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Preface

As risk is undeniably a part of human life, a considerable share of economic decisions is made under some form of risk. Taking decision makers' risk preferences into account can substantially change the validity of theoretical economic predictions. The *revenue equivalence theorem,* for instance, one of the major findings in auction theory stating that bids and revenues are equivalent in all common auction forms[1], loses its validity when bidders are assumed to exhibit preferences towards risk other than risk neutrality. Understanding how choice behavior is affected by risk is thus crucial to predict market behavior and to make correct policy decisions. Economic research contributed a lot to develop models capturing basic human tendencies under risk in order to better understand market behavior. These models are, for instance, necessary to explain the existence of insurance markets. More elaborate models are also capable of capturing more puzzling findings such as the demand for insurances and lottery tickets at the same time, or the return-premium of stocks over bonds usually observed in financial markets.

Laboratory experiments play an important part in testing and refining these models as well as their implications. An analysis of behavior under risk with real world data is aggravated by the decision makers' unknown level of information, a self-selection of decision makers into risky situations, and the impracticality to control for motives other than risk preferences. In contrast, the laboratory environment allows controlling for various factors such as available information, it averts self-selection by assigning participants exogenously, and enables the elicitation of context-free preferences over probabilities and outcomes.

All studies presented in this dissertation report results of laboratory experiments. The first two chapters explore how human behavior under risk may change under certain circumstances. While the first chapter investigates how choices concerning risk change when a decision maker decides not only for herself but is responsible for another person, the second chapter examines how choices made under risk change when the decision maker faces severe time pressure. Chapter three and four

[1] All auction forms where the bidder with the highest signal wins the auction and the bidder with the lowest signal has an expected surplus of zero.

investigate how the introduction of risk changes behavior in two well-studied economic settings allowing for a more realistic approach: Chapter three investigates how bidding behavior changes in a first-price sealed bid auction when the auctioned good exhibits an ex-post risk, meaning that the true value of the good is revealed only some time after the auction. Chapter four analyzes how individuals' willingness to cooperate in the framework of a linear public goods game is affected if the goal of cooperation is not to create a gain but to prevent a loss and if a cooperative contribution has no deterministic impact on final outcomes but exhibits an outcome risk itself, i.e. a contribution only influences the probability of the desired outcome to occur.

In *Chapter 1,* which is joint work with Sebastian Strasser and Ferdinand Vieider, we explore how a decision maker's risk preferences change when she decides not only for herself but for another person as well. So far, economic research has focused mainly on exploring and describing risk preferences when decisions are made individually. However, in reality a large part of decisions individuals face either personally or professionally are made under responsibility for other persons, e.g. as managers for a company, as parents for children, or as politicians for the society. To the extent that decisions under responsibility differ from decisions commonly found in the individual decision making literature, findings from the latter will only constitute an imperfect predictor of attitudes under responsibility. Given that decisions under responsibility constitute an important class of decision situations - and indeed one that in its economic importance may even surpass individual decisions - additional evidence on any differences can provide important insights for descriptive as well as prescriptive policy purposes.

In a between subjects design we compare lottery decisions observed in an *individual treatment*, i.e. a decision maker's choice only affects her own income, with those made in a *responsibility treatment*, i.e. a decision maker's choice affects her own income as well as the income of an anonymous other (*the recipient*) in exactly the same way. The perfect income matching for the decision maker and her recipient allows us to study possible behavioral changes in a clean way, excluding issues deriving e.g. from preferences over outcome distributions that may cause inequality concerns. As the decision maker's choice does not only affect the recipient´s outcome but also her own, she will not be able to exactly follow her own preferences and thus will bear an actual cost when accommodating any presumed preferences of the recipient or when following some social norm. Therefore, our findings constitute only the lower bound on the effects we aim to investigate.

As individual risk attitude has been found to differ systematically across probability and outcome spaces we confront individuals with risky choices in the gain domain, the loss domain, and the mixed domain including both gains and losses. In a second experiment we also systematically vary probabilities in the gain domain.

