective results are discussed. Sections four and five present the second and third experiment and their results. Section six concludes.

2. Related literature

Our research is closely related to previous literature that studies the role of outcome fairness, besides selfishness, in majority decision-making.[2] Sauermann and Kaiser (2010) find in a laboratory experiment that applying the social preference model suggested by Bolton and Ockenfels (2000) to behavioral laboratory data significantly adds explanatory power to the standard rational choice model in majority decision-making. Similarly, Tyran and Sausgruber (2006) show that a preference for fairness in collective decision-making tends to promote equality. We build on these findings and show that also *expected* inequality affects behavior in a group choice context. Our study thereby extends and confirms the role of outcome fairness in collective decision-making.

We further investigate whether a decision procedure that should be perceived as fair makes expected inequality more acceptable compared to an unfair aggregation mechanism. In this sense, we study whether a fair procedure substitutes for a fair outcome. The idea behind this variation relates to the literature on procedural fairness in social psychology and economics (e.g., Thibaut and Walker 1975; Tyler and Cain 1981; Tyler et al. 1985; Barrett-Howard and Tyler 1986; Bolton et al. 2005)[3] which emphasizes the importance of two distinct fairness dimensions: outcome fairness and procedural fairness. Examples from this literature include Tyler and Cain (1981) who show that procedures have a strong effect on leader-evaluations that is independent of outcome fairness and Tyler et al. (1985) who find that also voice, the possibility to express one's view about a decision, is closely related to the perceived fairness. Barrett-Howard and Tyler (1986) directly compare the importance of procedural and outcome fairness in allocation decisions. Their results suggest that both fairness dimensions are equally important. While the aforementioned studies investigate how different procedures and outcomes are *perceived* in terms of fairness, Bolton et al. (2005) test whether the fairness of a

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[2] See Wilson (2007) for a general overview on majority decision-making in experiments.

[3] Much of the psychological literature on procedural fairness is summarized in Lind and Tyler (1988) and Tyler and Lind (2000).

procedure also affects *behavior*. They find that procedural fairness, in their case a fair rather than an unfair lottery, substitutes to some extent for outcome fairness when it comes to allocation decisions. In sum, these studies show that not only outcome fairness is important in social contexts but also the fairness of how these outcomes came to be. This supports the idea that also social risk taking may be affected by the fairness of the aggregation mechanism.

The effects of different mechanisms in collective decision-making have also been studied in political science and political economy. The respective literature finds that a system and its decisions are perceived as more legitimate when people can actively participate in the process of arriving at a decision (Weatherford 1992; Hibbing and Theiss-Morse 2001; Gangl 2003; Frey and Stutzer 2005; Birch 2010; Olken 2010; Doherty and Wolak 2012). Our research is in particular related to studies investigating the effects of majority decision-making. Here, Dal Bó et al. (2010) show in a laboratory experiment that a policy to increase cooperation is more successful when it is decided on democratically rather than being exogenously imposed. Baldassarri and Grossman (2011) show that the legitimation of sanctioning mechanisms via majority vote leads to a more successful promotion of cooperation in public good settings as compared to a random choice mechanism. Finally, majority decisions are also found to increase welfare by installing institutions that commit societies to efficient sanctioning devices (Hilbe et al. 2013). This literature suggests that different aggregation mechanisms can have substantial effects on both fairness perceptions and outcomes. The current research extends it in investigating whether the fairness of a procedure also affects choices in social risk taking and how this interacts with outcome fairness.

Our study is further related to the literature comparing group decision-making under uncertainty to individual choices (Charness and Sutter 2012) and to the literature investigating how risk taking is affected by the social environment the decision is taken in (e.g., Bolton and Ockenfels 2010). Importantly, so far, research comparing individual and group willingness to take risks has mainly focused either on a collective or on an individual resolution of the risk and on only one or another social choice mechanism. Hence, what has been largely neglected is how the behavior of group members depends on the way they may be differently affected by a decision and on the way the will of the group is aggregated.

Notable exceptions from the former statement can be found in the literature on decision-making in social contexts, where ‘groups’ usually consist of two players or where decisions are just taken in the explicit presence of others. Here, some studies observe that correlated are preferred to uncorrelated risks. Friedl et al. (2014) find that individuals invest more in insurance when risks are uncorrelated rather than correlated while Adam et al. (2014) find that positively correlated risks are preferred to negatively correlated risks, with the individual decision lying in between. In both studies also correlated risks result in inequality as in Friedl et al. (2014) the decisions are taken only for self while in Adam et al. (2014) two types of players are involved who receive different outcomes in the good and the bad state. López-Vargas (2014) and Gaudeul (2015) show, in studies where only the expected inequality is varied between conditions, that social risks are indeed the more preferred the less inequality they cause.

To our best knowledge, the fairness of aggregation mechanisms in social risk taking has not been studied systematically. Still, several mechanisms have been used in the literature. Studies relying on one group member to make the decision come to varying conclusions, ranging from individual and group decisions are similar (Brennan et al. 2008) to social risk taking being more conservative (Bolton et al. 2015) to finding an influence that we would anticipate from social preference models (Bolton and Ockenfels 2010) to finding an influence that appears different from what social preference models would imply (Rohde and Rohde 2011[4]). When decided by consensus, group risk taking tends to be more risk averse than

[4] Specifically, Rohde and Rohde (2011) observe that their results are consistent with social preferences that are lexicographic, with the self-interest of the decider being served first.

individual risk taking[5] (Baker et al. 2008; Shupp and Williams 2008; Masclet et al. 2009; He et al. 2012). What drives this result is unclear, although inequality aversion and the social context per se are mentioned as candidates. When decisions are taken by majority vote, individuals and groups are found similar in risk taking proclivity (Harrison et al. 2013). In Reynolds et al. (2009), group decisions are taken by a third party with no material stake in the outcome. The results are similar to the consensus studies.

In sum, these two literatures show that correlated risks are preferred to uncorrelated risks and investigate how individual and group decisions under one particular aggregation mechanism compare. In the present study, we test whether the reluctance to choose risks that cause inequality extends to decision-making in larger groups and whether choices are also affected by the fairness of the decision procedure.

3. Experiment I

Experimental design. The first experiment consists of a 2*2 between-subjects design and a baseline treatment (see Table 1). In each variation, participants indicate whether they prefer a sure payoff of 6€ or a lottery that either pays 0€ or 13€ with equal probability.

In the first two treatments (*VoteCorr* and *VoteUncorr*) a majority vote determines which of the two options is realized for all participants (Voting) while in two further treatments (*DicCorr* and *DicUncorr*) the decision of one player is randomly chosen to determine the relevant option for the whole group (Dictator). If the safe option is chosen, each participant gets 6€ and if the lottery is chosen, the payoffs are determined by chance.

In the *VoteCorr* and the *DicCorr* treatments, participants are told that the lottery (if it is chosen) is conducted publicly for all participants at once (socially correlated). In the *VoteUncorr* and the *DicUncorr* treatments however, they are told that the lottery is conducted

[5] At least when the risk is high, i.e., groups are more risk averse when the probability of the good outcome is low.

independently for each participant (socially uncorrelated). We implement the lottery by throwing a six-sided die. If it shows 1, 2, or 3, all members of the group/the respective participant get(s) the high payoff and if it shows 4, 5, or 6, they/she get(s) the low one.

TABLE 1. EXPERIMENTAL DESIGN CONSISTING OF A 2*2 DESIGN VARYING THE CHOICE MECHANISM AND THE SOCIAL CORRELATION OF RISKS AND A BASELINE TREATMENT.

Experimental treatments

Correlation of risks	**Choice mechanism**		
	Voting	Dictator	Baseline
Correlated	*VoteCorr*	*DicCorr*	Individual decision without social context *Baseline*
Uncorrelated	*VoteUncorr*	*DicUncorr*	

In the *Baseline* treatment, participants have to choose only for themselves between the sure payoff and the lottery. After taking their decision, those who prefer the lottery have to state for which three numbers (either 1, 2, and 3 or 4, 5, and 6) they want to receive 13€ while they would get a payoff of 0€ if one of the other numbers was realized. Subsequently, an experimenter approaches each participant in her booth in order to determine the result of the lottery and to enter the respective number into the computer. The payoff for those who preferred the sure payoff is independent of their number while participants who chose the lottery either receive the low or the high payoff conditional on the result of their lottery and their initial choice.[6]

The experiment took place between April of 2012 and September of 2014 in the Cologne Laboratory for Economic Research (CLER). Participants were invited using ORSEE (Greiner

[6] We decided to roll the die for all participants, regardless of them taking the risk or not, so that they would know that others could not observe whether they chose the lottery or the fix payoff, making the decision more private.

2015) and the experiment was programmed with zTree (Fischbacher 2007). We conducted 12 sessions with 21 participants each, two sessions for treatments *VoteCorr*, *DicCorr* and *Baseline* and three for treatments *VoteUncorr* and *DicUncorr*. Thus, we have 252 observations in total. Before taking their decision, participants were to pass a quiz to make sure that they understood the rules of the experiment. After the experiment, they answered a short questionnaire on demographics. Each session lasted about 20 minutes. The average payoff was 7.85€ including a show-up fee of 2.50€.

Results. Across all treatments, 148 participants choose the sure thing (59%) while 104 (41%) prefer the lottery. Importantly, and contrary to what would be implied by standard theory, the share of participants that chooses the risky option differs significantly across treatments (p = 0.002, chi-squared test).

In the *Baseline* treatment, subjects take a private decision. In this case, we find that about half of them go for the risky option (52%). In the other four treatments, participants make their choices in a social context. While in the *VoteCorr* treatment 57% choose the risky option, in the *DicCorr* treatment 50% do so. In the *VoteUncorr* and the *DicUncorr* treatments the shares of decisions for the lottery are, with 35% and 24% respectively, substantially lower.

These descriptive results indicate that an individual's risk taking behavior is similar to the behavior in social contexts when risks are socially correlated while it seems to differ from behavior under socially uncorrelated risks. In fact, in the *Baseline* 52% of all decision makers go for the risky option, compared to 54% pooled across both treatments with socially correlated risks and to 29% pooled across both treatments with socially uncorrelated risks. The share of participants choosing the risky option in *Baseline* does not differ from the two treatments with socially correlated risks (p = 0.661 for *VoteCorr* vs. *Baseline*; p = 0.827 for *DicCorr* vs. *Baseline*; and p = 0.900 for a pooled set of *VoteCorr* and *DicCorr* vs. *Baseline,* two-tailed two sample tests of proportions) while it differs from the share of decisions for the lottery in the treatments with socially uncorrelated risks (p = 0.076 for *VoteUncorr* vs.

Baseline; p = 0.003 for *DicUncorr* vs. *Baseline*; and p = 0.007 for *VoteUncorr* and *DicUncorr* vs. *Baseline*, two-tailed two sample tests of proportions). These results suggest that, independent of the choice mechanism, behavior does not differ between an individual decision and a decision in a social context with socially correlated risks while decision makers are substantially more risk averse when risks are uncorrelated as this allows for unequal outcomes. This is evidence supporting **Hypothesis 1**.

We now go on to investigate how the two factors that we vary in the social treatments, the correlation of risks and the fairness of the choice mechanism, influence behavior across treatments in the 2*2 design. While the lottery is chosen by 57% of the participants in the *VoteCorr* treatment, it is chosen by only 35% in the *VoteUncorr* treatment. This is an increase of 63% due to correlated instead of uncorrelated risks. When the dictator mechanism is applied, 50% of the participants choose the risky option in the *DicCorr* treatment and 24% in the *DicUncorr* treatment. This equates to an increase of 108%, again, only due to correlated instead of uncorrelated risks. Both differences are significant (p = 0.025 for *VoteCorr* vs. *VoteUncorr*, and p = 0.006 for *DicCorr* vs. *DicUncorr*, two-tailed two sample tests of proportions). These results strongly support the idea that social risks get less attractive when they affect the members of a group differently (in line with **Hypothesis 2**).

Finally, we also find some descriptive evidence suggesting that the influence of social preferences may not be independent of the social choice mechanism: comparing the two mechanisms, we find an increase of 46% in risk taking in *VoteUncorr* compared to *DicUncorr* while we only find an increase of 14% in *VoteCorr* in relation to *DicCorr*. Still, neither of the two differences is significant on conventional levels (p = 0.171 for *VoteUncorr* vs. *DicUncorr* and p = 0.512 for *VoteCorr* vs. *DicCorr*, two-tailed two sample tests of proportions). This lends no conclusive support to **Hypothesis 3**.

Discussion. Summarizing, we find that decision makers behave similar when making an individual decision or when taking social risks that are correlated. When risks are socially uncorrelated however, they choose more conservative. Both findings are independent of the choice mechanism. No conclusive evidence is found suggesting that the reluctance to take social risks that cause inequality differs between the two aggregation mechanisms. In the following, we conduct a second experiment to get a clearer picture with regard to the respective effect.

4. Experiment II

Experimental design. The second experiment consists of two treatments which are variations of *VoteUncorr* and *DicUncorr*. It is conducted as the dependent variable of the first experiment requires very large differences between treatments and/or a very large set of data to identify an effect. Importantly, given the results of the first experiment, we can neither convincingly argue that there is no effect of the procedure nor that there is an effect, as the difference is not significant (but close to weak significance) but quite large. Therefore, we set up an experiment that is similar to the first one but makes use of a risky choice variable that allows for more variation in subjects' choices.[7]

Specifically, in this second experiment, subjects do not choose between one lottery and one sure thing but take 15 decisions in total. The sure payoff is different in each of these decisions: while it is 10€ in Decision 1, it decreases in 50-cent-steps down to 3€ in Decision 15. The lottery is the same in each decision: it pays 0€ with 50% probability and 13€ with 50% probability (hence, this design similar to the choice-list introduced in Holt and Laury 2002). This design makes sure that the expected inequality of the uncorrelated lottery does not change across decisions while it still provides a measure of a participant's willingness to take social risks.

[7] We could have conducted more data using the design of the first experiment to clarify this issue. However, assuming that we observe the true difference between treatments, we would need at least four more sessions per treatment *VoteUncorr* and *DicUncorr* to observe a significant effect. Therefore, we decided to make use of a more efficient design in a second experiment.

In the beginning of the experiment the rules are explained on the computer screen and read out loud to the participants. Subjects learn in the instructions about the decisions they are going to make, about the mechanism that decides on the option for all and how the payoffs are determined. After giving them enough time to familiarize with the rules and to ask questions, the experiment is started. When all decisions are made, subjects are presented all their decisions on one screen: on this screen, they can either confirm their choices or make final changes before entering them. Only then, one of the 15 decisions is randomly chosen for payoff by publicly drawing a number (1-15) from a box. For the respective decision, it is checked whether the majority chooses the safe option or the lottery (*VoteUncorr*) or which option the random dictator chooses (*DicUncorr*). If the sure thing is chosen, each subject receives the same payoff which is the sure amount of the respective decision. If the lottery is chosen, an experimenter approaches each subject, throws a die and enters the number which determines the payoff of the respective participant. Subsequently, all subjects fill out a questionnaire asking for demographics and are then paid out privately in cash.

This second experiment took place in December of 2015 in the CLER. Participants were invited using ORSEE (Greiner 2015) and the experiment was programmed with zTree (Fischbacher 2007). We conducted four sessions with 21 participants each, two per treatment. Thus, we have 84 observations in total, 42 per cell. Each session lasted about 30 minutes. Average payoff was 11.86€, including a show-up fee of 4€.

Results. If they behaved consistently, subjects would either switch only once from the safe to the risky option or always choose the one or the other. This is the case for 79 out of 84 observations. Three participants behave inconsistently in *VoteUncorr* and two in *DicUncorr*. In the following, we only use data from participants who behave consistently.[8]

[8] If we use the sum of risky decisions rather than the threshold, the results are similar to what we report in this section.

On average subjects take 9.92 safe decisions which implies that the mean subject prefers the risky option as soon as the sure payoff is only 5€ whereas a risk-neutral decision maker is indifferent between the two options when she receives 6.50€. However, we find no difference between treatments: in treatment *VoteUncorr* subjects take on average 10.13 safe options, with the corresponding value being 9.72 in treatment *DicUncorr* (p = 0.395, two-tailed Mann-Whitney U test). Descriptively, this difference even points in the opposite direction of what we would have expected. Considering only the amount of the sure thing that was used in the first experiment (6€ - Decision 9), we neither find evidence for differences between treatments (26% in *VoteUncorr* vs 28% in *DicUncorr,* p = 0.852, two-tailed two sample test of proportions).

Discussion. The results of the second experiment do neither suggest that there is a difference between *VoteUncorr* and *DicUncorr* in subjects' willingness to take risks that cause unequal outcomes. This could either be due to the fairness of the procedure not affecting behavior in this context or our 'unfair' mechanism may not be perceived as such. One potential reason for the latter is the following: in a **random** dictator mechanism, all members of the group are treated equally as they all have the same low probability of being chosen as the dictator. This might also be perceived as fair. Following this reasoning, it may not be the lack of voice in the final decision that causes perceived unfairness but different treatments of the group members.[9]

5. Experiment III

Experimental design. In the third experiment, we therefore test whether a mechanism that might actually be perceived as unfair, as subjects are not treated equally, affects social risk taking. To do so, we set up an experiment that is identical to the dictator treatments of the first

[9] One could also argue that subjects in the random dictator mechanism actually do have voice as all submit their preference to the system which then randomly chooses a decision maker.

experiment with the difference being that one participant per session has an 80% probability of being chosen as the dictator whereas all others have only a 1% probability.[10] We call these treatments *UnfairdicCorr* and *UnfairdicUncorr*. The hypotheses are still that people are more willing to take risks when outcomes are equal ex-post and that a fair aggregation mechanism mitigates this effect of outcome inequality on choices. To test the latter hypothesis, the results of the third experiment are also compared to the results of the first.

We conducted 42 observations for both treatments in April of 2016 in the CLER. Subjects were invited using ORSEE (Greiner 2015) and the experiment was programmed with zTree (Fischbacher 2007). Average payoff was 11.75€ which includes a show-up fee of 4.00€.

Results**.** In both treatments, we find that 33% of the subjects go for the risky option, indicating no difference conditional on the correlation of risks when this unfair procedure is applied (p = 1.000 for *UnfairdicCorr* vs. *UnfairdicUncorr*, two-tailed two sample test of proportions). Comparing these values also to the data of the fair *majority vote* treatments from the first experiment further provides no support for **Hypothesis 3** while instead suggesting another form of interaction between outcome and procedural fairness in social risk taking (see Figure 1).

First, choices in *UnfairdicUncorr* do not differ from *VoteUncorr* (p = 0.867 for *VoteUncorr* vs. *UnfairdicUncorr,* two-tailed two sample test of proportions) which provides no evidence that the fair procedure substitutes for fair outcomes. Second, the unfair mechanism makes subjects less likely to choose the risk compared to the fair mechanism when risks are correlated (p = 0.028 for *VoteCorr* vs. *UnfairdicCorr,* two-tailed two sample test of proportions). Third, social risk taking is more pronounced under correlated than under uncorrelated risks when decided on via *majority vote* (p = 0.025 for *VoteCorr* vs. *VoteUncorr,* two-tailed two sample test of proportions). Taken together, these findings seem to suggest that

[10] We have no hypothesis regarding the two probabilities of being chosen as the dictator. Therefore, the two types of subjects are not treated differently in the analysis. If we only make use of those subjects who have the low probability of being chosen as the dictator, the results remain similar.

both kinds of fairness are required to foster risk taking in a social context as more conservatism is introduced if either outcomes or procedures are unfair.

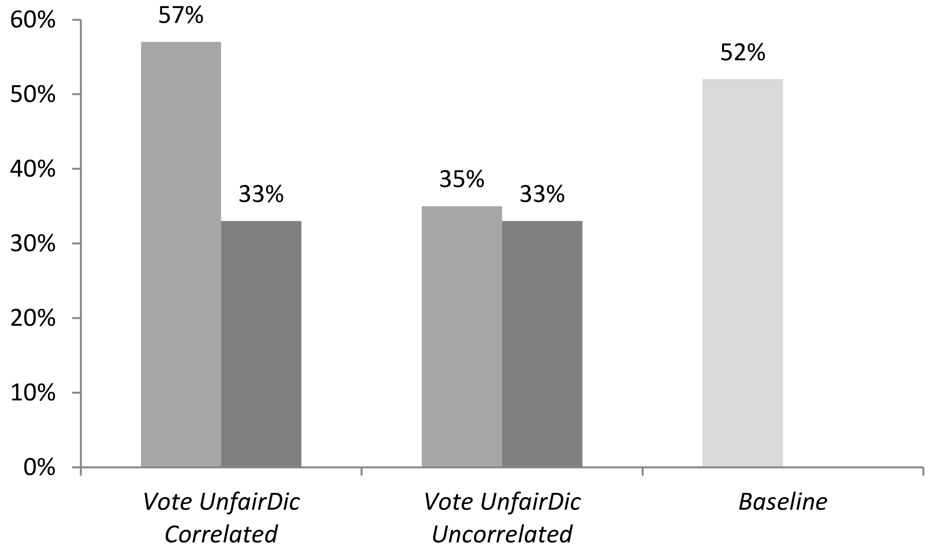

FIGURE 1. SHARE OF RISKY CHOICES ACROSS TREATMENTS WITH THE BASELINE IN LIGHT GREY, THE VOTING TREATMENTS IN MEDIUM GREY AND THE UNFAIR DICTATOR TREATMENTS IN DARK GREY.

