

NING LIU

Behavioral biases in interpersonal contexts



Behavioral Biases in Interpersonal Contexts

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Fouten in keuzegedrag naar interpersoonlijke situaties

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Chapter 1

Introduction

Behavioral Economics incorporates psychological insights into economics. For more than half a century, it has identified a series of systematic violations of the neoclassical economic theories. Inspired by these anomalies in the neoclassical contexts, behavioral economists construct alternative theories with more realistic psychological foundations. These theories have been shown to generate useful theoretical insights, make better predictions of field phenomena, and suggest better policies (Loewenstein et al., 2004).

One of the first challenges to the neoclassical economic theories focused on the expected utility theory, in the context of decision making under risk. Expected utility makes testable implications based on precise assumptions, and is therefore suitable for behavioural economics to build upon. Studies followed the seminal papers by Allais (1953) and Ellsberg (1961) have documented when and how decisions deviate from expected utility. In 1979, Kahneman and Tversky proposed prospect theory as an alternative descriptive model to the expected utility theory. More than 35 years have passed and prospect theory has proved its value in a rich body of literature as the best descriptive model in decision making under risk and uncertainty.

Most of the behavioral insights in decision making under risk and uncertainty come from studies focusing on individual decisions in isolated contexts. This thesis expands the current knowledge base by examining these insights in interpersonal

contexts. It discusses in what interpersonal contexts and how people attenuate, amplify, or replicate the biases they exhibit when making decisions separately.

Chapter 2 investigates the rationality of group decisions versus individual decisions under risk. I use an experiment to study two group decision rules, majority and unanimity, in stochastic dominance and Allais paradox tasks. I distinguish communication effects (the effects of group discussion and interaction) from aggregation effects (the impact of pure voting), which makes it possible to better understand the complex dynamics of group decision making. I find both effects occur, but these effects were stronger and occur more often under the unanimity rule. Communication effects always lead to more rational choices; aggregation effects do so sometimes, but not always. Groups violate stochastic dominance less often than individuals do, which is due to both aggregation and communication effects. In the Allais paradox tasks, there are almost no communication effects, and aggregation effects make groups deviate more from expected utility than individuals.

Communication has an impact on not only collective decisions but also individual ones. Interpersonal communication is an important means of collecting information. Chapter 3 is concerned with the role of communication in individual judgment revision tasks. Except for an exchange of estimates, my design also allows the exchange of supportive evidence underlying the estimates in a controlled manner. Compared with control, the exchange of estimates and supportive evidence together improves judgment quality at both the individual level and the crowd level. On the other hand, the exchange of estimates or supportive evidence separately has either no or even a negative impact.

Chapter 4 is a theoretical paper focusing on interpersonal comparison of risk attitudes. Yaari (1969) defined person A as being more risk averse than person B under uncertainty as A rejecting all bets that B rejects, and showed that, under expected utility, this definition implies that A has more concave utility, and that A and B share the same beliefs. Extended from Yaari's results, my project proposes weaker definitions of comparative risk aversion that are applicable for people who hold different beliefs. Under subjective expected utility (SEU), these definitions by

themselves no longer imply any constraint on beliefs. They are all necessary for A to have more concave utility than B, and they are sufficient under additional belief assumptions. The most general definitions require so weak belief assumptions that they can also be used to compare risk aversion (possibly of a single person) towards different sources of uncertainty.

In Chapter 5, I investigate the effects of cash on people's risk attitudes. I ask I ask subjects of two treatments to value the same set of lotteries in the experiment. There is no communication between the subjects. All settings in the two treatments are identical except for one: in the cash treatment, all possible outcomes of the lotteries are presented with real cash notes, whereas in the control treatment, outcomes are denoted by written numbers. This chapter tests the effects of cash, the crucial payment instrument in trading, on individual valuations.

The results suggest that presenting the lotteries with real cash lowers participants' valuations, but does not affect their utility function. What drives the decrease of valuation is likelihood insensitivity in probability weighting. Towards the same lottery, subjects who value the one presented with cash exhibit less sensitivity towards changes in likelihood than participants in the control treatment do. The mere presentation with monetary currency could, without any real transaction, make a psychological impact on people's risk attitudes.

Chapter 2

Group decision rules and group rationality under risk

2.1 Introduction

Many economic decisions - e.g. family financial planning, corporate strategies, national laws - are made by groups. The literature comparing individual and group decision making is rich (Kugler et al., 2012). Groups have been found to attenuate, amplify, or replicate the biases found for individual decisions (Kerr et al., 1996), and these diverse findings highlight the closing remark of a recent review paper: “Ultimately, the goal of comparing individual and group decision making is to identify the contexts and types of decisions where each is likely to work best” (Charness and Sutter, 2012, p. 174).

This chapter compares group and individual decision making for three tasks, a test of non-transparent stochastic dominance, Allais’ common consequence paradox, and Allais’ common ratio paradox. Stochastic dominance is an objective, generally

This chapter is based on the the homonymous paper, co-authored with Aurélien Baillon, Han Bleichrodt, and Peter P. Wakker.

accepted criterion of rationality but recognizing it may require intellectual effort. We will therefore refer to this type of task as *intellective*. Previous research has shown that groups violate stochastic dominance less frequently than individuals (Charness et al., 2007), suggesting that group decisions improve rationality. Expected utility (EU) violations are to a larger extent due to motivational or subjective aspects of human behavior, for example the nonlinear weighting of probabilities. We will, therefore, call the Allais tasks *judgmental*. In this, we follow the psychological literature on groups (e.g. Laughlin and Ellis 1986; Kerr and Tindale 2011), where problems with a demonstrably correct answer are called intellective and tasks in which one cannot objectively defend one’s preferred alternative as correct (e.g. aesthetic judgments or matters of personal taste) are called judgmental. In Allais’ common consequence and common ratio paradoxes (Allais, 1953), previous research has found that groups violate EU as often as individuals do (Rockenbach et al. 2007; Bone et al. 1999; Bateman and Munro 2005).

We distinguish two components in group decision making: *aggregation* and *communication*. Aggregation refers to the direct effect of the procedure on the group decision, without involving any change of an individual or any communication. In single choice tasks, aggregation effects tend to amplify the patterns exhibited by the majority, as we will show; these effects are merely procedural and statistical. For tasks involving two choices (as in the Allais paradoxes), however, aggregation effects can reverse the majority pattern. It then is, for example, possible that, while a majority of the group members behave according to EU, the group decision violates EU. Aggregation effects are fleeting and do not affect individual attitudes or subsequent individual decisions.

Communication effects do affect individuals. Communication effects are lasting and capture the effects of group decision making beyond pure aggregation. These effects include the impact of learning what others prefer and why they prefer it. For instance, group decision making can foster discussion and this may change members’ preferences. Communication effects will persist after the group process has ended (Maciejovsky et al., 2013).

In our experiment, we measured the strength of aggregation and communication effects for majority and unanimity group decisions. We elicited individual preferences before and after the group decision stage. Changes in individual preferences were interpreted as communication effects. We also used a second approach to measure communication effects. To control for the effect of aggregation, we aggregated the individual decisions before the group decision stage into simulated group decisions. Differences between these simulated decisions and the actual group decisions constitute communication effects. For instance, in the actual group decisions a minority sometimes convinced a majority to change their preferences, which was clearly a communication effect.

The effects of decision rules are central in political economics (Feddersen and Pesendorfer 1998; Messner and Polborn 2004), and have also been studied in psychology (Kerr and Tindale, 2004). However, they have rarely been considered for decisions under risk. An exception is Brunette et al. (2014), who implemented majority and unanimity voting rules, but did not permit verbal communication between group members. Most existing studies considered groups of two individuals, for which majority and unanimity rules make the same predictions and, therefore, cannot be distinguished. We used groups of three individuals, for which these rules can be distinguished. In intellectual tasks, the results indicate that both rules led to aggregation and communication effects, which improved both group rationality and individual rationality. In the judgmental tasks there were mainly aggregation effects, which led to more deviations from expected utility for groups than for individuals in one of the tasks (the common ratio task).

2.2 Literature

According to social-decision scheme theory (Davis, 1973), group decisions are determined by the majority view unless the minority view is demonstrably correct, which is the case where “truth wins” (Laughlin and Ellis 1986; Davis 1992; Kerr and Tindale 2011). This suggests that communication effects will be stronger when the

reasons for choosing one option are easy to defend. Because answers in intellectual tasks are easier to defend than answers in judgmental tasks, we expect that communication effects will be stronger in the non-transparent stochastic dominance (NTSD) tasks than in the Allais tasks.

The literature on group decision processes shows that the unanimity rule typically leads to more communication than the majority rule. Moreover, group members identify themselves more with unanimous decisions than with majority decisions (Kameda, 1991). The deliberations of unanimity groups show more conflict and more changes of opinion (Nemeth, 1977). Accordingly, we hypothesize that the unanimity rule leads to more discussion than the majority rule and, consequently, to stronger communication effects.

Several studies have investigated the effect of different aggregation rules on the outcome of the decision process, both theoretically and experimentally. Dasgupta and Maskin (2008) showed theoretically that the majority rule satisfies five appealing conditions (Pareto optimality, anonymity, neutrality, independence of irrelevant alternatives, and decisiveness) over a larger class of preferences than any other voting rule does and, in this sense, is the most robust rule.

For jury decision making, Feddersen and Pesendorfer (1998) showed theoretically that the unanimity rule is more likely to convict the innocent and to acquit the guilty if jury members vote strategically. However, Goeree and Yariv (2011) found in an experiment that such differences in jury decisions vanish when deliberation before voting is allowed. Miller (1985) and Ohtsubo et al. (2004) showed experimentally that majority group decisions tend to ignore the minority's preferences whereas the unanimity decisions incorporate the minority's preferences when information about each group member's preference is available. Finally, Stasson et al. (1991) showed that subjects from majority groups performed marginally better than subjects from unanimity groups in mathematical tasks. However, groups consisting only of members who had answered incorrectly in the individual tasks were more likely to find the correct solution under the unanimity rule.

2.3 Examples of aggregation effects

To clarify the role of aggregation and to show how it can distort group decisions, we will give two simple examples, one involving a single choice (as in the NTSD task) and one involving two choices (as in the Allais paradoxes).

Example 1: Aggregation effects in a single choice

Consider a choice between two lotteries where one lottery stochastically dominates the other. Assume that a minority of the population violates stochastic dominance. If people in this population form groups and the group choice is made by the majority rule then sufficiently large groups will almost always satisfy stochastic dominance. Aggregation effects amplify the majority choice, and provided the majority is rational, will amplify rationality. This results purely from the aggregation procedure, without involving any change of any person or any communication.

Example 2: Aggregation effects in a pair of choices

Aggregation effects are more complex when two choices have to be made as in the Allais paradoxes. Consider two choices, each between a risky lottery R and a safe lottery S. Then there are four possible choice patterns: SS, RR, SR, and RS. The SS and RR patterns are consistent with EU (EU-consistent), and the SR and RS patterns are EU-inconsistent. Suppose that 30% of the sample chooses SS, 30% chooses RR, and 40% chooses SR. Then the majority of the population (60%) is EU-consistent. If we randomly draw groups of three persons from this population and let the group choice be determined by majority then SR will be chosen in 56.8% of the cases and the majority, thus, exhibits an EU-inconsistent pattern. The opposite case (a majority of EU-inconsistent people leading to a majority of EU-consistent groups) can also occur¹. Again, this results purely from the aggregation procedure.

¹If the individuals exhibit patterns SS, RR, SR, and RS with probability 40%, 0%, 30%, 30% respectively, then the groups will exhibit the same patterns with probability 56.8%, 0%, 21.6%, and 21.6%.

2.4 Method

2.4.1 Experimental Design

156 students of Erasmus University Rotterdam participated in the experiment (63% males). We organized 52 sessions of three subjects each.

Table 2.1: Structure of the experiment

	<i>Stage 1</i>	<i>Stage 2</i>	<i>Stage 3</i>
Control	Individual	Individual	Individual
M-treatment	Individual	<i>Majority Rule</i>	Individual
U-treatment	Individual	<i>Unanimity Rule</i>	Individual

The experiment consisted of three stages (within-subjects) and three treatments (between-subjects; see Table 2.1). A stage consisted of a set of instructions and decision tasks, which subjects received on paper (called answer sheets hereafter). All decision tasks were choices between two lotteries. The subjects in a session had their own colored pen (blue, red, or black) to mark their preferred lottery on the answer sheet so that we could identify individual choices.

In the first and third stages of the experiment, subjects made individual choices and were not allowed to talk with each other. Only the second stage of the experiment differed between treatments. In the *control treatment*, subjects made individual decisions (without communication), as in the other two stages. This permitted us to check for any learning effect. In the *M-treatment*, the three subjects in a session made group decisions using the majority rule and they could communicate face-to-face in English. All subjects, after (possible) discussion, marked their preferred lotteries. The option with at least two ticks was taken as the group choice, but minority subjects could still express disagreement. In the *U-treatment* subjects had to reach unanimity. All three subjects of a session had to tick the same lottery, otherwise the choice was invalid and would not be paid (see the section on incentives below). The groups of the U-treatment always reached unanimity. Subjects could freely and directly communicate during the group decisions (Stage 2 of M- and U-treatment). In the control treatment, answering all questions took 21 minutes on

average. In the other two treatments, subjects needed about 7 minutes more due to group deliberations.

Stage 1 gave us information on subjects' behavior before any treatment manipulation. We used these choices to simulate the group decisions they would have made based on the majority rule and, thereby, we inferred the effect of pure aggregation without any communication. We could then compare the actual group decisions in stage 2 with these simulated decisions to measure the effects beyond pure aggregation. Finally, stage 3 gave information about whether any changes in preferences that we observed in stage 2 were temporary (and thus probably due to aggregation effects) or lasting (which is likely for communication effects).

Each session of three subjects was randomly assigned to one treatment. We ran 13 sessions for the control treatment, 21 sessions for the M-treatment, and 18 sessions for the U-treatment. There were fewer sessions of the control treatments to have more observations in M- and U-treatments, which concerned the main research questions.

2.4.2 Stimuli

Subjects faced three types of decision tasks in each stage of the experiment: NTSD tasks, CR tasks, and CC tasks. We present these tasks below. The order of the choices was randomized within each of the three stages (it was therefore possible that two questions for the same CR task were far apart in the experiment), but the order was the same for all subjects. Huber et al. (2008) showed that choices are affected by the first and the last choice made. No distortions result when there are no more than three choices, as in our experiment.

Subjects had to choose between two lotteries, A and B. Table 2 gives an example of the way choices were displayed. Uncertainty was resolved using the roll of a twenty-sided die. The first row of Table 2.1 shows the outcomes of the roll of the die. Rows 2 and 3 show the payoffs of the two lotteries under each roll of the die. Subjects indicated their choice by ticking the cell of the last column of their preferred lottery.

NTSD tasks

Charness et al. (2007) found that groups are more likely to satisfy stochastic dominance than individuals, but did not specify the aggregation rule that groups should follow and did not check whether the group effects persisted in later individual choices. Maciejovsky et al. (2013) found that group effects persisted in intellectual tasks other than NTSD, but they did not distinguish between the majority rule and the unanimity rule as their group size was two, for which the two rules lead to the same results. We adapted the NTSD tasks first used by Tversky and Kahneman (1986).

Table 2.2: An NTSD task

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Lottery A	€0												€35				€30	€5			
Lottery B	€5	€10	€35				€0														

Table 2.2 is an example of a NTSD task faced by the subjects. Lottery B results from lottery A by improving €30 into €35 and by improving two outcomes €5 into €10. Consequently, lottery B stochastically dominates lottery A. However, this dominance is not immediately obvious and a subject who neglects probability differences and only compares the outcomes of the lotteries may have the false impression that A is better than B because it yields money amounts €5, €30, and €35, whereas Lottery B yields money amounts €5, €10, and €35. Because the violation of stochastic dominance in Table 2 is non-transparent, we refer to these tasks as NTSD. Some theories allow for such violations of stochastic dominance (Kahneman and Tversky 1979; Viscusi 1989). Table 2.3 summarizes the tests of non-transparent stochastic dominance that we carried out. Note that the tests are comparable in structure but differ across the three stages.

CC tasks

The CC tasks were close to those of Huck and Müller (2012) with a small adjustment to fit into our 20-sided die format.

Table 2.3: NTSD questions

	Dominated Lottery				Dominant Lottery			
	20%	5%	15%	60%	25%	10%	5%	60%
Stage 1	35	30	5	0	5	10	35	0
	40	30	8	0	8	12	40	0
	40	35	5	0	5	10	40	0
	45	40	10	0	10	15	45	0
	35	28	5	0	5	10	35	0
Stage 2	35	32	8	0	8	12	35	0
	35	30	8	0	8	12	35	0
	45	35	5	0	5	10	45	0
	40	35	10	0	10	15	40	0
	45	28	8	0	8	12	35	0
Stage 3	40	28	5	0	5	12	40	0
	45	30	8	0	8	15	45	0
	45	25	10	0	10	15	45	0
	40	25	5	0	5	10	40	0
	35	30	8	0	8	12	35	0

The second row indicates the probabilities of the outcomes; the numbers below the probabilities indicate payoffs in €

Table 2.4: A CC task

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Lottery S	€_s																				
Lottery R	€_0					€_s												€_r			

a) First Choice

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Lottery S	€_s								€_0												
Lottery R	€_0																€_r				

b) Second Choice

Notes: Actual payoffs s and r in Table 2.5. In the experiment, lotteries were presented as Lottery A and Lottery B.

Table 2.4 shows the structure of the CC tasks. Table 2.5 presents the different values of s , r , and q that we used in the experiment². Each CC task involved two choices between Lottery S and Lottery R with the second choice similar to the first except that the (common) q chance (60% in the above example) of €_s was changed

²Stage 1 had four CC tasks and Stages 2 and 3 had three CC tasks.

into a q chance of nothing.

According to the sure-thing principle (Savage, 1954) of EU, replacing a common payoff by another common payoff does not affect preference. Hence EU predicts the same preference in both choices, either SS or RR. However, empirically, many subjects violate EU and display the pattern SR, which can be due to the certainty effect (see Starmer 2000, for a survey). The opposite pattern, RS, has also been observed (Starmer 1992; Wu et al. 2005; Blavatsky 2013) although less often.

Table 2.5: Payoffs and common probability of the CC tasks

	Stage 1				Stage 2			Stage 3		
s	8	8	9	8	10	9	10	10	8	10
r	25	25	30	20	45	30	45	40	20	40
q	0.60	0.55	0.55	0.55	0.60	0.60	0.55	0.60	0.60	0.55

Notes: The first column means that in Table 2.4, we used $s=\text{€}8$, $r=\text{€}25$, and $q=60\%$. The first choice was therefore between a Lottery S yielding $\text{€}8$ for sure and a Lottery R yielding $\text{€}25$ with probability 20%, $\text{€}8$ with probability 60% and $\text{€}0$ otherwise. The second choice involved Lottery S , yielding $\text{€}8$ with probability 40% ($=100\%-q$) and nothing otherwise, and Lottery R yielding $\text{€}25$ with probability 20% and nothing otherwise.