For medium probabilities we can confirm the intuition that being responsible for somebody else's payoffs increases risk aversion for gains, while in the loss domain we find increased risk seeking. In our second experiment we replicate the finding of increased risk aversion for large probabilities of a gain, while for small probabilities we find an increase of risk seeking under conditions of responsibility. This discredits hypotheses of a *cautious shift*, and indicates an accentuation of the fourfold pattern of risk attitudes predicted by prospect theory.

Our results indicate that typical risk attitudes found for individual decision makers can not only be generalized to decision makers bearing responsibility for others, such as professional agents, but are even reinforced to some extent. These findings may have important consequences for economic prediction and policy design. As such patterns are usually seen as suboptimal from a (risk neutral) principal's point of view, special care should be taken when designing contracts, and training programs for managers may seem desirable in order for them not to follow these unwanted decision making patterns.

In *Chapter 2*, which is joint work with Martin Kocher and Stefan Trautmann, we explore how the presence of severe time pressure affects individual decision making under risk. Time pressure is common to many crucial economic decisions. Whether in financial markets, in auctions, or in negotiations – individuals often have to make their decisions immediately after new information becomes available. In most experimental studies on individual decision making under risk, however, subjects have plenty of time to make their decisions and face no severe time restrictions. Instead, experimental subjects are usually even urged to consider their choices carefully. If there is a change in behavior under time pressure, existing behavioral predictions and empirical evidence based on data without time pressure may not be valid in fast-paced markets. The effects of time pressure on decision making under risk are also important from a methodological perspective, as it is inevitable in some disciplines such as the quickly emerging field of neuro-economics to restrict decision times for individuals to only a few seconds in order to measure neural activity during the decision process. If decisions under risk are affected by time pressure, the observed patterns of neural activity may also be specific to this condition. If, in contrast, they are not or only partially affected, the results can claim wider applicability.

In a between subjects design we confront individuals with risky decisions in the gain domain, the loss domain, and the mixed domain varying the time available for making the decision and the degree of decision aid in terms of expected value information. We find that risk aversion in the gain domain is robust under time pressure whereas risk seeking in the loss domain turns into risk aversion under time pressure. For mixed prospects, subjects become more loss averse and more gain seeking under time pressure, depending on the framing of the prospects. The availability of expected value information affects choices concerning mixed prospects, reducing both loss averse and gain seeking behavior irrespectively of the presence of time pressure. In contrast, we find decisions for pure gain or pure loss gambles not to be affected by expected value information.

Our findings provide reasserting evidence for the generalizability of elicited risk attitudes from the laboratory to real world environments exhibiting time pressure as long as the gain domain is concerned. If losses and mixed outcomes are involved, results with existing elicitation methods seem to be only partially valid. Our results show that typical non-expected utility patterns as modeled by prospect theory may not provide an appropriate description of choice behavior when time pressure becomes important. Instead, recently developed models of expected utility with an aspiration level (Diecidue and van de Ven 2008) may be more suitable to capture behavioral patterns in such situations. Our finding of expected value information to reduce both loss averse and gain seeking behavior indicates that subjects benefit from such decision aids. Our results suggest that subjects are aware of their sensitivity to framing and aspiration levels, and try to avoid such effects by falling back on presumably objective measures. Given that in many decision situations outside the lab a wide range of decision aid, e.g. summary statistics, is available to decision makers, actual behavior may thus be closer to the risk neutral benchmark than laboratory studies sometimes suggest.

In *Chapter 3*, which is also joint work with Martin Kocher and Stefan Trautmann, we study how bidding behavior in an affiliated private value first-price auction is affected when adding an ex-post risk to the value of the auctioned good. The expression *ex-post risk* reflects the fact that the stochastic event which determines the good's value either takes place or is at least revealed only some time after the good is auctioned off. Real life auctions often involve goods exhibiting such a common knowledge ex-post risk that is independent of buyers' private values or their signals regarding common value components. For instance, the final value of construction procurements might depend on future weather conditions, the value of mining- or drilling-rights on the outcome of a future political election in the target country, and the value of antiques and paintings on a future verification of authenticity. However, theoretical as well as empirical economic studies mostly ignore the presence of ex-post risk and focus on auctions for *deterministic* objects, i.e. objects exhibiting no such risk.