Comparing these results also to the *Baseline* further provides some evidence that decision makers in social contexts actually only choose according to their private risk preferences when outcomes *and* procedures are fair. While we observe no difference between *Baseline* and *VoteUncorr* (p = 0.661, two-tailed two sample test of proportions), we observe evidence for differences between social and individual risk taking both due to the unfair mechanism (p = 0.078 for *UnfairdicCorr* vs. *Baseline*; p = 0.078 for *UnfairdicUncorr* vs. *Baseline*, and p = 0.039 for *UnfairdicCorr* and *UnfairdicUncorr* vs. *Baseline,* two-tailed two sample tests of proportions) and due to unfair outcomes (p = 0.076 for *VoteUncorr* vs. *Baseline*; p = 0.078 for *UnfairdicUncorr* vs. *Baseline*, and p = 0.043 for *VoteUncorr* and *UnfairdicUncorr* vs. *Baseline*, two-tailed two sample tests of proportions). These results indicate that outcome and procedural fairness may in fact be complements rather than substitutes in making people choose according to their private risk preferences when taking a decision on behalf of their group.

From the test analysis we surmise that social and individual risk taking are similar when outcomes and procedures are fair whereas more conservatism is introduced if either dimension is unfair. To substantiate this idea, we make use of linear probability models (LPM) to estimate a participant's propensity to choose the risky option. A pooled set of *Baseline* and *VoteCorr* serves as the reference group. The respective results are presented in Table 2.

TABLE 2. LPM MODELS ESTIMATING THE PROBABILITY THAT THE RISKY OPTION IS CHOSEN.

Dependent var.: **Risky choice**	(1)	(2)
Unfair outcomes (UO)	-0.198** (0.082)	--
Unfair procedure (UP)	-0.214** (0.092)	--
UO x UP	0.198 (0.132)	--
At least one type of unfairness	--	-0.205*** (0.072)
Two types of unfairness	--	-0.010 (0.087)
Observations	231	231
R-squared	0.041	0.041

Notes: LPM models; dependent variable: risky choice (1 being the risky option and 0 the sure thing); robust standard errors in parentheses; Levels of significance: **$p < 0.05$; ***$p < 0.01$.

Model 1 shows the effects of outcome unfairness, of procedural unfairness and of their interaction on the probability that a subject chooses the risky option. We find that both kinds of unfairness have significant effects on risk taking, making decisions more conservative. Their respective coefficients do not differ ($p = 0.868$, Wald test) which indicates that both fairness dimensions are somewhat equally important. We also observe that the two types of unfairness being present at once does not further change behavior as the effect of the interaction is opposite and similar in size when compared to the respective unfairness coefficients. In Model 2, we estimate the corresponding effects of either at least one or two types of unfairness being present. When at least one is present, decision makers are substantially less likely to choose the risky option – in fact, the difference is about 20.5 percentage points – whereas when two are present behavior seems not further affected.

Discussion. In sum, these results are not in line with the idea that fair procedures substitute for fair outcomes. In contrast, they suggest that outcome and procedural fairness are complements rather than substitutes in fostering social risk taking as both kinds of fairness seem required to make decision makers choose according to their private risk preferences, and hence more risk seeking, in a social context.[11]

6. Conclusion

In the present paper, we investigate how outcome and procedural fairness influence social risk taking and how this compares to individual decision-making under risk. To do so, we set up a laboratory experiment collecting individual and group decisions while varying the correlation of risks within a group and the choice aggregation mechanism to determine the relevant option for its members. We expect that social is similar to individual risk taking when outcomes are equally distributed within a group ex-post while group members should get more conservative when the risk causes inequality. We hypothesize that a fair decision procedure mitigates this effect of expected inequality on social risk taking.

We indeed find an interaction between outcome and procedural fairness. However, it is different from what we hypothesize. In fact, our results suggest that a fair procedure does not substitute for fair outcomes but that both fair outcomes and fair procedures are required to foster social risk taking and to make decision makers choose according to their private risk preferences when taking a decision on behalf of their group. If either of the two kinds of fairness is missing, individual preferences seem distorted in a social context, making choices more conservative. Even though these findings do not support our hypotheses entirely, they seem somewhat in line with previous results. While some prior studies where one predetermined decision maker decides for the whole group, which might also be perceived as

[11] These findings further limit the evidence supporting **Hypothesis 2** from the first experiment as subjects seem not further affected by outcome unfairness in case the aggregation mechanism is unfair.

unfair, show that the respective decisions are more conservative compared to individual decisions (Bolton et al. 2015), majority decision-making with correlated risks seems similar to individual risk taking (Harrison et al. 2013). In addition, correlated risks were found to be preferred to uncorrelated risks (e.g., López-Vargas 2014 and Gaudeul 2015). These prior findings indicate that both fairness dimensions may be important in social risk taking. In the present study, we show how the two dimensions interact while also confirming that both affect behavior. In particular, our results suggest that subjects react to unfairness in general, regardless of the dimension, in taking less risk. We conclude that the two fairness dimensions are complements rather than substitutes in social risk taking.

Our research provides important implications for the study of collective decision-making under risk. It shows that both fair outcomes *and* fair procedures are required to make people choose according to their private risk preferences in social contexts and suggests that, as soon as outcomes or procedures are unfair, far less risk is taken. This potentially reduces the efficiency of collective decision-making under risk as people might be less willing to choose a risky but efficient option when confronted with either kind of unfairness.

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Appendix

A. Instructions given to subjects in Experiments I and III

Treatment Baseline

Welcome and thanks for your participation in our experiment. You are participating in a study in which you can earn some money. At the end of the experiment, your earnings will be added to a show-up fee of 2.50 Euros, and you will be paid in cash.

Experiment. The study is conducted anonymously. No one gets to know the identities of the other participants, their decisions or their payoffs. Communication between participants is not permitted throughout the experiment. If you have any question after reading these instructions, please raise your hand. An experimenter will then come to you to answer your question.

Decision pattern. Each participant is to choose between two options. These options are Option 1 and Option 2. Your payoffs are affected by your decision as follows:

Option 1: You get 6 Euros.

Option 2: A die determines your payoff.

- With 50% probability, you get 13 Euros.
- With 50% probability, you get 0 Euro.

The experiment is conducted in three steps.

Step 1: The participants choose one of both options.

Step 2: If you have chosen Option 1, you do not have to take a decision in this step. If you have chosen Option 2, you decide in this step whether you want to get the high payoff (13 Euro) for the numbers 1, 2, and 3 or for the numbers 4, 5, and 6 of the six-sided die. If another number is realized you receive the low payoff (0 Euro). In both cases the probability for both results is 50%.

Step 3: The payoffs are determined. Therefore, an experimenter will come to each participant to throw a die and to enter the result. This is independent of your choice in the first step as the experimenter will not know at this stage which Option was chosen.

→ If you have chosen **Option 1**, you get 6 Euros independent of the results of you die

→ If you have chosen **Option 2**, your payoff is determined by chance. Conditional on your decision in step 2 and the result of your die, you receive 13 Euro or 0 Euro.

Before the experiment begins, you get some questions in order to test whether you understood the instructions. When the experiment is finished we ask you to fill out a short questionnaire. Subsequently, you get your payoffs.

Treatment VoteCorr

Welcome and thanks for your participation in our experiment. You are participating in a study in which you can earn some money. At the end of the experiment, your earnings will be added to a show-up fee of 2.50 Euros, and you will be paid in cash.

Experiment. The study is conducted anonymously. No one gets to know the identities of the other participants, their decisions or their payoffs. Communication between participants is not permitted throughout the experiment. If you have any question after reading these instructions, please raise your hand. An experimenter will then come to you to answer your question.

Decision pattern. You and all other participants of the experiment are one group. This group consists of 21 persons. Each participant is to choose between Option 1 and Option 2. The majority rule is implemented: The option that is chosen more frequently within the group is relevant for each member of the group.

Option 1: All participants get 6 Euros.

Option 2: A die determines the payoffs of all participants.

- If it shows 1, 2, or 3 (with 50% probability), all participants get 13 Euros.
- If it shows 4, 5, or 6 (with 50% probability), all participants get 0 Euro.

The experiment is conducted in three steps.

Step 1: The participants choose one of both options.

Step 2: The result of the election is announced.

Step 3: The payoffs are determined.

→ If a majority has chosen **Option 1**, all participants get 6 Euros.

→ If a majority has chosen **Option 2**, the payoffs are determined by chance. A die is thrown publicly so that anyone can see it. The result determines the payoffs of all participants. This means, that all participants get the same payoff (either 13 Euro, if it shows 1, 2, or 3 or 0 Euro, if it shows 4, 5, or 6).

Before the experiment begins, you get some questions in order to test whether you understood the instructions. When the experiment is finished we ask you to fill out a short questionnaire. Subsequently, you get your payoffs.

Treatment DicUncorr

Welcome and thanks for your participation in our experiment. You are participating in a study in which you can earn some money. At the end of the experiment, your earnings will be added to a show-up fee of 2.50 Euros, and you will be paid in cash.

Experiment. The study is conducted anonymously. No one gets to know the identities of the other participants, their decisions or their payoffs. Communication between participants is not permitted throughout the experiment. If you have any question after reading these instructions, please raise your hand. An experimenter will then come to you to answer your question.

Decision pattern. You and all other participants of the experiment are one group. This group consists of 21 persons. Each participant is to choose between Option 1 and Option 2. Subsequently, one participant is randomly chosen whose decision is relevant for each participant; the decision of each other participant is irrelevant for the payoffs of this experiment.

Option 1: All participants get 6 Euros.

Option 2: Dice determine the payoffs.

- If your die shows 1, 2, or 3 (with 50% probability), you get 13 Euros.
- If your die shows 4, 5, or 6 (with 50% probability), you get 0 Euro.

The experiment is conducted in three steps.

Step 1: The participants choose one of both options.

Step 2: One participant is chosen whose choice is relevant for all participants.

Step 3: The payoffs are determined.

→ If the participant has chosen **Option 1**, all participants get 6 Euros.

→ If the participant has chosen **Option 2**, the payoffs are determined by chance. For each participant a die is thrown independently. This means, that all participants can get different payoffs depending on the result of their personal die (either 13 Euro, if it shows 1, 2, or 3 or 0 Euro, if it shows 4, 5, or 6).

Before the experiment begins, you get some questions in order to test whether you understood the instructions. When the experiment is finished we ask you to fill out a short questionnaire. Subsequently, you get your payoffs.

Treatment UnfairdicUncorr

Welcome and thanks for your participation in our experiment. You are participating in a study in which you can earn some money. At the end of the experiment, your earnings will be added to a show-up fee of 4.00 Euros, and you will be paid in cash.

Experiment. The study is conducted anonymously. No one gets to know the identities of the other participants, their decisions or their payoffs. Communication between participants is not permitted throughout the experiment. If you have any question after reading these instructions, please raise your hand. An experimenter will then come to you to answer your question.

Decision pattern. You and all other participants of the experiment are one group. This group consists of 21 persons. Each participant is to choose between Option 1 and Option 2. Subsequently, one participant is randomly chosen whose decision is relevant for each participant; the decision of each other participant is irrelevant for the payoffs of this experiment.
The probability of being chosen is different for the involved participants. One participant is chosen, whose probability of taking the decision for the whole group is 80% whereas all other participants have a probability of 1% of taking the decision for the whole group. Your personal probability of being chosen is **1%/80%**.

Option 1: All participants get 6 Euros.

Option 2: Dice determine the payoffs.

- If your die shows 1, 2, or 3 (with 50% probability), you get 13 Euros.
- If your die shows 4, 5, or 6 (with 50% probability), you get 0 Euro.

The experiment is conducted in three steps.

Step 1: The participants choose one of both options.

Step 2: One participant is chosen by chance. Her choice is relevant for all participants. The members of the group have different probabilities of being chosen.

Step 3: The payoffs are determined.

> → If the participant has chosen **Option 1**, all participants get 6 Euros.
>
> → If the participant has chosen **Option 2**, the payoffs are determined by chance. For each participant a die is thrown independently. This means, that all participants can get different payoffs depending on the result of their personal die (either 13 Euro, if it shows 1, 2, or 3 or 0 Euro, if it shows 4, 5, or 6).

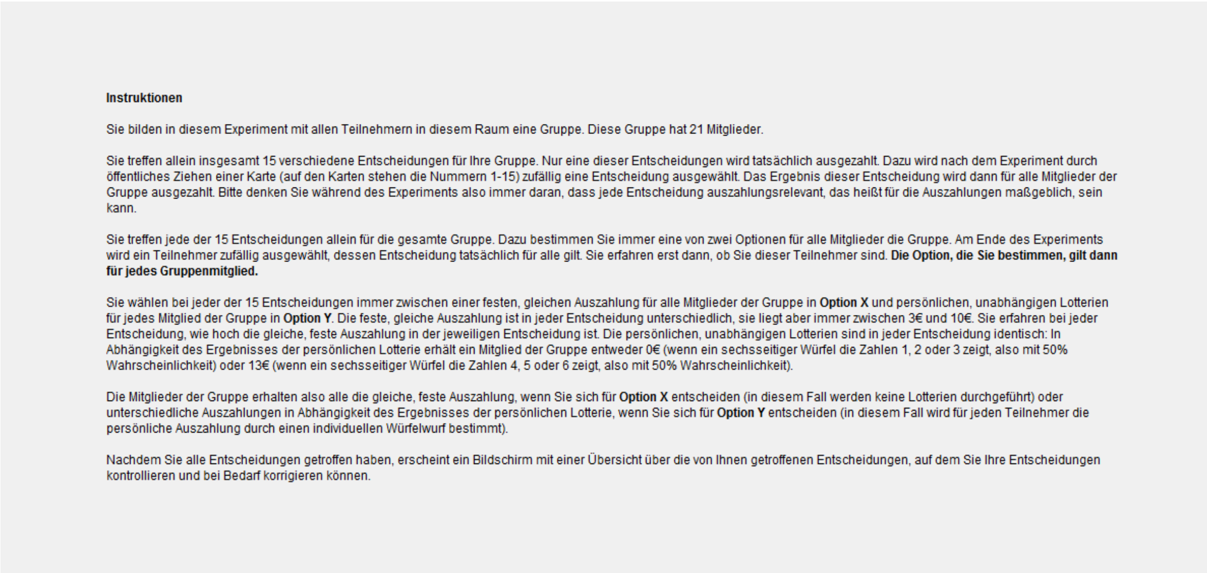
Before the experiment begins, you get some questions in order to test whether you understood the instructions. When the experiment is finished we ask you to fill out a short questionnaire. Subsequently, you get your payoffs.

Treatments VoteUncorr, DicCorr and UnfairdicCorr

Re-combining the upper instructions yields the instructions of the other treatments. One only has to rearrange the parts on the resolution of risk and on the aggregation mechanism.

B. Instructions and screenshots of Experiment II

Screenshot 1. Instructions of the experiment.



Instruktionen

Sie bilden in diesem Experiment mit allen Teilnehmern in diesem Raum eine Gruppe. Diese Gruppe hat 21 Mitglieder.

Sie treffen allein insgesamt 15 verschiedene Entscheidungen für Ihre Gruppe. Nur eine dieser Entscheidungen wird tatsächlich ausgezahlt. Dazu wird nach dem Experiment durch öffentliches Ziehen einer Karte (auf den Karten stehen die Nummern 1-15) zufällig eine Entscheidung ausgewählt. Das Ergebnis dieser Entscheidung wird dann für alle Mitglieder der Gruppe ausgezahlt. Bitte denken Sie während des Experiments also immer daran, dass jede Entscheidung auszahlungsrelevant, das heißt für die Auszahlungen maßgeblich, sein kann.

Sie treffen jede der 15 Entscheidungen allein für die gesamte Gruppe. Dazu bestimmen Sie immer eine von zwei Optionen für alle Mitglieder die Gruppe. Am Ende des Experiments wird ein Teilnehmer zufällig ausgewählt, dessen Entscheidung tatsächlich für alle gilt. Sie erfahren erst dann, ob Sie dieser Teilnehmer sind. **Die Option, die Sie bestimmen, gilt dann für jedes Gruppenmitglied.**

Sie wählen bei jeder der 15 Entscheidungen immer zwischen einer festen, gleichen Auszahlung für alle Mitglieder der Gruppe in **Option X** und persönlichen, unabhängigen Lotterien für jedes Mitglied der Gruppe in **Option Y**. Die feste, gleiche Auszahlung ist in jeder Entscheidung unterschiedlich, sie liegt aber immer zwischen 3€ und 10€. Sie erfahren bei jeder Entscheidung, wie hoch die gleiche, feste Auszahlung in der jeweiligen Entscheidung ist. Die persönlichen, unabhängigen Lotterien sind in jeder Entscheidung identisch: In Abhängigkeit des Ergebnisses der persönlichen Lotterie erhält ein Mitglied der Gruppe entweder 0€ (wenn ein sechsseitiger Würfel die Zahlen 1, 2 oder 3 zeigt, also mit 50% Wahrscheinlichkeit) oder 13€ (wenn ein sechsseitiger Würfel die Zahlen 4, 5 oder 6 zeigt, also mit 50% Wahrscheinlichkeit).

Die Mitglieder der Gruppe erhalten also alle die gleiche, feste Auszahlung, wenn Sie sich für **Option X** entscheiden (in diesem Fall werden keine Lotterien durchgeführt) oder unterschiedliche Auszahlungen in Abhängigkeit des Ergebnisses der persönlichen Lotterie, wenn Sie sich für **Option Y** entscheiden (in diesem Fall wird für jeden Teilnehmer die persönliche Auszahlung durch einen individuellen Würfelwurf bestimmt).

Nachdem Sie alle Entscheidungen getroffen haben, erscheint ein Bildschirm mit einer Übersicht über die von Ihnen getroffenen Entscheidungen, auf dem Sie Ihre Entscheidungen kontrollieren und bei Bedarf korrigieren können.

Translation for treatment DicUncorr [VoteUncorr in brackets]:

In this experiment you and all other participants in this room form a group. This group consists of 21 members.

You take in total 15 decisions for your group [The members of the group take in total 15 decisions together]. Only one of these decisions will actually be paid out. In order to determine the respective decision, a sheet of paper is randomly drawn from an urn by the end of the experiment (with the numbers 1-15 on them). The result of this decision is paid out to all participants. Please do not forget throughout the experiment that each decision may be relevant for the payoffs.

You take each of the 15 decision alone for the whole group. To do so, you choose for each decision one of two options for all members of your group. At the end of the experiment one member is randomly chosen whose decision is actually relevant for all. You only get to know then whether you are this member. In this case, the option you choose is implemented for all members of the group.

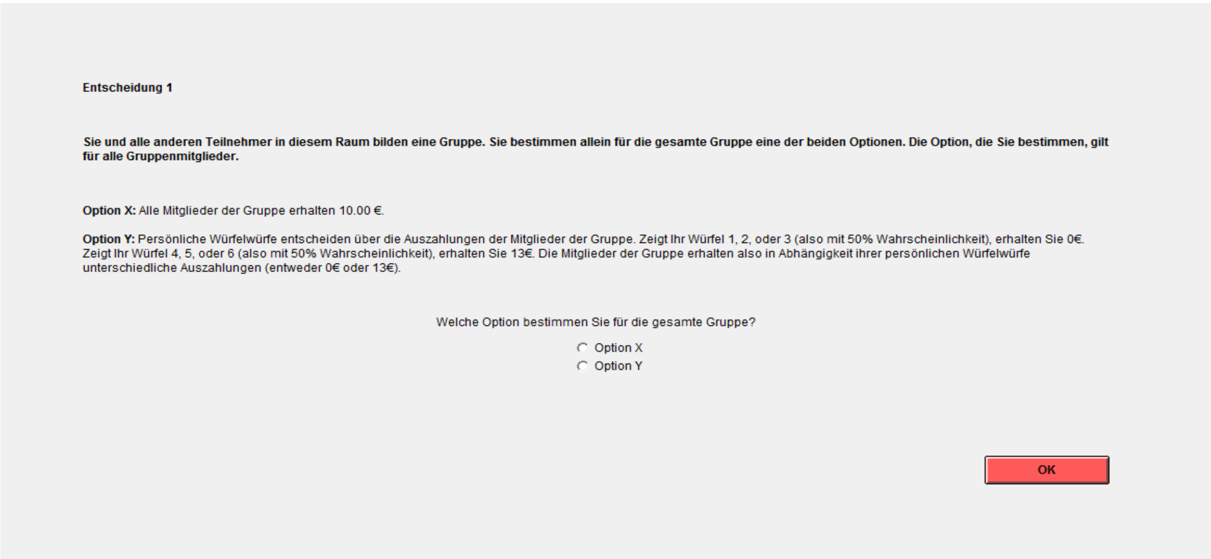
[You and the other members of your group take each of the 15 decisions together. To do so, you always cast your vote for one of two options. The majority rule is used: this means that the option which is preferred by the majority is implemented for each member of the group.]

In each of the 15 decisions, you [the members of the group] choose between a safe and equal payoff for all members of the group in **Option X** and individual, independent lotteries in **Option Y.** The safe and equal payoff changes in each decision but it is always between 3€ and 10€. You get to know in each decision how much the safe and equal payoff is in the corresponding decision. The individual, independent lotteries are the same in each decision: conditional on one's individual result of the lottery, a member of the group gets 0€ (in case a six-sided die shows 1, 2 or 3, with 50% probability) or 13€ (in case a six-sided die shows 4, 5 or 6, with 50% probability).

Thus, the members of the group receive equal and safe payoffs in case you [the majority] choose[s] **Option X (**in this case no lotteries are conducted) or different payoffs conditional on the result of the individual lottery in case you [the majority] choose[s] Option Y (in this case the individual payoff is determined by individually throwing a die.)