CR tasks

The CR questions were adapted from Loomes (1988), using the design of Van de Kuilen and Wakker (2006). Table 2.6 shows the way the CR choices were presented

Table 2.6: A CR task

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Lottery S	€_s																				
Lottery R	€_r																$\text{€}0$				

a) First Choice

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Lottery S	€_s					$\text{€}0$															
Lottery R	$\text{€}0$																€_r				

b) Second Choice

Notes: Actual payoffs s and r can be found in Table 2.7. In the experiment, lotteries were presented as Lottery A and Lottery B.

to the subjects. The second choice follows from the first by dividing all probabilities at nonzero outcomes by 4. According to the independence condition of EU (Fishburn and Wakker, 1995), this should not affect preferences. Hence EU predicts the same choice in both situations, either SS or RR. However, empirically, the prevailing pattern is SR, which violates EU. The opposite violation RS, has rarely been observed.

Table 2.7: Payoffs of the CR tasks

	Stage 1					Stage 2					Stage 3				
<i>s</i>	18	9	14	10.5	20	16.5	15	9.5	11.5	12.5	13	10	15.5	17.5	19
<i>r</i>	24.5	12	18	14.5	25.5	22.5	20.5	13	15	17.5	18	14	21.5	24	25

Incentives

Upon completion of the experiment, subjects received a €5 participation fee. In addition, they played out one of their choices for real. First, one of the 3 stages was randomly selected. All subjects in a session played out a choice that was made in this stage. If the selected stage was an individual stage (Stages 1 and 3 for the M- and U-treatments, and all stages for the control treatment), each subject randomly drew one choice and played out the lottery they preferred in that choice.

If the second stage was selected in the M- and U-treatments, the lottery the group had chosen was played out and the three subjects received the same payoff. In the group decision making stage, subjects shared the consequences of their choice. We explained this group incentive procedure in the M- and U-treatments at the beginning of Stage 2. The instructions were the same across treatments, except for Stage 2.

2.4.3 Analysis

In the individual decisions (all three stages of the control and stage 1 and 3 of the M- and U-treatments), we used subjects' reported choices as input in the analysis. In the group decisions (stage 2 for the M- and U-treatments), we assigned the choices of his group to each subject. For the M-treatment in stage 2, we could also use the choices reported by each subject, since they were allowed to express disagreement

with the group choice. Thirty-one percent of the majority group decisions revealed disagreement. Our conclusions did not depend on whether we analyzed the group choices or the reported choices in the second stage of the M-treatment (Alternative results are available upon request).

We used probit³ models to study the likelihood of violations of stochastic dominance and multinomial probit models to study the likelihood of the four CC and CR patterns. We used clustered standard errors in the probit regressions. For the NTSD tasks, the dependent (binary) variable indicated whether or not the choice satisfied stochastic dominance. For the CC and the CR tasks, the dependent (categorical) variable described the choice pattern (SS, SR, RS, or RR).

We used the dummy variables *majority*, *unanimity*, *stage2*, and *stage3* to code stages and treatments. The main effects *stage2* and *stage3* captured the effect of learning. Our main independent variables represented the interaction terms of the treatments with the stages. The terms *majority*stage2* and *unanimity*stage2* identified the full effect of group decision making (both aggregation and communication effects). The terms *majority*stage3* and *unanimity*stage3* captured the changes in preferences after the group stage and provided a first method to estimate communication effects.

As explained before, the second method to disentangle aggregation and communication effects used the individual choices (of stage 1 for the M- and U-treatments and stages 1 and 2 for the control treatment) to simulate aggregate choices. For the majority rule, as each session consisted of three subjects, we could in each case simulate the majority group choice. These simulated group choices captured the pure aggregation effects. The difference between the simulated and the actual group choices provided an estimate of communication effects. The majority preference is also the most plausible benchmark for the U-treatment. A deviation from the majority choice then can only occur if some group members were willing to change their mind, and there must then have been communication effects. The analysis of the simulated and the group choices was the same as the analysis of the individual and the group

³Results from alternative logit models are available upon request.

choices, which was described above, except that we excluded stage 3 and, thus, used fewer data points. The interpretation of the results of the third stage is unclear for the simulated data. Finally, we also recorded and studied the group discussions. In nearly 40% of the groups, arguments based on stochastic dominance were used (without using this term though). The dummy variable *dominance_discussion* identified the members of such groups.

2.5 Result

2.5.1 *NTSD* tasks

Figure 2.1 displays the proportion of choices satisfying stochastic dominance, for the actual choices (panel A) and when individual choices are replaced by the simulated choices (panel B). Panel A shows that only a minority of subjects chose the dominant lottery in the first stage. In the control treatment, the proportion of choices satisfying stochastic dominance increased from around 40% in stage 1 to around 50% in stage 2 to slightly below 60% in stage 3, suggesting modest learning. In the M- and U-treatments, we see a faster increase in the second stage, due to aggregation and communication effects. The comparison of the group choices with the simulated choices (panel B) confirms that even though subjects of all treatments better identify dominant lotteries as the experiment progresses, this improvement is faster in the M- and U-treatments than in the control treatment.

Table 2.8 reports the results of the probit regressions. The first column, which reports the results for the actual choices, shows that the use of stochastic dominance arguments in the group discussion was crucial. Groups in which no stochastic dominance arguments were used did not differ from the control treatment. However, there were strong group effects in stages 2 and 3 for groups that did talk about stochastic dominance (captured by the three-way interaction terms between stages, treatments and *dominance_discussion*). Hence, a subject who used stochastic dominance arguments strongly affected not only his group's choices in stage 2 but also the individual choices of his fellow group members in stage 3, indicating communication effects.

Table 2.8: Probit regressions for the NTSD tasks

	Group vs. individual choices	Group vs. simulated choices
<i>stage2</i>	0.06 (0.04)	0.14** (0.07)
<i>stage3</i>	0.13** (0.06)	
<i>majority</i>	-0.09 (0.09)	-0.05 (0.17)
<i>unanimity</i>	-0.17* (0.10)	-0.17 (0.17)
<i>stage2*majority</i>	0.02 (0.07)	0.01 (0.12)
<i>stage2*unanimity</i>	-0.16 (0.10)	-0.14 (0.14)
<i>stage3*majority</i>	-0.02 (0.08)	
<i>stage3*unanimity</i>	-0.11 (0.09)	
<i>majority*dominance_discussion</i>	0.24** (0.11)	0.34* (0.18)
<i>unanimity*dominance_discussion</i>	0.12 (0.12)	0.08 (0.17)
<i>stage2*majority*</i>	0.44*** (0.14)	0.50** (0.24)
<i>dominance_discussion</i>		
<i>stage2*unanimity*</i>	0.84*** (0.15)	0.89*** (0.21)
<i>dominance_discussion</i>		
<i>stage3*majority*</i>	0.34*** (0.13)	
<i>dominance_discussion</i>		
<i>stage3*unanimity*</i>	0.47*** (0.14)	
<i>dominance_discussion</i>		
<i>No. of observations</i>	2340	520
<i>Wald chi2</i>	149.69	74.69
<i>p-value</i>	0.00	0.00

Notes: Reported numbers are the marginal effects at the means of covariates, followed by significance and clustered standard errors between brackets. The standard errors in the left column are clustered at the individual level⁴, and those in the right column are clustered at the group level.

* indicates significant at 10% (two-sided test)

** indicates significant at 5% (two-sided test)

*** indicates significant at 1% (two-sided test)

In the M-treatment, the significant interaction term *dominance_discussion*majority* captures the difference in the proportions who satisfied stochastic dominance in stage 1 between subjects who talked about stochastic dominance in stage 2 and those who did not.

The comparison of group choices with simulated choices confirmed that communication effects played a role in both the M-treatment and the U-treatment. Once aggregation effects were ruled out, groups that talked about stochastic dominance were still more likely to choose the dominant lotteries in all stages than those that did not talk about stochastic dominance. The three-way interactions between *majority*, *dominance_discussion*, and *stage2* and between *unanimity*, *dominance_discussion*, and *stage2* were significant, showing that the choices of groups that talked about stochastic dominance were affected by communication effects beyond pure aggregation effects. Majority and unanimity seemed to have fostered discussions, which

increased subjects' rationality. The importance of avoiding biases and increasing rationality has often been emphasized (Viscusi, 1995, p. 108) and our analysis shows that group decision making may contribute to such increasing rationality.

2.5.2 *CC* tasks

Neither the majority nor the unanimity group rule led to more EU-consistent choices in the common consequence Allais paradoxes. In the first stage, half of our subjects' choices were consistent with EU (32% of the choice patterns were RR and 18% were SS). Surprisingly, the violations of EU were mainly of the RS type: 45% of the patterns were RS and only 5% of the empirically common SR. This finding does not confirm the certainty effect. The low occurrence of the SR pattern led to estimation problems and we excluded it from the analysis reported below. Keeping the SR pattern whenever possible gave similar results as those reported next (these results available upon request).

The proportion of EU violations slightly dropped in the second stage, but this held for all treatments and it disappeared in the third stage. The control and the M-treatment look very similar. In the U-treatment, there was a more pronounced increase in the proportion of RR choices, but, again, the effect did not last. Several studies have found that risk aversion is negatively related to cognitive ability (Frederick 2005; Dohmen et al. 2010; Dohmen et al. 2011). This finding could suggest that the unanimity rule also led to more rationality in the CC task. It is consistent with Keck et al. (2014) finding that groups are closer to EU (ambiguity neutrality) than individuals. However, the effect did not last⁵.

Table 2.9 shows the results of the multinomial probit regressions. Subjects became more risk averse during the experiment as indicated by the positive coefficient of stage 3 in the SS choices. Groups were more risk seeking than individuals: in stage 2, the prevalence of the SS pattern was less in the M- and U-treatments than in

⁵The literature that compares risk attitudes between groups and individuals gives mixed results. Rockenbach et al. (2007) and Viscusi et al. (2011) found that groups were more risk seeking than individuals. On the other hand, Masclet et al. (2009) found that groups were more risk averse than individuals. Adams and Ferreira (2010) found that groups made decisions closer to risk neutrality than individuals do.

Table 2.9: Probit regressions for the CC tasks

Choice pattern	Group vs. individual choices			Group vs. simulated choices		
	SS	RR	RS	SS	RR	RS
<i>stage2</i>	0.02 (0.02)	0.08 (0.05)	-0.10* (0.05)	0.02 (0.04)	0.12 (0.10)	-0.13 (0.11)
<i>stage3</i>	0.06** (0.03)	-0.01 (0.06)	-0.06 (0.06)			
<i>majority</i>	-0.02 (0.06)	-0.04 (0.07)	0.07 (0.07)	-0.01 (0.08)	-0.03 (0.12)	0.04 (0.12)
<i>unanimity</i>	-0.04* (0.06)	-0.02 (0.07)	0.07 (0.07)	-0.15 (0.10)	0.03 (0.13)	0.12 (0.12)
<i>stage2*majority</i>	-0.10** (0.05)	0.03 (0.07)	0.07 (0.08)	-0.06 (0.06)	0.02 (0.13)	0.04 (0.14)
<i>stage2*unanimity</i>	-0.29*** (0.07)	0.25*** (0.07)	0.04 (0.08)	-0.08 (0.05)	0.18 (0.12)	-0.10 (0.13)
<i>stage3*majority</i>	-0.05 (0.04)	0.00 (0.07)	0.05 (0.07)			
<i>stage3*unanimity</i>	-0.05 (0.06)	-0.00 (0.08)	0.05 (0.08)			
<i>No. of observations</i>		1481			350	
<i>Wald chi2</i>		86.83			32.94	
<i>p-value</i>		0.00			0.00	

Notes: Reported numbers are the marginal effects at the means of covariates, followed by significance and clustered standard errors between brackets. The standard errors in the left three columns are clustered at the individual level, and those in the right three columns are clustered at the group level. Missing choices and SR patterns are excluded.

* indicates significant at 10% (two-sided test)

** indicates significant at 5% (two-sided test)

*** indicates significant at 1% (two-sided test)

the control treatment (see model 1). The RR pattern was also more common in the U-treatment than in the control group in Stage 2 (model 3). However, the two-way interactions in stage 3 (*stage3*majority* and *stage3*unanimity*) were not significant and there was no evidence of communication effects. When comparing group choices with simulated choices, the treatment effects on the SS and RR patterns at stage 2 disappeared, confirming the absence of communication effects. The tendency to make riskier group decisions was thus mainly due to aggregation effects.

2.5.3 CR tasks

In the CR tasks, we again found that about half of the subjects satisfied EU in the first stage (29% SS pattern, 22% RR pattern). This time we found the usual SR violation of EU (41%). We do not report the results about the infrequent RS pattern, because it led to estimation problems in one of the regressions. As in the CC tasks, group choices tended to be more risk seeking than individual choices. However, this time the shift was from SS to the EU-inconsistent SR pattern and less to the EU-consistent RR pattern.

Table 2.10: Probit regressions for the CR tasks

Choice pattern	Group vs. individual choices			Group vs. simulated choices		
	SS	RR	RS	SS	RR	RS
<i>stage2</i>	-0.04 (0.03)	0.05 (0.04)	-0.01 (0.06)	-0.03 (0.04)	0.03 (0.06)	-0.00 (0.09)
<i>stage3</i>	-0.00 (0.03)	0.05 (0.04)	-0.05 (0.05)			
<i>majority</i>	-0.07 (0.06)	0.05 (0.07)	0.02 (0.08)	-0.06 (0.09)	0.03 (0.11)	0.04 (0.13)
<i>unanimity</i>	-0.10* (0.06)	0.03 (0.08)	0.07 (0.08)	-0.09 (0.09)	-0.07 (0.11)	0.16 (0.13)
<i>stage2*majority</i>	-0.19*** (0.06)	0.03 (0.06)	0.16** (0.08)	-0.10 (0.08)	0.06 (0.08)	0.05 (0.12)
<i>stage2*unanimity</i>	-0.19*** (0.06)	0.04 (0.07)	0.14* (0.08)	-0.11 (0.09)	0.15* (0.08)	-0.04 (0.12)
<i>stage3*majority</i>	-0.08 (0.04)	0.01 (0.07)	0.07 (0.07)			
<i>stage3*unanimity</i>	-0.05 (0.06)	-0.00 (0.06)	0.05 (0.08)			
<i>No. of observations</i>		2202			505	
<i>Wald chi2</i>		53.82			24.70	
<i>p-value</i>		0.00			0.00	

Notes: Reported numbers are the marginal effects at the means of covariates, followed by significance and clustered standard errors between brackets. The standard errors in the left three columns are clustered at the individual level, and those in the right three columns are clustered at the group level. Missing choices and RS patterns are excluded.

* indicates significant at 10% (two-sided test)

** indicates significant at 5% (two-sided test)

*** indicates significant at 1% (two-sided test)

The multinomial probit regressions in Table 2.10 confirmed that the M- and U-

treatments decreased the number of SS patterns and increased the number of SR patterns. These effects did not last in stage 3, suggesting that they were aggregation effects. This suggestion was confirmed by the comparison between the group choices and the simulated choices, in which these effects disappeared: the prevalence of the SS and SR patterns in the actual group choices (stage 2 of the M- and U-treatments) did not differ from the prevalence of these patterns in the simulated choices.

In the regression comparing groups with simulated choices, there were marginally more RR-choices in stage 2 than in stage 1 for treatment U, suggesting that communication increased risk seeking. This was not observed in the regression comparing groups with individual choices. This result indicates that aggregation effects and communication effects had opposite effects on the proportion of RR choices. Communication effects led to an increase in RR choices, whereas aggregation effects reduced them. These effects could not be observed in the actual choices because the aggregation effects and communication effects were close in magnitude and cancelled out. It shows the added value of analyzing the simulated choices. That communication effects led to more risk seeking suggests that individual choices may be too risk averse and (some) more risk seeking may be rational. On the other hand, the effect was only modest and in contrast with the findings on NTSD, it did not persist in the third stage.

2.5.4 *Additional analysis*

We recorded the decision time at each stage. Table 2.11 reports the results of a linear regression of decision times on treatments and on stages. The M- and U-treatments took more time than the control treatment in stage 2, but they did not differ from each other ($p=0.20$). Response time decreased for the second and third stages of the control treatment and we also observed that the response time was less in the third stage than in the first stage in the majority and the unanimity treatments.

Table 2.11: Linear regression on decision time

<i>stage2</i>	-1.77*	(0.96)
<i>stage3</i>	-2.73***	(0.96)
<i>majority</i>	0.05	(0.86)
<i>unanimity</i>	0.50	(0.89)
<i>stage2*majority</i>	3.72***	(1.22)
<i>stage2*unanimity</i>	5.16***	(1.26)
<i>stage3*majority</i>	-0.27	(1.22)
<i>stage3*unanimity</i>	-0.57	(1.26)

Notes: Reported numbers are regression coefficients, followed by significance and standard errors. The unit of the dependent variable is minute.

* indicates significant at 10% (two-sided test)

** indicates significant at 5% (two-sided test)

*** indicates significant at 1% (two-sided test)

2.6 Conclusion

We disentangled communication effects and aggregation effects in group decisions. This allowed us to analyze the effects of unanimity and majority rules on intellectual and judgmental tasks under risk. Our results show that:

a) Both aggregation effects and communication effects occurred but these effects were stronger and occurred more often under the unanimity rule.

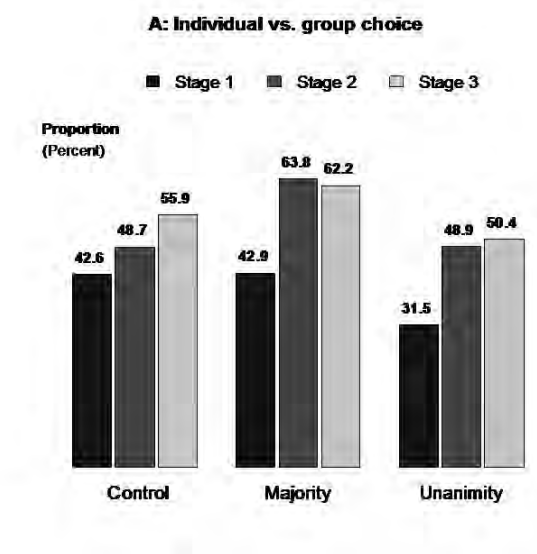
b) Aggregation effects were mixed and did not always lead to more rational choices. Aggregation effects reduced violations of stochastic dominance, but they also reduced EU-consistent patterns in Allais' common ratio tasks, and they changed the distribution of EU-consistent choice patterns in Allais' common consequence task without affecting the overall proportion of EU-consistent choice patterns.

c) Communication effects favored the justifiable (rational) choices and had more impact in intellectual tasks than in judgmental tasks. When there was a clear argument for a particular choice, such as in the nontransparent stochastic dominance tasks, then communication effects could strongly increase the proportion of such choices in the group decisions. Communication effects led to more risk seeking in the common ratio tasks (with marginal significance), but not in the common consequence

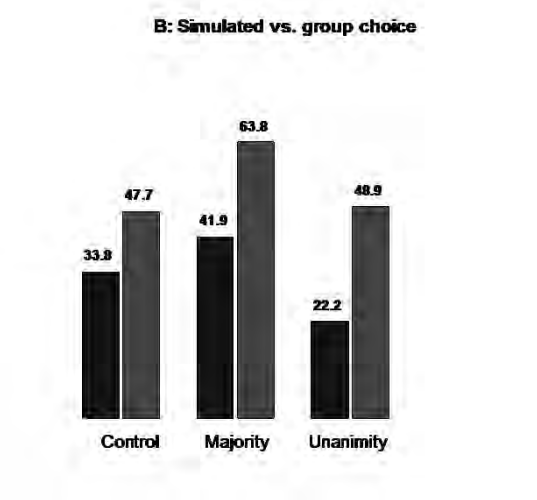
tasks. Communication effects increased rationality but they also increased decision times.