From a theoretical perspective the introduction of an ex-post risk should have three effects concerning the bidding decision of a risk averse agent: First, the bidder´s valuation for the auctioned good should be reduced by her risk premium. Second, as the riskiness of the object introduces a background risk, the bidder should become more risk averse regarding other risks and should consequently reduce her bid shading in order to lower the risk of losing the good. Third, due to prudence, the bidder should value each extra unit of wealth more in case she wins the auction and has to face the ex-post risk and should consequently lower her bid. Esö and White (2004) theoretically show for bidders exhibiting decreasing absolute risk aversion (DARA) that the last effect which they call the *precautionary effect* overweighs the second effect, thus, predicting bidders to reduce their bids by more than their appropriate risk premium when ex-post risk is involved. We are the first to provide an empirical study exploring the effect of ex-post risk on bidding behavior. Although deriving our hypotheses from Esö and White's theoretical result which is based on the assumption of expected utility theory, we provide a model-free test and a behavioral definition of precautionary bidding.

In a within subjects design we conduct experimental first-price auctions that allow us to compare subjects' bids for risky objects, i.e. lotteries, with bids for their respective certainty equivalents which we have elicited beforehand. We find subjects to place significantly lower bids on risky objects than on equally valued sure objects. Our results are robust when controlling for potentially confounding decision biases such as loss aversion. As we find precautionary bidding to occur already for the small stakes employed in our experimental setting, it is natural to expect such behavior to be an important factor in real world auctions where stakes are usually much higher. Thus, our results have implications for auction design in general, and more specifically, for information revelation by sellers and information acquisition by buyers.

In *chapter 4,* I investigate whether cooperation in social dilemmas is more likely if cooperation is either framed as an opportunity to create a value (*creation frame*) or to prevent the loss of a value (*prevention frame*) and if cooperative behavior increases the group's outcome either in a deterministic way (*no outcome risk condition*) or in a non-deterministic way affecting only outcome-probabilities (*outcome risk condition*). Distinguishing within these two dimensions allows capturing crucial characteristics of various real world social dilemmas. While the provision problem of public parkways, for instance, can most likely be classified as a creation frame social dilemma, the provision of public safety is rather an example for a prevention frame social dilemma. However, both examples share the existence of an almost infinite number of provision levels allowing each invested monetary unit to deterministically increase the quality of provision. In contrast, the funding of research and development projects or the construction of a dyke are examples for social dilemmas exhibiting only two possible provision levels: A scientific breakthrough is either achieved or it is not, a dike either bursts with the next flood or it does not. Contributions only increase the probability of the desired event. However, the former of these two examples has a creative character while the latter exhibits a preventive nature. Although ubiquitous in real world social dilemmas, the impact of such variations has never been analyzed in depth.

In a between subjects design I systematically vary a linear public goods game in the two dimensions introduced above. Keeping marginal incentives equivalent in expectations, I find both a preventive character and the presence of outcome risk to increase contributions. Consequently, the highest degree of cooperation can be found when combining both characteristics. My findings contribute to a better understanding of why some real world social dilemmas are more easily overcome than others. The results furthermore suggest interest groups concerned with the provision of a specific public good containing a preventive character and/or outcome risk to emphasize these characteristics as much as possible.

All four chapters contain their own introductions and appendices so they can be read independently.

Chapter 1

Responsibility Effects in Decision Making under Risk

1.1 Motivation

Economic situations in which an agent takes decisions that affect others' outcomes as well as her own constitute a common class of phenomena. For instance, they represent situations in which a decision maker’s choices affect not only her own outcomes, but those of her family as well. Another common instance of such decision problems is the one of financial agency contracts in which the incentive structure of the agent coincides with the one of the principal. An example may be the one of executives that are compensated through company shares, or the one of a stock broker whose payoffs are determined by the outcomes of the investments she undertakes.