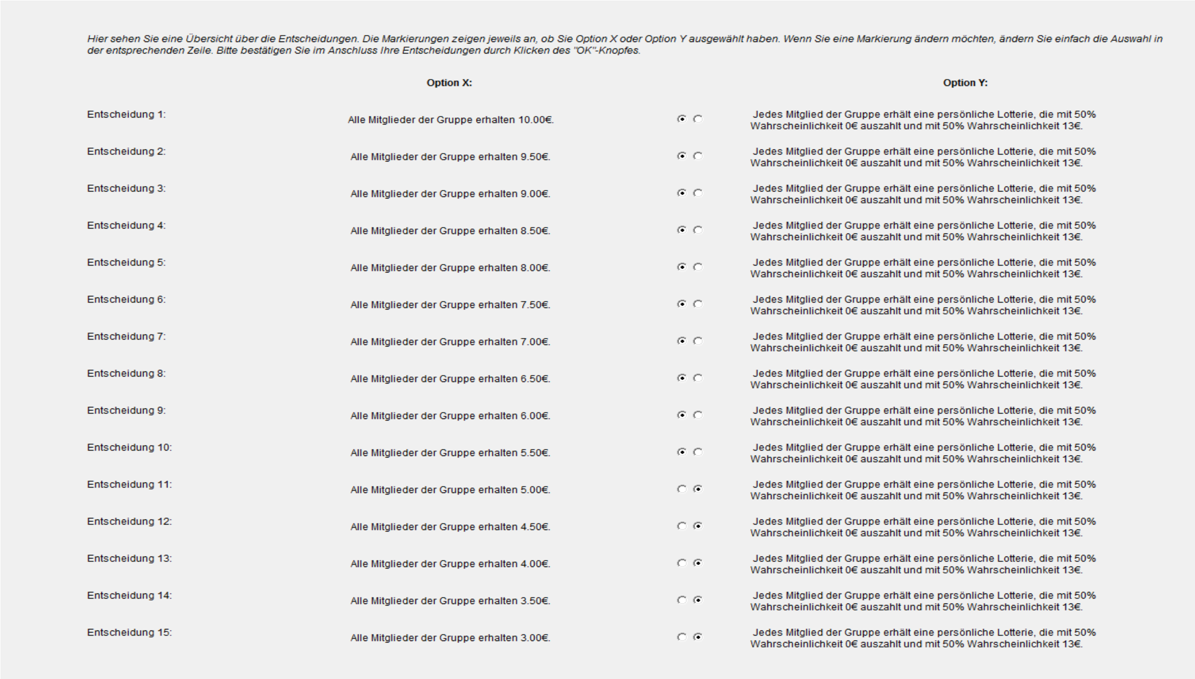
When all decisions are made, one screen summarizes all your decisions. On this screen you can check your choices and make corrections of required.

Screenshot 2. Decision stage.



This is a screen-shot of the decision screen that shows and describes the options that can be chosen in this experiment.

Screenshot 3. Overview of decisions.



Hier sehen Sie eine Übersicht über die Entscheidungen. Die Markierungen zeigen jeweils an, ob Sie Option X oder Option Y ausgewählt haben. Wenn Sie eine Markierung ändern möchten, ändern Sie einfach die Auswahl in der entsprechenden Zeile. Bitte bestätigen Sie im Anschluss Ihre Entscheidungen durch Klicken des "OK"-Knopfes.

	Option X:		Option Y:
Entscheidung 1:	Alle Mitglieder der Gruppe erhalten 10.00€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 2:	Alle Mitglieder der Gruppe erhalten 9.50€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 3:	Alle Mitglieder der Gruppe erhalten 9.00€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 4:	Alle Mitglieder der Gruppe erhalten 8.50€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 5:	Alle Mitglieder der Gruppe erhalten 8.00€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 6:	Alle Mitglieder der Gruppe erhalten 7.50€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 7:	Alle Mitglieder der Gruppe erhalten 7.00€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 8:	Alle Mitglieder der Gruppe erhalten 6.50€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 9:	Alle Mitglieder der Gruppe erhalten 6.00€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 10:	Alle Mitglieder der Gruppe erhalten 5.50€.	◉ ○	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 11:	Alle Mitglieder der Gruppe erhalten 5.00€.	○ ◉	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 12:	Alle Mitglieder der Gruppe erhalten 4.50€.	○ ◉	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 13:	Alle Mitglieder der Gruppe erhalten 4.00€.	○ ◉	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 14:	Alle Mitglieder der Gruppe erhalten 3.50€.	○ ◉	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.
Entscheidung 15:	Alle Mitglieder der Gruppe erhalten 3.00€.	○ ◉	Jedes Mitglied der Gruppe erhält eine persönliche Lotterie, die mit 50% Wahrscheinlichkeit 0€ auszahlt und mit 50% Wahrscheinlichkeit 13€.

Summary screen where all decisions can be checked and corrected if required.

Chapter 3

Anchoring vs focality in coordination: Evidence from minimum effort games*

Abstract

I compare the effect of private vs public anchors on decision-making in minimum effort games. While private anchors are strategically irrelevant as they should not rationally affect a decision maker's beliefs, public anchors are strategically relevant as they constitute a focal point. I find that subjects' choices are affected by both irrelevant and relevant anchors with the extent being amplified when anchors are public. Some evidence is found suggesting that the respective effects interact with the degree of strategic uncertainty.

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1. Introduction

The current research investigates the effects of private vs public anchors in strategic interaction. Thereby, I examine whether both cognitive and strategic considerations are relevant when it comes to the effects of focal points in coordination. In addition, I test how the reactions towards anchors interact with the degree of strategic uncertainty.

I utilize a set of minimum effort games (MEG) to study behavior under strategic uncertainty. The MEG is a coordination game with Pareto-ranked equilibria where decision makers face a trade-off between payoff and risk (Van Huyck et al. 1990; Goeree and Holt 2005). In the MEG, the members of a group choose from their set of 'effort levels' with everyone's payoffs increasing in the minimum of the group while decreasing in their individual deviation from that minimum. It is hence in all players' interests to have a high minimum while they are also individually trying not to choose more than the minimum. The risk is caused by the fact that choosing more than the minimum is costly as the additional effort is in vain. Even though the MEG includes a Pareto-efficient social optimum where all choose the highest effort, it is often found in empirical investigations that groups tend converge towards the secure and least-efficient equilibrium over the course of time (Van Huyck et al. 1990; Devetag and Ortmann 2007).[1]

In the literature, the minimum effort game is used to resemble a variety of coordination scenarios such as global public good games (Harrison and Hirshleifer 1989), team production (Brandts and Cooper 2006), bank runs (Diamond and Dybvig 1983) and many more. What all these scenarios have in common is that the involved decision makers know what would be best for all but at the same time they have to be convinced that all act accordingly. Experimental evidence suggests that this conviction is very sensitive to variations in the context which implies that the beliefs players form crucially depend on the details of a decision situation.

[1] Most experimental evidence on the minimum effort game stems from repeated interactions. See Ochs (1995) and Devetag and Ortmann (2007) for surveys on the minimum effort and the closely related stag hunt game.

There are many structural details that have been identified to affect behavior in the MEG in particular and in coordination in general. Closely related to this research is the literature on suggestions, cheap talk and sunspots in (minimum effort) coordination. Both Chaudhuri and Paichayontvijit (2010) and Devetag et al. (2012) find that an explicit (public) suggestion to play the maximum can help groups overcome coordination failures. Chaudhuri et al. (2009) further observe that intergenerational recommendations improve coordination while Devetag (2005) and Cason et al. (2012) show that also successfully coordinated precedents do so. Also related is the literature on cheap talk in the MEG (Charness 2000; Duffy and Feltovich 2002; Blume and Ortmann 2007) which finds that communication helps groups to coordinate on the Pareto-dominant equilibrium. Similarly, in other coordination games, Van Huyck et al. (1992) and Brandts and Macleod (1995) show that coordination on the respective equilibrium increases in case it is externally suggested while Beugnot et al. (2012) find that sunspots, a public random announcement of one strategy, can cause coordination failure in a game where efficient coordination is designed to be very simple. Finally, also explicit recommendations to introduce correlated equilibria (Cason and Sharma 2007; Kuang et al. 2007; Duffy and Feltovich 2010) tend to shape behavior.

This study is further related to research emphasizing the context-dependence of choices in the MEG. Manzini et al. (2009) find that even minor cues such as ‘smiles’ are sufficient for affecting behavior. They argue that such cues make decision makers more trusting concerning the behavior of their opponent(s) and hence more willing to take a risky option. The underlying model implies that choosing the highest effort is perceived as the natural standard with decision makers only deviating due to a ‘lack of trust’. Dugar (2010) and Galbiati et al. (2013) show that (also non-monetary) rewards and sanctions can foster coordination. In addition to such cues, group decision-making and (social) preferences affect behavior in the MEG. Feri et al. (2010) find that groups coordinate more successfully than individual decision makers while Chen and Chen (2011) show that also group identity shapes equilibrium selection as a shared identity makes subjects choose higher effort levels.[2] Feldhaus et al. (2015) provide evidence concerning the importance of equal payoffs for equilibrium selection in the MEG, further emphasizing the relevance of social preferences. Goeree and Holt (2001, 2005) show that higher costs of miscoordination make participants choose lower effort levels, with this finding support also in larger groups in Brandts and Cooper (2006).[3]

In sum, the empirical literature on the MEG suggests that the behavior of subjects strongly depends on the specific context and that even minor variations can have substantial effects on choices. However, the psychological mechanisms behind these effects often remain unclear. In fact, it is basically always shown that behavior reacts in response to possibly strategically justified changes in the beliefs as the variations are usually public knowledge and the game is mostly played repeatedly. Also, to the best of my knowledge, no attempt has been made to investigate how the effects of external cues or recommendations vary conditional on the gains and losses from (mis)coordination in the MEG. In the current paper, I hence broaden the literature in (1) investigating whether also cognition affects behavior in the presence of focal points and in (2) testing whether the reaction towards external 'cues' depends on the degree of strategic uncertainty.

Specifically, I investigate whether behavior in the MEG is influenced by anchors and by the degree of public knowledge about these anchors. Anchors have been studied extensively in both social psychology and economics starting with the seminal paper of Tversky and Kahneman (1974).[4] Anchoring suggests that even random but highly accessible and related 'values' can affect behavior. I test whether anchors may also shape the beliefs and the corresponding choices in social interaction. Importantly, if this was the case, anchoring might

[2] The finding that group identity influences effort choices is not successfully replicated in Camerer et al. (2016).

[3] Also, some mechanisms have been identified that improve coordination even in large groups: while Weber (2006) shows that slowly increasing group-size can help to promote stable coordination on the Pareto-dominant equilibrium, Riedl et al. (forthcoming) observe that groups coordinate more successfully when all group members can freely choose with whom they want to play the game with.

[4] In the economics literature, anchoring was mainly investigated in terms of how it shapes valuations in purchasing decisions (e.g., Ariely et al. 2003) and how it affects outcomes in auctions (e.g., Beggs and Graddy 2009) and bargaining (e.g., Galinsky and Mussweiler 2001). See Furnham and Boo (2011) for a recent literature review on anchoring effects.

also (partly) explain the effects of focal points as these points are also more accessible due to their prominence. In order to disentangle cognitive and strategic reasons for reactions towards focality, I make use of treatments with private (*Individual*) and public (*Common*) anchors. While cognition research predicts changes in behavior also in case of private anchors, an economic rationale is only available for public anchors.

In a first set of treatments, I hence provide participants with a private anchor in asking them to state whether they think that their opponent chooses at least some random value as her effort level. This random value is individually determined. From the psychological literature, I conjecture that the beliefs about the opponent's behavior and the corresponding choices might be affected by such a 'private anchor'. In fact, the mechanism suggested in Strack and Mussweiler (1997) and Mussweiler and Strack (1999, 2001) as a now well-established explanation for anchoring effects seems also applicable to this strategic setting. They argue that decision makers confronted with an anchor engage in hypothesis-consistent testing and assume that the anchor, as it and its associations are highly available due to being present, triggers more arguments in line with the respective anchor as compared to other feasible values. In the MEG, this means that relatively more arguments are put forward that suggest a behavior of the opponent(s) close to the anchor compared to arguments in line with other strategies she/they might play when forming a belief. The reason is termed 'selective accessibility' (Mussweiler and Strack 1999): while evidence supporting the anchor is easily thought off (as it is more accessible), non-consistent evidence comes to mind less easily (as it is less accessible). This effect should cause both beliefs and choices to be biased towards the anchor value (*cognition hypothesis*).

From an economic perspective, one would not expect effects of such private anchors as they should not affect the behavior of the opponent and hence neither a decision maker's beliefs. To test whether the reasoning behind the reactions towards focal points is cognitive or strategic (or both) in nature, I conduct another set of treatments with public instead of private anchors. Now, the random value is the same for the members of a group. This constitutes a focal point. Since Schelling (1960), "focal points" are discussed as a potential means for overcoming coordination failures:[5] as coordination games include many rational strategies, prominence of particular equilibria and public knowledge of this prominence is an important property for equilibrium selection as it may serve as a coordination device. This prominence can, e.g., be due to the payoff structure of a specific strategy combination (e.g., safe strategy, equal outcomes, Pareto-dominance) or due to its representation (e.g., color or position).

In this second set of treatments, I therefore provide participants with an anchor while making sure that it is public knowledge that all members of a group are provided with the same anchor. Here, I expect that subjects' choices are also biased towards the anchor and, as this variation combines cognitive and strategic considerations, that the effect of public anchors is stronger than the effect of private ones (*strategic hypothesis*).

In addition, to evaluate the importance of uncertainty for the effects of anchors, I investigate whether the extent of the reaction towards anchors depends on the degree of strategic uncertainty. Mussweiler and Strack (2000) show in guessing tasks that the effect of anchors is the stronger the more uncertain a guessing target is. In the present study, I test whether this finding also extends to social interaction in the sense that choices are more affected by anchors as strategic uncertainty increases. It seems reasonable to expect different reactions towards anchors in the MEG conditional on the degree of uncertainty as under low uncertainty decision makers should have a quite clear idea about what their opponent plans to do and thus act accordingly while this might change as uncertainty increases. I therefore expect that the reaction towards anchors gets the more pronounced the more uncertain the respective decision is (*uncertainty hypothesis*).

[5] See Mehta et al. (1994a) and Mehta et al. (1994b) for experimental evidence on the effects of focal points in coordination. Focal points are mainly discussed in the realm of pure coordination games as here focality is the only selection principle. Crawford et al. (2008) provide evidence that even minor asymmetries can undermine the effects of focality.

In the experiment, I find evidence for both anchoring on private as well as on public anchors with the effect being amplified when they are public. These results emphasize the relevance of strategic reasons for the effects of focal points. Still, they also indicate that part of the effect of focal points may actually be driven by mere prominence. I further find that anchoring gets more pronounced as the gains from coordination decrease and the costs of miscoordination increase. This finding further suggests that the extent of anchoring may indeed be affected by the degree of strategic uncertainty associated with a decision.

The remainder of the paper is organized as follows: Section 2 presents the framework which includes the minimum effort game and two measures of strategic uncertainty. Section 3 presents the experimental design and summarizes the hypotheses. In Section 4, I present the results and Section 5 concludes.

2. Framework and measures of uncertainty

Minimum effort game. In a symmetric minimum effort game $N = \{2, 3, \ldots, n\}$ players form a group and $E = \{e^{min}, \ldots, e^{max}\}$ is a set of effort levels. Each group member simultaneously chooses an effort level $e_i \in E$. The action profile $p = (e_i)_{i \in N}$ denotes the choices of the members of the group, r the marginal return from the minimum, c the marginal cost of effort provision and a is a constant. The payoff of a player i is given by

$$\pi_i(p) = r \min_{j \in N}\{e_j\} - ce_i + a.$$

The relation $r > c > 0$ causes the additional gain from a higher minimum to be more than the additional cost from choosing a higher effort. This makes sure that players always have an incentive to choose exactly the minimum of their group. Therefore, all strategy profiles p where all group members choose the same effort constitute the set of pure-strategy equilibria of the MEG. Unilaterally decreasing one's effort would reduce one's payoff as the minimum decreases while increasing one's effort would decrease one's payoff as the additional effort is in vain. The pure-strategy equilibria can be Pareto-ranked as everyone's payoffs increase in the minimum.[6]

Two equilibria stand out. First, the equilibrium where all players choose the highest effort is prominent: this equilibrium is Pareto-efficient and results in the highest individual and overall payoff. However, the corresponding strategy is also 'risky' as the actual outcome strongly depends on the choices of others. In fact, the least payoff is realized if one player chooses the highest effort while some other chooses the lowest one. Second, the equilibrium where all choose the lowest effort is prominent: this equilibrium features the lowest in-equilibrium payoffs while in turn being 'secure' as a player's payoff does not depend on the choices of others. In the MEG, decision makers hence face a trade-off between payoff and risk.

The discussed properties make the game an ideal setting to vary strategic uncertainty in social interaction and to test for anchoring effects. First, in changing the gains from coordination and the losses from miscoordination, it seems reasonable to suspect that the degree of uncertainty is affected (see discussion below) while one still provides an otherwise identical setting in terms of strategies and equilibrium predictions. Second, the MEG provides two opposing reasons to choose high or low efforts, one of which can be highlighted by a high or a low anchor.

Measures of uncertainty. I propose two measures of strategic uncertainty across different parameterizations of the MEG. Two measures are proposed as there is no established one yet.

First, I briefly introduce the "potential" as a proxy for uncertainty. I call the corresponding measure *Potential-based measure*. Potential games are first discussed in detail and related to the MEG in Monderer and Shapley (1996). Importantly, the authors show that the symmetric MEG is a potential game and suggest that this concept can be used as an equilibrium refinement for this setting. A potential game is a game that admits a potential function *P* that

[6] The game also contains a continuum of Pareto-ranked mixed-strategy equilibria. However, experimental research on the MEG mainly focusses on pure strategies as the mixed-strategy equilibria have some implausible comparative static predictions such as that higher effort costs *decrease* the probability that a low effort is chosen (Anderson et al. 2001).

maps the action profiles p into the real numbers such that all changes in the potential are identical to the changes in the payoffs of the deviator. Hence, P is the potential function of the MEG if $\pi_i(e_i, e_{-i}) - \pi_i'(e_i', e_{-i}) = P(e_i, e_{-i}) - P(e_i', e_{-i})$ for every $e_i, e_{-i}, e_i' \in E$. For the MEG, this concept predicts that the Pareto-dominant equilibrium is played for some parameter configurations while making no prediction or proposing the secure equilibrium for others.

The potential function is a global payoff function that captures the costs and gains from unilateral deviations for all involved players on the group level. It in particular coincides with the predictions of *risk-dominance* in symmetric 2x2 games, an established concept that takes into account the tradeoff between payoffs and risks in some coordination settings (Harsanyi and Selten 1988; Goeree and Holt 2005). However, in contrast to risk-dominance, the potential can also be applied to many games with larger action spaces and more players while also weighting payoffs and risks.

Monderer and Shapley (1996) show that the potential function of the MEG can be depicted as $P(p) = rmin\{p\} - c\sum_{i \in N} e_i$. All equilibria of the MEG are local maximizers of this function. In addition to these local maxima, one can deduce a threshold benefit from coordination r^{thres} (Monderer and Shapley 1996), which depends on the costs of effort provision c and the group-size N, such that the secure equilibrium is the *unique* maximizer of the potential function if $r < r^{thres}$ and the Pareto-dominant equilibrium is the unique maximizer if $r > r^{thres}$. If $r = r^{thres}$, the potential does not make a prediction with regard to equilibrium selection.[7] Hence, "*the Nash equilibrium that maximizes the potential in this game is sensitive to parameters that may actually affect behavior*" (Goeree and Holt 2005, p. 352) which is also documented by the empirical success of the concept.[8]

[7] Similar to risk dominance, the potential is most often used to study behavior in repeated interaction (e.g. Goeree and Holt 2005; Chen and Chen 2011). Both concepts have in common that many learning processes converge towards the respective suggested equilibrium (Monderer and Shapley 1996; Goeree and Holt 2005).

[8] Evidence on the predictive power of the potential in the MEG can be found in Monderer (1996), Anderson et al. (2001), Goeree and Holt (2001; 2005), and Chen and Chen (2011).

Apart from refining the set of equilibria, I suggest that the potential may also serve well as a proxy for strategic uncertainty. There are two reasons for this. First, the potential weights payoffs and risks from coordination and second, it identifies a parameterization where no prediction is made which can be thought off as the one involving the most uncertainty. Thereby, it provides an intuitively appealing structure with regard to uncertainty across different versions of the MEG: if the gains from coordination are low and the costs from miscoordination are high, the potential proposes the play of the secure equilibrium while it proposes the Pareto-dominant equilibrium when the gains from coordination are high and the costs are low. It further makes no prediction for one parameterization in between. These behavioral predictions of the potential are clear-cut in the sense that the theory always suggests the one or the other extreme equilibrium in case r and r^{thres} differ, while not refining the set of equilibria in case they are equal. In terms of uncertainty, I interpret this more loosely in assuming that it increases the closer the parameterization gets towards $r = r^{thres}$ which in turn means that it decreases as the difference between r and r^{thres} increases. E.g., if the *Potential-based measure* was good measure of uncertainty, I would expect that uncertainty is low when (1) the gains from coordination are large and the losses from miscoordination small as decision makers should be quite sure that the opponent chooses a high effort and when (2) the gains from coordination are small and the losses from miscoordination large as decision makers should be quite sure that the opponent chooses a low effort while uncertainty should be high when gains and losses are similar in size.

As a second measure of strategic uncertainty, I propose the costs of effort provision. I call this measure *Trust-based measure*. While all strategies of the game are rational, choosing the maximum is sometimes associated with trust (e.g., Manzini et al. 2009, Engelmann and Normann 2010, Cartwright et al. 2013). This suggests that in cases where trust is established, decision makers should always go for the Pareto-dominant equilibrium which could therefore be interpreted as a natural standard. That choosing the maximum might indeed be perceived as such a standard is also somewhat implied by empirical evidence that shows that subjects often go for high effort levels in one-shots or in the beginning of repeated encounters (see, e.g., Van Huyck et al. 1990 and Feldhaus et al. 2015). That subjects tend to choose this standard under a lack of experience is further also in line with research from social psychology which suggests that trusting is the default state of mind (e.g., Schul et al. 2008; see also Légal el al. 2012).