The separation of communication and aggregation effects introduced in this chapter sheds new light on the pros and cons of group decision rules and on their differences with individual decisions. In particular, we can test whether these rules increase the rationality of group decisions, and have a lasting impact on individual decisions after the group process is over. Our results show that communication effects play an important role and deserve further study.

Figure 2.1: Proportion of choices satisfying stochastic dominance across treatments and stages.



(a)



(b)

Figure 2.2: Proportion of choices in the Allais common consequence tasks per treatment and stage.

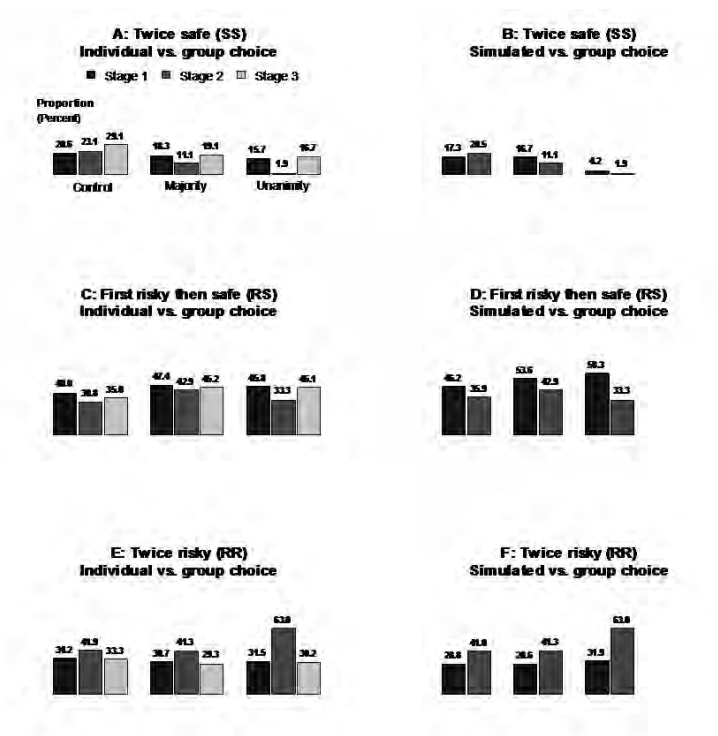
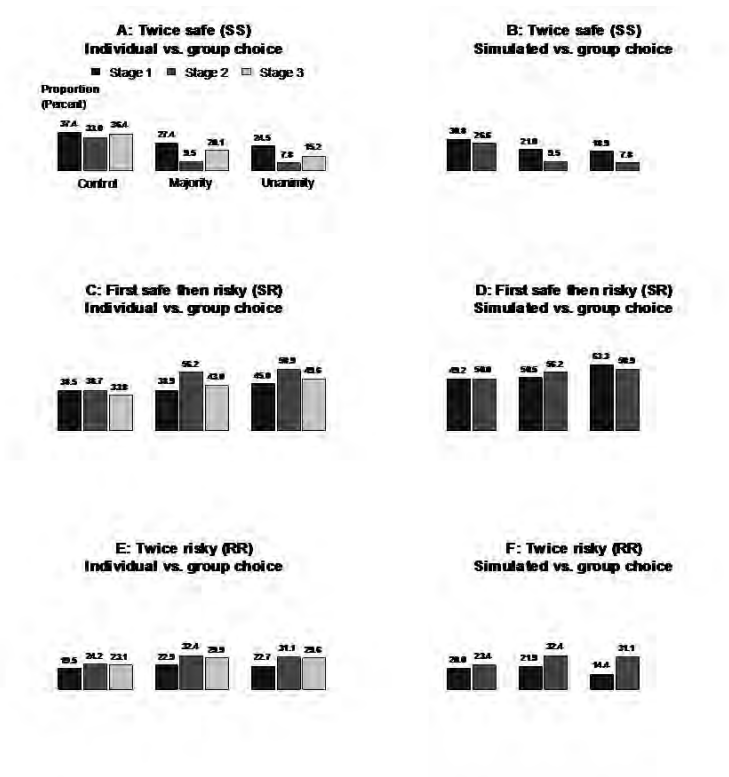


Figure 2.3: Proportion of choices in the Allais common ratio tasks per treatment and stage.



Chapter 3

Individual decision making with controlled communication

3.1 Introduction

Combining estimates from different people, even by simply taking the average, can generate a surprisingly accurate aggregate estimate under certain circumstances. This phenomenon was first observed more than one hundred years ago (Galton 1907), and is often referred to as the wisdom of the crowd (WoC). WoC has been observed in many scientific studies and general observations (Surowiecki 2005), and its application has been studied in various tasks and environments (Laughlin et al. 2006; Chen 2007; Pentland 2007; Nguyen 2008; Krause et al. 2010; Lee and Chang 2010; Lykourantzou et al. 2010; Beer 2013). Although WoC is of a statistical nature (Stroop 1932), taking full advantage of WoC requires behavioral insights to design proper processes where WoC is likely to work best.

One of the claimed cornerstones of WoC is independence, which requires that indi-

This chapter is based on the homonymous paper, co-authored with Chen Li.

viduals form and express their opinions independent of others' influences (Surowiecki, 2005; Wagner and Vinaimont, 2010). In his best seller, Surowiecki (2005) documented a series of collective failures showing the possible downside of communication.

In many real life situations, decisions are rarely made by one individual acting alone. Blocking communication is unrealistic. Projects from "Corporation Wikipedia" (Lykourantzou et al., 2010) to the recent plan of "Listening government" in the UK (Coleman and Blumler, 2011) highly depend on the internet to harness the WoC, and communication is ubiquitous in such environments. Moreover, it is premature to see communication as a problem. Two questions have to be answered first. How important is independence for judgment aggregation and how does communication affect independence?

When individual judgments are used as input of a combination process that takes the average of individual judgments, independence is valuable as it cultivates diversity and makes the crowd more accurate. In this special case, the magnitude of individual errors matters little as long as these errors cancel out. In many cases, however, when other aggregation rules, such as approval voting, are used, individual accuracy also matters. For instance, imagine that two committees, each with 21 experts, are forecasting the GDP growth next year. In committee A, the judgment distribution is: 10 members predict 1.7%; 10 members predict 3%; and 1 member predicts 2.3%. Whereas the judgment distribution in committee B is: 10 members predict -5%; 10 members predict 9%; and 1 member predicts 2%. It turns out that GDP grows 2%. The average predictions of both committees are equally accurate. But if the committees take a poll to determine their final predictions, committee A is more likely to reach a more accurate consensus than committee B is. Hence, a comprehensive evaluation of the communication effects requires measuring the quality of both individual and aggregate judgments.

The prevailing finding on communication is that judgment communication improves individual accuracy (Yaniv and Kleinberger, 2000; Bonaccio and Dalal, 2006; Mannes, 2009; Soll and Larrick, 2009; Soll and Mannes, 2011). The effects of communication on crowd accuracy (we shall refer to the accuracy of the average as crowd

accuracy), however, are less conclusive. Although closely related, only a few studies on wisdom of the crowd jointly investigated the effect of social interaction on individual accuracy and crowd accuracy. In an experiment with face-to-face communication (Jenness, 1932), individual accuracy was found to increase substantially after discussion, whereas crowd accuracy decreased undesirably. In a recent study (Lorenz et al., 2011) with non-verbal communication, the authors found that communication decreased crowd diversity, and boosted individual confidence without improving crowd accuracy. A re-analysis of the same data showed that individual accuracy increased after communication, and challenged the results of Lorenz et al. (2011) on the ground that the crowd diversity criteria they adopted favored excessive variance (Farrell, 2011).

This paper investigates the impact of communication on quality of individual and crowd judgments jointly. We introduce measures capturing different aspects of judgment quality and related psychological traits at both the individual and the crowd level, aiming to provide a more comprehensive picture of how communications of different information contents influence a variety of important aspects of judgment quality. We found that, when judgment was exchanged together with supporting evidence, both individual and crowd accuracy improved, without harming the diversity of opinion. However, when individual judgments or supportive evidence were exchanged separately, there was no impact on judgment quality at both individual and crowd levels.

Method: Experimental Design

Subjects

Subjects were 252 high school students from Donghu Middle School (101) and Caidian No.2 Middle School (151) in Hubei province in China. They were from the 10th grade, with age ranging from 15 to 17.

Tasks

Following the literature (Galton, 1907; Lorenz et al., 2011), we use simple esti-

mation tasks on general knowledge to study the communication effects. For every subject in each treatment, three types of questions were asked. A full list of questions are in Appendix A.

- Estimation questions (ESQ): 15 general knowledge questions about the total number of elements in a specified set, such as “How many countries are there in Asia?” and “How many fictional characters have appeared in the Harry Potter series?”
- Confidence questions (CQ): 15 questions asking subjects to indicate their confidence in their answer to each corresponding estimation question on a 7-point scale.
- Evidence questions (EVQ): 15 questions asking subjects to provide supporting evidence of their answers to each corresponding estimation question. A typical evidence question asks the subjects to state 3 elements that they think others are most likely not to be able to think of. One example of an evidence question is: Please name 3 Asian countries that you think the others most likely will not think of.

Treatments with communication of different information contents

We have three main between-subjects treatments where we vary the information content during the communication, and a control treatment.

- Judgment Exchange (JE): estimation and confidence questions asked; estimates exchanged.
- Evidence and Judgment Exchange (EJE): all three types of questions asked; evidence and estimates exchanged.
- Evidence Exchange (EE): all three types of questions asked; evidence exchanged.

We have a control treatment to account for the learning effects of repeating the tasks.

- Control (CO): ESQ and CQ asked; no communication.

In addition, we have another treatment (ECO) without communication, similar to the control treatment, but we ask all three types of questions (ESQ, CQ and EVQ). This treatment accounts for the possible effects of asking the EVQ questions.

The most commonly studied communication is the exchange of individual judgment, for instance, individual estimates in estimation tasks (Larrick et al., 2011). Although judgment exchange is the most straightforward way of communication, in every day life people often add supporting arguments or evidence. In certain cases, people only exchange evidence and/or reasoning without providing their definite judgments. Communication does not always lead to convergence of opinions, and in cases when the shared information is open to different interpretations, it can even lead beliefs to diverge due to the confirmation bias (Loewenstein and Moore, 2004). Therefore, in this paper, we consider communications of different information contents: exchanging estimates alone (JE), exchanging evidence alone (EE), and exchanging estimates with evidence (EJE). Apart from the three treatment groups, we have two control treatments, where the first one is to account for the effects of merely repeating the tasks (without communication) on individual and crowd judgment quality, and the second control treatment is to account for the additional impact of writing down evidence (without communication).

Procedure

The experiment was paper and pencil based, run in classrooms. All subjects were randomly assigned subject ID's which they used to identify themselves throughout the experiment. The experimental instructions are in Appendix B.

The sessions of all treatment groups consist of two within-subject stages. In the control treatment, questionnaires 1 consisting of 15 estimation questions and the corresponding confidence questions were handed out and recollected after the subjects finished answering all the questions in stage 1. In stage 2, questionnaires 2 with the identical questions were handed out and recollected.

In the other treatments, an answer sheet was handed out together with questionnaire 1. The content of the answer sheet differs across treatments. On the answer sheet, subjects were asked to write down answers to estimation questions in the

judgment exchange treatment, evidence questions in the evidence exchange treatment, and answers to both estimation and evidence questions in the judgment and evidence exchange.

At the end of stage 1, the experimenter collected questionnaires 1 and the separate answer sheets. The answer sheets were randomly redistributed among the class and it was ensured that one subject did not receive her own answer sheet. Stage 2 was the same with that of the control treatment except that subjects answered the questions again in presence of pieces of information from another subject. A summary of the experimental design is presented in table 1.

The procedure of non-verbal information exchange paradigm that we adopted is similar to that in Soll and Larrick (2009), where communication among subjects is mediated by the experimenters. It ensures more control over the contents of communication, and is in line with the *Delphi method*, introduced to overcome the potential drawbacks of face-to-face communication (Linstone and Turoff, 1975). One example of such drawbacks is the “normative influence” (Deutsch and Gerard, 1955) of social communication, which may have led to excessive conformity to others’ opinion out of social concerns. Unlike the traditional Delphi method, where communication is in free form, we gave specific instructions to the subjects so that they provide and receive supporting evidence in one standard format.

Table 3.1: Summary of the experimental design

Treatment	Questionnaire 1			Exchange		Questionnaire 2	
	ESQ	CQ	EVQ	ESQ	EVQ	ESQ	CQ
EJE (n=49)	✓	✓	✓	✓	✓	✓	✓
EE (n=52)	✓	✓	✓	x	✓	✓	✓
JE (n=51)	✓	✓	x	✓	x	✓	✓
CO (n=47)	✓	✓	x	x	x	✓	✓
ECO (n=53)	✓	✓	✓	x	x	✓	✓

Incentives

Each subject received 10 yuan (about 1.5 USD at the time of the experiment) as show-up fee. In each treatment, 8 subjects were randomly selected to be paid an extra bonus depending on the accuracy of their estimates. For each selected subject,

one number is randomly drawn. The number determines which question on which questionnaire will be used for the subject's bonus payment. Extra bonus was 100 yuan, 50 yuan or 20 yuan if the estimate differ from the true value by no more than 10%¹, more than 10% but no more than 20% or more than 20% but no more than 40%, respectively; otherwise, they receive no extra bonus payment.

3.2 Theory and Measures

3.2.1 Individual-level measure

At the individual level, we are interested in the change in both individual accuracy and confidence after communication of different information content.

Individual Accuracy

Let x_{iq}^s denote the estimate of individual i for estimation question q in stage s ($s = 1, 2$). Let $t_q > 0$ be the true answer to question q . Define (relative) individual error as : $e_{iq}^s = \frac{|x_{iq}^s - t_q|}{t_q}$, so that the errors are comparable across different questions. Define *individual accuracy* as $a_{iq} = 1 - e_{iq}^s$; and *individual accuracy change* as $\Delta a_{iq} = a_{iq}^2 - a_{iq}^1$. A positive change in accuracy ($\Delta a_{iq} > 0$) corresponds to a decrease in errors, and hence improvement in accuracy.

Individual Confidence

Let c_{iq}^s denote the confidence level of individual i for estimation question q in stage s ($s = 1, 2$). Similarly, we define *individual confidence change* as $\Delta c_{iq} = c_{iq}^2 - c_{iq}^1$, where a positive change in confidence corresponds to increase in confidence after judgment revision.

3.2.2 Crowd-level measure

We refer to subjects in the same treatment as one *crowd*. At the crowd level, we analyze the change of the crowd performance in three aspects.

Crowd Accuracy

¹It is 10% of the true value.

Crowd accuracy measures how accurate the crowd would be had it averaged the estimates of its members. Simple average is one of the most popular opinion aggregation rules, and many studies have documented its superiority to individual estimates in various tasks (e.g. Laughlin et al. 2006; Chen 2007; Pentland 2007; Nguyen 2008; Krause et al. 2010; Lee and Chang 2010; Lykourantzou et al. 2010; Beer 2013; Bolger and Rowe 2015).

For crowd c (with n_c individuals) and estimation task q , we define *crowd error* as the distance between the crowd average and the correct answer, adjusted by the correct answer: $ce_q^s = \frac{|\frac{1}{n_c} \sum_i x_{iq}^s - t_q|}{t_q}$. Define *crowd accuracy* as $ca_j = 1 - ce_q$, and the *crowd accuracy change* as $\Delta ca_q = ca_q^2 - ca_q^1$.

Elite Percentage

We define the *Elite Percentage* (ep) as the proportion of high accuracy individuals: $ep_q^s = \sum_{i=1}^{n_c} 1_{(e_{iq}^s < l)} / n_c$, where $1_{(\cdot)}$ is an indicator function, which takes value 1 if the individual error is smaller than a chosen threshold level l , and 0 otherwise. This measure captures the proportion of individuals with highly accurate estimate in the crowd. The *elite percentage change* is $\Delta ep_q^s = ep_q^2 - ep_q^1$.

Crowd Diversity

We define the crowd diversity cd_q^s as the standard deviation of individual answers adjusted by the true answer $ax_{iq}^s = \frac{x_{iq}^s}{t_q}$:

$$cd_q^s = \sqrt{\frac{1}{n_c - 1} \sum_i (ax_{iq}^s - \overline{ax}_q^s)^2},$$

where $\overline{ax}_q^s = \frac{1}{n_c} \sum_{i=1}^{n_c} ax_{iq}^s$. The greater the diversity measure is, the more diversified the crowd's judgments are. The *crowd diversity change* is $\Delta cd_q = cd_q^2 - cd_q^1$.

3.3 Results

3.3.1 Individual-level

Figure 3.1 shows the impact of different contents of communication on the change of individual accuracy and confidence. In the control treatments CO individual ac-

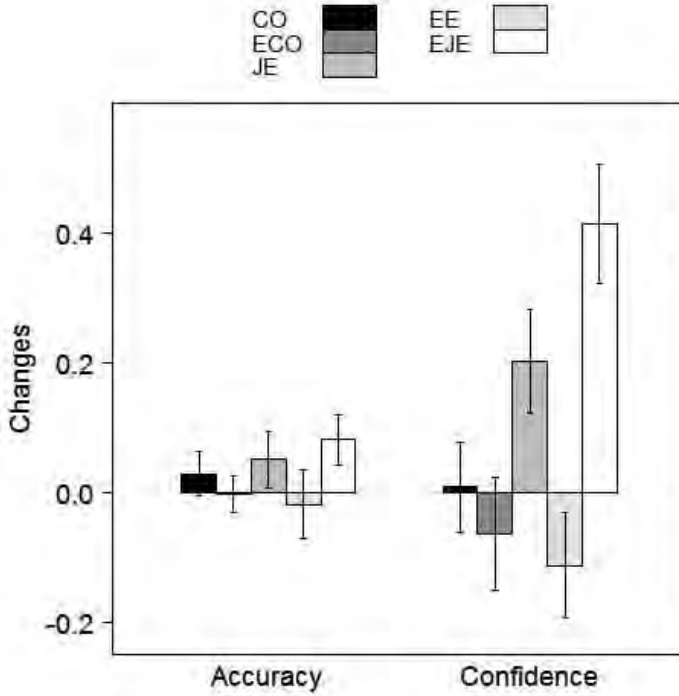


Figure 3.1: Means of changes in individual accuracy and confidence. Error bars indicate the 90% confidence interval. Means are calculated by pooling accuracy (confidence) changes for all subjects in all questions in each treatment.

curacy and confidence stayed unchanged across the two stages. The same holds for the ECO treatment. Individual accuracy improved in EJE when judgment was exchanged with evidence (t-test $p < 0.01$) and marginally in treatment JE when judgment was exchanged (t-test $p = 0.05$). Accuracy did not change in EE when only evidence was exchanged (t-test $p = 0.58$). A similar pattern is observed for individual confidence: confidence increased in treatment JE and EJE (t-test $p < 0.01$), but marginally decreased in EE (t-test $p = 0.07$).