There is an extensive literature on individual decision making under risk and uncertainty (Abdellaoui *et al.*, 2011; Post *et al.*, 2008), as well as a substantial literature on risk attitude in agency problems and how to influence it through performance-contingent pay (Wiseman and Gomez-Mejia, 1998). What is missing, however, is a direct comparison of risk attitudes when decisions are individual to situations of responsibility. Indeed, to the extent that decisions under responsibility differ from decisions commonly found in the individual decision making literature, findings from the latter will only constitute an imperfect predictor of attitudes under responsibility. Given that the latter constitute an economically important class of decision situations - and indeed one that in its economic importance may even surpass individual decisions - additional evidence on any differences can provide important insights for descriptive, prescriptive, and policy purposes.

We thus explore the difference in risk attitudes between situations of decision-making for oneself and situations of *responsibility*, i.e. situations in which the decision maker decides for others as well as for herself. We explore such decisions for situations in which an anonymous other (the *recipient*) is affected by any outcomes in exactly the same way as the decision maker herself. This allows us to study possible changes in behavior in a clean way, excluding issues deriving e.g. from preferences over outcome distributions that may cause inequality concerns.

Also, by making both the decision maker's outcome and the recipient's outcome dependent on the decision makers' choice, the latter will bear an actual cost in terms of her own preferences by accommodating any presumed preferences of the recipient or by following some social norm. Any findings should thus constitute a lower bound on the effects we want to investigate.

To our best knowledge, the only paper that reports results about this issue under equal payoff assumptions in a non-strategic setting is Bolton and Ockenfels (2010), although the authors report these results as an afterthought to their main results about inequality concerns and do not find statistically significant results due to their small sample size. They also discuss only the case of decisions in the gain domain. We explore the issue systematically for risky choices in the gain domain, the loss domain, and the mixed domain. Individual risk attitudes have been found to differ systematically in the different domains (Abdellaoui, 2000; Booij *et al.*, 2010; Schoemaker, 1990). To the extent that individual risk attitudes have been found to differ systematically across the probability and outcome spaces, responsibility may well have different effects across these dimensions. While we adopt a theory-neutral approach in our exploratory efforts, the inclusion of different decision domains will allow us to capture any richness in behavior as predicted by descriptively more complex theories such as prospect theory.

We find that in the gain domain, being responsible for others as well as oneself does indeed increase risk aversion for medium to large probabilities, thus, showing that Bolton and Ockenfels' (2010) intuition was correct. In addition, we show that for pure loss prospects, subjects become more risk seeking when responsible for others. Loss aversion on the other hand, being already very strong in individual decisions, does not seem to increase when subjects are responsible for others. In a second experiment aimed at exploring social norms on risk taking in the gain domain in more detail, we replicate the finding that risk aversion increases under responsibility for large probabilities. When choices regard small probability prospects, however, we find increased *risk seeking* under conditions of responsibility. Overall, our results thus point to an accentuation of the fourfold pattern of risk attitudes typically found in individual decision making when subjects are responsible.

This chapter proceeds as follows. Section 1.2 discusses risk attitudes and how they may be influenced by social contexts. Section 1.3 describes the first experiment, with section 1.3.1 describing the methodology and section 1.3.2 presenting the results; section 1.3.3 discusses the result of experiment 1 and derives hypotheses for experiment 2. Section 1.4 introduces experiment 2, with section 1.4.1 describing the methodology and section 1.4.2 presenting the results. Section 1.4.3 discusses the results of experiment 2 as well as the overall results. Section 1.5 concludes this chapter.

1.2 Risk Attitudes in Social Contexts

In recent years, there has been a growing interest by economists in how social factors may influence decision making under risk (Bohnet *et al.*, 2008; Bolton and Ockenfels, 2010; Goeree and Yariv, 2008). Such *social factors* could take various forms, ranging from whether a decision is observed by somebody else or whether the decision maker observes somebody else's decision, to whether one's outcome depends on somebody else or whether one's decision influences the outcome of somebody else (Trautmann and Vieider, 2010). We are interested in the latter category: do preferences over risky choices change when the decision influences somebody else's outcomes as well as the ones of the decision maker? And if so, how?