I hence propose that uncertainty about the other(s) actually choosing a high effort and thus the standard may also increase in the costs of effort provision. Following this idea, people should be the more affected by external cues rather than having an inherent idea about the effort to choose as the costs increase as this also increases the uncertainty that their opponent(s) actually follow the standard. This, in turn, would also leave more room for reactions toward anchors which either reinforce the standard or provide a reason not to choose it. E.g., if the *Trust-based measure* was a good measure of uncertainty, I would expect that people do not react to their anchors for low costs, as they choose the high standard anyways, while this inclination should get weaker as costs increase.

In sum, the *Potential-based measure* suggests that uncertainty is strongest for intermediate costs while the *Trust-based measure* suggests that uncertainty increases as costs increase. As neither of the two measures is standard in the literature, I will look for evidence for either of them in the experimental data.

3. Experimental design, hypotheses, and experimental procedures

Experimental design. In the experiment, subjects play the MEG in randomly matched pairs of two as in Goeree and Holt (2001, 2005), Chen and Chen (2011) and Feldhaus et al. (2015). In contrast to the original research in Van Huyck et al. (1990), who present the MEG in tables, I use a representation as equation while also extending the strategy space as, e.g., also done in Goeree and Holt (2001). I decided to do so to make the extrema less prominent and to leave more room for anchors and choices. In addition, I make use of a one-shot version of the game rather than repeated encounters as for the given research question the presence of strategic uncertainty is important and it hence should not be resolved by experience.

In the experiment, I combine a between-subjects and a within-subject design (see Table 1). First, I vary the height of the anchor between subjects within one session. Second, I vary whether the two members of a group receive private anchors or whether they have a public, common anchor between sessions. Third, I vary the costs of effort provision within subjects; they make five decisions in total (see also Table 2).

TABLE 1. EXPERIMENTAL VARIATIONS.

Variation	Variation-level	Specifications
Anchor value	Between	110, 111, ... 170
Kind of anchor	Between	Private (*Individual*) or Public (*Common*)
Costs of effort provision (c)	Within	0.1, 0.3, 0.5, 0.7, 0.9

In my MEG, subjects simultaneously choose an integer effort level $e_i \in \{110, 111, \dots, 170\}$. However, before making their decisions, subjects have to guess the behavior of their opponent. Importantly, they cannot freely enter the number for their guess but have to answer the question whether they think that their opponent chooses at least some specific *value* – this *value* serves as the anchor. It is drawn from a uniform distribution $u[110,170]$ and is also the default when the decision is made. The anchor is drawn individually for all decision makers in treatment *Individual* while it is drawn for each pair in *Common*. This is known by the participants. Only after their guess is entered, subjects choose their effort. The payoff of a subject depends on both her own effort and the effort of the person she is matched with as described by $\pi_i(p) = r \min_{j \in N}\{e_j\} - ce_i + a$.

In the following, I call the five variations of the MEG that subjects make decisions for Game 1 to Game 5. Participants are confronted with the five variations in a random sequence to control for order effects. Across games, both the costs of effort provision c and the constant a are varied. The parameter r, representing the marginal return from the minimum, is always 1.

A subject's or a pair's anchor is the same throughout the five decisions.[9] They thus answer for each game whether their opponent will at least choose the anchor before deciding on their own effort. Each time, they can answer either "yes" or "no".

TABLE 2. PARAMETERS USED IN THE EXPERIMENT AND OUTCOMES FOR SOME HYPOTHETICAL CHOICES.

Decision	Game 1	Game 2	Game 3	Game 4	Game 5
Parameters	c = 0.1; a = -44	c = 0.3; a = -22	c = 0.5; a = 0	c = 0.7; a = 22	c = 0.9; a = 44
Choices	Payoff in ECU				
110, 110	55, 55	55, 55	55, 55	55, 55	55, 55
110, 140	55, 52	55, 46	55, 40	55, 34	55, 28
110, 170	55, 49	55, 37	55, 25	55, 13	55, 1
140, 140	82, 82	76, 76	70, 70	64, 64	58, 58
140, 170	82, 79	76, 67	70, 55	64, 43	58, 31
170, 170	109, 109	97, 97	85, 85	73, 73	61, 61

Table 2 shows the parameters c and a used across the five games and also provides examples on how these parameters affect outcomes for some hypothetical choices of two matched subjects. The costs of effort provision c increase from Game 1 to Game 5 while the constant a is always chosen such that the payoffs are normalized for choosing the minimum of 110. Subjects hence receive, regardless of the decision of their respective opponent and the game, a payoff of 55 ECU when choosing the minimum. Relative to this, payoffs can be increased or decreased when other effort levels are chosen, either by successful coordination or by miscoordination. When c is low, subjects can earn a lot from coordination while losing little from miscoordniation. When c is high, only little can be earned from coordination while subjects lose substantially from miscoordination (for examples see Table 2).

The parameter c is chosen such that potential game theory suggests the play of the maximum when $c = 0.1$ and $c = 0.3$, makes no prediction for $c = 0.5$ and proposes the minimum for $c = 0.7$ and $c = 0.9$. Hence, if the *Potential-based measure* was a good measure of uncertainty, I would expect that subjects are less uncertain about the behavior of their opponent for extreme costs while they should be the more uncertain the closer the costs are to

[9] I decided to provide subjects only with one anchor in order to avoid a multiplicity of competing anchors.

the intermediate value $c = 0.5$. If, on the other hand, the *Trust-based measure* was a good measure of uncertainty, I would expect that decision makers get the more uncertain about their opponent playing a high effort the higher the costs of effort provision.

When all decisions are made, beliefs are elicited more precisely. Here, subjects are asked to estimate the behavior of their respective opponent for each of the five games. To do so, they have to distribute 100 percentage points on the four effort intervals 110-125, 126-140, 141-155 and 156-170. The more likely they find an interval, the more percentage points should be assigned to it.

Hypotheses. In the following, I summarize the hypotheses that can be tested with this experimental design. They concern subjects' reactions towards anchors and the interaction of anchors and the degree of strategic uncertainty.

Hypothesis 1 (cognition): *Participants' choices are biased towards their anchor in Individual.*

As the presence of an anchor causes selective accessibility with regard to reasons for the respective anchor being a useful belief about the other's behavior, I expect that subjects' choices are biased towards their private anchor.

Hypothesis 2 (strategic): *The effect that subjects' choices are biased towards their anchor is more pronounced in Common than in Individual.*

A common anchor provides a group with a focal point to coordinate on and also serves as a cognitive anchor. Hence, if also strategic reasons played a role in the presence of focal points, I would expect that the effect of the public anchor is stronger when compared to the private anchor.

Hypothesis 3 (uncertainty): *The effect of anchors gets more pronounced as strategic uncertainty increases.*

Mussweiler and Strack (2000) find that uncertainty reinforces the effects of anchors. If this finding also extended to strategic interaction, I would expect more anchoring as strategic uncertainty increases.

Experimental procedures. I conducted the experiment in the Cologne Laboratory for Economic Research (CLER) in November and December of 2015. It was programmed with zTree (Fischbacher 2007) and participants were invited using ORSEE (Greiner 2015). I conducted 4 sessions with 32 participants each, two sessions per treatment *Individual* and *Common*.[10] Thus, I collected 64 observations per between-treatment.

During the experiment, subjects received their instructions on screen.[11] Due to the complicated structure of the MEG and in particular the different parameters used, the experiment is quite demanding. To facilitate understanding, subjects had to pass several tasks before the experiment was started. First, they had to enter three different cases of hypothetical choices of two matched participants where in one case the one chooses the higher number, in one case the other and in one case both choose the same number. Then they were asked to calculate the corresponding earnings for one specific cost parameter while being provided with an on-screen calculator. After that, they were to identify several outcomes for further pre-determined hypothetical choices of two matched players for two other cost parameters. From this stage onwards and then throughout the experiment, participants were provided with two sliders which could be used to calculate all feasible payoffs for a given game. To do so, they had to enter the efforts of two matched players by positioning the sliders with the corresponding payoffs being instantly calculated. These sliders were always adjusted to the relevant cost parameter of the respective decision.

[10] Two more sessions (one per treatment) were conducted initially. However, the respective data are not included in the analysis as these sessions took far longer than expected (60 minutes were announced while the sessions took about 90). Participants got nervous, one decided to leave early. One of the sessions was stopped before the belief-elicitation stage. Afterwards, I changed the announcement to 90 minutes. Including the data that could be collected in these initial sessions yields similar results to what is reported in the following.

[11] See Appendix B for screenshots of the instruction screens and of some crucial decisions screens.

Only one of the five games was paid out. This game was randomly determined at the end of the experiment. Until the end of the experiment no information about the behavior of the opponent was given. This makes the data multiple one-shot decisions. After the experiment, subjects were asked to answer a structured questionnaire on demographics and an open question on reasons for their choices. The experimental payoffs were calculated in Experimental Currency Units (ECU). The exchange rate was 7.5 ECU = 1 EUR. The average payoff was 12.90 EUR for an experiment that took about 90 minutes. Subjects were paid privately in cash after the experiment. 57% were female (43% male) and they were 22 years on average.

4. Results

In this section, the results of the experiment are discussed. First, I investigate whether the effort choices are affected by the costs of effort provision and whether evidence for differences in strategic uncertainty across games is found. Only then, I test how anchors affect behavior and whether the reaction towards anchors depends on the degree of strategic uncertainty.

The effect of effort costs on effort provision. As first shown in Goeree and Holt (2001), also my results indicate that subjects react to increasing effort costs in decreasing their effort choices. On average, taking all decisions into account, subjects choose a mean effort of 144. The corresponding value is 158 when costs are lowest ($c = 0.1$), with the average effort then decreasing to 150 ($c = 0.3$), 147 ($c = 0.5$), 135 ($c = 0.7$), 129 ($c = 0.9$) as costs increase.

Table 3 presents the corresponding regression results. In Model 1, I find an estimated decrease in effort choices of about 7.30 effort units per increase in effort costs in a linear specification. Furthermore, I observe significant differences between all games when adding them as dummies (Model 2). The estimated difference between the lowest and the highest cost of effort provision is 29 effort units. From this, I conclude that behavior in the MEG depends on the cost of effort provision even though standard theory does not predict differences conditional on the costs. This further suggests that the beliefs about the other's behavior should vary across games with me conjecturing that also the uncertainty in beliefs may be affected as costs change.

TABLE 3. EFFECTS OF EFFORT COSTS ON EFFORT CHOICES.

Dependent var.: **Effort choice**	(1)	(2)
Effort costs	-7.30*** (0.57)	--
Game 2 ($c = 0.3$)	--	-7.66*** (1.53)
Game 3 ($c = 0.5$)	--	-10.71*** 1.77
Game 4 ($c = 0.7$)	--	-22.48*** (2.10)
Game 5 ($c = 0.9$)	--	-29.07*** (2.37)
Test: Game 2 = Game 3 (p-value)	--	0.05
Test: Game 3 = Game 4 (p-value)	--	0.00
Test: Game 4 = Game 5 (p-value)	--	0.00
Observations	128/640	128/640
R-squared overall	0.22	0.22

Notes: Coefficients of random effects OLS regressions with 'Effort choice' as dependent variable. Standard errors clustered on the individual level in parentheses. Level of significance: ***p < 0.01.

Result 1: Decision makers in the MEG react to increasing effort costs in decreasing their effort choices.

Strategic uncertainty across games. I proposed two measures of strategic uncertainty: the *Potential-based measure* and the *Trust-based measure*. In order to test for empirical evidence on how the five games actually differ in terms of strategic uncertainty, I make use of two empirical proxies. The first proxy is the uncertainty in beliefs (what subjects say they think – *Belief proxy* in the following) and the second is the variability in choices (what subjects should think – *Choice proxy* in the following).[12]

Participants stated their beliefs in distributing 100 percentage points for each game on the four intervals 110-125, 126-140, 141-155 and 156-170 (with $a_1, \ldots, a_4$ being the percentage points

[12] Both measures are noisy as they are only collected after subjects are confronted with the treatment variations which might hence also cause the relations between the costs and the proxies. Still, despite this limitation, it seems reasonable to seek for empirical evidence for changes in uncertainty across games as there is no established way to measure it. However, one should keep this in mind when interpreting the data.

attributed to the first, …, fourth interval). The *Belief proxy* is calculated by summing up the absolute differences between the percentage points attributed by an individual i to an interval k and 25 (as equation: $Belief\ proxy_i = \sum_{k=1}^{4} |a_{ki} - 25|$). The resulting variable is low for high uncertainty and high for a low uncertainty and lies between 0 and 150. In fact, it is 0 if a subject expects that each interval is chosen with 25% probability while it is 150 if she is sure that the opponent's choice lies in one particular interval. Hence, a low value implies a high degree of uncertainty as the belief suggests that it is unlikely that the opponent's choice is in one particular interval whereas a high value implies a low degree of uncertainty as the corresponding decision maker is sure about the other's behavior.

TABLE 4. RELATION BETWEEN EFFORT COSTS AND THE BELIEF PROXY.

Dependent var.: **Belief proxy**	(1)	(2)
Effort costs	0.88	-32.36***
	(0.82)	(3.73)
Effort costs sq.	--	5.54***
		(0.65)
Observations	128/640	128/640
R-squared overall	0.00	0.06

Notes: Coefficients of random effects OLS regressions with the 'Belief proxy' as dependent variable. Standard errors clustered on the individual level in parentheses. Level of significance: ***p < 0.01.

Analyzing this *Belief proxy*, I observe substantial differences across games with its value being 84 in Game 1 and 67, 64, 70, and 87 in Games 2-5. The corresponding regressions can be found in Table 4. I regress the *Belief proxy* on the effort costs only (in Model 1) and on the effort costs squared (Model 2) and find a strong effect of the non-linear specification in Model 2. The analysis reveals the relationship between the costs of effort provision and the *Belief proxy* that I would expect from the *Potential-based measure* of uncertainty: decision makers seem quite uncertain about the behavior of their opponent for medium costs of effort provision while being less uncertain for both very low and very high costs. In contrast, if uncertainty was described well by the *Trust-based measure*, uncertainty in the beliefs should increase as effort costs increase. However, this is not found to be significant in Model 1.[13]

If the *Potential-based measure* was actually a good measure of uncertainty and potential game theory a good predictor for behavior in the one-shot MEG, I would expect not only more uncertainty in the beliefs but also more variability in behavior for medium costs while the respective variability should be lower for extreme costs. This is my second empirical measure of uncertainty – the *Choice proxy*. It represents how uncertain subjects *should* be about their opponent's behavior.

TABLE 5. RELATION BETWEEN EFFORT COSTS AND THE CHOICE PROXY.

Dependent var.: **Choice proxy**	(1)	(2)
Effort costs	1.35***	1.57
	(0.34)	(1.30)
Effort costs sq.	--	-0.04
		(0.21)
Observations	128/640	128/640
R-squared overall	0.03	0.03

Notes: Coefficients of random effects OLS regressions with the 'Choice proxy' as dependent variable. Standard errors clustered on the individual level in parentheses. Level of significance: ***p < 0.01.

To calculate the *Choice proxy*, I take the absolute difference between an individual i's effort choice for a given game G and the mean of all decisions in that game as dependent variable (as equation: $Choice\ proxy_i = |effort_{iG} - \overline{effort_G}$). This variable is on average 13 effort units in Game 1, 15 in Game 2, 16 in Game 3, and 18 and 19 in Game 4 and Game 5 respectively. Using the same empirical strategy as in Table 4, regressing the *Choice proxy* on the effort costs (Model 1) and the respective squared term (Model 2), I find just the opposite of what was found before: while a significant linear effect is observed, no effect is found with regard to the non-linear specification. This is not in line with the *Potential-based measure* but

[13] The *Belief proxy* seems a good measure of uncertainty as it attributes reasonable values to many distributions of percentage points across intervals. Still, it has some flaws: e.g., cases where 50 percentage points are attributed to two categories each, have the same *Belief proxy* value as cases where one category receives 75 points and one other 25, even though the latter decision maker seems less uncertain. However, if I replace the *Belief proxy* by the highest amount of percentage points attributed to *one* interval (*Max proxy*), which can also be interpreted as a proxy for belief uncertainty, the results remain similar. See Table A.1 in the Appendix for details.

suggest that uncertainty *should* increase in the costs of effort provision as suggested by the *Trust-based measure*.

In sum, I find conflicting evidence with regard to the two measures of uncertainty in the *Belief proxy* and the *Choice proxy*. I hence conclude that both measures should be considered when testing whether the reactions towards anchors vary in the degree of strategic uncertainty.

Result 2: While the Belief proxy is well in line with the Potential-based measure of strategic uncertainty, this does not hold for the Choice proxy which rather supports the Trust-based measure as a measure of strategic uncertainty.

Anchors and their effects on choices. I now go on to measure the effect of anchors on effort choices. In the first step, I only look at cases where subjects choose exactly their anchor and test whether more subjects choose it than would be suggested by a random matching of 'normal' choices and anchors absent any anchoring. In the second step, I investigate whether I observe a correlation between choices and anchors and test how this differs between *Individual* and *Common*.

Choosing exactly the anchor. In treatment *Individual* subjects choose their anchor in 11.6% of all cases (37 out of 320) while in treatment *Common* the corresponding value is with 25.6% (82 out of 320 cases) substantially higher. Taking all five decisions on the subject level into account, subjects in *Individual* choose their anchor 0.58 times and they do so 1.28 times in *Common*. These values differ when compared with a Mann-Whitney U test (MWU, $p < 0.01$), indicating more anchoring in *Common*.

In addition, I find that in *Individual* 29.7% of all subjects (19 of 64) choose their anchor at least once with this share being 57.8% (37 of 64) in *Common*. These values can be compared to the expected share of subjects whose normal choice is matched by the random anchor at least once across the five games. The corresponding probability is $1 - \left(1 - {}^{1}\!/_{61}\right)^{5} \approx 0.079$, with the 61 being all possible anchors and 5 the games for which a decision is made. This corresponds to about 5.08 in 64 subjects. Rounded up, I would hence expect that 6 out of 64 decision makers should choose their anchor at least once even if they are not affected by it.[14] The corresponding null-hypothesis is that subjects choose their effort independent of their anchor value, i.e., in this case, one would expect that six subjects choose exactly their anchor. However, I find that this value differs from both the share of decision makers choosing their anchor at least once in *Individual* (6 out of 64 vs 19 out of 64, chi-squared test, $p < 0.01$) and from the share of decision makers doing so in *Common* (6 out of 64 vs 37 out of 64, chi-squared test, $p < 0.01$). Also, the treatments *Individual* and *Common* differ in this respect (19 out of 64 vs 37 out of 64, chi-squared test, $p < 0.01$). These results lend strong support to both **Hypothesis 1** and **Hypothesis 2**.

Result 3: Subjects' choices are affected by both private and public anchors when it comes to exactly choosing the anchor. The effect is amplified when anchors are public.

TABLE 6. EFFECTS OF PRIVATE VS PUBLIC ANCHORS ON EFFORT CHOICES.

Dependent var.: **Effort choice**	(1)	(2)	(3)
Anchor	0.26***	0.38***	0.19**
	(0.07)	(0.08)	(0.09)
Anchor x Individual	--	-0.28**	-0.20
		(0.13)	(0.14)
Individual	--	37.01**	24.40
		(18.08)	(18.81)
Observations	128/640	128/640	123/521
R-squared overall	0.05	0.07	0.02

Notes: Coefficients of random effects OLS regressions with 'Effort choice' as dependent variable. Standard errors clustered on the individual level in parentheses. Levels of significance: $^{**}p < 0.05$; $^{***}p < 0.01$.

Correlation of choices and anchors. I next observe whether these findings translate into a correlation of anchors and choices. Table 6 presents the corresponding regression results. In Model 1, the effort choice is regressed on the anchor only. I find a highly significant correlation between the anchor and the corresponding effort choice. This effect is not only statistically significant but also economically large – the model implies that for every increase in the anchor by 1, the effort choice increases by 0.26. Model 2 further shows that this effect

[14] This is a very conservative measure as decision makers may want to choose same values several times as, for example, suggested by potential game theory. If this would be taken into account, the probability that the random anchor matches the normal choice at least once can only be lower.

differs between treatments, being weaker in case of an individual anchor.[15] Finally, in Model 3, I only make use of observations that do not exactly choose their anchor. Again, I find a significant effect (only in case of a common anchor) of the anchor on the corresponding choice.[16] This suggests that also in cases where subjects do not exactly choose their anchor, they seem influenced by its value when making their choice.

Result 4: Higher anchors imply higher choices. This effect is stronger for public than for private anchors and it is also found for subjects not exactly choosing their anchor in case it is public.

Anchoring and the degree of strategic uncertainty. Finally, I test for evidence concerning **Hypothesis 3** which claims that anchoring depends on the degree of strategic uncertainty. Whereas the *Potential-based measure* suggests that uncertainty is high for intermediate costs of effort provision and low for extreme costs, the *Trust-based measure* suggests that uncertainty increases in the costs. I again investigate first the cases where decision makers choose exactly their anchor and then the correlation between anchors and choices.

The share of subjects exactly choosing their anchor is similar across games. In Game 1, 14.1% (18 out of 128) choose exactly their anchor, with the corresponding values being 21.1% (27 out of 128), 18.8% (24 out of 128), 19.5% (25 out of 128), and 19.5% (25 out of 128) in Games 2 – 5. These values suggest no systematic differences across the costs of effort provision in a subject's propensity to choose exactly the anchor.[17]

I finally test whether I find evidence for differences conditional on the degree of strategic uncertainty when investigating the correlation of anchors and choices. I generate a variable reflecting strategic uncertainty as suggested by the *Potential-based measure*. This variable

[15] In fact, when running the regression for both treatments separately, I only find a significant correlation in *Common*. See Table A.2 in the Appendix.

[16] Given the finding that subjects react to their private anchors when it comes to exactly choosing it, this somewhat suggests that they consider it only when it is close to their normal choice.