Studies in the literature have found that communications may boost individual confidence even though judgment quality is not much improved (Heath and Gonzalez, 1995; Lorenz et al., 2011). Our finding confirms this, but with more perspectives added. We observe that on the one hand, confidence increased when individual

accuracy increased (in JE and EJE). On the other hand, it decreased, when accuracy did not improve (in EE). In our experiment, subjects did not receive feedback on their accuracy. The same direction in the changes of accuracy and confidence that we found suggested that people have a good intuition about their own performance.

Table 3.2 presents regression results, where individual accuracy changes (Δa_{iq}) and individual confidence changes (Δc_{iq}) are regressed on treatment dummy variables, with the control treatment (CO) as the reference group. Fixed effect for each question was included, and standard errors were clustered at both question and individual level using the two-way clustering strategy of ?.

Results show no significant effect of answering evidence questions (ECO) on people’s judgment quality at the individual level. Further, the exchange of estimate alone (JE) does not improve individual accuracy. This finding does not contradict the findings in the literature of advice taking, which reported positive influence of communication on individual accuracy (Yaniv and Kleinberger, 2000; Bonaccio and Dalal, 2006). Studies in this literature often report the change in accuracy before and after, without comparing to a control group. We also find significant increase in absolute accuracy, if not compared to the control group (see figure 3.1), consistent with the findings in the literature.

On the other hand, when exchange of estimate is coupled with evidence (EJE), such communications improve individual accuracy. These results suggest that subjects’ revised judgments are more accurate when provided access to others’ judgments together with supplementary information that helps them better incorporate the judgments they receive.

The improvement of individual accuracy in the EJE treatment could come from the exchange of evidence alone. Receiving supportive evidence from others without their corresponding judgments or eliciting supportive evidence alone could provide information to the receivers to rethink and improve their own judgments. Our results were against both conjectures. The improvement of accuracy in the ECO and EE treatments was not higher than that in the CO treatment (see table 3.2), suggesting that subjects could not effectively benefit from the evidence elicitation or evidence

Table 3.2: Regression Results: Individual Level

	<i>Dependent variable:</i>	
	Δ Accuracy	Δ Confidence
ECO	-0.032 (0.025)	-0.073 (0.060)
JE	0.022 (0.025)	0.190*** (0.063)
EE	-0.047 (0.030)	-0.120 (0.088)
EJE	0.052** (0.023)	0.400*** (0.067)
Observations	3,660	3,646
R ²	0.003	0.021
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

exchange.

Another possibility is that receiving evidence which reminds subjects of their limited knowledge may lead them to adjust their estimates upward. If they tend to underestimate initially, such upward adjustment will lead to more accurate judgments. Again, we did not find support for this conjecture. The proportion of underestimation did not change in the EE treatment (from 72% to 71%, $p=0.32$ in McNemar test). One possible explanation of people's no response to the evidence information provided by others is that, when faced with the rather ambiguous evidence provided by others, subjects interpreted it as evidence supporting their own judgments (Lord et al., 1979; Plous, 1991), leading to little adjustment of their own initial judgments.

After ruling out the explanation of the pure effects from eliciting evidence and exchanging evidence, we attribute the improvement of individual accuracy in the EJE treatment to the combination of estimate and evidence exchange. We conjecture that, when evidence was coupled with others' estimate, people can better evaluate the quality of others' judgments, leading to better judgment revisions. "Egocentric discounting", a systematic bias of giving inadequate weights to others' judgments (Yaniv and Kleinberger, 2000), has often been documented in the literature. Following the definition in the literature, for individual i who receives the estimate from individual i' ($i' \neq i$), the weight individual i puts on her own estimate is

$WOE = \frac{|x_{i'q}^1 - x_{iq}^2|}{|x_{i'q}^1 - x_{iq}^1|}$. Test results show that WOE is lower in the EJE treatment than in the JE treatment (one-sided Wilcoxon rank sum test, $p=0.04$). This suggests that subjects in the EJE treatment assigned more weight to others' judgment, which may have led to the improvement in their individual accuracy.

3.3.2 Crowd-level

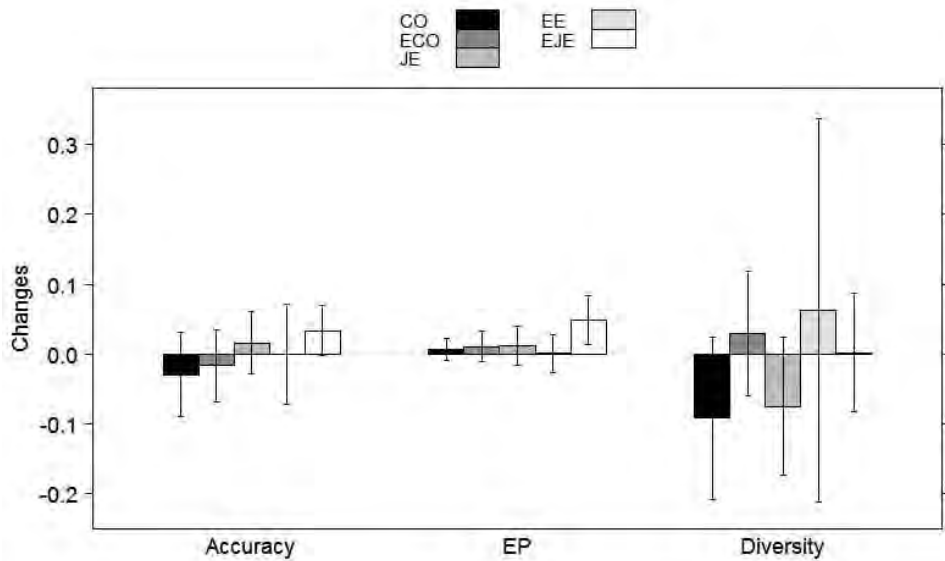


Figure 3.2: Means of change in crowd accuracy, elite percentage and diversity. Error bars indicate the 90% confidence interval. Means of changes in each measure are calculated for all questions in each treatment.

Figure 3.2 shows the impact of communication of different information contents on the change in crowd accuracy, elite percentage (the threshold being 10%), and crowd diversity for all treatments. Elite Percentage increased when both judgment and evidence were exchanged.

Table 3.3 presents the regression results on the treatment effects on the crowd level measures, where question fixed effects are included. When judgment and evidence were both communicated (EJE), crowd accuracy improved without hurting crowd diversity. Lorenz et al. (2011) termed the decrease in diversity without improvement

Table 3.3: Regression Results: Crowd Level

	<i>Dependent variable:</i>		
	Δ Accuracy	Δ EP	Δ Diversity
ECO	0.013 (0.033)	0.004 (0.020)	0.120 (0.110)
JE	0.046 (0.033)	0.005 (0.020)	0.017 (0.110)
EE	0.029 (0.033)	-0.006 (0.020)	0.150 (0.110)
EJE	0.063* (0.033)	0.042** (0.020)	0.094 (0.110)
Observations	75	75	75
R ²	0.075	0.110	0.046

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

in accuracy the “social influence effect”, where “[groups] engage in a convergence process that does not yield improvements of the collective.” Our findings on the effect of judgment and evidence exchange (EJE) suggest that this form of communication is not susceptible to the social influence effect. In everyday communication, judgments are exchanged often with supporting evidence and arguments. Our findings show that “social influence” would not always lead to the undesirable convergence to a wrong judgment as suggested by Lorenz et al. (2011).

Further, apart from the increase in crowd accuracy, there were increases in the elite percentage² in the EJE treatment. The findings on the elite percentage gave us more information on the quality of the judgments in the crowd. With more individuals with high accuracy, the group judgment aggregation is more likely to go in the right direction.

3.3.3 Summary and Discussion

We found that communication with both judgment and evidence has a positive impact on the quality of judgment at both the individual and crowd levels. Our findings suggest that, when consulting the opinion of a committee, exchanging judgments

²This finding is robust to different thresholds: $l = 0.2$ and $l = 0.4$.

among its members coupled with their most valuable information (valuable in the sense of being least likely to be shared by other members) can help the group form a better judgment. After sharing judgments and (potentially) idiosyncratic information with each other, the group may still preserve its diversity in opinions, but at the same time arrive at a better average judgment and have more individuals with high accuracy. With these aspects improved, the group is more likely to arrive at a more accurate judgment, and hence at a socially and economically more efficient decision.

Our findings also suggest that communication with only judgment or with only evidence is not ideal. Neither of these communication forms provide information that others can utilize in forming a better judgment. The latter, communication with evidence alone, even undermined individual accuracy. The former has a psychological side effect of boosting individual confidence not matched by the improvement in the judgment qualities.

The out-performance of communication of judgment with supporting evidence over the other two forms of communication may stem from the observation that people can better appreciate (assigned more weight to) others' judgments when they were accompanied by supporting evidence. The accompanying evidence can serve as an extra cue on the accuracy of others' judgments, helping people make better use of others' judgments in the revision process than when such evidence is absent. In certain cases, evidence may also explain the differences between their own and others' judgments. However, when evidence was provided without others' judgment, people may not be able to interpret the evidence in the right direction. As suggested by Lord et al. (1979) and Plous (1991), people may interpret the same ambiguous information as evidence supporting their own judgments, going even further in their already wrong direction.

3.4 Conclusion

This paper made a first attempt to better simulate real life communication by incorporating not only judgment but also supporting evidence as the information

content of communication in a controlled manner. Using proper controls, we find that exactly this extra piece of information improves judgment quality.

Our findings have implications on communication of judgments and opinions in general. If you have an opinion, then support it with your arguments. It helps others to better appreciate it.

Chapter 4

Comparing risk aversion when beliefs differ

4.1 Introduction

Consider two decision makers, person A and person B . Imagine that we want to compare the degree of risk aversion between these two individuals. Under risk (known probabilities) we can say that person A is *more risk averse* than person B if person A rejects all bets that person B rejects.¹ Under expected utility (EU) this is equivalent to saying that person A has more concave utility than person B . Under uncertainty, however, things are less straightforward. Yaari (1969) showed that under subjective expected utility (SEU), A rejecting all bets that person B rejects is equivalent to A having more concave utility than B *and* them sharing the same beliefs. A large body of work has used this approach to define comparative risk aversion, or has expanded

This chapter is based on the paper “Comparing uncertainty aversion towards different sources”, co-authored with Aurélien Baillon and Dennie van Dolder.

¹As noted by Yaari (1969), it would be both more accurate and more cumbersome to say that person A is at least as risk averse as person B . For sake of brevity, we will follow Yaari and simply say that person A is more risk averse than person B .

on by constructing comparative definitions of uncertainty/ambiguity and loss aversion (Kihlstrom and Mirman, 1974; Roth, 1985; Epstein, 1999; Nau, 2003; Köbberling and Wakker, 2005; Nau, 2006; Olszewski, 2007; Blavatskyy, 2011; Jewitt and Mukerji, 2011; Bommier et al., 2012; Chambers and Echenique, 2012). The restriction that the two individuals share the same beliefs is, however, highly restrictive as individuals will often hold different beliefs when assessing the likelihood of particular events.

In this chapter, we propose definitions of comparative risk aversion for people who hold different beliefs. Without placing any constraints on beliefs, all our definitions are necessary for A to have more concave utility than B under subjective expected utility. Furthermore, they are sufficient for A having more concave utility than B under additional, relatively weak, belief assumptions.

Next to comparing risk aversion between individuals holding different beliefs, our definitions also give more leeway to study how an individual's risk preferences vary across conditions. As noted by Yaari (1969), person A and person B can refer to the same person at, for instance, different wealth levels or different information levels. However, with Yaari's definition such a comparison required that the conditions under consideration did not affect the individual's beliefs regarding the likelihood of events. This is problematic when comparing the same person at different information levels as it would contradict Bayesian updating. By letting go of this *same belief requirement*, our definitions allow for the comparison of an individual's risk preferences across conditions that do affect beliefs.

Besides relaxing the same belief requirement, most of our definitions do not even require that person A and B face the same event space. This property creates further opportunities to compare risk aversion both between and within individuals. Between individuals, it allows us to compare risk aversion of individuals operating on different markets. For example, not only can we tell whether A or B is more risk averse when trading on the Dow Jones Index, we can also compare the risk aversion of person A who is trading on the Dow Jones to that of person B who is trading on the Nikkei. Within an individual, our definitions can be used to study an individual's source preferences. A large body of work suggests that people prefer to bet on some

sources of uncertainties over others (Ellsberg, 1961; Heath and Tversky, 1991; Keppe and Weber, 1995; Tversky and Fox, 1995; Resende and Wu, 2010; Abdellaoui et al., 2011). Models capturing source preference through source-dependent utility have been proposed by Kreps and Porteus (1978) when sources are different time points and by Klibanoff et al. (2005), Nau (2006), and Neilson (2010) to study ambiguity attitudes. Chew et al. (2008) investigate source-dependent utility in a neuroimaging experiment.² Our conditions can be used to compare the concavity of the utility of a person for different sources of uncertainty, without assuming that probabilities are known (unlike Kreps and Porteus, 1978) or that subjective probabilities are observable (unlike Klibanoff et al., 2005).

Yaari's condition can be extended to Hansen and Sargent (2001) *multiplier preferences* model in order to compare policy makers' confidence in the model they employ to define their policies (or alternatively: their concern for model misspecification). With Yaari's original condition, this was only possible if the policy makers had the same baseline model (Baillon et al., 2014). Our definitions allow for comparisons between agents with different baseline models, possibly facing different situations.

The rest of this chapter is organized as follows. The next section introduces the theoretical framework and Yaari's original results. Section 4.3 presents our main definitions for binary acts. Section 4.4 extends these definitions to general acts and Section 4.5 shows how our definitions can be used to compare risk attitudes across sources of uncertainty and Section 4.6 describes how they can be employed to compare policy makers' concern for model specification under multiplier preferences. Section 4.7 concludes the chapter. The proofs of the propositions in this chapter can be found in Appendix III.

4.2 Background

Let S be a finite or infinite *state space*, containing all *states of nature* s . The agents do not know which state is true. An event E is a subset of S . Let the set of

²See Baillon et al. (2012) for a discussion of the descriptive appropriateness of utility to capture source preference.

events considered by the agent be a sigma-algebra denoted Σ . The complementary event of E is denoted E^c and $\{E_i\}_n$ denotes a partition of S in n events. The *outcome set* is X , an open interval of the reals. The agents can choose between *acts*, which are finite Σ -measurable mappings from S to X . Acts are typically denoted f or g and the set of all acts is \mathcal{F} . We write $f = (E_1 : x_1, \dots, E_n : x_n)$ to indicate that f assigns outcome x_i to event E_i for all $1 \leq i \leq n$. The *bet* x_{Ey} is a binary act yielding outcome x if event E occurs and y otherwise. When $x > y$, we call x_{Ey} a *bet on* E and y_{Ex} a *bet against* E . Acts that yield the same outcome z for all $s \in S$ are referred to as z .

Agent $i \in \{A, B\}$ has preferences \succsim_i over \mathcal{F} , with \sim_i, \succ_i, \prec_i , and \precsim_i defined as usual. We will say that \succsim_i satisfies *subjective expected utility* (SEU) if there exist a countably additive subjective probability measure P_i and a continuous and strictly increasing utility function u_i uniquely defined up to an affine transformation such that $f \succsim_i g \Leftrightarrow \int_S P_i(s) u_i(f(s)) ds \geq \int_S P_i(s) u_i(g(s)) ds$. We will say that u_A is *more concave than* u_B if there exists a concave function φ such that $u_A = \varphi \circ u_B$. Finally, we will say that P_i is *nonatomic* if for all $E \in \Sigma$ such that $P_i(E) > 0$, there exists $F \in \Sigma$ such that $F \subset E$ and $0 < P_i(F) < P_i(E)$. Nonatomicity is guaranteed, for instance, by Savage's (1954) axiomatization of SEU.

For Yaari (1969), A is more risk averse than B if, starting from the same initial situation, the acceptance set of A is a subset of the acceptance set of B . Figure 4.1 illustrates this. Axes represents the outcomes that events E and E^c yield. Indifference curves of A and B that cross the diagonal at z are the acceptance frontiers of both agents at situation z . Among all bets on and against E , A 's acceptance set at z (striped area) is a subset of B 's (shaded area). Any bet that B will not accept will also not be accepted by A . Formally, $z \succsim_B x_{Ey} \Rightarrow \text{not}[z \prec_A x_{Ey}]$.

Under subjective expected utility, the slope of the tangent of an indifference curve where it crosses the diagonal is equal to minus the odds for E , i.e. $-\frac{P_i(E)}{1-P_i(E)}$. If agents A and B have different beliefs, their indifference curves will have different tangents at z and will therefore cross each other (assuming differentiability). Hence, there will exist a bet that A accepts and B does not and another bet that B accepts and A

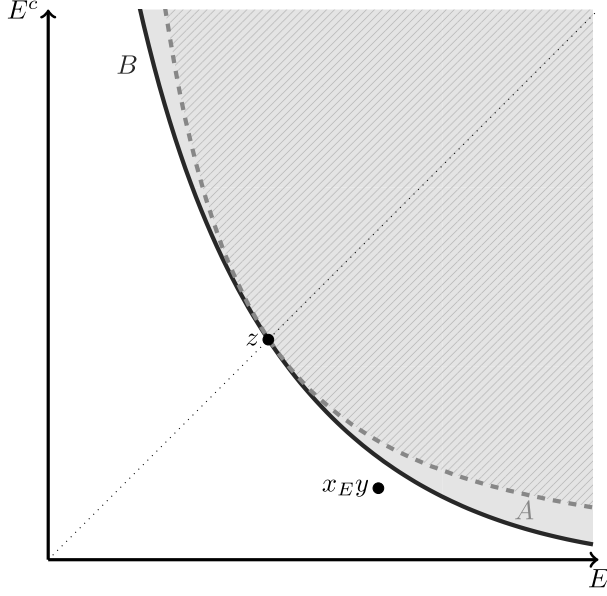


Figure 4.1: Yaari's (1969) condition

does not. We could not conclude who, from A and B , is more risk averse. Yaari's condition is not necessary for the utility of A being more concave than that of B , unless $P_A = P_B$. Moreover, it implies both u_A being more concave than u_B and the agents sharing the same beliefs.

Proposition (Yaari, 1969). *Assume \succsim_A and \succsim_B satisfy subjective expected utility with u_A and u_B being differentiable on an interval $I \subset X$. The following two statements are equivalent:*

(i) $\forall E \in \Sigma$, and x, y , and z in X , $z \succsim_B x_E y \Rightarrow \text{not}[z \prec_A x_E y]$.