To date there is very little evidence on this issue, with the existing evidence appearing inconclusive. Bolton and Ockenfels (2010) hypothesize that risk aversion will increase under responsibility. However, their result fails to reach statistical significance. Indeed, their main results concern the effect of social comparison, so that they mainly examine choice behavior when outcomes may differ between the decision maker and the recipient. They find an increase in risk taking under conditions of responsibility when the safe option yields unequal payoffs, and particularly when such payoff asymmetry is unfavorable to the decision maker. In contrast, they find that under responsibility risk taking does not depend on whether the risky option yields unequal payoffs.

In a somewhat related study from the game-theoretic literature, Charness and Jackson (2009) have subjects play Rousseau's stag hunting game against each other. They compare conditions in which one subject simply plays against another, to one in which a second, *passive*, subject depends on each player. They find that under responsibility for someone else the efficient equilibrium obtains less frequently. While this may again be an indication for increased risk aversion under responsibility, it is not clear where such a risk may actually come from since it is not in the interest of any of the players to deviate from the efficient equilibrium unless they think the other player may deviate. Furthermore, the setup of the study again creates issues of inequality aversion. Even if the passive recipient obtains the same payoffs as the decision maker, the strategic nature of the game implies that the decision maker can influence the payoffs of her opponent and the latter's passive recipient –which may affect her choices ex-ante.

We aim to specifically exclude inequality concerns to filter out the pure effect of being responsible for somebody else's payoffs. In order to achieve this, the exact choice that determines the decision maker's payoff also determines the recipient's payoff, resulting in exactly the same outcome for the decision maker and the recipient. This design thus allows us to isolate the effect of being responsible for somebody else as well as for oneself from any distributional issues (Rohde and Rohde, 2010). Furthermore, there are costs for the decision maker in adapting her preferences under conditions of responsibility in terms of sacrificing her own preferences. In this sense, we believe that our design constitutes a lower bound on any effects of responsibility that could be found employing alternative designs, such as salaried agents.

Given the lack of conclusive evidence to date, we propose to systematically explore the effect of responsibility on risk preferences throughout the outcome and probability domains. In order to facilitate that task, in what follows in this chapter we will adopt a behavioral, and hence theory-neutral, definition of risk aversion. A decision maker will be defined as risk averse whenever she prefers the expected value of a prospect to the prospect itself; conversely, she will be defined as risk seeking whenever she prefers the prospect to a sure amount equivalent to the prospect in terms of expected value (Wakker, 2010, p.52). Risk aversion and risk seeking are thus relative terms, such that a decrease in risk aversion can be seen as equivalent to an increase in risk seeking, regardless of absolute levels of risk taking[2].

[2]This means that saying that choices under condition A are more risk averse than under condition B is taken as equivalent to saying that they are less risk seeking under A than under B, regardless of the

In our presentation of the results we recur to prospect theory - the prevalent descriptive theory of choice under risk and uncertainty today (Starmer, 2000; Wakker, 2010). Under prospect theory, risk attitudes are described by utility curvature, loss aversion, and probability weighting (Köbberling and Wakker, 2005). Since prospect theory is more general than other theories of decisions under risk such as expected utility theory, we can thus capture richer risk attitudes if present, without however imposing a theory on our data a priori.

In individual decision making under risk, the typical finding is a fourfold pattern of risk attitudes: risk aversion for medium to large probabilities of gains; risk seeking for small probability gains; risk aversion for small probability losses; and risk seeking for medium to large probability losses (Abdellaoui, 2000; Abdellaoui *et al.*, 2010; Bleichrodt and Pinto, 2000; Tversky and Kahneman, 1992). In addition to this fourfold pattern, for mixed prospects involving both gains and losses, risk attitudes are significantly influenced by loss aversion - the phenomenon according to which monetary losses are usually attributed greater weights than equivalent monetary gains (Abdellaoui *et al.*, 2007; Schmidt and Zank, 2005; Tversky and Kahnemann, 1992).