[17] This can be substantiated using LPM models. In Table A.3 in the Appendix, I provide models estimating the probability that decision makers choose exactly their anchor. No systematic differences are found across costs of effort provision and neither Model 1 nor Model 2 is significant as a whole.

(*PG uncertainty*) is 0 for Game 1 and Game 5 as subjects should be least uncertain about the behavior of their opponent and hence not react strongly to the anchor, it is 1 for Game 2 and Game 4 as intermediate uncertainty would be suggested by the *Potential-based measure* and it is 2 for Game 3 as uncertainty should be highest in this case. Concerning the *Trust-based measure* of uncertainty, I would expect that uncertainty increases in the costs of effort provision (which is reflected in the variable *Trust uncertainty* which is identical to the linear effort cost variable). The results of the regressions are shown in Table 7.

TABLE 7. EFFECTS OF ANCHORS ON EFFORT CHOICES CONDITIONAL ON THE DEGREE OF STRATEGIC UNCERTAINTY.

Dependent var.: **Effort choice**	(1)	(2)
Anchor	0.28***	0.05
	(0.06)	(0.10)
PG uncertainty	5.65	--
	(5.67)	
Anchor x PG uncertainty	-0.03	--
	(0.04)	
Trust uncertainty	--	-17.00***
		(4.06)
Anchor x Trust uncertainty	--	0.07**
		(0.03)
Observations	128/640	128/640
R-squared overall	0.05	0.27

Notes: Coefficients of random effects OLS regressions with ‘Effort choice’ as dependent variable. Standard errors clustered on the individual level in parentheses. Levels of significance: **p<0.05; ***p < 0.01.

In Model 1, I interact the anchor value with the *PG uncertainty* variable. While I still observe a substantial effect of the anchor per se, no evidence is found that anchoring gets more pronounced as uncertainty according to the *Potential-based measure* increases. To test whether uncertainty as suggested by the *Trust-based measure* affects anchoring, I conduct the same analysis substituting the *PG uncertainty* with the *Trust uncertainty* variable. The respective results are shown in Model 2. I observe first, that the direct effect of the anchor vanishes and second, a negative effect of the *Trust uncertainty* variable which confirms that decision makers choose lower effort levels as the costs of effort provision increase. Importantly, the analysis further reveals an interaction between the anchor and *Trust* *uncertainty*. This shows that subjects react the more to their anchor the higher the degree of strategic uncertainty as suggested by the *Trust-based measure*.

Result 5: Decision makers react the more to their anchors the higher the costs of effort provision and hence the Trust-based measure of uncertainty.

5. Conclusion

The present paper investigates the relevance of private vs public anchors in social interaction. To approach this realm, I set up a laboratory minimum effort game experiment varying the height of the anchor, whether they are private or public and the costs of effort provision. In doing so, I am able to disentangle cognitive and strategic reasons for reactions towards focality in coordination and to investigate how anchoring is related to the gains and risks from (mis)coordination and hence the degree of strategic uncertainty.

The experimental results provide evidence for both cognitive and strategic reasons causing subjects' reactions towards public anchors. Thereby, this research suggests that not only strategic considerations play a role when decision makers are confronted with focal points but that also cognition might be relevant for the respective findings. In addition, I observe empirical evidence that the extent of such effects increases as strategic uncertainty increases.[18]

In this paper, I propose different measures of strategic uncertainty for the MEG. As there is no established way to measure it, I provide arguments as well as empirical evidence for two measures which are both defined in terms of the gains and risks from (mis)coordination. However, the current study seems not conclusive in this respect and further research is needed to get a better understanding about how strategic uncertainty can be measured in general and in the MEG in particular. E.g., the fact that uncertainty in beliefs seems well in line with the

[18] As I only find a difference in anchoring conditional on the degree of strategic uncertainty when analyzing the correlation but not when looking at the cases where exactly the anchor is chosen, the respective evidence seems somewhat limited. However, it might well be that the amount of data is not sufficient to identify the respective effect or the degree of uncertainty might be more important in cases where the 'normal' choice is not close to the anchor.

Potential-based measure of uncertainty is an interesting finding to start research concerning this question.

With this in mind, it remains unclear why subjects react stronger to anchors when uncertainty increases according to the *Trust-based measure* while this is not found for the *Potential-based measure*. This is in particular relevant as I find empirical evidence in favor of the latter measure that seems to indicate that uncertainty in beliefs is described well by the *Potential-based measure*. These results hence somewhat suggest that decision makers in the MEG often decide heuristically while, in case they are explicitely asked to state and think about their beliefs, they know under which circumstances they *should* be more or less uncertain. To provide a clearer picture regarding the relationship of uncertainty and beliefs vs behavior might be a fruitful topic for future research.

In sum, this paper can only serve as a first step towards the study of anchoring in belief-formation in the MEG. However, it seems a promising endeavor to further investigate how non-rational cognitive effects of, e.g., different types of information-processing, emotions, or frames affect beliefs and choices in social interaction (see, e.g., Cohn et al. 2014, 2015; LeCoq et al. 2015; Berger et al. 2015; Bolton et al. forthcoming for examples from the economic literature).

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Appendix

A. Additional analyses

TABLE A.1. RELATION BETWEEN EFFORT COSTS AND THE MAX PROXY.

Dependent var.: **Max proxy**	(1)	(2)
Effort costs	0.42	-18.14***
	(0.43)	(2.15)
Effort costs sq.	--	3.09***
		(0.37)
Observations	128/640	128/640
R-squared overall	0.00	0.06

Notes: Coefficients of random effects OLS regressions with the 'Max proxy' as dependent variable. Standard errors clustered on the individual level in parentheses. Level of significance: ***p < 0.01. Average maximum number of percentage points attributed to one category (*Max proxy*) in Games 1-5: 64, 54, 53, 56, 66.

TABLE A.2. EFFECTS OF ANCHORS ON EFFORT CHOICES BY TREATMENT: MODEL 1 REGARDS INDIVIDUAL ANCHORS AND MODEL 2 COMMON ANCHORS.

Dependent var.: **Effort choice**	(1)	(2)
Anchor	0.09	0.38***
	(0.11)	(0.08)
Observations	64/320	64/320
R-squared overall	0.00	0.13

Notes: Coefficients of random effects OLS regressions with 'Effort choice' as dependent variable. Standard errors clustered on the individual level in parentheses. Level of significance: ***p < 0.01.

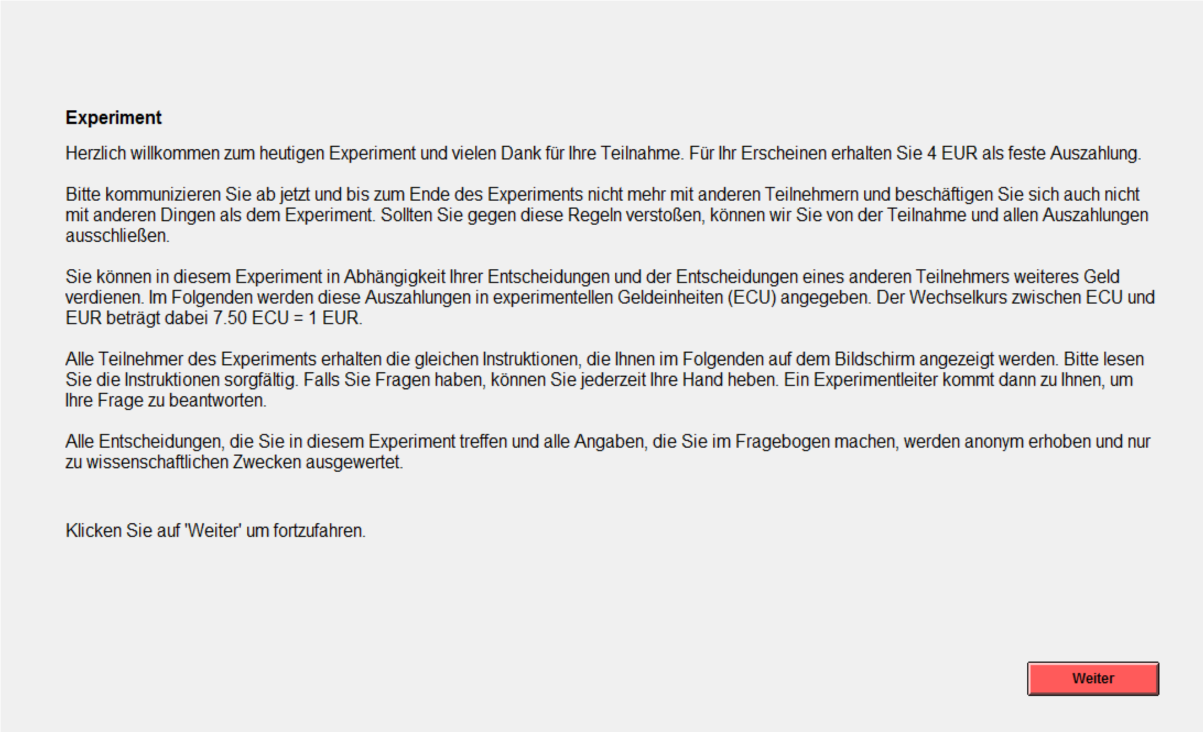
TABLE A.3. EFFECTS OF EFFORT COSTS ON THE PROBABILITY TO CHOOSE EXACTLY THE ANCHOR.

Dependent var.: **Chooses anchor**	(1)	(2)
Effort costs	0.01	--
	(0.01)	
Game 2 ($c = 0.3$)	--	0.07**
		(0.04)
Game 3 ($c = 0.5$)	--	0.05
		(0.03)
Game 4 ($c = 0.7$)	--	0.05
		(0.04)
Game 5 ($c = 0.9$)	--	0.05
		(0.04)
Observations	128/640	128/640
R-squared overall	0.00	0.00

Notes: Coefficients of random effects LPM regressions with 'Chooses anchor' as dependent variable. Standard errors clustered on the individual level in parentheses. Level of significance: **p < 0.05. Overall p-value Model 2: 0.34.

B. Screenshots: Instructions and experimental design (relevant translations are below the respective screenshot)

Screenshot 1. General instructions



Experiment

Herzlich willkommen zum heutigen Experiment und vielen Dank für Ihre Teilnahme. Für Ihr Erscheinen erhalten Sie 4 EUR als feste Auszahlung.

Bitte kommunizieren Sie ab jetzt und bis zum Ende des Experiments nicht mehr mit anderen Teilnehmern und beschäftigen Sie sich auch nicht mit anderen Dingen als dem Experiment. Sollten Sie gegen diese Regeln verstoßen, können wir Sie von der Teilnahme und allen Auszahlungen ausschließen.

Sie können in diesem Experiment in Abhängigkeit Ihrer Entscheidungen und der Entscheidungen eines anderen Teilnehmers weiteres Geld verdienen. Im Folgenden werden diese Auszahlungen in experimentellen Geldeinheiten (ECU) angegeben. Der Wechselkurs zwischen ECU und EUR beträgt dabei 7.50 ECU = 1 EUR.

Alle Teilnehmer des Experiments erhalten die gleichen Instruktionen, die Ihnen im Folgenden auf dem Bildschirm angezeigt werden. Bitte lesen Sie die Instruktionen sorgfältig. Falls Sie Fragen haben, können Sie jederzeit Ihre Hand heben. Ein Experimentleiter kommt dann zu Ihnen, um Ihre Frage zu beantworten.

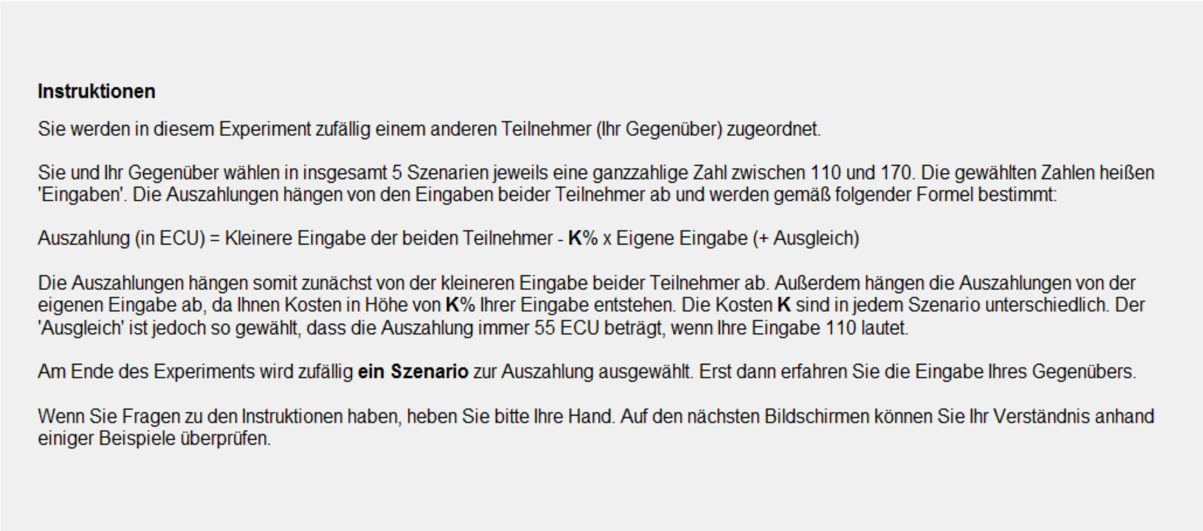
Alle Entscheidungen, die Sie in diesem Experiment treffen und alle Angaben, die Sie im Fragebogen machen, werden anonym erhoben und nur zu wissenschaftlichen Zwecken ausgewertet.

Klicken Sie auf 'Weiter' um fortzufahren.

Weiter

Standard screen welcoming the participants and explaining general rules such as prohibition of communication.

Screenshot 2. Instructions explaining the setting



Instruktionen

Sie werden in diesem Experiment zufällig einem anderen Teilnehmer (Ihr Gegenüber) zugeordnet.

Sie und Ihr Gegenüber wählen in insgesamt 5 Szenarien jeweils eine ganzzahlige Zahl zwischen 110 und 170. Die gewählten Zahlen heißen 'Eingaben'. Die Auszahlungen hängen von den Eingaben beider Teilnehmer ab und werden gemäß folgender Formel bestimmt:

Auszahlung (in ECU) = Kleinere Eingabe der beiden Teilnehmer - **K%** x Eigene Eingabe (+ Ausgleich)

Die Auszahlungen hängen somit zunächst von der kleineren Eingabe beider Teilnehmer ab. Außerdem hängen die Auszahlungen von der eigenen Eingabe ab, da Ihnen Kosten in Höhe von **K%** Ihrer Eingabe entstehen. Die Kosten **K** sind in jedem Szenario unterschiedlich. Der 'Ausgleich' ist jedoch so gewählt, dass die Auszahlung immer 55 ECU beträgt, wenn Ihre Eingabe 110 lautet.

Am Ende des Experiments wird zufällig **ein Szenario** zur Auszahlung ausgewählt. Erst dann erfahren Sie die Eingabe Ihres Gegenübers.

Wenn Sie Fragen zu den Instruktionen haben, heben Sie bitte Ihre Hand. Auf den nächsten Bildschirmen können Sie Ihr Verständnis anhand einiger Beispiele überprüfen.

Instructions (translation)

In this experiment, you are randomly assigned to another participant (your counterpart).

You and your counterpart choose in 5 scenarios integer numbers between 110 and 170. These numbers are called “inputs”. The payoffs of both participants are dependent on the inputs of both participants as described by the following equation:

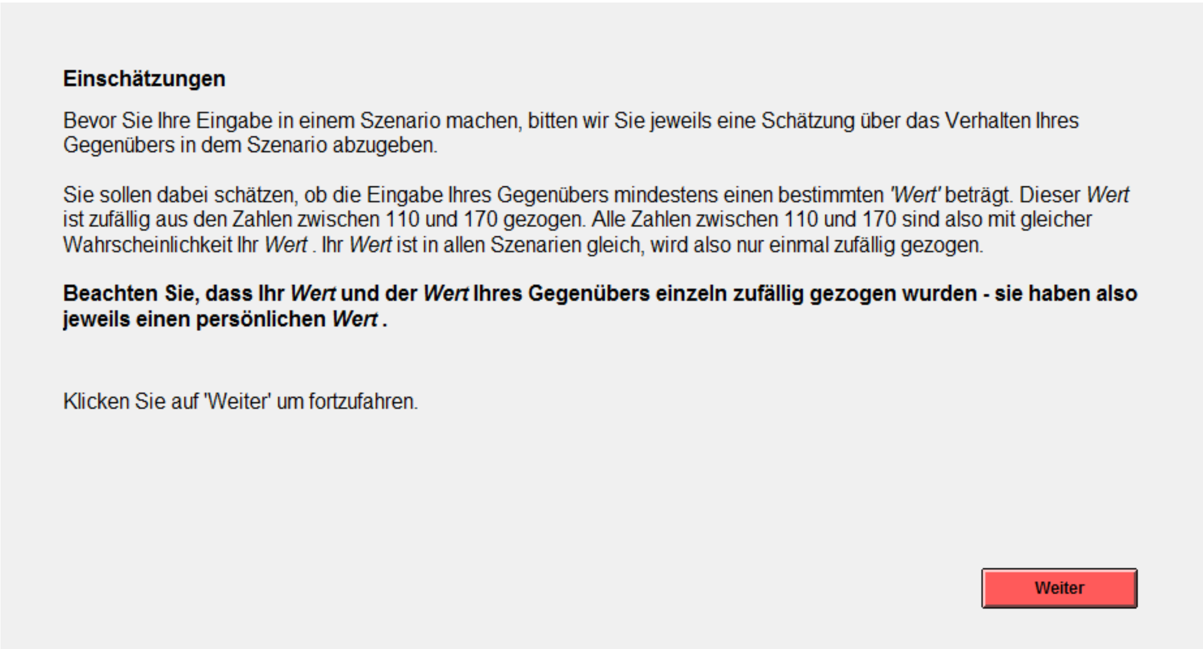
$$\begin{aligned} Payoff\ (in\ ECU) \\ &= Smaller\ Input\ of\ both\ participants \\ &- K\%xOwn\ Input(+\ compensation) \end{aligned}$$

Hence, payoffs are first determined by the smaller input of both participants. Furthermore, the payoffs depend on your own input as your input costs you K% of your input. The cost parameter K is different in each scenario. However, the compensation is always chosen such that your payoff is 55 ECU when choosing 110.

At the end of the experiment one scenario is randomly chosen for payoff. Only then, you get to know the input of your counterpart.

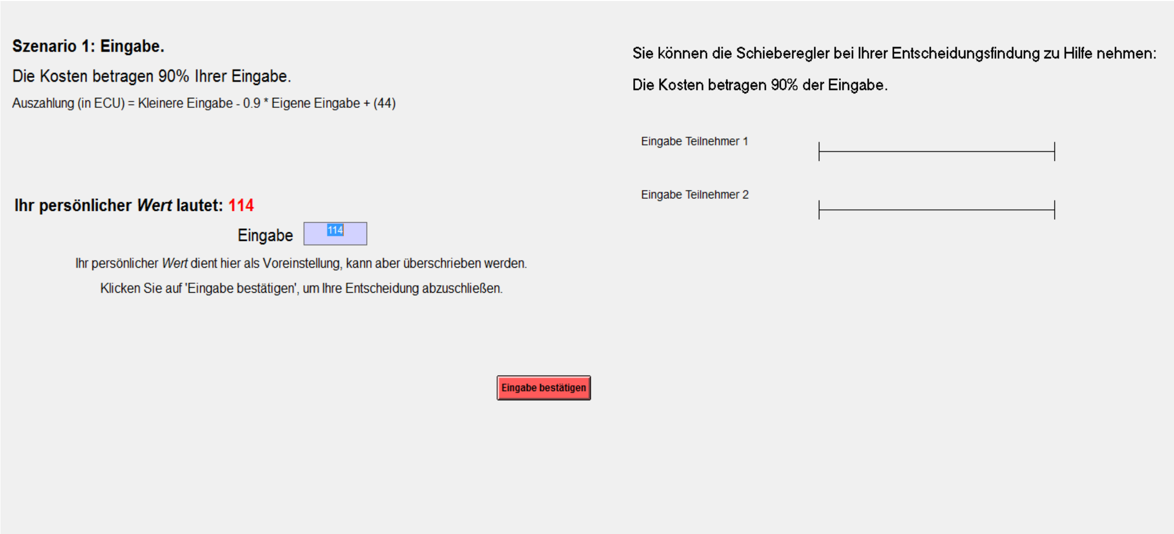
If you have any question, please raise your hand. On the next screens, you can test your understanding in several examples.

Screenshot 3. Treatment variation private vs public

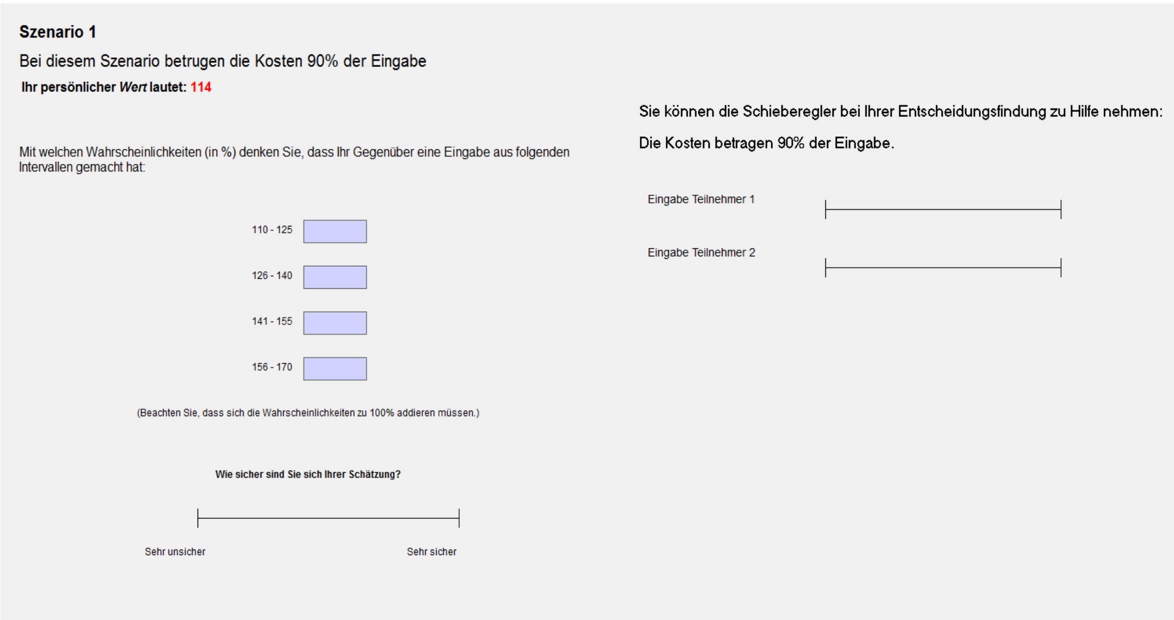


This screen describes how participants have to guess the choices of their counterpart (see section 3). Importantly, in one treatment (*Individual*), they get to know that their random value is determined independently of their counterpart’s value whereas in the other treatment (*Common*), they get to know that the random value is the same for both participants.