(ii) u_A is more concave than u_B **and** $P_A = P_B$.

A consequence of this approach is that it is common to think that risk aversion cannot be compared for agents with different beliefs. Below we propose an alternative approach, only slightly departing from Yaari's. Our definition of risk aversion will not have any implications regarding beliefs and will be necessary for A 's utility being more concave than B 's. We will build it stepwise, starting from the simplest case.

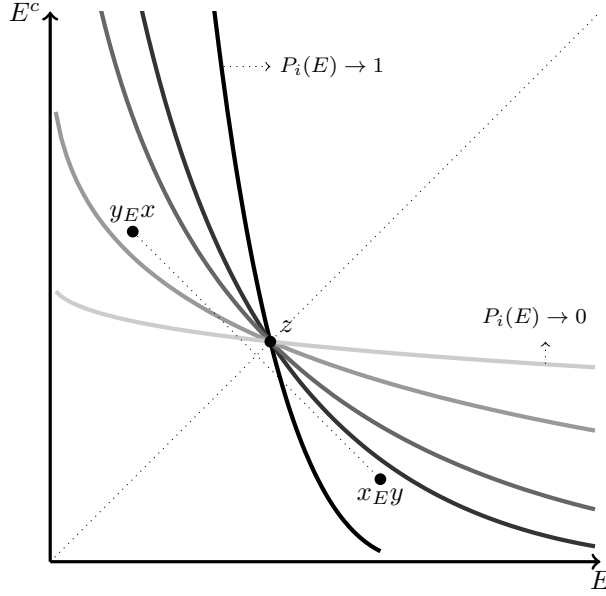


Figure 4.2: Effect of beliefs

4.3 Relaxing the "same belief" requirement for binary acts

Figure 4.2 shows how the acceptance frontier at z is affected by changes in beliefs. If agent i is certain that E will occur ($P_i(E) = 1$), she will be indifferent to any change of outcomes on E^c and her acceptance frontier will be vertical. It will be horizontal in the opposite case ($P_i(E) = 0$). A switch to a higher $P_i(E)$ will make a bet on E (e.g., $x_E y$) more attractive, but will make the symmetric bet against E ($y_E x$) less attractive. We propose to use this effect of changes in beliefs to adapt Yaari's definition of "more risk averse".

We say that A is more risk averse than B if there are no pairs of symmetric bets on and against E that B would not accept while A would. In terms of acceptance set, it means that if A 's acceptance set exceeds B 's below the diagonal, it should not exceed it in the symmetric region above the diagonal. Figure 4.3.a illustrates such a case, while Figure 4.3.b displays a counter-example (in both figures, agents A

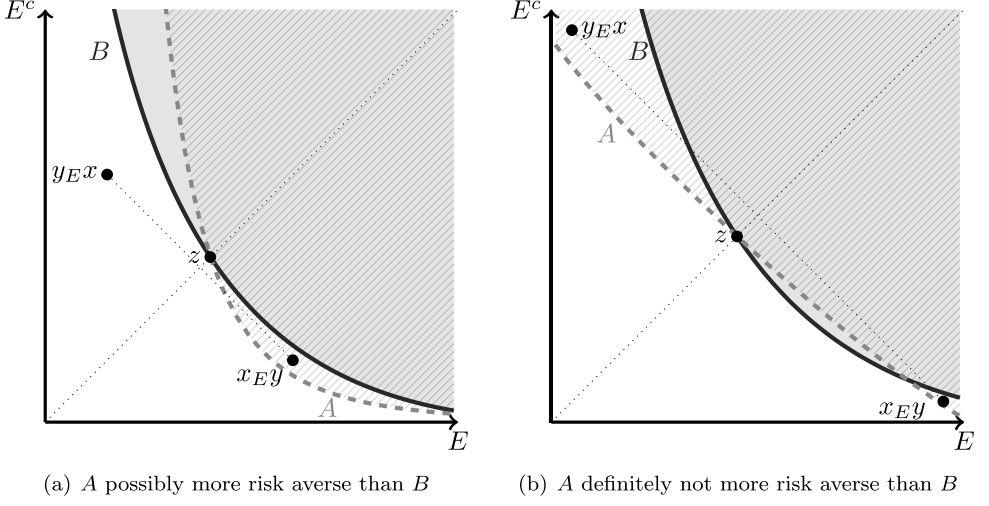


Figure 4.3: Comparing risk attitudes of two agents holding different beliefs

and B hold different beliefs). In Figure 4.3.b, there is a pair of bets that A would accept while B would not. This is evidence that A is not more risk averse than B . Our definition excludes such cases. It is obviously a weaker requirement than Yaari's condition. On Figure 4.3, the segment from $x_E y$ to $y_E x$ serves as a gauge for the acceptance set: if its two end points do not fit in B 's acceptance set, they should not fit in A 's. If A has a convex acceptance set, thus being risk averse under subjective expected utility, this is equivalent to saying that the entire gauge line should not fit in A 's acceptance set. Our definitions allow for risk seeking preferences.

Proposition 1. Assume \succsim_A and \succsim_B satisfy subjective expected utility:

(i) $\forall E \in \Sigma$, and x, y , and z in X ,

$$[z \succsim_B x_E y \quad \& \quad z \succsim_B y_E x] \Rightarrow \text{not}[z \prec_A x_E y \quad \& \quad z \prec_A y_E x].$$

(ii) u_A is more concave than u_B .

(i) is necessary for (ii). It is also sufficient if there exists $F \in \Sigma$ such that $P_A(F) = P_B(F) = \frac{1}{2}$.

As shown by proposition 1, our new definition of “more risk averse” does not have any implication on beliefs. Yet, we do need an event that both A and B believe to

have probability of $\frac{1}{2}$. In other words: we need them to agree on a “fair coin”. Note that we do not require them to agree on *all* events with probability $\frac{1}{2}$. If P_A and P_B are nonatomic, requiring them to agree on all events with probability $\frac{1}{2}$ would be equivalent to requiring that P_A and P_B are identical, as was shown by Marinacci (2002, Theorem 2). The richness requirement of proposition 1 may still be deemed too demanding and we will therefore propose a way to relax it.

Assume that on Figures 4.1 and 4.3, the axes represent the outcomes obtained on event F and F^c for A and on E and E^c for B (for some arbitrary E and F). If $P_A(F) = P_B(E)$, our definition would draw the same conclusion as comparing which acceptance set, between A 's and B 's, is a subset of the other one. If $P_A(F) \neq P_B(E)$, the acceptance frontiers would cross each other but our definition could still work. It would say that if there are some outcomes x and y such that B would accept neither $x_E y$ nor $y_E x$, then A should not find both $x_F y$ and $y_F x$ acceptable. If the two end points of the gauge from $x_E y$ to $y_E x$ do not fit in B 's acceptance set, the end points of the equivalent gauge from $x_F y$ to $y_F x$ (the scale on the axes being the same) should not fit in A 's. These gauge lines allow us to compare acceptance sets for bets on different events.

Proposition 2. *Assume \succsim_A and \succsim_B satisfy subjective expected utility:*

$$(i) \ \forall E, F \in \Sigma, \text{ and } x, y, \text{ and } z \text{ in } X, \\ [z \succsim_B x_E y \ \& \ z \succsim_B y_E x] \Rightarrow \text{not}[z \prec_A x_F y \ \& \ z \prec_A y_F x] .$$

$$(ii) \ u_A \text{ is more concave than } u_B .$$

(i) is necessary for (ii). It is also sufficient if there exist $E, F \in \Sigma$ such that $P_A(F) = P_B(E) = \frac{1}{2}$.

Statement (i) is necessary and sufficient to compare the utility functions of A and B if both agents assign probability $\frac{1}{2}$ to some, possibly different, events. No agreement is required anymore. The existence of such events only requires the state space to be rich enough. This richness condition is automatically satisfied if P_A and P_B are nonatomic, as follows from Savage's (1954) axiomatization. In the following,

we will further weaken the richness requirement on the state space by considering general acts instead of bets.

Observation 1. *If, in Proposition 2, P_A and P_B are nonatomic, then (i) is equivalent to (ii).*

4.4 Extension 1: Comparing risk aversion with general acts

To adapt the notion of more risk averse to general acts, we can consider what happens to two events only for each agent and keep everything else (what happens on the other events) constant. For acts with three outcomes, the acceptance sets can be represented on a 3D graph. We can keep the outcome on one state constant (we "slice" the acceptance set) to obtain 2D acceptance sets and use similar gauges as above (see Figure 4.4.a). Proposition 3 applies this approach. We denote $x_E y_F f$ the act yielding x on E , y on F , and $f(s)$ for all $s \notin E \cup F$.

Proposition 3. *Assume \succsim_A and \succsim_B satisfy subjective expected utility:*

(i) $\forall E, F, G, H \in \Sigma, f, g \in \mathcal{F}, \text{ and } x, y, \text{ and } z \text{ in } X,$

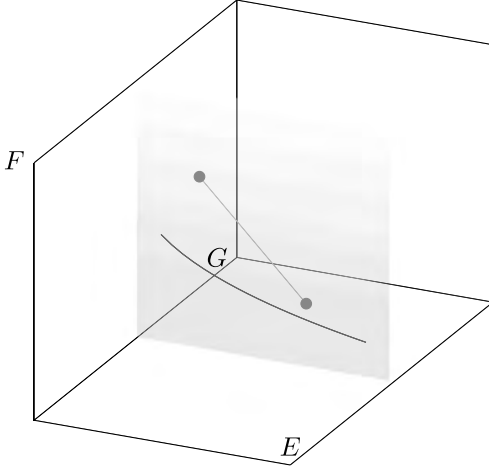
$$[z_{E \cup F} f \succsim_B x_E y_F f \quad \& \quad z_{E \cup F} f \succsim_B y_E x_F f] \Rightarrow \text{not}[z_{G \cup H} g \prec_A x_G y_H g \quad \& \quad z_{G \cup H} g \prec_A y_G x_H g].$$

(ii) u_A is more concave than u_B .

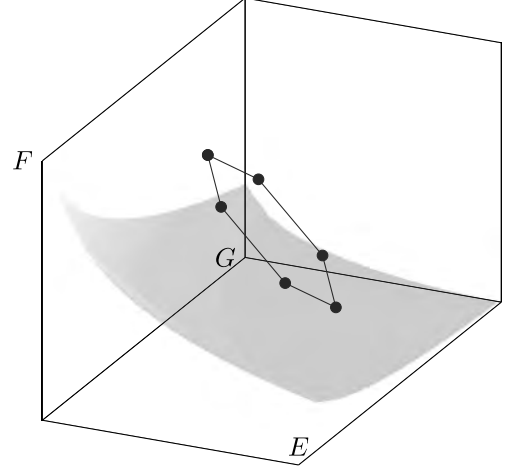
(i) is necessary for (ii). It is also sufficient if there exist $E, F, G, H \in \Sigma$ such that $P_B(E) = P_B(F)$ and $P_A(G) = P_A(H)$.

As can be seen, we have weakened the richness requirement on the state space: it is no longer necessary that each agent finds two complementary events that are equally likely; it only requires two equally likely events for each agent.

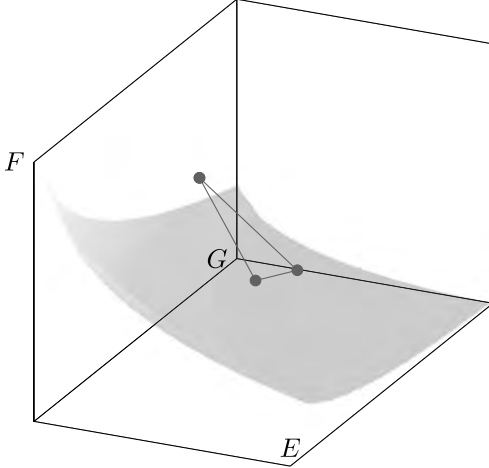
In all statements (i) above, we permuted two outcomes to build gauges. We can also permute all outcomes of the act. If the acts have three outcomes, we obtain six permutations and a *gauge hexagon* (see Figure 4.4.b). Agent A is then more



(a) Slicing and gauge line



(b) Gauge hexagon



(c) Gauge triangle

Figure 4.4: Possible gauges for acceptance sets over three-outcome acts

risk averse than B if all gauge hexagons whose corners are not in B 's acceptance set do not have all corners in A 's either. Consider $f = (E_1 : x_1, \dots, E_n : x_n)$ and $g = (F_1 : x_1, \dots, F_n : x_n)$ (i.e., f and g yield the same n outcomes but need not assign these outcomes to the same events). Let $\Pi(x_1, \dots, x_n)$ be a permutation of (x_1, \dots, x_n) . For simplicity, we use the $\Pi(f)$ to denote the act assigning $\Pi(x_1, \dots, x_n)$ to (E_1, \dots, E_n) and $\Pi(g)$ to denote the same permutation of the outcome of g .

Proposition 4. Assume \succsim_A and \succsim_B satisfy subjective expected utility:

$$(i) \forall f, g \in \mathcal{F} \text{ yielding the same outcomes } (x_1, \dots, x_n) \text{ and for all } z \in X, \\ [z \succsim_B \Pi(f) \quad \forall \Pi] \Rightarrow \text{not } [z \prec_A \Pi(g) \quad \forall \Pi].$$

(ii) u_A is more concave than u_B .

(i) is necessary for (ii). It is also sufficient if there exist, for some integer n , two n -fold partitions $\{E_i\}_n$ and $\{F_i\}_n$ of S such that $P_A(F_i) = P_B(E_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$.

We can also decide not to consider all permutations, but only those which "maximally differ" from each other. For three-outcome acts, we can consider only three corners of the hexagon which differ in all coordinates. We obtain such a triangle (see Figure 4.4.c). We can obtain this triangle by using one type of permutation that we will call cyclic. Consider the act $f = (E_1 : x_1, E_2 : x_2, \dots, E_{n-1} : x_{n-1}, E_n : x_n)$. Let π be a *cyclic permutation function*, defined as $\pi(f) = (E_1 : x_2, E_2 : x_3, \dots, E_{n-1} : x_n, E_n : x_1)$. It moves each outcome one event to the left. We denote π^m the compound function that applies π m times. For instance, $\pi^3 = \pi \circ \pi \circ \pi$.

Proposition 5. Assume \succsim_A and \succsim_B satisfy subjective expected utility:

$$(i) \forall f, g \in \mathcal{F} \text{ yielding the same outcomes } (x_1, \dots, x_n) \text{ and for all } z \in X, \\ [z \succsim_B \pi^m(f) \quad \forall m \in \{1, \dots, n\}] \Rightarrow \text{not } [z \prec_A \pi^m(f) \quad \forall m \in \{1, \dots, n\}].$$

(ii) u_A is more concave than u_B .

(i) is necessary for (ii). It is also sufficient if there exist, for some integer n , two n -fold partitions $\{E_i\}_n$ and $\{F_i\}_n$ of S such that $P_A(F_i) = P_B(E_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$.

In Propositions 4 and 5, the additional requirement for (i) to be sufficient for (ii) is stronger than that of Proposition 3. The approach of Proposition 3 is therefore the least demanding in terms of richness of the state space(s) and beliefs of the decision makers. For all three propositions, the requirements are trivially satisfied by nonatomic probability measures.

Observation 2. *If, in Proposition 3, 4 and 5, P_A and P_B are nonatomic, then (i) is equivalent to (ii).*

4.5 Extension 2: Comparing risk aversion across sources of uncertainty

In his seminal work Ellsberg (1961) provides convincing examples suggesting that individuals will prefer to bet on risk over uncertainty. In his simplest example, people prefer to bet on an urn with 50 red and 50 black balls over an urn containing a 100 red or black balls in unknown proportions, irrespective of the winning color. While Ellsberg presented this as a thought experiment, subsequent work has convincingly shown his intuition to be correct (Camerer and Weber, 1992). Further work suggests that people prefer to bet on sources of uncertainty for which they feel competent, and can prefer uncertainty over risk in such cases (Heath and Tversky, 1991; Keppe and Weber, 1995; Tversky and Fox, 1995; Resende and Wu, 2010). Such tendencies are not without consequence in the real world. In finance, it is a well-known pattern that people tend to invest and trade more in their own country than we would expect given the gains to be had from diversification (French and Poterba, 1991; Obstfeld et al., 2001). This *home bias* can be explained by a preference to take risk on more familiar sources of uncertainty (Kasa, 2000; Kilka and Weber, 2000; Uppal and Wang, 2003; Huang, 2008). Sources of uncertainty have been used in theoretical studies of ambiguity, in which decision makers face two stages of uncertainty (Nau, 2006; Ergin and Gul, 2009; Strzalecki, 2011), one of these stages possibly being risky (Klibanoff et al., 2005).

From Proposition 2 on, we allowed agent A and agent B to bet on different events from the same state space S . We can also assume they are facing different sources of uncertainty S_A and S_B , with their respective Σ_A , Σ_B , \mathcal{F}_A , and \mathcal{F}_B . Below we adapt our second proposition, apart from Proposition 1, all other propositions can be adapted in a similar fashion.

Proposition 6. *Assume \succsim_A and \succsim_B satisfy subjective expected utility:*

(i) $\forall F \in \Sigma_A, E \in \Sigma_B$, and x, y , and z in X ,

$$[z \succsim_B x_E y \quad \& \quad z \succsim_B y_E x] \Rightarrow \text{not}[z \prec_A x_F y \quad \& \quad z \prec_A y_F x] .$$

(ii) u_A is more concave than u_B .

(i) is necessary for (ii). It is also sufficient if there exist $F \in \Sigma_A$ and $E \in \Sigma_B$ such that $P_A(F) = P_B(E) = \frac{1}{2}$.

This allows us to compare risk aversion between agents facing different sources of uncertainty, for example operating on different markets. Furthermore, A and B could refer to the same agent facing different sources of uncertainty. For each agent, we can then characterize her willingness to take risk across different sources of uncertainty. We could rank these sources in terms of the agent's willingness to take risk and compare the decision maker's willingness to take risk in each source to the case of known probabilities to classify a decision maker as ambiguity seeking, neutral or averse towards a given source.

4.6 Extension 3: Comparing robustness of policies under model uncertainty

When formulating policies to achieve specific aims, policy makers need to have some kind of model in mind (either explicitly or implicitly) that defines the likelihood of particular outcomes to result from different policies. Policy makers will generally face considerable uncertainty regarding the correct model.

In the following, we consider two policy makers maximizing the same objective function W (defined over X) representing, for instance, well-being. Hansen and Sargent (2001) proposed *multiplier preferences* to model a policy maker who wants to make decisions robust to possible misspecification of the model (s)he uses to define policies. The policy maker's follows the decision rule $V_i(f) = \min_{p \in \Delta(S)} \int_S W(f(s)) dp(s) + \theta_i R(P||Q_i)$ where $\Delta(S)$ is the set of all countably additive probability measures on (S, Σ) , the function $R(P||Q)$ is the relative entropy of P with respect to Q , Q_i is the policy maker's baseline model, and $\theta_i \geq 0$ represents the degree of confidence

the policy maker has in the baseline model (or alternatively: the concern she has for model misspecification). If θ_i is small, the agent will give considerable weight to alternative models. If θ_i tends to infinity, the agent fully accepts the baseline model.