The question of whether and how being responsible for others changes choice behavior also raises interesting questions about rationality concepts, social norms on risk taking, and the perceived acceptability of attitudes towards risks. This in turn has implications for *debiasing*, or simply changing risk attitudes in ways that may seem socially desirable. By comparing situations of individual decision making to situations of responsibility for different probabilities and in different domains, we are able to examine the perceived acceptability of common individual decision making patterns under risk. To the extent that being responsible for others may act as a cognitive motivator for more careful consideration of the decision alternatives, we can draw conclusions about the perceived acceptability of a type of behavior by observing if and in what direction people move from the individual baseline when responsible for others.

absolute level of risky or safe choices (i.e, regardless of whether safe choices are more or less than 50% in both cases, or whether they cross the 50% mark).

1.3 Experiment 1: Responsibility for Gains, Losses, and Mixed Prospects

1.3.1 Experimental Design

We designed a laboratory experiment in which we asked subjects to take binary decisions between two alternatives that are presented to them on a computer screen. Payoffs always affect the decision maker and the recipient in a perfectly parallel manner in the responsibility treatment, so as to avoid issues of payoff inequality (Bolton and Ockenfels, 2010; Rohde and Rohde, 2010).

Subjects: Overall, 144 subjects were recruited from a subject pool of the experimental laboratory MELESSA of the University of Munich, Germany, via ORSEE (Greiner, 2004). The experiment took roughly 1.5 hours, and average earnings were €22.49. The experiments were run on computers using z-Tree (Fischbacher, 2007). 46% of subjects were female, and the average age was 24.07 years.

Task: Subjects were asked to choose between a safe prospect and a risky prospect. The safe prospect usually consisted in a sure amount of money, and sometimes in a prospect with lower volatility compared to the risky prospect. The risky prospect always gave a 50–50 chance to obtain one of two outcomes. The prospects could comprise only positive amounts, only negative amounts, or both positive and negative amounts (see below). Overall, subjects had to make 40 choices, with the order of presentation as well as the position of the two prospects randomized for each subject. Subjects took decisions sequentially and had no opportunity to return to an earlier decision to revise it. All of the above was explained in the instructions (see appendix A1).

Prospects: The 40 choices to be made by all subjects in the experiment were constructed systematically in the following way: We chose five different stake levels that we denote henceforth by b where $b = \{2, 4, 6, 8, 10\}$. For every stake level, we had subjects chose between the following eight different prospect pairs:

Base Case: These prospect pairs offered a choice between the safe payment *b* and a prospect providing a 50% chance to win twice the safe amount *b* or zero otherwise.

Sensitivity up: Compared to the *base case*, the safe payment is increased by 25% to assess the degree of risk aversion of subjects. The risky option remained unchanged.

Sensitivity down: Similar to *Sensitivity up*, but the safe payment is reduced by 25%, again in order to measure the degree of risk aversion. The risky option remained unchanged.

Positive shift: Every amount is increased by 50% of the safe payment in the *base case*. These choices were included to see how choices changed when shifting away from the €0 outcome.

Lottery choice: The risky prospect now remains identical to the *base case*, but the safe payment is replaced by a prospect with a lower variance (0.5 b and 1.5 b) than the risky prospect (0 and 2 b)

Mixed prospects**:** To obtain these prospects, the safe amount in the *base case* was subtracted from all outcomes, thus obtaining a prospect with an expected value of €0. The safe amount was therefore always €0, the prospect always a lottery between -b and b.

Mean-preserving spread (MPS): To obtain this prospect, the two risky outcomes of the *base case* were respectively increased and decreased by 50% of the sure amount. The expected value of the prospect thus remains the same; however, the variance of the prospect increases, and a loss equal to 50% of the sure amount is introduced into the prospect.

Loss Shift: The mirror image of the base case where every amount was negative instead of positive. These prospects were inserted to directly compare risk taking behavior for gains and losses.[3]

Table 1-1 gives an overview of the eight different prospect pairs as a function of the stake level *b*.

[3]Additional prospect in the gain domain were not mirrored for ethical reasons - indeed, replicating all gain prospects for losses would have resulted in a high chance of overall losses during the experiment.