Screenshot 4. Choice stage and decision support (on the right).



Screenshot 5. Belief elicitation and decision support (on the right).



Chapter 4

Social interaction promotes risk taking in a stag hunt game*

Co-authors: Gary Bolton and Axel Ockenfels

Abstract

We demonstrate that people are more willing to take risks in a stag hunt game when the agent of uncertainty is another person, thereby promoting cooperation. Recent social cognition research suggests an explanation for this pattern, which is based on the idea that games that align interests between subjects activate a trust mindset.

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1. Introduction

It is risky to rely on others. Yet voluntary cooperation requires individuals to take the chance. Voluntary cooperation requires an individual investment of resources that pays only if others choose to invest as well. What makes people more or less likely to accept the 'strategic uncertainty' – risk based in the uncertainty about the actions of others – is a central issue in all the games commonly used to study economic cooperation, including the prisoner's dilemma, trust games and coordination games. In these games, the success or failure of the cooperative venture hinges critically on expectations that others will participate.

Recent laboratory studies of trust games find that the human factor inherent in strategic uncertainty is an important element in people's choice to cooperate or not (Bohnet and Zeckhauser 2004; Bohnet et al. 2008; Bohnet et al. 2010; Aimone and Houser 2011; Aimone and Houser 2012; Fetchenhauer and Dunning 2012; Aimone and Houser 2013; Butler and Miller 2015). The trust game has two players. The first player decides whether to invest in cooperation. Doing so increases the size of the social pie. The second player decides (assuming the first player invests) how to divide the pie between the players. Whether and how much the first player profits from her investment thus depends on the second player's trustworthiness. The finding in previous research is that the first player is less likely to invest in cooperation when he or she knows that the risk is generated from (strategic) uncertainty about the choice of the second player than from 'ordinary risk', in this case risk is generated by a mechanical device choosing on behalf of the second player, even though the objective probability of cooperating is the same in the two cases. This diminishment of trust in the face of social risk is termed 'betrayal aversion' (Bohnet and Zeckhauser 2004).

Betrayal aversion has potentially important implications for the nature of human cooperative behavior and for the degree of institutional intrusion necessary for cooperation to succeed. The nature of these implications, however, depends critically on the underlying behavioral mechanisms. In the present research we investigate whether, in addition to betrayal aversion as studied before, behavior in trust-related contexts might also be affected by the cognitive process they trigger. Here we consider two hypotheses, both consistent with the aforementioned observation in the trust game.

One mechanism is motivational in nature: betrayal aversion is triggered by disappointment about untrustworthiness. This disappointment has two dimensions. One is disappointment about the other's choice to selfishly increase *his or her* payoff. The other one is disappointment about the other's failure to meet *one's own* expected payoffs. (While untrustworthiness in the trust game implies both kinds of disappointment, this is not the case in the coordination game as we will see below.) This line of reasoning implies that strategic uncertainty per se, doubt about the action the other player will choose (Van Huyck et al. 1990), hinders the propensity to cooperate, which is our Hypothesis 1 (H1).[1]

A second potential behavioral mechanism is cognitive in nature: recent social cognition research shows that human information processing differs substantially under a 'trust state of mind' compared to a 'distrust state of mind'. Specifically, starting with a seminal paper by Schul et al. (2004), it has been consistently shown that "*the cognitive system reacts to distrust by automatically inducing the consideration of incongruent associations — it seems designed to ask 'and what if the information were false?'*" (p. 668).[2] Though this literature has not yet been applied to behavioral economics research paradigms, it strongly suggests that games that pose a conflict of interest (where there is reason to distrust another person) induce a distrust state of mind and thus a different kind of information processing than games that align interests (where there is reason to trust another person). From this perspective, (part of) the

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[1] In a somewhat related study, Butler and Miller (2015) emphasize the role of intentions in social risk taking. In particular, they show that the inability to intentionally betray (as the agent does not know which of the two options is 'betray' and 'not betray') makes subjects take even more risks as compared to a mere risky decision in a social context.

[2] The processing difference for trust vs. distrust manifests on different levels of information processing. Even at the most basic level of semantic activation, distrust seems to entail non-routine effects: Under distrust, participants activate incongruent and remote associations (Mayer and Mussweiler 2011; Schul et al. 2004). At higher levels of information processing, distrust increases cognitive flexibility, creativity (Mayer and Mussweiler 2011), sensitivity to non-routine contingencies (Schul et al. 2008), information interpretation in multiple frames (Schul et al. 1996), and multiple category activation (Friesen and Sinclair 2010). Furthermore, distrust reduces stereotyping (Posten and Mussweiler 2013).

reason why people trust less in a trust game when the agent of uncertainty is another person is that this variant of cooperation games, the trust game, poses a conflict of interests and thus a different, more ‘skeptical’ processing of the probability about the other’s trustworthiness compared to an otherwise identical game where the agent of uncertainty is a lottery and thus no conflict of interests is involved (H2).[3] Our experiment provides a test for these two hypotheses.

While little attention has been paid to the influence of cognition on coordination game behavior, a number of investigations have focused on the influence of structural elements in coordination games with Pareto-rankable equilibria (Devetag and Ortmann 2007; Weber 2008) and on the measurement of strategic uncertainty in coordination games (Heinemann et al. 2009). With regard to the stag hunt game, the structural elements known to influence whether there is cooperation or not include communication (Cooper et al. 1992; Clark et al. 2001), knowledge of the past play or truthfulness of the other player (Duffy and Feltovich 2002; Duffy and Feltovich 2006), credible strategy assignments (Bangun et al. 2006), avoidance of loss payoffs (Rydval and Ortmann 2005; Feltovich et al. 2012), the pecuniary incentives to play best response (Battalio et al. 2001; Dubois et al. 2011), whether the game is one shot or repeated (Clark and Sefton 2001), conventions (Rankin et al. 2000), the opportunity to learn (Stahl and Van Huyck 2002), and group decision-making (Charness and Jackson 2009; Feltovich and Grossman 2014).[4]

[3] The processing of probabilities has previously been shown to depend on context (Windschitl and Weber 1999; see also Cohn et al. 2015). We note that, instead of differences in the processing of given probabilities, another mechanism that could explain the predicted pattern is that the social context might affect risk attitudes which, in turn, may affect behavior. However, it has been shown that the decision to trust is hardly related to risk attitudes (Eckel and Wilson 2004; Houser et al. 2010).

[4] Structural elements have also been studied in the closely related minimum effort game. As examples: the advice from other players (Chaudhuri et al. 2009), recommendations (Chaudhuri and Paichayontvijit 2010), rewards and sanctions (Dugar 2010; Galbiati et al. 2013), the history of play (Al-Ubaydli 2011; Cason et al. 2012), as well as group decisions (Feri et al. 2010) and a common identity (Chen and Chen 2011) were found to influence behavior.

2. The experiment

To separate the two sources of betrayal aversion, we conducted an experiment with human subjects and monetary incentives using the stag hunt coordination game. Stag hunt, as the name implies, was originally framed in terms of cooperative hunting (Rousseau 1984/1755). Figure 1 illustrates the normal-form representation of the stag hunt game investigated in this research. In the game, players (simultaneously) choose between a safe option that pays a fixed but moderate payoff and a risky one that pays more if the other player also goes for the risky option but less if she goes for the safe one. In terms of hunting, they can either elect to hunt stag – risky because it requires the cooperation of the other player in order to be effective – or they can choose to hunt hare – an activity that can be done as effectively alone as in a group. Whether a player selects hunting stag or hare depends critically on his or her expected probability that the other player will choose to cooperate. In fact, both players hunting stag and both players hunting hare each constitute a Nash equilibrium, the former case representing mutually optimistic expectations for cooperative effort, the latter case representing mutually pessimistic expectations. While the former equilibrium is payoff dominant, the latter is risk dominant (Harsanyi and Selten 1988; Van Huyck et al. 1990). The stag hunt serves as a prototype for many kinds of social contracts. It also serves as a reduced form representation of repeated games where cooperation can be exploited, such as the indefinite horizon repeated prisoner's dilemma in which players can choose either to play tit-for-tat or not to cooperate (Skyrms 2004).

Importantly, a player in the stag hunt game cannot be exploited for individual gain: while the failure to cooperate imposes a loss on the cooperator, it provides no gain for the non-cooperator (in fact, in terms of lost opportunity, the non-cooperator also incurs a loss). That is, the stag hunt game perfectly aligns players' interests, and according to our second hypothesis may thus induce a different kind of information processing as compared to the trust game, where the interests of players are conflicting.

		Y/Lottery chooses			
		Option Y1/L1		Option Y2/L2	
X chooses	Option X1	X gets 11 €	Y gets 11 €	X gets 1 €	Y gets 7 €
	Option X2	X gets 7 €	Y gets 1 €	X gets 7 €	Y gets 7 €

FIGURE 1. LABORATORY NORMAL FORM GAMES.

Returning to the structure of the game and the parameters used in our experiment (Figure 1). The row players are X and the column players are Y. X chooses between the two options X1 and X2. While option X1 results either in a payoff of 11 or 1 €, depending on the decision of Y, option X2 guarantees a payoff of 7 €.[5] Thus, X takes a decision for either a risky or a safe option, with another participant being the source of the risk. The game is fully symmetric, meaning Y chooses between the same options (Y1 and Y2).

The test pivots on the behavior of the X players. We use the technique pioneered by Bohnet and Zeckhauser (2004) to separate actions due to strategic risk from those due to ordinary risk or social context. We implement three treatments (T1, T2 and T3). In each of these treatments the X players play a variation of the coordination game shown in Figure 1, such that they take their decision with the risk being due to another person (T1), due to a lottery but still in the social context of a human Y player (T2), and due to a lottery but absent social context (T3). The test allows us to investigate how uncertainty due to human behavior compares to an otherwise identical risky decision (comparing T1 and T2), and how such a risky decision compares to an equally risky decision without social context (comparing T2 and T3). Specifically:

Treatment 1 (T1 – strategic uncertainty): In T1, we initially ask the 16 Y players whether they prefer the risky option Y1 or the safe option Y2. We call the share of Y players choosing the

[5] Given our parameters a risk neutral decision maker plays stag for any p > 60% that the other person also goes for stag. The corresponding value in Bohnet and Zeckhauser (2004) is p > 29%. The reason we deviated from the latter value is that, in stag hunt games, the potential gain from coordination is typically relatively small and the potential loss from a coordination failure is relatively large (see, e.g., Cooper et al. 1992).

risky option p*, which can take one of the values 0/16, 1/16, 2/16, ... 16/16. Subsequently, X players are asked to state, conditional on each possible p*, whether they prefer X1 or X2.[6] Then, all participants get to know the actual share of players Y choosing option Y1, the actual decision of their respective opponent, and their payoff.

Treatment 2 (T2 – lottery in a social context): There are human Y players in these sessions as in T1, but they do not choose between the risky and the safe option. Instead, a lottery chooses option L1, equivalent to option Y1 in Figure 1, with probability p* which again can take the value of either 0/16, 1/16, 2/16, ... or 16/16. To determine p*, we put 17 envelopes into an urn. Each of them contains a number stating a possible probability that the lottery chooses option L1. Then, one envelope is randomly drawn from the urn and stuck to a blackboard in the lab until it is opened after all decisions are made, so that everybody can verify that p* was chosen prior to X players making their decisions. Then, as in T1, X players choose, conditional on each possible p*, whether they prefer option X1 or X2. When all decisions are made, p* is announced, the lottery is resolved and the participants get to know the decision of their respective opponent and their payoffs.

Treatment 3 (T3 – lottery absent social context): T3 is identical to T2, save that there is no human player Y, which is known to X players.

Hypotheses: According to H1, if a personal disappointment caused by the other's intention to exploit X for his or her own benefit causes the betrayal aversion effect, we would expect player X's willingness to play the risky option to be similar across all three treatments because this kind of exploitation is not possible in the stag hunt game. Alternatively, if disappointment arises when one's expectations regarding one's own payoffs are not met due to the opponent's (intentional) choice, we would expect player X's willingness to play the

[6] We decided to ask subjects for each given probability whether they want to play X1 or X2 in order to facilitate decision making.

risky option to be smaller in T1 compared to T2 and T3 because (similar to the trust game) only the safe choice guarantees that one's expectations cannot be betrayed.

If, on the other hand, different modes of information processing under trust and distrust mindsets (H2) were relevant, we would expect the opposite pattern compared to what has been observed in trust game experiments, namely that players X will exhibit a larger willingness to accept risk in T1 than in T2 and T3. The reason is that T1 aligns interests and thus induces a trust mindset, which — as suggested by social cognition research — results in more 'confident' information processing of a given probability about the other's willingness to coordinate efficiently compared to an otherwise identical game where the agent of uncertainty is a lottery and thus no interpersonal trust is involved.

The experiment took place between April and June of 2012 in the Cologne Laboratory for Economic Research (CLER), participants were invited using ORSEE (Greiner 2015) and it was programmed with zTree (Fischbacher 2007). In each treatment, all participants received identical instructions and all of them had to answer some quiz questions to make sure that they understood the rules of the experiment. Only after that, the player roles, X and Y, were randomly determined. Pairs of X and Y were then randomly matched. In total, we collected 48 independent decisions made by X per treatment (and 48 observations of Y in T1 and T2). Each session lasted about 40 minutes and participants were asked to fill a questionnaire on demographics after they got to know their payoffs. Most participants studied economics, business or related fields. 121 were female and 119 were male. The average payoff was 9.26 € including a show-up fee of 2.50 €, with a standard deviation of 3.75 €.

3. Results

In all treatments, each X player makes 17 decisions, conditional on p*, for either the safe or the risky option. We analyze the "minimal acceptable probability" (MAP) for the opponent, or the lottery, to select the risky strategy, that is needed to induce risk taking. The lower a player's MAP, the more she is willing to take risks.[7]

In T1, the average MAP is 64 %, in T2 it is 75 %, and in T3 it is 80 %. Using a non-parametric Kruskal-Wallis-tests, we find that the distributions of MAPs differ significantly across treatments (Kruskal-Wallis-test, $P < 0.01$, two-tailed). Looking more closely at the data, we find that the distributions differ between T1 and T2 (Mann-Whitney test, $P < 0.01$, two-tailed), while they do not differ between T2 and T3 (Mann-Whitney-test, $P = 0.30$, two-tailed).[8] The higher willingness to take risks and go for the risky option is hence not driven by a player X's desire to reach the efficient outcome in a social context per se, but is rather consistent with H2: people process information differently — more trustfully — under a trust mindset and are thus more risk seeking when the uncertainty is caused by the social interaction context.

Figure 2 shows the share of players X choosing the risky option per possible probability p* that their opponent (either human or computer) also chooses to do so. It illustrates the large differences between T1 and T2/3 across the whole distribution of probabilities. Moreover, these differences are particularly strong in the range of the distribution which reflects 'reasonable beliefs' about the probability that the opponent in this setting chooses the risky option. Across our three sessions of T1, 32 out of 48 players Y choose the risky strategy, the lowest share being 9/16 and the highest 12/16. The lowest and the highest share are shown as

[7] A consistent participant has exactly one switching point from the safe to the risky option when her MAP is reached. More than 90 % of the subjects in our sample have such a unique switching point, while those who deviate exhibit no particular pattern. In T1, five players always choose the risky strategy, even when the probability that Y1 is chosen is 0, while two players never choose it even when it is 1. In T2, one player always chooses the risky strategy and one player switches more than once between the safe and the risky option. In T3, two participants switch more than once between the options while two switch once but the wrong way (from the risky to the safe). The analysis we present here is confined to these unique switchers that first go for the safe and then for the risky option. Proceeding this way simplifies the exposition, although we emphasize that our results and conclusions do not change if we include all individual data by replacing the MAP by the sum of risky decisions.

[8] We seem to observe somewhat less risk taking in our sample compared to what has been observed in previous studies on the trust game. One potential reason is the very low payoff that results from a coordination failure. Table A.1 in the Appendix uses OLS regressions to corroborate the main result controlling also for personal characteristics of subjects.

vertical lines in Figure 2 with the range between these two values representing reasonable beliefs in our setting.

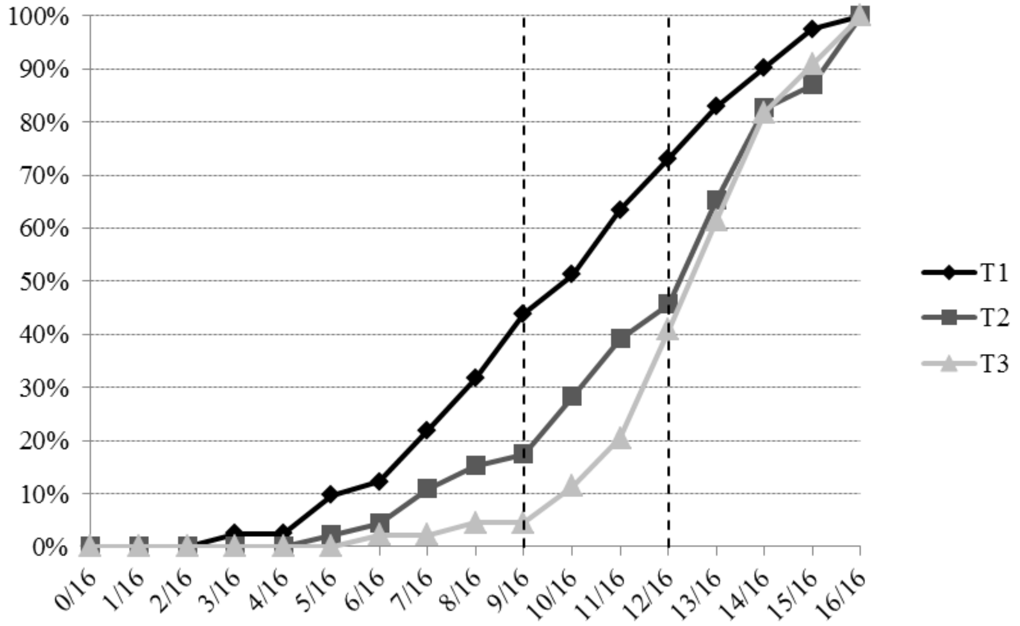

FIGURE 2. CHOICE DATA BY TREATMENT.

Notes: Cumulated share of players X choosing the risky option X1 per probability that Y1/L1 is chosen across treatments. The vertical lines illustrate the lowest and the highest probability found across our coordination-treatment sessions that a randomly chosen player Y goes for the risky option.

In this part of the distribution, the shares of players X choosing the risky strategy indeed differ very substantially between treatments. While in T1 44 % of players X choose the risky option when the actual probability is 9/16, in the natural risk treatments this share is considerably lower (17 % in T2 and 5 % in T3). When the probability is 12/16, which is the highest found across our sessions, 73 % of players X choose the risky strategy while the respective values are with 46 % in T2 and 41 % in T3 again considerably lower. For both actual probabilities, these shares differ significantly between T1 and T2 and between T1 and T3 ($P < 0.01$, all pairwise comparisons, two-tailed).[9.] Furthermore, they differ weakly between T2 and T3 in the former case ($P < 0.10$, two-tailed) but not in the latter ($P = 0.65$, two-tailed). In fact, the shares of X players choosing the risky option are significantly higher in T1 than in T2 and T3 ($P < 0.10$, all pairwise comparisons, two-tailed) for all actual probabilities from 8/16 to 13/16.

[9] Here and in the following we use the chi-squared test if there are at least 5 observations per cell while we use Fisher's exact test in all other cases.

We conclude that the differences in risk taking, when the risky outcome is determined either by another human or by nature, are large and in particular so in an area of reasonable beliefs of X players about the probability p* that the opponent in the coordination game goes for the risky strategy.

4. Conclusion

We find that the stag hunt coordination game where the source of uncertainty is another player, promotes risk taking and social efficiency compared to an otherwise identical decision where risk is determined by nature. This is the opposite pattern in terms of risk taking and efficiency compared to the same treatment variation previously found in the trust game. Our results suggest that, in line with recent social cognition research, one underlying source of the betrayal aversion effect is at least partly due to different information processing modes in games with conflict of interests (such as the trust game) versus alignment of interests (such as the stag hunt game). There are plausible reasons why people behave this way. To give one: Humans might have developed behavioral and cognitive heuristics (Mussweiler and Ockenfels 2013) that allow to better overcome coordination problems in games with aligned interests while at the same time protect against exploitation in games with conflicting interests.

From a general economics perspective, our finding suggests a conceptualization of *"games as frames"*: Not only does the way of presenting strategies and outcomes of a given game affect the construction of beliefs and preferences (Tversky and Kahneman 1981; Dufwenberg et al. 2011; Ellingsen et al. 2012), but the game itself can provoke different construction processes. We believe that the underlying hypothesis, namely that the nature of social interaction inherent to a game affects beliefs and preference formation, is a promising research topic, and that betrayal aversion as well as our finding provide a good starting point for further efforts along those lines.