Yaari's condition permits a comparison of θ_i , but only if both policy makers have the same baseline model (Baillon et al., 2014). Our conditions extend the comparison to agents with different baseline models. Proposition 7 shows how we can adapt Proposition 2 to multiplier preferences.

Proposition 7. *Assume \succsim_A and \succsim_B are multiplier preferences:*

(i) $\forall E, F \in \Sigma$, and x, y , and z in X ,

$$[z \succsim_B x_E y \quad \& \quad z \succsim_B y_E x] \Rightarrow \text{not}[z \prec_A x_F y \quad \& \quad z \prec_A y_F x] .$$

(ii) $\theta_A \leq \theta_B$.

(i) is necessary for (ii). It is also sufficient if there exists $E, F \in \Sigma$ such that $Q_A(F) = Q_B(E) = \frac{1}{2}$.

All other propositions discussed above can also be directly applied to multiplier preferences by replacing their respective condition (ii) by that of Proposition 7. As extension 3 has shown, we can also have events from different sources of uncertainty and therefore compare the decisions of a policy maker in one situation with those (s)he made in another situation with different model uncertainty.

4.7 Conclusion and Discussion

There are many instances when we want to compare risk aversion between decision makers. When probabilities are known, this is a straightforward exercise as person A being more risk averse than person B can be defined as person A rejecting all gambles that person B rejects. Under expected utility, this choice pattern implies that person A has more concave utility than person B . When probabilities are unknown, the rejection of a bet is no longer determined by risk aversion alone, but also by subjective beliefs regarding the likelihood of uncertain events. Under subjective expected utility, Yaari (1969) showed that person A rejecting all gambles that person

B rejects is equivalent to A having more concave utility than B and A and B sharing the same beliefs. This latter assumption narrows down the domain in which risk aversion can be compared as beliefs about the likelihood of uncertain events may very well differ across individuals, and even for the same individual across different conditions.

In the this chapter, we proposed a number of definitions that can be used to compare risk aversion of person A and B without assuming that they hold the same beliefs. Without any constraints on belief, all our definitions imply that A has more concave utility than B under subjective expected utility. Additional, relatively weak, belief assumptions make our definitions sufficient for A having more concave utility than B .

Being able to compare risk aversion between decision makers for cases when probabilities are unknown is important as almost all real world scenarios fall in this category. An individual's risk preferences under known, exogenously given probabilities are likely to be a poor proxy for her risk preferences towards other sources of uncertainty, as an individual's willingness to take risk tends to differ across different sources of uncertainty (Ellsberg, 1961; Heath and Tversky, 1991; Keppe and Weber, 1995; Tversky and Fox, 1995; Resende and Wu, 2010; Abdellaoui et al., 2011). If person A is more risk averse than person B when facing gambles with known probabilities, this does not automatically imply that person A will be more risk averse than person B when investing on the Dow Jones. Our definitions allow us to compare risk aversion towards specific sources of uncertainty between decision makers without imposing that they hold the same beliefs.

Most of our definitions do not require that person A and B face the same event space. This allows us to compare risk aversion between decision makers facing different sources of uncertainty. For example, we can compare the risk aversion of person A and person B trading on different financial markets. Not only does this relaxation make our definitions applicable to a broader domain, but also allows for more informative comparisons. Consider comparing risk preferences regarding the stock market for investors from different countries. One option would be to compare their

risk preferences on a single market, such as the Dow Jones. However, such a market will potentially have a substantial different meaning for investors from different countries. As a result, it will arguably be more informative to compare the investors' willingness to take risk on markets that carry similar meaning to the investors, such as their respective home markets.

In addition to opening up opportunities to compare risk aversion between individuals holding different beliefs, our definitions are suitable for studying how an individual's risk preference varies across conditions that affect beliefs. One example is information level. If people adopt Bayesian updating, the same individual would hold different beliefs at different levels of information. Our definition allows for comparisons of one's risk aversion across conditions that influence beliefs. Furthermore, the fact that person A and person B do not have to face the same event space opens up the possibility to map an individual's source preferences. In particular, our method allows for the ranking of sources of uncertainty on the basis of the decision maker's willingness to take risk under each source. Our conditions allow for comparing utility between sources of uncertainty and thereby complement the definitions of Tversky and Wakker (1995), which allowed for comparing decision weights between sources of uncertainty in non-expected utility models.

Finally, Yaari's condition has previously been extended to compare policy makers' confidence in the model they use to define policies. Due to the restrictions on beliefs, however, this was only possible between policy makers that share the same baseline model (Baillon et al., 2014). Our conditions allow to draw this comparison between agents who have different baseline models, possibly facing different situations altogether.

Chapter 5

Individual risk attitudes and the effects of payment instruments

5.1 Introduction

Nowadays, consumers are indulged with many payment instruments: cash, checks, debit cards, credit cards, online/mobile banking, Apple Pay, etc. Cash is no longer the most common payment instrument in Europe or in the U.S. (Bagnall et al., 2014). Non-cash payment instruments enable consumers to make payments without exchange of cash notes, which substantially simplify the payment process. Non-cash payment instruments obviously change how we pay. What is not as obvious but no less important is, they also change how much we pay.

When payments are made in cash, consumers tend to spend less (Hirschman, 1979; Feinberg, 1986). Even for hypothetical questions, consumers cued with credit

This chapter is based on the homonymous paper, co-authored with Yu Gao.

card logo in sight are willing to spend more and have shorter decision time (Feinberg, 1986; Raghurir and Srivastava, 2008). The substantial gap between cash and non-cash payment cannot be fully explained by the convenience and potential money saving of credit-card usage. A behavioral explanation explains the gap because the thoughts of payment can undermine the pleasures of consumption, and the psychological distance created by non-cash payment could alleviate the pain of paying (Prelec and Loewenstein, 1998).

To test the effects in incentivized transactions of high value, Prelec and Simester (2001) conducted experiments comparing consumers' willingness-to-pay for tickets to sporting events with different payment instruments. Consistent with the literature, consumers were willing to pay substantially more with their credit card. The large credit card premium (up to 100%) could not plausibly be explained by liquidity constraints. However, the effect seemed to depend on the characteristics of the products under valuation. In their second study, subjects were asked to value products of either certain (a restaurant gift certificate) or unknown market value (tickets to a sold-out sporting event, for which the value was unstated). The gap between cash and non-cash instruments only existed for the products with unknown market value, but not for those with certain market value.

If the effects of payment instruments are moderated by the feeling of uncertainty, the question arises as to the channel through which the payment instruments work. Prelec and Simester (2001) did not provide a theory explaining the presence of the observed effect. In this chapter, we propose that payment instruments change valuations of lotteries by shaping consumers' risk attitudes. In particular, consumers' probability weighting might be affected by payment instruments in two ways. On the one hand, payment instruments could affect consumers' allocation of their attention. As Kahneman (2011) put it, "our mind has a useful capability to focus on whatever is odd, different or unusual". The attention paid to the colorful cash occupies cognitive resources. Different notes and coins make it one step harder to calculate EV of the lottery. The depletion of cognitive resources reduce people's reliance on the analytic, calculating, and deliberative part, the so called "System 2", and rely more

on the instinctive “System 1”. Therefore people would be less sensitive to probability differences. On the other hand, the risk-as-feelings hypothesis (Loewenstein et al., 2001) postulates that responses to risky situations result in part from direct emotional influences, including negative feelings such as worry, fear, or anxiety, and such feelings can be influenced by how an outcome is presented (vividness). Compared with non-cash presented lotteries, cash presented ones might trigger stronger anticipatory emotions and therefore make people more pessimistic towards risk.

We test the hypotheses above using a controlled laboratory experiment. We ask the subjects to value lotteries with known probabilities under two treatments, one with cash and the other with a non-cash instrument (number). We found that the valuations given by subjects in the cash treatment were lower than those in the number treatment. We use a binary rank-dependent utility (RDU) model to explain the certainty equivalents (CEs) given under the two treatments. Since we used binary lotteries in the experiment, many non-expected utility theories do not diverge (Gul, 1991; Luce and Fishburn, 1991; Miyamoto, 1988), and therefore the results from binary RDU also apply to them. By eliciting the parameters of the utility function and probability weighting function under each treatment, we identified that the gap in CEs between the treatments was due to the difference in probability weighting functions under the cash and non-cash treatments. Subjects were less sensitive to changes in likelihood when valuing cash lotteries, but there is no difference in pessimism.

5.2 Experimental design

Ninety-two students at Erasmus University Rotterdam participated in the experiment (37% female). Each subject received a show-up fee of €5. On top of that, each subject received additional payment (up to €30) determined by their choice in a randomly drawn question answered in the experiment.

Subjects were assigned to one of the two treatments randomly, and were interviewed individually by one of the two researchers randomly determined, independent

of the treatment. In each session, the researcher presented a series of lotteries to the subject, and recorded the subject's valuation of each one. To familiarize subjects with the tasks and payment procedure, the instructions contained examples and trial problems. The subjects could ask the researcher clarification questions any time during the experiment. To minimize the difference between the two researchers, a strict protocol (see Appendix IV) about what to tell subjects and how to answer their questions was adopted. Subjects could work at their own speed. On average, it took them 45 minutes to complete the experiment.

In both treatments, subjects were asked to give valuations to binary lotteries. We denote $L = x_py$ (with $x > y > 0$). The lottery gave the subject the better outcome x with probability p , and y otherwise. There were in total 12 such lotteries, varying p , x and y (see Table 5.1). Such variation enables us to estimate the utility function and the probability weighting function for each subject. The lotteries appeared in individualized random orders.

Table 5.1: The lotteries used in the valuation task

	p	x	y
1	0.05	20	5
2	0.05	30	10
3	0.1	10	5
4	0.25	20	5
5	0.25	30	10
6	0.5	10	5
7	0.5	20	5
8	0.75	20	5
9	0.75	30	10
10	0.9	10	5
11	0.95	20	5
12	0.95	30	10

We implemented the Becker-DeGroot-Marschak (BDM) procedure (Becker et al., 1964) to elicit CEs for lotteries with compatible incentives. First, the lottery to be implemented for real was randomly determined at the end of the experiment. Second, the BDM procedure was conducted by drawing one number z between the lowest prize (y) and the highest prize (x) of the chosen lottery. If z was larger than

the subject’s evaluation, the subject would receive z ; otherwise, the subject would be paid by running the lottery.

Figure 5.1: Examples of lottery presentation

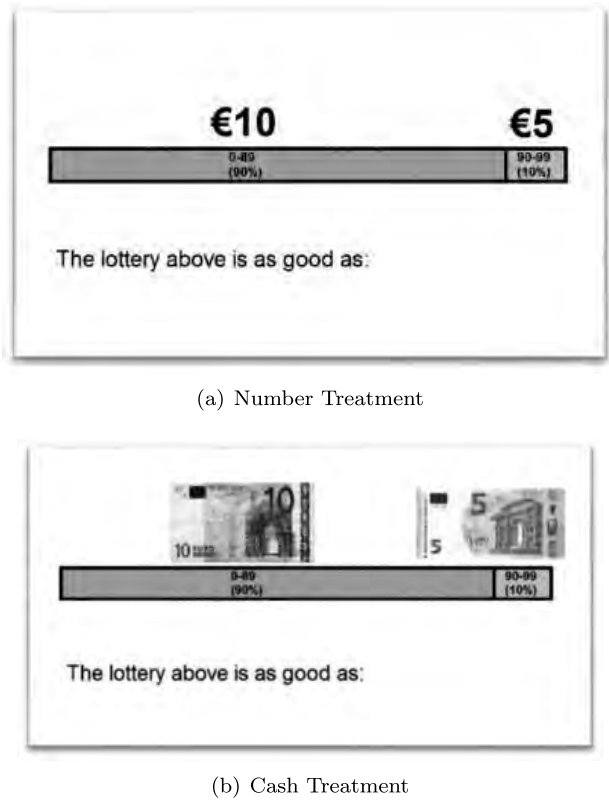


Figure 5.1 shows how the lotteries were presented to subjects in the two treatments. The only difference between the two treatments is that in the number treatment, money amounts were presented with the currency symbol and a number, as in most of the experiments in decision studies; in the cash treatment, money amounts were presented with real cash attached to the questionnaire without the number written down¹.

¹In the cash treatment, the same money amount could be presented in different ways. For instance, €13 could be presented with two €5 notes plus three €1 coins, or with six €2 coins plus one €1 coin, or other possible ways. We apply the rule that in the cash treatment, money amount is presented with the fewest number of notes and coins. In this case, €13 is presented with one €10

Subjects were asked to either specify their evaluation to the lottery by writing down the number in the number treatment, or put down the corresponding amount of cash below the lottery in the cash treatment. Particularly, subjects in the cash treatment were given a box with one €20 note, one €10 note, one €5 note, two €2 coins, one €1 coin, one 50-cent coin, two 20-cent coins and one 10-cent coin in it, so that they can make different combinations to present all possible evaluations (precision up to 10 cents) to lotteries, ranging from €41 to 0.

5.3 Analysis

5.3.1 Decision-model-free Analyses

We analyze the reported CEs without assuming any specific decision model. We use a simple linear mixed-effects model, with fixed effects of treatments and task dummies and with subject random effect. The certainty equivalent of lottery j given by subject i is modeled as: $CE_{ij} = \beta \text{Treatment}_i + \delta_j + \epsilon_{ij}$, where $i = 1, \dots, 92, j = 1, \dots, 12$, and ϵ_{ij} is a normally distributed within-subject error term.

We also calculate Relative Risk Premium ($RRP = \frac{EV - CE}{EV}$) for each valuation. The positive, zero, and negative RRP suggest risk aversion, risk neutrality and risk seeking respectively. We model the RRP with the same mixed effect model as the one for CE above: $RRP_{ij} = \beta \text{Treatment}_i + \delta_j + \epsilon_{ij}$, where $i = 1, \dots, 92, j = 1, \dots, 12$, and ϵ_{ij} is a normally distributed within-subject error term.

5.3.2 Binary RDU analysis

Under binary RDU, for a given binary lottery $L = x_p y$ ($x > y \geq 0$), the CE shall satisfy: $CE = u^{-1}(w(p)u(x) + (1 - w(p))u(y))$ (Eq. 5.1), where u is a utility function, with $u(0) = 0$ and $u'(x) > 0$, describing how a monetary outcome x is subjectively valued, and w is an increasing probability weighting function that assigns subjective weight to probabilities, with $w(0) = 0$ and $w(1) = 1$.

note, one €2 coin and one €1 coin.

Stott (2006) compared combinations of different utility functions and weighting functions for choice data, and found that the combination of power utility function (Wakker, 2008) and the compound invariance family (Prelec, 1998) the most predictive. We use the power utility function $u(x) = x^\gamma$ if $\gamma > 0$; $\ln x$ if $\gamma = 0$; $-x^\gamma$ if $\gamma < 0$ (Eq. 5.2), and Prelec’s compound invariant probability weighting function $w(p) = ((\exp(-(-\ln p)^a))^b$ ($0 < a < 1, b > 0$) (Eq. 5.3) to analyze our data. In particular, we use Prelec’s two parameter probability weighting function that decomposes probability weighting into likelihood-sensitivity and pessimism.

The parameter a is an index of likelihood-sensitivity, which points to a psychological phenomenon which reflects “diminishing sensitivity” for probabilities. A smaller a indicates less distinction between different levels of likelihood. The parameter b is an index of pessimism, and a bigger b indicates that the subject pays more attention to the worst outcome.

Using maximum-likelihood estimation, we estimate Eq. 5.1 with the specific u and w for each individual separately, and obtain parameters γ , a , and b for each individual. We will compare the estimates to their benchmark and between the two treatments using Wilcoxon tests.

5.4 Results

5.4.1 Decision-model-free Analyses

Table 5.2 shows the means (and standard deviations) of CEs for each lottery in the two treatments. The EVs, winning probability of the larger outcome, the mean differences between treatments normalized by EVs of the lotteries are also provided in the table.

It can be observed that *CEs* in the number treatment are larger than *CEs* in the cash treatment for every lottery. As described in Section 5.3.1, we subject the *CEs* to a linear mixed-effects model. The model shows that the CEs in the cash treatment are on average 0.77 euro lower than those in the number treatment ($p = 0.002$).

If look at the columns of *EV* and $\frac{Difference}{EV}$, an increasing trend can be detected:

Table 5.2: CEs for each lottery

	<i>EV</i>	<i>Probability</i>	<i>Number</i>	<i>Cash</i>	$\frac{\text{Difference}}{EV}$
1	5.5	0.1	6.20 (0.94)	6.05 (0.82)	2.72%
2	5.75	0.05	7.60 (2.66)	7.50 (2.34)	1%
3	7.5	0.5	7.60 (0.86)	7.39 (0.55)	3.87%
4	8.75	0.25	9.58 (2.75)	9.17 (2.33)	4.69%
5	9.5	0.9	8.55 (0.99)	8.40 (1.09)	1.58%
6	11	0.05	13.64 (3.01)	12.61 (2.26)	9.36%
7	12.5	0.5	12.50 (2.42)	12.30 (1.60)	1.60%
8	15	0.25	15.98 (2.76)	15.22 (2.71)	5.07%
9	16.25	0.75	15.50 (1.95)	14.65 (2.59)	5.23%
10	19.25	0.95	17.71 (1.58)	17.34 (2.31)	1.92%
11	25	0.75	23.10 (2.43)	20.46 (4.21)	10.56%
12	29	0.95	26.97 (2.25)	24.47 (4.94)	8.62%

the normalized difference between two treatments is increasing with *EV*. This trend is shown with a fitted line in Figure 5.2. Pearson correlation test also confirms this trend ($= 0.62, p - value = 0.03$).

Figure 5.2: Scatter plot of proportional difference sorted by EVs.

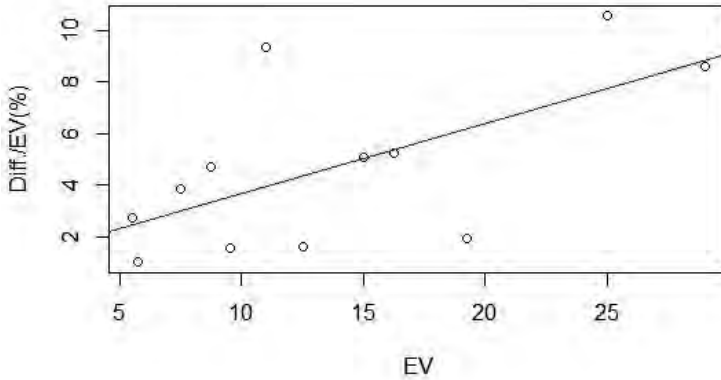
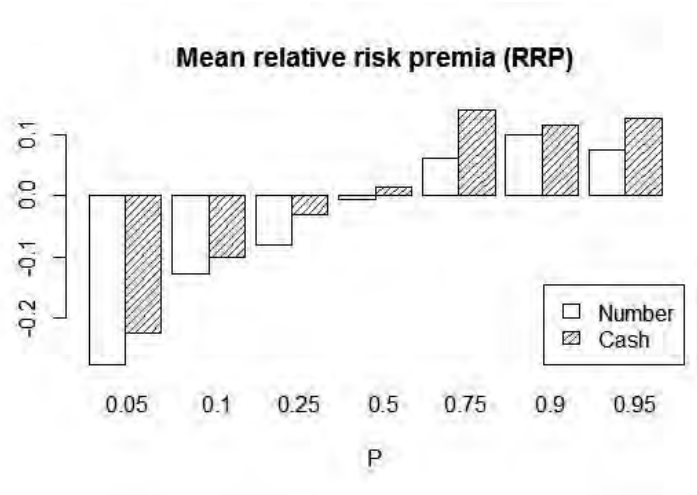


Figure 5.3 shows mean relative risk premium for lotteries with different probabilities. The mean *RRPs*, sorted by the probability p of outcome x_1 , show a systematic relationship between risk attitudes and probabilities of outcomes, which is also con-

sistent with the typical empirical findings: On average, people are risk seeking for small probabilities, and risk averse for large probabilities.

We subject the *RRPs* to a linear mixed effects model, as described in Section 5.3.1. The model shows that the cash treatment increases the *RRP* by 0.05 ($p = 0.03$).

Figure 5.3: RRP by the probabilities of the better outcome.



5.4.2 Binary RDU analysis

Table 5.3 summarizes the results from the maximum likelihood estimation specified in Section 5.3.2.

The results above show that, at the aggregate level, subjects in the number treatment exhibit linear utility, likelihood insensitivity and no pessimism. The cash treatment does not change the utility curvature, but changes the probability weighting. In particular, subjects in the cash treatment are less sensitive to probability changes than those in the number treatment.

In Figure 5.4, we plot the probability weighting curves (based on medians of individual parameters from maximum likelihood estimation) for the two treatments. The curve of the cash treatment is more pronounced in its inverse S shape.

Table 5.3: parameters from maximum likelihood estimation

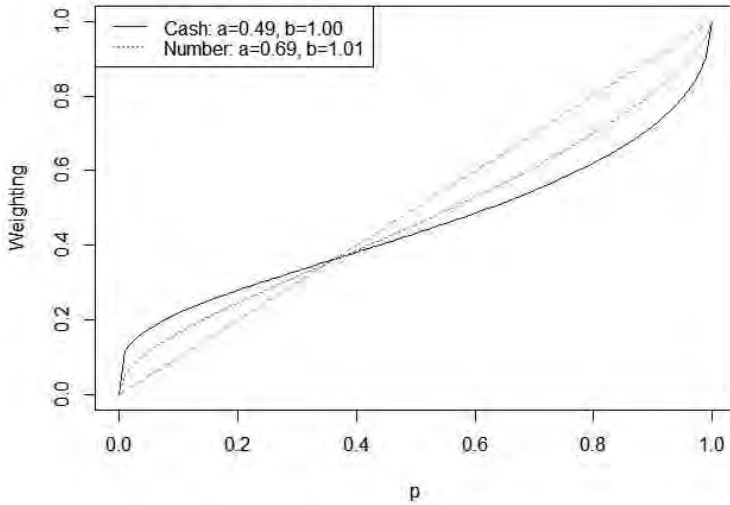
	Cash treatment	Number treatment	p-value
Utility Curvature γ	1.09	1.07	0.97
Likelihood Sensitivity a	0.49***	0.69***	0.02
Pessimism b	1.00	1.01	0.46
Number of Observations	46	46	

Notes: Reported numbers are the median of estimated coefficients in the corresponding treatment, followed by significance from one-sample Wilcoxon signed rank test. The benchmarks for the coefficients (γ, a, b) are 1.

The last column gives p-values from Wilcoxon rank-sum test.

*** indicates significance at 1% (two-sided test)

Figure 5.4: Probability weighting curves of the two treatments.



5.5 Conclusion

It has been noticed in the literature that the gap between valuations made of cash and non-cash instruments is more prominent for products of unknown value than for those of clear market value. Using a simple experiment, we test how payment instruments influence valuation through affecting people's risk attitudes, which can

be reflected by utility curvature and probability weighting.

The results show that valuers' utility functions elicited for cash and non-cash payment instruments do not differ from each other. The difference in valuations is driven by probability weighting. Presenting lotteries with cash makes valuers less sensitive to changes in likelihood, which leads to less variation in valuations of different lotteries.

Chapter 6

General Conclusion

This thesis presents evidence suggesting that the same types of biases in individual decision making under uncertainty pertain in interpersonal contexts. The chapters above demonstrate in specific contexts how specific interpersonal factors attenuate, amplify, or replicate these biases.

One of the most natural interpersonal contexts is group decision making. The group effects on decisions consist of aggregation and communication, and disentangling these could help us improve our understanding of the group decision making process and make better predictions. Because the two measures of rationality applied in Chapter 2, namely stochastic dominance and EU-compliance, are both dichotomous, we use the simulated decisions from pure voting as benchmarks to isolate the communication effects. Identifying group communication effects in tasks with continuous variables, such as lottery valuation or matching probabilities, would be more complicated. The challenge is to find a proper benchmark accounting for the pure aggregation effects. To simulate the group valuation from pure voting, one would need to make assumptions about the probability of one person voting for a given proposal depending on her prior. An alternative way without making such assumptions is to have a treatment where group members vote on individual valuations to determine the group valuation.

The content of communication could make a lot of difference. Chapter 3 aims

at providing simple information to help individuals evaluate the quality of the estimates they received in interpersonal communication. The improvement of estimates, after interpersonal exchange of information, depends on those with more accurate estimates sticking more to their estimates than those with less accurate estimates. Exchange of supporting evidence is one way to help individuals approximate the comparative accuracy between their own estimates and the estimates they receive.

Chapter 4 extends Yaari's definition of comparative risk aversion to enable comparisons between people holding different beliefs. The intuition behind our main result is simple: in order to compare uncertainty aversion of two agents, one should not only consider their bets on events alone, since different beliefs about the events are playing roles, but consider bets on and against events in pairs. The paired bets together serve as a benchmark set that controls for confounding from unmeasured different beliefs.

The last chapter presents evidence that the mere presentation with cash could make a substantial psychological impact on people's risk attitudes by changing their likelihood insensitivity in probability weighting. The cash effects that lower valuations could potentially come from a buyer's evolutionary instinct in bargaining triggered by cash. Testing the existence of such instincts and, if they exist, finding what triggers them could be complementary to the current study. Potentially, understanding such triggers could provide guidance for presenting uncertain prospects (e.g., lotteries, insurance, or financial plans) in certain ways to tune the strings of instincts to be ready for the melody of rationality.

Chapter 7

Appendix I: Questions in Chapter 3

There are in total 15 questions. Please make sure you have answered all the questions before handing in the questionnaire.

- 1. There are currently in total _____ countries in the Asia.
- 2. There are in total _____ triangles in Figure A.1.
- 3. There are in total _____ triangles in Figure A.2.
- 4. Feng Xiaogang has so far directed _____ movies.
- 5. Romance of the Three Kingdoms has in total _____ characters.
- 6. Yang Mi has so far played in _____ different movies and TV dramas.
- 7. Harry Potter (book series) has in total _____ characters.

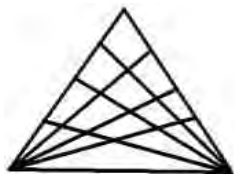


Figure A.1

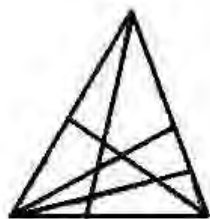


Figure A.2

8. China has won in total _____medals at the 2012 Summer Olympics in London.
9. There are currently in total _____ NBA teams.
10. There are currently in total _____countries in Europe.
11. Water Margin has in total _____characters.
12. Tom Cruise has so far played in _____different movies.
13. Dream of the Red Chamber has in total _____characters.
14. Zhao Wei has so far played in _____different movies and TV dramas.
15. Zhang Yimou has so far directed _____movies.

An example of a confidence question:

Please rate from 1 (not confident at all) to 7 (extremely confident) about how confident are you with your answer.

An example of an evidence question (for the first question of number of Asian countries):

Please list three countries that you think others are most likely not able to think of:

a)_____ b)_____ c)_____

Chapter 8

Appendix II: Experiment instructions in Chapter 3

Thank you for your participation. This experiment consists of two stages.

During the first stage, you will fill in questionnaire 1 and also write down your answers on a separate answer sheet. You have 25 minutes to complete this stage, after which the experimenter will collect the questionnaire 1 and the answer sheet.

During the second stage, you will receive questionnaire 2 AND one of your classmates' answer sheet from the previous stage. When filling in questionnaire 2, you can refer to the answer sheet you received. You have 10 minutes to finish stage 2. Afterwards, the experimenter will collect all documents and proceed to the payment procedure.

Payment procedure:

Please fill the subject ID assigned to you in the top left corner blank on **both** questionnaires and answer sheets. Questionnaires without a subject ID will not be paid. At the end of the experiment, we will randomly draw 8 subject numbers. Students with the 8 numbers will be paid according to the accuracies of their answer. Others will receive 10 RMB show-up fee. Each student has an equal probability to be drawn.

In case your subject ID is drawn, please go to the payment desk, where you will randomly draw a question number from the 30 questions (15 on Questionnaire 1 and 15 on Questionnaire 2). The experimenter will reveal the correct answer to the question drawn and your payment will be determined as follows:

Your prize is 100 RMB if your answer is within the 5% interval around the correct answer.

Your prize is 50 RMB if your answer is within the 10% interval but out of the 5% interval around the correct answer.

Your prize is 20 RMB if your answer is within the 20% interval but out of the 10% interval around the correct answer.

Your prize is 10 RMB if your answer is out of the 20% interval around the correct answer.

No communication is allowed during the experiment. Students who talk with others will be disqualified from the experiment.

Chapter 9

Appendix III: Proofs in Chapter 4

9.1 Proof of Proposition 1 and Proposition 2

The proof of Proposition 1 can be obtained by replacing all F with E in the following proof.

$(ii) \Rightarrow (i)$

Proof. By the definition of "more concave", u_A more concave than u_B implies that there exists a concave function φ such that $u_A = \varphi \circ u_B$. Moreover, φ is strictly increasing because u_A and u_B are strictly increasing.

Consider any $z, x, y \in X$ and events $E, F \in \Sigma$. Without loss of generality, we assume $x \geq y$.

We first consider the case $P_B(E) \geq P_A(F)$.

$$\begin{aligned} & z \succsim_B x_E y \\ \Rightarrow & u_B(z) \geq P_B(E) u_B(x) + (1 - P_B(E)) u_B(y) \\ \Rightarrow & u_B(z) \geq P_A(F) u_B(x) + (1 - P_A(F)) u_B(y) \\ \Rightarrow & \varphi(u_B(z)) \geq P_A(F) \varphi(u_B(x)) + (1 - P_A(F)) \varphi(u_B(y)) \text{ because } \varphi \text{ is strictly in-} \end{aligned}$$

creasing and concave

$$\Rightarrow u_A(z) \geq P_A(F) u_A(x) + (1 - P_A(F)) u_A(y)$$

$$\Rightarrow z \succsim_A x_F y$$

$$\Rightarrow \text{not } [z \prec_A x_F y \ \& \ z \prec_A y_F x]$$

The case $P_B(E) < P_A(F)$ can be derived in the same way by starting from $z \succsim_B y_E x$. □

$$(i) \Rightarrow (ii)$$

Below we prove $\text{not } (ii) \Rightarrow \text{not } (i)$ if there exist $E, F \in \Sigma$ such that $P_A(F) = P_B(E) = \frac{1}{2}$.

Proof. Remember that u_A and u_B are strictly increasing. We can therefore define φ over the image of u_B by $\varphi = u_A \circ u_B^{-1}$. Consequently, φ is also strictly increasing.

Not $(ii) \Rightarrow$ there exists b and c in the image of u_B such that $\varphi(\frac{1}{2}(b+c)) < \frac{1}{2}\varphi(b) + \frac{1}{2}\varphi(c)$

Let $x, y, z \in X$ be uniquely defined by $u_B(x) = b$, $u_B(y) = c$, $u_B(z) = \frac{b+c}{2}$.

Consider an event E such that $P_B(E) = \frac{1}{2}$ (it must exist according to the richness condition). Consequently, we have $z \sim_B x_E y$ and $z \sim_B y_E x$

Now consider event F such that $P_A(F) = \frac{1}{2}$ (it must also exist according to the richness condition).

$$\begin{aligned} u_A(z) &= \varphi(u_B(z)) = \varphi\left(\frac{1}{2}(b+c)\right) \\ &< \frac{1}{2}\varphi(b) + \frac{1}{2}\varphi(c) &= \frac{1}{2}u_A(x) + \frac{1}{2}u_A(y) \\ &= P_A(F) u_A(x) + (1 - P_A(F)) u_A(y) \\ &= (1 - P_A(F)) u_A(x) + P_A(F) u_A(y) \\ &\Rightarrow u_A(z) \prec_A x_E y \text{ and } u_A(z) \prec_A y_E x. \end{aligned}$$

□

9.2 Proof of Proposition 3

(ii) \Rightarrow (i)

Proof. By the definition of "more concave", u_A more concave than u_B implies that there exists a concave function φ such that $u_A = \varphi \circ u_B$. Moreover, φ is strictly increasing because u_A and u_B are strictly increasing.

Consider any $z, x, y \in X$, $f, g \in \mathcal{F}$, and events $E, F, G, H \in \Sigma$. Without loss of generality, we assume $x \geq y$.

We first consider the case $\frac{P_B(E)}{(P_B(E)+P_B(F))} \geq \frac{P_A(G)}{(P_A(G)+P_A(H))}$.

$$\begin{aligned}
 & z_{E \cup F} f \succsim_B x_E y_F f \\
 \Rightarrow & u_B(z) \geq \frac{P_B(E)}{(P_B(E)+P_B(F))} u_B(x) + \frac{P_B(F)}{(P_B(E)+P_B(F))} u_B(y) \\
 \Rightarrow & u_B(z) \geq \frac{P_A(G)}{(P_A(G)+P_A(H))} u_B(x) + \frac{P_A(H)}{(P_A(G)+P_A(H))} u_B(y) \\
 \Rightarrow & \varphi(u_B(z)) \geq \frac{P_A(G)}{(P_A(G)+P_A(H))} \varphi(u_B(x)) + \frac{P_A(H)}{(P_A(G)+P_A(H))} \varphi(u_B(y)) \text{ because } \varphi \text{ is} \\
 & \text{strictly increasing and concave} \\
 \Rightarrow & u_A(z) \geq \frac{P_A(G)}{(P_A(G)+P_A(H))} u_A(x) + \frac{P_A(H)}{(P_A(G)+P_A(H))} u_A(y) \\
 \Rightarrow & z_{G \cup H} g \succsim_A x_G y_H g \\
 \Rightarrow & \text{not}[z_{G \cup H} g \prec_A x_G y_H g \ \& \ z_{G \cup H} g \prec_A x_H y_G g].
 \end{aligned}$$

The case $\frac{P_B(E)}{(P_B(E)+P_B(F))} < \frac{P_A(G)}{(P_A(G)+P_A(H))}$, can be derived in the same way by starting from $z_{E \cup F} f \succsim_B x_F y_E f$. \square

(i) \Rightarrow (ii)

Below we prove not (ii) \Rightarrow not (i) if there exist $E, F, G, H \in \Sigma$ such that $P_B(E) = P_B(F)$ and $P_A(G) = P_A(H)$.

Proof. Remember that u_A and u_B are strictly increasing. We can therefore define φ over the image of u_B by $\varphi = u_A \circ u_B^{-1}$. Consequently, φ is also strictly increasing.

Not (ii) \Rightarrow there exists b, c in the image of u_B such that $\varphi(\frac{1}{2}(b+c)) < \frac{1}{2}\varphi(b) + \frac{1}{2}\varphi(c)$.

Let outcomes $x, y, z \in X$ be uniquely defined by $u_B(x) = b, u_B(y) = c, u_B(z) = \frac{b+c}{2}$. Consider events E, F such that $P_B(E) = P_B(F)$ (they must exist according to the richness condition) and any act f . Consequently, we have $z_{E \cup F} f \sim_B x_E y_F f$ and

$$z_{E \cup F} f \sim_B x_E y_F f.$$

Now consider events G, H such that $P_A(G) = P_A(H)$ (they must also exist according to the richness condition) and any act g .

$$\begin{aligned} u_A(z) &= \varphi(u_B(z)) = \varphi\left(\frac{1}{2}(b+c)\right) \\ &< \frac{1}{2}\varphi(b) + \frac{1}{2}\varphi(c) = \frac{1}{2}u_A(x) + \frac{1}{2}u_A(y) \\ \Rightarrow (P_A(G) + P_A(H))u_A(z) &< P_A(G)u_A(x) + P_A(H)u_A(y) \\ \text{and } (P_A(H) + P_A(G))u_A(z) &< P_A(H)u_A(x) + P_A(G)u_A(y) \\ \Rightarrow z_{G \cup H} g \prec_A x_G y_H g \text{ and } z_{G \cup H} g \prec_A x_H y_G g. \end{aligned}$$

□

9.3 Proof of Proposition 4

$$(ii) \Rightarrow (i)$$

Proof. By the definition of "more concave", u_A more concave than u_B implies that there exists a concave function φ such that $u_A = \varphi \circ u_B$. Moreover, φ is strictly increasing because u_A and u_B are strictly increasing.

Consider any $f = (E_1 : x_1, \dots, E_n : x_n) \in \mathcal{F}$ and $z \in X$.

$$z \succsim_B \Pi(f) \quad \forall \Pi$$

\Rightarrow

$$\begin{aligned} u_B(z) &\geq P_B(E_1)u_B(x_1) + P_B(E_2)u_B(x_2) + P_B(E_3)u_B(x_3) + \dots + P_B(E_n)u_B(x_n) \\ \text{and } u_B(z) &\geq P_B(E_1)u_B(x_2) + P_B(E_2)u_B(x_1) + P_B(E_3)u_B(x_3) + \dots + P_B(E_n)u_B(x_n) \\ &\vdots \end{aligned}$$

$$\text{and } u_B(z) \geq P_B(E_1)u_B(x_n) + P_B(E_2)u_B(x_{n-1}) + P_B(E_3)u_B(x_{n-2}) + \dots + P_B(E_n)u_B(x_1)$$

In total there are $n!$ inequalities, and each outcome has been assigned to each event $(n-1)!$ times.