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Appendix

A. Additional analysis

TABLE A.1. OLS REGRESSIONS ON MAP.

	(1)	(2)
T2 - lottery in a social context	1.85***	1.88***
	(0.66)	(0.66)
T3 - lottery absent social context	2.62***	2.60***
	(0.59)	(0.61)
Female	--	0.22
		(0.09)
T2 = T3	P = 0.23	P = 0.20
Observations	131	131
R-squared	0.14	0.14

Notes: The dependent variable is the minimal acceptable probability (MAP). T1 – strategic uncertainty serves as the reference category. Robust standard errors are given in parentheses. *** denotes 1% significance-level.

B. Instructions

T1 – strategic uncertainty

Welcome and thanks for participating in this experiment. You are participating in a study in which you will earn some money. At the end of the experiment, your earnings will be added to a show-up fee of 2.50 €, and you will be paid in cash.

Experiment. The study is conducted anonymously. Participants will neither get to know with whom they interact in the course of the experiment nor the identities of the other participants. Communication between participants is not permitted throughout the experiment. If you have any question after reading these instructions, please raise your hand. The experimenter will then come to you in order to answer your question.

In this experiment there are two types of participants, person X (16 participants) and person Y (16 participants). You will learn in the experiment which type you are. In this experiment one person X and one person Y are matched randomly. Person X has to decide between option X1 and option X2. Person Y chooses either option Y1 or option Y2.

The payoffs depend on the decisions of both as follows:

		Person Y chooses			
		Option Y1		Option Y2	
Person X chooses	Option X1	X gets 11 €	Y gets 11 €	X gets 1 €	Y gets 7 €
	Option X2	X gets 7 €	Y gets 1 €	X gets 7 €	Y gets 7 €

The payoff table reads as follows:

- If X chooses option X1 and Y option Y1, X gets 11 € and Y 11 €.
- If X chooses option X1 and Y option Y2, X gets 1 € and Y 7 €.
- If X chooses option X2 and Y option Y1, X gets 7 € and Y 1 €.
- If X chooses option X2 and Y option Y2, X gets 7 € and Y 7 €.

Decision pattern.

The experiment consists of four stages.

Stage 1: Person Y either chooses Option Y1 or Option Y2.

Stage 2: Person X can make her decision depending on the probability **p** that the person Y with whom she is randomly matched chooses option Y1. Therefore, the share of the 16 persons Y that chose option Y1 in stage 1 is determined. This share equals the probability **p**. The probability **p** can take 17 values, one for each possible share 0/16, 1/16, 2/16, …, 16/16.

As the actual value for **p** is not known in stage 2, person X decides for each possible outcome by answering questions of the following form.

“If the probability that the person Y I am matched with chooses option Y1 is p, I choose … Option X1 or Option X2.”

She does so for each possible probability 0/16, 1/16, 2/16, …, 16/16 (Thus, person X has to make 17 decisions).

Stage 3: The actual value of **p** is announced. It is determined by the share of persons Y choosing option Y1 in stage 1. It hence equals the probability that a randomly chosen person Y chooses option Y1.

Stage 4: The payoffs of person X and person Y are determined depending on the decision of person X for the actual probability **p** and the decision of the person Y with whom she is matched.

Before the experiment begins you are asked to answer some questions of understanding. When the experiment is over, you can see your payoffs, the decision of your opponent as well as her payoffs on the screen. While your payoffs are prepared, we ask you to fill out a short questionnaire.

T2 – lottery in a social context

Welcome and thanks for participating in this experiment. You are participating in a study in which you will earn some money. At the end of the experiment, your earnings will be added to a show-up fee of 2.50 €, and you will be paid in cash.

Experiment. The study is conducted anonymously. Participants will neither get to know with whom they interact in the course of the experiment nor the identities of the other participants. Communication between participants is not permitted throughout the experiment. If you have any question after reading these instructions, please raise your hand. The experimenter will then come to you in order to answer your question.

In this experiment there are two types of participants, person X (16 participants) and person Y (16 participants). You will learn in the experiment which type you are. In this experiment one person X and one person Y are matched randomly. Person X has to decide between option X1 and option X2. Person Y does not have to make a decision in this experiment.

The payoffs of person X and person Y depend on the decision of person X and the result of a lottery that chooses option L1 with some probability and option L2 with the reverse probability.

The payoffs depend on the decision of player X as well as on the result of a lottery that chooses option L1 with some probability **p** and option L2 with the inverse probability 1-**p**.

		Lottery chooses			
		Option L1		Option L2	
Person X chooses	Option X1	X gets 11 €	Y gets 11 €	X gets 1 €	Y gets 7 €
	Option X2	X gets 1 €	Y gets 7 €	X gets 7 €	Y gets 7 €

The payoff table reads as follows:

- If X chooses Option X1 and the lottery chooses Option L1, X gets 11 € and Y 11 €.
- If X chooses Option X1 and the lottery chooses Option L2, X gets 1 € and Y 7 €.
- If X chooses Option X2 and the lottery chooses Option L1, X gets 7 € and Y 1 €.
- If X chooses Option X2 and the lottery chooses Option L2, X gets 7 € and Y 7 €.

Decision pattern.

The experiment consists of four stages.

Stage 1: The probability that the lottery chooses option L1 is determined. This probability **p** can take 17 different values, which are 0/16, 1/16, 2/16, …, 16/16. We put 17 envelopes into an urn, one for each possible probability. Then one envelope is randomly drawn from the urn. It determines the actual probability **p** that the lottery chooses option L1. It is announced at the beginning of stage 3.

Stage 2: Person X can make her decision depending on the probability **p** that the lottery chooses option L1. It was determined in stage 1. The probability **p** can take the values 0/16, 1/16, 2/16, …, 16/16.

As the actual value for **p** is not known in stage 2, person X decides for each possible outcome by answering questions of the following form.

"If the probability that the lottery chooses option L1 is p, I choose … Option X1 or Option X2."

She does so for each possible probability 0/16, 1/16, 2/16, …, 16/16 (Thus, person X has to make 17 decisions).

Stage 3: The actual value of **p** is announced. It was determined in stage 1. Subsequently white and orange balls are put into an urn whereby the share of white balls is equal to **p**. Then one ball is randomly drawn. If it is white the lottery chooses option L1. Otherwise it chooses option L2.

Stage 4: The payoffs of person X and person Y are determined depending on the decision of person x for the actual probability **p** and the result of the lottery.

Before the experiment begins you are asked to answer some questions of understanding. When the experiment is over, you can see your payoffs, the decision of your opponent as well as her payoffs on the screen. While your payoffs are prepared, we ask you to fill out a short questionnaire.

T3 – lottery absent social context

The instructions for T3 differ from T2 only in the absence of a person y and that person X is called 'you'.

Questionnaire

T1 – strategic uncertainty

- How is determined which of the 17 decisions by person X is the one relevant for the payoffs?

  ____

- The share of decisions for option Y1 may be 6/16. For this probability that the person she is matched with chooses option Y1, person X may choose option X2. The person Y she is matched with may choose option Y1.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.
- The share of decisions for option Y1 may be 14/16. For this probability that the person she is matched with chooses option Y1, person X may choose option X1. The person Y she is matched with may choose option Y1.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.
- The share of decisions for option Y1 may be 10/16. For this probability that the person she is matched with chooses option Y1, person X may choose option X2. The person Y she is matched with may choose option Y2.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.
- The share of decisions for option Y1 may be 3/16. For this probability that the person she is matched with chooses option Y1, person X may choose option X1. The person Y she is matched with may choose option Y2.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.

T2 – lottery in a social context

- How is determined which of the 17 decisions by person X is the one relevant for the payoffs?

  ____

- How is the result of the lottery (Option L1 of Option L2) determined?

  ____

- What is the probability that the lottery chooses option L1? How is this probability determined?

  ____

- The probability that the lottery chooses option L1 may be 6/16. For this probability person X may choose option X2. The lottery may choose option L1.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.

- The probability that the lottery chooses option L1 may be 14/16. For this probability person X may choose option X1. The lottery may choose option L1.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.
- The probability that the lottery chooses option L1 may be 10/16. For this probability person X may choose option X2. The lottery may choose option L2.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.
- The probability that the lottery chooses option L1 may be 3/16. For this probability person X may choose option X2. The lottery may choose option L2.
  - What is person X's payoff? ____ Euro.
  - What is person Y's payoff? ____ Euro.

T3 – lottery absent social context

The questions for T3 differ from T2 only in the absence of a person y.

C. Experimental procedure

In each treatment players X first choose for all probabilities from $0/16^{th}$ to $16/16^{th}$ whether to choose the risky thing or the sure option. Subsequently, they get to see one screen with all decisions where they can change their decisions if wanted. If they switch from the sure to the risky option and back they are asked to note that they do so when they click to continue and can then either choose to proceed or to change their choices. Only then, all decisions are locked in.

Chapter 5

Norm uncertainty and voluntary payments in the field*

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Abstract

We investigate the behavioral reactions towards exogenous changes of implicit norm-relevant information in a natural field setting. Customers who are exposed to subtle information cues implying a higher norm increase their voluntary payments for a service by about 25%. Moreover, consistent with the conjecture that this effect is predominantly driven by customers who are uncertain about the actual payment norm, this effect vanishes when explicit norm-relevant information is provided. Additional analyses suggest that customers tend to pay more when the probability of being observed by others increases.

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1. Introduction

The present paper investigates how economic decisions respond to an exogenous shift in a perceived norm. Our central hypothesis is that subjects who are willing to adhere to norms respond to subtle variations in the environment when the relevant norm is unclear. In a setting where subjects can freely decide if and how much to pay for a service, we find that payments increase substantially when customers are exposed to information that implies a higher norm. This effect vanishes when explicit norm-relevant information is provided. Thereby, our study provides field evidence that subjects exhibit a preference for norm conformity and, if the norm is uncertain, actively search for norm-relevant information.

Previous studies have established the importance of norms for economic behavior. For example, Sliwka (2007) and Fischer and Huddart (2008) study theoretically how a preference to adhere to norms affects the interaction between principals and agents in companies. Examples from the laboratory that establish the influence of norms on behavior include dictator games (Andreoni and Bernheim 2009; Krupka and Weber 2009 and 2013; Chang et al. 2015; Gächter et al. 2015; Kimbrough and Vostroknutov forthcoming), public goods games (Xiao and Houser 2011; Kessler and Leider 2012; Reuben and Riedl 2013; Kimbrough and Vostroknutov forthcoming), Bertrand competition (Kessler and Leider 2012) and principal-agent relationships (Danilov and Sliwka forthcoming).

As norms are often ambiguous and vary from case to case, behavior may depend heavily on the specific context, and subjects who are willing to adhere to norms may face substantial uncertainty about what the relevant norm is. Importantly, when confronted with uncertainty about the norm, the construction of beliefs about appropriate behavior might be fragile and easily accessible by subtle psychological techniques that exogenously alter information available to the subjects, such as anchoring or priming (Ariely et al. 2003; Mussweiler and Ockenfels 2013; Cohn et al. 2015; Feldhaus 2016). This is suggested for example by

Mussweiler and Strack (2000) who find that anchoring effects are the stronger the more uncertain the target in a guessing task is.

Our study builds upon these insights and investigates the effect of norm uncertainty and behavioral reactions towards exogenous changes of norm-relevant information in a natural field setting. Our approach to exogenously vary information follows research in social psychology suggesting that responses to survey questions are sensitive to the particular values shown in answer scales (Schwarz et al. 1985; Schwarz 1999). Importantly, survey respondents seem to use the value categories of answer scales as reference values that represent 'common' or 'normal' opinions or behaviors.[1] In our experiment, we use the information provided by such answer scales as our treatment manipulation. We ask customers about their opinions regarding appropriate payments by presenting them two different answer scales – a scale with a lower midpoint and a scale with a higher midpoint, implying a lower and a higher payment norm, respectively. Our hypothesis is that voluntary payments increase in response to a scale with a higher midpoint due to the customers' preference to adhere to the norm.

As our study focuses on payments that are made voluntarily, it is related to research on the impact of norm-relevant information on charitable giving. For instance, donations are strongly influenced by information about the behavior of others (Frey and Meier 2004; Alpizar et al. 2008; Shang and Croson 2009; Agerström et al. 2016) as well as by direct suggestions (Adena et al. 2014; Charness and Cheung 2013; Edwards and List 2014). Martin and Randal (2008) observe that behavior can also be affected by more indirect cues, as donations in a gallery respond to the content of a transparent donation box.[2] Also, in applied psychology and marketing science, several studies have investigated how reference scales in the context of door-to-door solicitation or personalized mailings have to be designed in order to increase

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[1] Schwarz et al. (1985) ask participants how much time they invest into watching TV per day and find that respondents' estimations differ significantly conditional on the scale used in the survey. Schwarz (1999) provides a survey regarding such scale effects.

[2] Our study is also somewhat related to theoretical and empirical research on tipping that shows the importance of norm-relevant information (Azar 2004, 2007).

charitable donations (Weyant and Smith 1987; Doob and McLaughlin 1989; Schibrowsky and Peltier 1995; Desment and Feinberg 2003; De Bruyn and Prokopec 2011).[3]

Since subjects in our natural field environment are free what to pay, the decision situation also shares important features with dictator games and related decision situations. In the context of dictator games, Ockenfels and Werner (2014) test the effect of varying response scales for belief elicitation questions on dictator transfers in a classroom experiment. Before making their decisions, dictators are asked to provide a guess either about the average transfer of dictators or the average expectation of recipients. Here, dictators who are unfamiliar with the game transfer more to recipients after observing a scale with a high midpoint when guessing the recipients' expectations, in line with guilt aversion (Battigalli and Dufwenberg 2007). The observation that only participants are affected who do not know the dictator game suggests that the presence of uncertainty might indeed be important for the effect of scale variations on behavior.[4]

Finally, a number of economic studies have investigated behavior in 'pay-what-you-want' schemes. These schemes offer customers the choice if and how much to pay for a particular good or service. Similar to dictator games, profit maximizing behavior implies paying nothing or the minimum possible amount. Yet, studies both from the laboratory and the field have shown that customers are willing to pay non-negligible prices under pay-what-you-want schemes, and that this behavior can be attributed, among other things, to social preferences. Regner and Barria (2009) analyze data of an internet platform that sells music under a pay-what-you-want scheme and find that the average payment is substantially higher than the minimal amount, which is explained by reciprocal preferences of customers. Gneezy et al. (2010) and Gneezy et al. (2012) investigate pay-what-you-want schemes in field experiments.

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[3] In these studies, potential donors are directly approached, and the scale's values are presented as choice sets where subjects can mark the amount they are willing to give while also being allowed to donate some other amount.

[4] Likewise, Schwarz and Bienias (1990) found in survey studies that the effect of scale variations becomes weaker when more relevant information is accessible to respondents.

The former study shows that combining a pay-what-you-want scheme with a charitable donation substantially increases revenues as compared to a fixed price with a charity component. Results from the latter study suggest that concerns for self-image are important drivers for customer behavior: Here, more customers decide *not* to buy a product if they can choose the price compared to a case where the same goods are sold at a low fixed price, suggesting some of them want to avoid paying 'inappropriately' low prices. Schröder et al. (2015) find that a pay-what-you-want scheme results in higher revenues compared to a mechanism that asks customers to freely reduce a price, indicating the importance of framing effects and the volatility of payments in such contexts. Schmidt et al. (2015) investigate underlying reasons for positive payments under pay-what-you-want schemes in a laboratory market environment and conclude that these payments can be partly attributed to outcome-based social preferences (Fehr and Schmidt 1999; Bolton and Ockenfels 2000).[5] Finally, using restaurant data over a two-year period, Riener and Traxler (2012) observe persistent positive payments under a pay-what-you-want pricing scheme and, importantly, the patterns in the data are in line with customers' focus on norms, as payments converge over time.[6]

In contrast to most previous economic studies about the impact of norms on behavior, the focus of our experiment is on the effect of a subtle and implicit norm variation in a natural field setting. Importantly, our experimental design has the advantage that it allows us to exogenously shift the perceived norm regarding appropriate payments and test whether subjects adjust their behavior in response to this variation. Unlike former studies that directly communicate reference payments (such as average prices, recommendations or explicit requests), subjects in our main experiment neither get actual information about others'

[5] However, strategic concerns by customers are also found to be important for behavior - some customers seem to pay predominantly in the beginning to keep the firm with the pay-what-you-want scheme in the market, and that the choice of a pay-what-you-want scheme is not a successful strategy when competition among firms is introduced.

[6] In addition, studies from marketing science also indicate that behavior under pay-what-you-want schemes is sensitive to reference values: Kim et al. (2009), Wiggins Johnson and Peng Cui (2013), Armstrong Soule and Madrigal (2015) and Kim et al. (2014) provide survey and field evidence that external reference prices can have a positive impact on actual prices.

behavior nor an explicit suggestion. Instead, they infer the norm-relevant information from the environment, in our case from the announcement of a survey about customers' opinions with regard to appropriate payments. Hence, subjects process this information only indirectly because the variation of the scales in our setting is not directly tied to payment decisions. Furthermore, to test the impact of norm uncertainty in our setting, we implement our experimental variation both in absence and in presence of explicit norm information.

We find that consumers are highly sensitive to an exogenous shift in the perceived norm: Customers who are confronted with a high reference scale pay on average about 25% more for the service than customers who are shown a low reference scale. This is evidence that, under uncertainty, a minor and subtle intervention can cause a shift in customers' beliefs about appropriate behavior and a corresponding substantial increase in payments, suggesting that norm-guided behavior in the field is very sensitive to changes in the context of the decision environment. Moreover, consistent with our conjecture that this effect is predominantly driven by customers who are uncertain about what to pay, the reference scale effect vanishes in a control experiment where customers additionally receive information about average payments.

The remainder of the paper is organized as follows. The next section presents the setting and discusses the experimental design. In Section 3 we discuss the results of the main and the control experiment. Section 4 summarizes and concludes.

2. Setting and experimental design

We conducted our experiment on a Saturday in April 2014 in the "Neumarkt Galerie", a shopping mall in Cologne, Germany. The mall has a public restroom where guests can freely choose how much they want to pay for usage. Payment for the service is expected but not enforced so that customers can also refuse to pay. Moreover, there is no clear reference price for the service which allows us to study behavior under uncertainty about the payment norm.

All guests pass a counter where they can place their payment on a plate. During the time of our experiment, an experimenter was seated right behind the counter who collected the payments by the customers and the data and changed the signs associated with our experimental treatments at specific times (see below).[7] The experimenter put all customer payments immediately into a drawer in the counter to ensure that the plate was filled with the same amount of money as change – four coins of 20 cents and two coins of 10 cents.[8] Also, he collected data about the amount given and basic demographic characteristics (gender and estimated age) of each guest as well as situational factors that might have an influence on payments (for example, whether the customer arrived in a group or alone).[9] The experimenter noted the relevant information on sheets of paper hidden in a second drawer.

The experimental variation of norm-relevant information was implemented with the help of printed signs that were placed in the restrooms and on the counter close to the plate for payments. The signs announced a survey that asked the customers to state their opinion regarding an 'appropriate payment' for the usage of the restroom, suggesting altogether five categories for possible answers (see Table 1 for the text printed on the signs).[10] If customers wanted to participate in the survey, they were asked to contact the experimenter at the counter who would provide them with a questionnaire form. However, customers' interest in the survey was only low - 9 customers actually completed the questionnaire form which accounts for a participation rate of 2.2%.[11] However, as we will describe in Section 3 below, the information provided on the signs had a significant impact on the behavior of customers.

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[7] This experimenter was the same across all treatments and experiments. As the facilities are cleaned during the day, members of the cleaning team were also present during the time of our experiment. However, the aim of the study was not revealed to them.

[8] It was not always possible to put the money into the drawer right after the payment, for example, in cases when a large number of customers left the restroom at the same time. Therefore, in the later analysis we also control for the sum of money left on the plate (see Section 3).

[9] For a description of the dataset see Table A.1 in the Appendix.

[10] During the time of the experiment, a barrier was installed in the restroom that can, if enabled, only be passed after inserting coins as payment. However, it was not yet in use when we conducted the experiment. Thus, it seems plausible that customers took the survey seriously and interpreted it as a trial of the shopping center management to elicit their willingness to pay before putting the barrier into operation.

[11] 4 persons participated in the low and 5 persons in the high scale treatment.

TABLE 1. WORDING AND SCALES SHOWN IN THE MAIN EXPERIMENT.

Treatment 1: *LOW*	Treatment 2: *HIGH*
Survey: Which amount do you think is appropriate as the payment for the usage of our service?	
☐ 0 cent	☐ up to 30 cents
☐ 10 cents	☐ 40 cents
☐ 20 cents	☐ 50 cents
☐ 30 cents	☐ 60 cents
☐ more than 30 cents	☐ more than 60 cents
Please contact us!	

We implemented two treatments – the low scale treatment (*LOW*) and the high scale treatment (*HIGH*) – which were identical except for the numeric scale shown on the signs that provided a range of potential answers to the question regarding appropriate payments. In a related experiment conducted in the same week we found that in the absence of an intervention customers paid on average 30 cents for the service. Therefore, we used this value as the switching point between the two scales. In treatment *LOW*, the scale started with the category '0 cents' and increased in 10-cent increments to 'more than 30 cents' while in treatment *HIGH* the scale started with the category 'up to 30 cents', going up in 10-cent increments to 'more than 60 cents'. By this variation, we expect to move the average perception of appropriate payments into the direction of the midpoint of the respective scale (Schwarz et al. 1985), thereby affecting the corresponding actual payments of people who are willing to conform to norms. To ensure that there would be no confound due to the fact that different groups of customers might visit the shopping center at different times, we further switched between treatments at specific times during the day: we conducted the low scale treatment from 11:30am-1:00pm and from 3:30pm-5:00pm while we conducted the high scale treatment from 1:15pm-2:45pm and from 5:15pm-6:45pm

3. Results

Descriptive results. We collected data on the payment decisions of altogether 403 customers – 199 participants in the low scale treatment and 204 people in the high scale treatment. Customers in *LOW* pay on average 30.33 cents and are therefore very close to the customer payments without interventions (see previous section). In contrast, the mean payment in *HIGH* is with 37.80 cents 24.6% higher than in the low scale treatment. The respective distributions of payments are different as indicated by a Mann-Whitney U test (MWU, $p < 0.01$, two-tailed). This shows that customers react to the implicit norm variation as predicted by our main hypothesis. Since payments substantially increase in the high scale treatment, many customers seem to adjust their own payment to the increase in the 'appropriate payment' suggested by the higher midpoint (see Figure 1 that visualizes the distribution of payments across the two treatments).

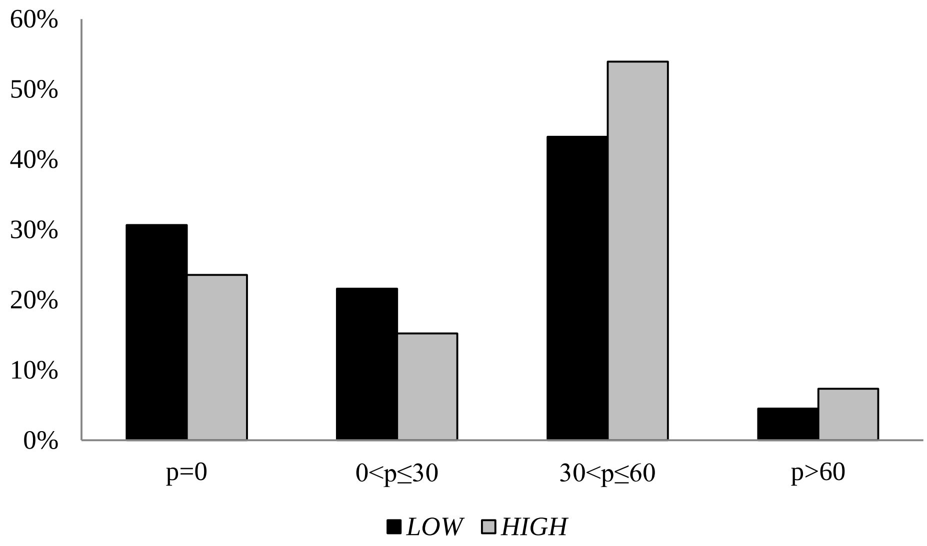

FIGURE 1. DISTRIBUTION OF PAYMENT DECISIONS (P) IN THE LOW AND HIGH TREATMENT (PAYMENT-INTERVALS IN EURO-CENTS).

Moreover, we find that the share of decision makers that gives more than the switching point of 30 cents is with 61.3% in the high scale treatment substantially higher compared to the low scale treatment (47.7%). These shares differ significantly between treatments as indicated by a two-sample test of proportions ($p < 0.01$, two-tailed). Also, we find a similar effect concerning the prominent center of the scales. While the share of payments of exactly 20 cents is higher in the low scale treatment (6.5% of the cases in *LOW* vs 2.5% of the cases in *HIGH*, two sample test of proportions, $p < 0.05$, two-tailed), payments of 50 cents are more frequently chosen in the high scale treatment (32.7% in *LOW* vs 42.2% in *HIGH*, two sample test of proportions, $p < 0.05$, two-tailed). Summarizing, we find significant differences in payments conditional on the scale shown to the customers.

Control experiment. Our conjecture is that the variation of scales affects people's beliefs about appropriate behavior in particular under norm uncertainty. To test this conjecture, we conducted a control experiment in the same setting in June 2014 and provided customers with explicit norm-relevant information, using exactly the same procedures, time slots and the schedule for treatment variations as in our first experiment.

In the second experiment, we added the average payment by customers without intervention to the description of the reference scales that were otherwise identical to the main experiment (see Table 2). If the effect of the scale variation is driven by norm uncertainty, it should become weaker once norm-relevant information is revealed.[12]

TABLE 2. WORDING AND SCALES SHOWN IN THE CONTROL EXPERIMENT.

Treatment 1: *LOW30*	Treatment 2: *HIGH30*
Survey: On average our guests pay **30** cents. Which amount do you think is appropriate as the payment for the usage of our service?	
☐ 0 cent	☐ up to 30 cents
☐ 10 cents	☐ 40 cents
☐ 20 cents	☐ 50 cents
☐ 30 cents	☐ 60 cents
☐ more than 30 cents	☐ more than 60 cents
Please contact us!	

[12] We acknowledge that customers might perceive a difference between the descriptive norm (the behavior by others) and the appropriate payment. If this was the case, responses to the scale variation should be less affected by the provision of additional norm information in the control experiment.

In the second experiment, we collected data for altogether 632 customers - 311 (321) for the *LOW30* (*HIGH30*) treatment.[13] Importantly, and in contrast to our main experiment, average customer behavior is very similar across treatments: In the *LOW30* treatment customers pay 30.58 cents on average while the corresponding value is 30.09 cents in the *HIGH30* treatment. The corresponding distributions of payments do not differ (MWU, $p = 0.89$, two-tailed). Testing for a difference between the treatments in customers' propensity to pay more than the cutoff value of 30 cents neither yields differences between the treatments (42.1% in the *LOW30* treatment vs 45.8% in the *HIGH30* treatment, two-sample test of proportions, $p = 0.35$, two-tailed).[14] This result is in line with our conjecture that the scale variation effect relies on the absence of explicit norm-relevant information.

One would expect a shift towards the actual mean payment in our control condition. Indeed, the share of customers who give exactly 30 cents is substantially higher, with 13.0% in the control experiment compared to 7.7% in the main experiment (two-sample tests of proportions, $p < 0.01$, two-tailed).

Summarizing, we find that voluntary payments are significantly affected by the variation in reference scales only when norm uncertainty is present.

In the next step, in order to control for additional factors that might influence customer behavior in our setting, we analyze the determinants of payments with the help of Tobit models. Here, we make use of both the data from the main experiment and the control

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[13] Due to constructional changes in the shopping mall and because of time constraints we conducted this experiment within the week (Wednesday) rather than on another Saturday. During our initial experiment, some of the shops in the center were not yet opened. However, at the time of the second experiment these shops were opened which led to a substantial increase of the customer volume in the center. For the second experiment, we were able to collect payment information from altogether 632 out of 641 customers. For 9 customers (1.4%) it was not possible to track payments. Our results do not change if we drop these customers from the analyses or when assuming that they all gave nothing. In the remainder of the paper, we drop these data points from the analysis.

[14] With respect to the midpoints of the scales, the probabilities to pay exactly 20 cents (7.7% in the *LOW30* treatment vs 6.5% in the *HIGH30* treatment, two-sample test of proportions, $p = 0.57$, two-tailed) or to give exactly 50 cents do neither differ (28.3% in the *LOW30* treatment vs 32.1% in the *HIGH30* treatment, two-sample test of proportions, $p = 0.30$, two-tailed).

experiment. We also test for further evidence corroborating our assumption that behavior in our setting is driven by a desire of customers to adhere to social norms.

We also collected data on the demographics of customers. These variables include gender and estimated age[15] of each customer, a dummy variable that takes the value of one if customers arrived in a group[16], and a dummy variable equal to one if people belonged to the staff of the shopping mall.[17] Moreover, we collected variables to control for situational factors that might have an influence on payments if behavior is guided by norms. First, we control for an effect of the observability of payments with the variables 'Observable by member of the team' and 'Observable by customer'. These variables are dummies that indicate whether a member of the cleaning team or another customer was close to the counter when a customer had to decide if and what to pay for the service. Second, to capture an effect of potentially competing reference values for payments, we control for the amount of money on the plate when a particular customer made her decision (see Footnote 8).

Model 1 in Table 3 includes only the dummy variables for the treatment variations (*LOW* and *HIGH* of the main experiment and *LOW30* and *HIGH30* of the control experiment; the *LOW* treatment of our main experiment serves as the reference category). Model 2 additionally controls for demographic characteristics of the customers and dummies for time intervals (the phases before 2pm, between 2pm and 4pm, and after 4pm) to control for daytime effects[18]. Model 3 further includes contextual effects described above.[19]

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[15] A person's age was estimated in intervals of 5 years starting with an age of 10 years.

[16] Groups are defined as people who jointly arrived in the restroom and obviously belonged together (for example, couples, families or friends). In cases when customers contributed as a group, every person was attributed the total payment of the group divided by sum of people who belonged to the group. This was done because if customers arrived in groups, they typically left only one payment at the counter. As customers, we consider only adults and adolescents older than 10 years.

[17] We assign people who work for one of the shops in the shopping center and construction workers from a building lot in the mall to this category. Due to their work clothes they could be easily identified.

[18] These time intervals overlap with the time slots for the experimental treatments.

[19] In Table A.2 in the Appendix we report additional Probit regressions with a dummy variable equal to one if the payment by a customer exceeds the threshold value of the scales (30 cents). These models support our main conclusion and also show the significant treatment effect. In these models, the signs of the additional control variables are similar to the specifications reported in Table 3, but the significance levels vary in some cases. For example, the dummy for male customers is always significantly negative.

TABLE 3. DETERMINANTS OF CUSTOMER PAYMENTS (EURO CENTS).

Dependent var.: **Payment**	(1)	(2)	(3)
Treatment *HIGH*	9.238***	10.906***	10.980***
	(3.289)	(3.172)	(3.170)
Treatment *LOW30*	2.384	3.890	2.948
	(3.000)	(2.829)	(2.839)
Treatment *HIGH30*	0.508	3.501	2.001
	(2.992)	(2.888)	(2.906)
Male	--	-3.467	-3.909*
		(2.221)	(2.217)
Age	--	0.484***	0.497***
		(0.069)	(0.069)
Staff	--	-71.092***	-69.772
		(7.831)	(7.768)
Group	--	--	-3.578*
			2.038
Observable by team member	--	--	3.390*
			(2.035)
Observable by customer	--	--	6.763**
			(2.639)
Money on plate (Euro cents)	--	--	0.092*
			(0.049)
Time dummies	No	Yes	Yes
Observations	1035	1031	1030
Log likelihood	-4068	-3958	-3946

Notes: Tobit models are calculated to account for the share of observations with zero transfers. The dependent variable is the payment of a customer in Euro cents. Standard errors are given in parantheses. ***, ** and * denote significance at the 1%-, 5%- and 10%-level, respectively.

Across all models we find a positive and highly significant effect of the *HIGH* dummy on decision makers' payments. This result confirms that subjects in the high scale treatment of the main experiment are indeed more generous than in the low scale treatment when uncertainty about the norm is present. As before, we find no difference between the treatments of the control experiment (Wald-tests, Models 1 to 3, $p > 0.10$), suggesting that customers do not react to the scale variation when they also receive information about average payments. Furthermore, the high scale treatment of the main experiment does not only differ from the respective low scale treatment but the values of the coefficients also exceed those of both treatments of the control experiment (Wald-tests, Models 1 to 3, $p < 0.05$, all pairwise comparisons). In line with the descriptive statistics, the coefficients *LOW30* and *HIGH30* for the control experiment are positive but not significant, indicating no substantial difference compared to *LOW*. With respect to the demographic variables, we find a positive effect of age suggesting that older people tend to pay more and a negative effect of the 'Staff'-variable, implying that people who work in the shopping mall pay less for the service. Moreover, the dummy variable for groups is negative and marginally significant, indicating that the individual payment becomes lower if customers arrive as a group. In Model 3, the dummy for male customers is negative and weakly significant.

Model 3 provides further indications that customers' willingness to adhere to norms is a relevant driver of behavior in our setting. In line with the notion that customers are more likely to follow norms when they are observed by others, we find that guests pay (weakly) significantly more when either a member of the cleaning team or another guest is close by and able to observe the payment. This observation is related to the formal arguments by Bernheim (1994) that subjects conform to norms due to their desire for social esteem and by Andreoni and Bernheim (2009) that the observability of one's action is an important prerequisite for inducing fair behavior. Also, it is related to empirical evidence on the impact of social pressure on giving behavior (see, for example, Andreoni and Bernheim 2009; DellaVigna et al. 2012). Finally, in line with the literature on the effect of seed money in the context of charitable giving (List and Lucking-Reiley 2002), customers tend to give more the more money is on the plate; yet, this effect is only marginally significant.

In a last step, we investigate the conjecture that the effect of the scale variation in our main experiment and hence the customers' tendency to adhere to the higher payment norm is related to the degree of observability. Therefore, we calculate additional Tobit models similar to those reported in Table 3 that control for different possible audience effects.

Model 4 and 5 exclude all cases where decision makers could be observed by a member of the cleaning team (Model 4) or by another customer (Model 5) when choosing their payment, whereas in the final step, Model 6 excludes both cases.

In Models 4 and 5, we find that the treatment effect in *HIGH* is still significant although the coefficients and significance levels seem to get somewhat smaller compared to the model including all observations (see Table 3). In Model 5, the difference between the coefficients of the two high scale treatments is not significant anymore (Wald-test, $p > 0.10$). Finally, in Model 6, the effect of the high scale treatment in our main experiment seems indeed substantially mitigated: Although the coefficient of *HIGH* is still positive, it is neither significant (Wald-test, $p = 0.142$) nor different from *HIGH30* (Wald test, $p = 0.220$) in this specification. This is an indication that the willingness to conform to the perceived payment norm in our field setting might be sensitive to the observability of actions.

TABLE 4. DETERMINANTS OF CUSTOMER PAYMENTS (EURO CENTS) – ROBUSTNESS CHECKS.

Dependent var.: **Payment**	(4)	(5)	(6)
Treatment *HIGH*	9.009**	7.75**	6.694
	(4.166)	(3.467)	(4.554)
Treatment *LOW30*	5.497	2.010	4.101
	(4.062)	(3.090)	(4.487)
Treatment *HIGH30*	0.877	2.637	1.836
	(4.082)	(3.235)	(4.495)
Male	1.167	-3.053	1.765
	(3.094)	(2.485)	(3.418)
Age	0.592***	0.540***	0.675***
	(0.094)	(0.077)	(0.102)
Staff	-69.036	-68.172***	-67.766
	(9.894)	(8.114)	(10.216)
Group	-4.388	-3.980*	-4.605
	(2.742)	(2.275)	(3.014)
Observable by team member	--	3.161	--
		(2.290)	
Observable by customer	5.368	--	--
	(3.780)		
Money on plate (Euro cents)	0.087	0.150	0.164
	(0.064)	(0.065)	(0.082)
Time dummies	Yes	Yes	Yes
Sample	Not observed by team member	Not observed by customer	Not observed by team member or customer
Observations	613	867	528
Log likelihood	-4068	-3958	-3946

Notes: Tobit models are calculated to account for the share of observations with zero transfers. The dependent variable is the payment of a customer in Euro cents. Standard errors are given in parentheses. ***, ** and * denote significance at the 1%-, 5%- and 10%-level, respectively.

4. Conclusion

In the present study we investigate whether subtle changes in norm-relevant information affect payment decisions in a natural field setting where the actual payment norm is unclear.

With the help of different reference scales for 'appropriate payments', we exogenously induce shifts in the perceived payment norm and find non-negligible effects on customers' choices: When observing a scale with high values, customers pay on average some 25% more than customers who see a scale with low values. Remarkably, this effect is found although customers only passively process the information from the scales. Our interpretation of the treatment effect is that people adjust their behavior in the direction of the values provided by the scales when they are uncertain about the relevant payment norm. This interpretation is supported in a control experiment in which between-treatment differences vanish as soon as customers receive explicit norm-relevant information (the mean amount paid by customers). Also, additional analyses suggest that customers tend to give more when being observed by others, and that the estimated effect of the scale variation becomes somewhat weaker when it is less likely that others see what the customer pays for the service.

A potential alternative explanation for the scale effect independent of norms is that the mere exposure to the numerical values of the different scales is enough to shift behavior under uncertainty. This could be caused by the fact that the scales make particular values more salient. To test for this possibility, we conducted a third experiment in the same setting and in exactly the same way as in the previous experiments. Here, the only difference to the main experiment was that the question for customers was unrelated to the service: In particular, the question displayed to customers was "What do you think does a one-minute phone-call from Germany to England cost on average?" (The scales were identical to those displayed in Tables 1 and 2). In this experiment, we find no effects of the scale variations, with average payments in the low scale (high scale) treatment of 27.39 cents (30.54 cents; MWU, $p = 0.98$, two-tailed).[20] Hence, it seems that it is indeed the perception of being confronted with norm-relevant information (due to the request to think about 'appropriate' behavior) that makes

[20] This experiment was also conducted in June 2014; in total, we were able to observe the payment decisions of 681 subjects.

customers more susceptible to use this information which, in turn, leads to shifts in payments in the direction of the 'suggested' appropriate payment.[21]

Our natural field setting does not allow us to isolate the underlying mechanisms through which the experimental information variation influences behavior of the customers. First, the effects of our intervention might be related to the particular characteristic of the norm that is made salient through the announcement of the survey. Bicchieri (2011) distinguishes between 'normative expectations' (what ought to be done) and 'empirical expectations' (what others do) as central elements of norms. The intervention in our experiment (focusing on the 'appropriate payment') potentially includes both aspects of the norm, and so we cannot determine which element of the norm predominantly triggers the response to the scale variation. It might therefore be interesting to investigate whether the impact of scale variations in the field is sensitive to which elements of a norm are highlighted.[22]

Also, we cannot disentangle between different motivations that may drive the scale variation effect. For example, a customer might have an inherent preference not to deviate from a given norm, as outlined by Kimbrough and Vostroknutov (forthcoming). Alternatively, a customer might interpret the information provided by the scale as expectations of the members of the shopping mall team concerning the payment for their services. Guilt aversion, the preference to avoid disutility caused by disappointing expectations of others (Battigalli and Dufwenberg 2007), might then motivate customers to increase their payments in response to the higher reference scale.[23] First evidence in line with the latter notion is provided by the dictator game

[21] Interestingly, we find that customers are less likely to pay for the service in the third experiment compared to the other conditions: while the probability of paying is 73.0% in the main experiment and 76.7% in the control experiment, it is only 63.1% in the experiment using the unrelated question, and the differences to the former experiments are significant (two-sample tests of proportions, $p < 0.01$, two-tailed, both pairwise comparisons). This suggests that increasing the saliency of potential payments for the service has a significant impact on customers' propensity to give.

[22] Armstrong Soule and Madrigal (2015) conclude from an experiment using hypothetical pay-what-you-want scenarios that descriptive norms - information about what other customers pay - influence price choices more than explicit price recommendations.

[23] Battigalli and Dufwenberg (2009) provide the framework for capturing the idea that beliefs directly enter a subject's utility function. Empirical evidence in line with guilt aversion is provided, for example, by the

experiment by Ockenfels and Werner (2014) where the effect of scale variations is only significant when the attention of the dictators is directed to recipients' expectations.

Overall, our study shows that in the presence of uncertainty about what is expected, customers' payment decisions might be rather volatile and easily affected by minor changes in the environment. In our setting, it takes only very subtle exogenous variations in the context to substantially increase payments made by customers. To better understand the effects of the context (in particular with respect to norm-relevant information) on voluntary payments is a promising avenue for further research and also an important prerequisite for the adequate design of pay-as-you-want pricing schemes and campaigns for charitable donations in practice.

experimental studies of Charness and Dufwenberg (2006, 2011), Dufwenberg et al. (2011), Bellemare et al. (2015), Khalmetski et al. (2015) and Khalmetski (2016).

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Appendix

TABLE A.1. DESCRIPTIVE STATISTICS.

	Frequency	Percent
treatment		
LOW	199	19.06
HIGH	204	19.54
LOW30	314	30.08
HIGH30	327	31.32
observable by member of the team		
no	618	59.59
yes	419	40.41
observable by customer		
no	880	84.29
yes	164	15.71
gender		
female	754	72.22
male	290	27.78
group		
no	576	55.17
yes	468	44.83
staff		
no	998	95.59
yes	46	4.41
time		
before 2pm	385	36.88
from 2pm to 4pm	215	20.59
after 4pm	444	42.53
	Mean	Standard Deviation
Variable		
payment (Euro cent)	31.80	25.35
money on plate (Euro cent)	104.29	19.16
estimated age (years)	35.80	14.53

TABLE A.2. CUSTOMERS' WILLINGNESS TO GIVE MORE THAN 30 EURO CENTS – PROBIT MODELS.

Dependent var.: **More than 30**	(1)	(2)	(3)
Treatment *HIGH*	0.343***	0.483***	0.505***
	(0.125)	(0.136)	(0.137)
Treatment *LOW30*	-0.142	-0.096	-0.109
	(0.114)	(0.119)	(0.121)
Treatment *HIGH30*	-0.049	0.084	0.051
	(0.113)	(0.121)	(0.123)
Male	--	-0.232**	-0.251***
		(0.095)	(0.095)
Age	--	0.018***	0.019***
		(0.003)	(0.003)
Staff	--	-2.306***	-2.268***
		(0.439)	(0.434)
Group	--	-0.230***	-0.234***
		(0.086)	(0.086)
Observable by team member	--	--	0.186**
			(0.088)
Observable by customer	--	--	0.139
			(0.114)
Money on plate (Euro cents)	--	--	0.002
			(0.002)
Time dummies	No	Yes	Yes
Observations	1035	1031	1030
Log likelihood	-707	-640	-636

Notes: The dependent variable is a dummy equal to one if the customer paid more than 30 Euro cents. Standard errors are given in parentheses. *** and ** denote significance at the 1%- and 5%-level, respectively.

Erklärung

nach §6 der Promotionsordnung vom 16. Januar 2008

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

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Unterschrift:

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