Summing up these $n!$ inequalities and dividing by $n!$, we have:

$$\begin{aligned} u_B(z) &\geq \frac{1}{n}u_B(x_1) + \frac{1}{n}u_B(x_2) + \dots + \frac{1}{n}u_B(x_n) \\ \Leftrightarrow \varphi(u_B(z)) &\geq \frac{1}{n}\varphi(u_B(x_1)) + \dots + \frac{1}{n}\varphi(u_B(x_n)) \text{ because } \varphi \text{ is strictly increasing and concave.} \end{aligned}$$

$$\Rightarrow u_A(z) \geq \frac{1}{n}u_A(x_1) + \dots + \frac{1}{n}u_A(x_n).$$

This inequality implies $\text{not}[z \prec_A \Pi(g) \quad \forall \Pi]$ for any $g = (F_1 : x_1, \dots, F_n : x_n)$ whose

outcomes are the same as those of f because:

$$[z \prec_A \Pi(g) \quad \forall \Pi]$$

\Rightarrow

$$u_A(z) < P_A(F_1)u_A(x_1) + P_A(F_2)u_A(x_2) + P_A(F_3)u_A(x_3) + \cdots + P_A(F_n)u_A(x_n)$$

$$\text{and } u_A(z) < P_A(F_1)u_A(x_2) + P_A(F_2)u_A(x_1) + P_A(F_3)u_A(x_3) + \cdots + P_A(F_n)u_A(x_n)$$

\vdots

$$\text{and } u_A(z) < P_A(F_1)u_A(x_n) + P_A(F_2)u_A(x_{n-1}) + P_A(F_3)u_A(x_{n-2}) + \cdots + P_A(F_n)u_A(x_1).$$

Summing these $n!$ inequalities, and dividing them by $n!$ implies

$$u_A(z) < \frac{1}{n}u_A(x_1) + \cdots + \frac{1}{n}u_A(x_n). \quad \square$$

$$(i) \Rightarrow (ii)$$

Below we prove not $(ii) \Rightarrow$ not (i) if there exist, for some integer n , two n -fold partitions $\{E_i\}_n$ and $\{F_i\}_n$ of S such that $P_A(F_i) = P_B(E_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$.

Proof. Remember that u_A and u_B are strictly increasing. We can therefore define φ over the image of u_B by $\varphi = u_A \circ u_B^{-1}$. Consequently, φ is also strictly increasing.

Not $(ii) \Rightarrow$ there exists u_1, \dots, u_n belonging to the domain of u_B such that

$$\varphi\left(\frac{1}{n}(u_1 + \cdots + u_n)\right) < \frac{1}{n}\varphi(u_1) + \cdots + \frac{1}{n}\varphi(u_n)$$

The outcomes x_1, \dots, x_n are uniquely defined by $u_B(x_1) = u_1, \dots, u_B(x_n) = u_n$ and z is defined by $u_B(z) = \frac{1}{n}(u_1 + \cdots + u_n)$.

Consider a partition $\{E_i\}_n$ such that $P_B(E_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$ (which is assumed to exist), the act f assigning (x_1, \dots, x_n) to (E_1, \dots, E_n) and all its permutations $\Pi(f)$. The equality $u_B(z) = \frac{1}{n}u_1 + \cdots + \frac{1}{n}u_n$ implies $z \sim_B \Pi(f)$ for all Π .

$$\text{Yet, } u_A(z) = \varphi(u_B(z)) = \varphi\left(\frac{1}{n}(u_1 + \cdots + u_n)\right)$$

$$< \frac{1}{n}\varphi(u_1) + \cdots + \frac{1}{n}\varphi(u_n) = \frac{1}{n}u_A(x_1) + \cdots + \frac{1}{n}u_A(x_n).$$

Consider a partition $\{F_i\}_n$ such that $P_A(F_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$ (which is also assumed to exist), the act g assigning (x_1, \dots, x_n) to (F_1, \dots, F_n) and all its permutations $\Pi(g)$.

The inequality $u_A(z) < \frac{1}{n}u_A(x_1) + \cdots + \frac{1}{n}u_A(x_n)$ implies $z \prec_A \Pi(g)$ for all Π , and

hence not (i). □

9.4 Proof of Proposition 5

(ii) \Rightarrow (i)

Proof. By the definition of "more concave", u_A more concave than u_B implies that there exists a concave function φ such that $u_A = \varphi \circ u_B$. Moreover, φ is strictly increasing because u_A and u_B are strictly increasing.

Consider any $f = (E_1 : x_1, \dots, E_n : x_n) \in \mathcal{F}$ and $z \in X$.

$$z \succsim_B \pi^m(f) \quad \forall m$$

\Rightarrow

$$u_B(z) \geq P_B(E_1)u_B(x_1) + P_B(E_2)u_B(x_2) + P_B(E_3)u_B(x_3) + \dots + P_B(E_n)u_B(x_n)$$

$$\text{and } u_B(z) \geq P_B(E_1)u_B(x_2) + P_B(E_2)u_B(x_3) + P_B(E_3)u_B(x_4) + \dots + P_B(E_n)u_B(x_1)$$

\vdots

$$\text{and } u_B(z) \geq P_B(E_1)u_B(x_n) + P_B(E_2)u_B(x_1) + P_B(E_3)u_B(x_2) + \dots + P_B(E_n)u_B(x_{n-1})$$

In total there are n inequalities, and each outcome has been assigned to each event 1 single time.

Summing up these n inequalities and dividing by n , we have:

$$u_B(z) \geq \frac{1}{n}u_B(x_1) + \frac{1}{n}u_B(x_2) + \dots + \frac{1}{n}u_B(x_n)$$

$\Leftrightarrow \varphi(u_B(z)) \geq \frac{1}{n}\varphi(u_B(x_1)) + \dots + \frac{1}{n}\varphi(u_B(x_n))$ because φ is strictly increasing and concave.

$$\Rightarrow u_A(z) \geq \frac{1}{n}u_A(x_1) + \dots + \frac{1}{n}u_A(x_n).$$

This inequality implies not $[z \prec_A \pi^m(g) \quad \forall m]$ for any $g = (F_1 : x_1, \dots, F_n : x_n)$ whose outcomes are the same as those of f because:

$$[z \prec_A \pi^m(g) \quad \forall m]$$

\Rightarrow

$$u_A(z) < P_A(F_1)u_A(x_1) + P_A(F_2)u_A(x_2) + P_A(F_3)u_A(x_3) + \dots + P_A(F_n)u_A(x_n)$$

$$\text{and } u_A(z) < P_A(F_1)u_A(x_2) + P_A(F_2)u_A(x_3) + P_A(F_3)u_A(x_4) + \dots + P_A(F_n)u_A(x_1)$$

\vdots

$$\text{and } u_A(z) < P_A(F_1)u_A(x_n) + P_A(F_2)u_A(x_1) + P_A(F_3)u_A(x_2) + \dots + P_A(F_n)u_A(x_{n-1}).$$

Summing these n inequalities, and dividing them by n implies

$$u_A(z) < \frac{1}{n}u_A(x_1) + \cdots + \frac{1}{n}u_A(x_n).$$

□

(i) \Rightarrow (ii)

Below we prove not (ii) \Rightarrow not (i) if there exist, for some integer n , two n -fold partitions $\{E_i\}_n$ and $\{F_i\}_n$ of S such that $P_A(F_i) = P_B(E_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$.

Proof. Remember that u_A and u_B are strictly increasing. We can therefore define φ over the image of u_B by $\varphi = u_A \circ u_B^{-1}$. Consequently, φ is also strictly increasing.

Not (ii) \Rightarrow there exists u_1, \dots, u_n belonging to the domain of u_B such that

$$\varphi\left(\frac{1}{n}(u_1 + \cdots + u_n)\right) < \frac{1}{n}\varphi(u_1) + \cdots + \frac{1}{n}\varphi(u_n)$$

The outcomes x_1, \dots, x_n are uniquely defined by $u_B(x_1) = u_1, \dots, u_B(x_n) = u_n$ and z is defined by $u_B(z) = \frac{1}{n}(u_1 + \cdots + u_n)$.

Consider a partition $\{E_i\}_n$ such that $P_B(E_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$ (which is assumed to exist), and the act f assigning (x_1, \dots, x_n) to (E_1, \dots, E_n) . The equality $u_B(z) = \frac{1}{n}u_1 + \cdots + \frac{1}{n}u_n$ implies $z \sim_B \pi^m(f)$ for all m .

$$\begin{aligned} \text{Yet, } u_A(z) &= \varphi(u_B(z)) = \varphi\left(\frac{1}{n}(u_1 + \cdots + u_n)\right) \\ &< \frac{1}{n}\varphi(u_1) + \cdots + \frac{1}{n}\varphi(u_n) = \frac{1}{n}u_A(x_1) + \cdots + \frac{1}{n}u_A(x_n). \end{aligned}$$

Consider a partition $\{F_i\}_n$ such that $P_A(F_i) = \frac{1}{n}$ for all $i \in \{1, \dots, n\}$ (which is also assumed to exist) and the act g assigning (x_1, \dots, x_n) to (F_1, \dots, F_n) . The inequality $u_A(z) < \frac{1}{n}u_A(x_1) + \cdots + \frac{1}{n}u_A(x_n)$ implies $z \prec_A \pi^m(g)$ for all m , and hence not (i). □

9.5 Proof of Proposition 6

Proof. It is enough to notice that the proof of Proposition 2 does not use the fact that E and F belongs to the same Σ . □

9.6 Proof of Proposition 7

According to Proposition 1.4.2 of Dupuis and Ellis (1997), for all countably additive probability measures $Q \in \Delta(S)$ and for all Σ -measurable functions f , $\min_{p \in \Delta(S)} \int_S W(f(s)) dp(s) + \theta_i R(P||Q_i) = \phi_{\theta_i}^{-1} \left(\int_S \phi_{\theta_i}(W(f(s))) dQ_i(s) \right)$ with $\phi_{\theta_i}(t) = -e^{-\frac{1}{\theta_i} t}$. Since $\phi_{\theta_i}^{-1}$ is strictly increasing, $\min_{p \in \Delta(S)} \int_S W(f(s)) dp(s) + \theta_i R(P||Q_i)$ is ordinally equivalent to $\int_S \phi_{\theta_i}(W(f(s))) dQ_i(s)$. Noticing that the lower θ_i , the more concave ϕ_{θ_i} , Proposition 7 follows from Proposition 2.

Chapter 10

Appendix IV: Experimenter's protocol in Chapter 5

1. Is this number how much I want to pay for / sell for this lottery?

“This is a valuation task, and you are asked to fill in how much is this lottery worth to you. Our payment procedure is designed to guarantee that it is for your best interest to fill in the exact valuation in your mind, which dominates both overstating and understating this value.” (Specifically avoid mentioning “buy” or “sell” in the explanation.)

2. In case the subject gives a valuation lower than the lower outcome in the lottery.

“Sorry to interrupt. You can surely put whatever amount you see proper as your valuation. This is just a reminder, because here you put a valuation lower than the lower possible outcome in the lottery, and I want to clarify the rules in case there is any misunderstanding. Since we will only randomly draw a number from the lower outcome and the higher outcome of a given lottery, in this case X and Y ($X < Y$ are the two outcomes of the lottery this subject is valuating), therefore giving a valuation lower than the lower outcome of the lottery means that all the random number we draw would be higher than your valuation and therefore you will be paid

that amount. In the extreme case, if we draw X , your valuation indicates that you prefer to be paid X , rather than receiving this lottery that gives you at least X . Is this what you prefer?"

3. In case the subject gives a valuation higher than the higher outcome in the lottery.

"Sorry to interrupt. You can surely put whatever amount you see proper as your valuation. This is just a reminder, because here you put a valuation higher than the higher possible outcome in the lottery, and I want to clarify the rules in case there is any misunderstanding. Since we will only randomly draw a number from the lower outcome and the higher outcome of a given lottery, in this case X and Y ($X < Y$ are the two outcomes of the lottery this subject is valuating), therefore giving a valuation higher than the higher outcome of the lottery means that all the random number we draw would be lower than your valuation and therefore you will receive the lottery. In the extreme case, if we draw Y , your valuation indicates that rather than receiving Y , you prefer to receive the lottery that gives you at most Y . Is this what you prefer?"

4. In the cash treatment, make sure the subject put all the notes and coins for valuation back to the box after finishing each valuation.

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Samenvatting

In dit proefschrift wordt getoond dat fouten in individueel keuzegedrag vaak voortvloeien naar interpersoonlijke situaties. De verschillende hoofdstukken tonen hoe de gedragsfouten worden verzwakt, versterkt of gerepliceerd door bepaalde interpersoonlijke factoren in specifieke situaties.

Een van de meest natuurlijke interpersoonlijke situatie is groepskeuzegedrag. De groepseffecten ten aanzien van keuzes bestaan uit aggregatie en communicatie, en het ontvlechten van deze twee kan helpen bij het begrijpen en voorspellen van groepskeuzes. Aangezien de twee maten van rationaliteit uit Hoofdstuk 2, stochastische dominantie en verwacht nut maximalisatie (Expected Utility), dichotoom zijn gebruiken we gesimuleerde keuzes van puur stemgedrag om communicatie-effecten te isoleren. Het zou lastiger zijn om groepscommunicatie-effecten te identificeren in taken met continue variabelen zoals loterijwaarderingen of kansequivalenten. De uitdaging ligt in het vinden van een juist criterium dat rekening houdt met pure aggregatie-effecten. Om de groepswaardering te simuleren op basis van puur stemgedrag zou men aannames moeten maken omtrent de kans dat een persoon afhankelijk van zijn prior stemt op een bepaald voorstel. Een alternatieve wijze waarop dergelijke aannames niet gemaakt hoeven worden is een opzet waarbij groepsleden individuele waarderingen noemen om tot de groepswaardering te komen.

De inhoud van de communicatie heeft mogelijk grote invloed. Hoofdstuk 3 beschrijft hoe simpele signalen aan individuen worden meegegeven met het doel hen te helpen de kwaliteit van hun ontvangen interpersoonlijke communicatie in te schatten. De verbetering van schattingen na interpersoonlijke uitwisseling van eerdere schattingen

is afhankelijk van de mate waarin personen met nauwkeurigere schattingen dichterbij hun originele schatting blijven dan personen met minder nauwkeurige schattingen. Het uitwisselen van ondersteunend bewijs is een manier om individuen te helpen met het inschatten van de relatieve nauwkeurigheid van hun schattingen t.o.v. de ontvangen schattingen.

In Hoofdstuk 4 wordt Yaari's definitie van betrekkelijke risico-aversie uitgebreid waardoor personen met verschillende kansinschattingen alsnog kunnen worden vergeleken. De intuïtie achter het hoofdresultaat is simpel: om de afkeer tegen onzekerheid van twee personen te vergelijken moet men niet slechts weddenschappen op aparte evenementen vergelijken, daarop hebben de eventueel verschillende kansinschattingen een invloed, maar "weddenschapsparen" waarbij op het evenement en het complement van het evenement wordt gewed. Het weddenschapspaar dient als maatstaf waarbij voor eventueel verschillende kansinschattingen wordt gecorrigeerd.

Het laatste hoofdstuk lever bewijs voor het "cash-effect": het louter tonen van contant geld heeft een substantiële psychologische impact op de risico aversie van mensen, doordat de waarschijnlijkheidsindifferentie in de kansweging wordt veranderd. Het feit dat het cash-effect lagere waarderingen tot stand brengt kan worden verklaard door het "afding-instinct": zodra men contant geld ziet is men geneigd een lagere prijs te noemen. In later onderzoek kan de aanwezigheid en de totstandkoming van dit instinct verder worden onderzocht.

About the author

Ning Liu was born on February 18, 1987 in Beijing, China. He received a bachelor degree in management at Renmin University of China. He came to Rotterdam, and joined the MPhil program of ERIM in 2009. After receiving his master degree in 2011, he continued to pursue his PhD in Prof. Peter P. Wakker's Behavioral Economics group at Erasmus School of Economics, supervised by Prof.dr. Aurélien Baillon, Prof.dr. Han Bleichrodt, and Prof.dr. Peter P. Wakker.

In 2014, Ning was a visiting PhD student at Harvard University, hosted by Prof. dr. Eric S. Maskin. His research focuses on decision making in interpersonal environments. He is interested in human's risk preference, time preference, and social preferences. In particular, he studies how human interaction affects these fundamental human preferences. He adopts both theoretical and empirical approaches. His current research also addresses the implicational issues of communicating uncertainty related to climate change. Ning's research has appeared in leading journals including *Journal of Risk and Uncertainty*, *Theory and Decision*, *Nature Climate Change*.

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PHD PORTFOLIO

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Research School: ERIM
PhD-period: October 1st 2011-September 1st 2014
Promotor: Prof. Aurélien Baillon, Prof. Han Bleichrodt
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Publication

Baillon, A., Bleichrodt, H., Liu, N., & Wakker, P. (2016). Group decision rules and group rationality under risk. *Journal of Risk and Uncertainty*, 52:99-116.

Baillon, A., Liu, N., & Van Dolder, D. Comparing uncertainty aversion towards different sources. *Theory and Decision (forthcoming)*.

Bosetti, V., Weber, E., Berger, L., Budescu, D., Liu, N., Tavoni, M. COP21 Climate Negotiators' Beliefs about Future Global Temperature Increases and their Responses to Climate Model Forecasts. *Nature Climate Change (forthcoming)*.

PhD training

Seminars and workshops

Risk, uncertainty and ambiguity workshop, Ein Bokek, Israel	January, 2014
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The Erasmus-Technion Workshop on Decisions and Predictions, Ein Bokek, Israel	January, 2012
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Presentations

See ‘(Inter)national conferences’

(Inter)national conferences

24th Subjective Probability, Utility, and Decision Making Conference (SPUDM) 2013, Barcelona, Spain	August, 2013
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24th Jerusalem School in Economic Theory, Jerusalem, Israel	June, 2013
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15th Conference on the Foundations on Utility and Risk (FUR), Atlanta GA, USA	July, 2012
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Teaching

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PROPOSITIONS

1. Pure group communication effects cannot be properly observed without controlling for aggregation effects (Chapter 2).
2. Group communication can more easily rectify errors in intellectual tasks than biases in judgmental tasks (Chapter 2).
3. Examples are by themselves weak in changing people's estimations, but they can make a difference when combined with estimations (Chapter 3).
4. Applying the right criteria, we can compare risk attitudes of people who hold different beliefs or face different sources of uncertainty (Chapter 4).
5. Non-cash payment instruments not only change how we pay, but also how much we pay (Chapter 5).
6. Decisions that are not made through reasoning are not likely to be improved by a bit more of that.
7. There is a fine line between rectifying biases and introducing other biases with the opposite effects.
8. The choice of control treatment in an experiment determines if the findings are inspiring, boring, or, in the worst case, misleading.
9. Advances in technology have provided great opportunities to conduct experiments with people in the field and test the external validity of our knowledge in human behavior.
10. The development of field experiment urges lab experimenters to create novel designs to test and develop economic theories. Using non-monetary outcomes is one of the promising directions for future lab experiments.
11. No one should trust a proposition that does not rhyme.

Ning Liu was born on February 18, 1987 in Beijing, China. He received a bachelor degree in management at Renmin University of China. He came to Rotterdam, and joined the MPhil program of ERIM in 2009. After receiving his master degree in 2011, he continued to pursue his PhD in Prof. Peter P. Wakker's Behavioral Economics group at Erasmus School of Economics, supervised by Prof.dr. Aurélien Baillon, Prof.dr. Han Bleichrodt, and Prof.dr. Peter P. Wakker.

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