Choice Type	Option A ("safe")		Option B ("risky")	
	50%	50%	50%	50%
Base Case	b		0	2b
Sensitivity Up	1.25b		0	2b
Sensitivity Down	0.75b		0	2b
Positive Shift	1.5b		0.5b	2.5b
Lottery Choice	0.5b	1.5b	0	2b
Mixed Prospect	0		-b	B
MPS	b		-0.5b	2.5b
Loss Shift	-b		-2b	0

Table 1-1: Prospects as functions of stake level b

For a complete overview of all prospect pairs, see Table 1-6 in appendix A2.

Treatments: Subjects were randomly assigned to one of two treatments. In the *individual treatment*, subjects took their decisions only for themselves. In the *responsibility treatment*, half of the subjects were randomly assigned the role of *decision maker* and the other half to the role of passive *recipient*. The decision maker was told that she had to take the decision on behalf of herself and another subject sitting in the laboratory, whose identity was not disclosed. All other subjects were told that they were in a passive role and that somebody else in the laboratory would take the decisions on their behalf. With a lag of one period, recipients were shown the decision problem and the choice of their corresponding decision maker. They could then indicate whether they were "satisfied" or "not satisfied" with the decision, but this did not affect payoffs nor was it shown to the decision maker.

Incentives: 3 out of the 40 decisions were randomly drawn for every subject to be payoff relevant once the experiment was over. Subjects did not learn about any payoffs or extractions before the very end of the experiment. The random incentive system was chosen in order to avoid possible income effects, and because it is the standard procedure used in this kind of tasks. We extracted 3 out of the 40 choices in order to reduce the probability that subjects would actually lose money in the experiment. To make the random mechanism behind lotteries as transparent as possible, we had one participant throw a dice for every lottery that determined what outcome of the lottery is obtained. In the responsibility treatment, we implemented the payout procedure such that always three identical decisions were randomly chosen for the two paired subjects. A decision maker and her passive recipient would thus always obtain the same payoff from a choice. Subjects were told that it was possible - though unlikely - that they would lose money in the experiment. They could either pay such losses directly or work them off in the lab for a wage of €5 per half hour.

1.3.2 Results: Choices under Responsibility

Prospect Choices: overview

Before discussing treatment effects, it seems desirable to discuss general risk attitudes and how they change for the different types of prospects employed when we look at the individual treatment only. In the base case we find a considerable degree of risk aversion across all stake levels, with about 73% of subjects choosing the sure amount over the prospect with equal expected value ($p<0.001$[4]). As one would expect, choices of the sure amount further increase when the sure amount is higher than the expected value of the prospect (Sensitivity Up), and decrease when the sure amount is lower (Sensitivity Down) in which case we observe a majority of choices for the prospect ($p<0.01$). When, compared to the base case, all outcomes are moved upward by 50% of the sure amount (Positive Shift), we observe increased choices of the prospect, although choices still display significant risk aversion ($p<0.01$). This can be explained by aspiration level theory, whereby subjects aspire to win at least some money, thus making a prospect with a non-zero minimal outcome more attractive (Payne *et al.*, 1980; 1981).

When the choice is between two non-degenerate prospects (Lottery Choice), choice frequencies of the safe prospect are further increased relative to the base case, indicating a similar heuristic, since the safe choice now provides a combination between a safe minimum amount and a potentially higher outcome. For mixed prospects, the choice frequency of safe choices is only slightly increased compared to the base case (this, however, underestimates the effect given the lowering of the stake levels: see below as well as appendix A3 for a more nuanced discussion). For the mean-preserving spread, choices of the risky prospect increase, but risk aversion remains the dominant pattern ($p<0.001$). This may indicate that the increase in the good outcome more than makes up for the slight loss that has been introduced in the

[4]P-values reported are two-sided and refer to binomial tests for intermediate stakes, with a safe amount of $b=6$, unless specified otherwise.

bad outcome. Finally, for pure loss choices, subjects are considerably more risk seeking than for gains, and in absolute terms risk neutrality cannot be rejected (p=0.19).

It is also commonly found in the literature that risk attitudes are influenced by stake levels (Abdellaoui *et al.*, 2011; Binswanger, 1980; Holt and Laury, 2002; Kachelmeier and Shehata, 1992). We thus take a look at the influence of the different stake levels on decisions. Figure 1-1 shows choices for the safe alternative separately for the basic case and the pure loss pairs.

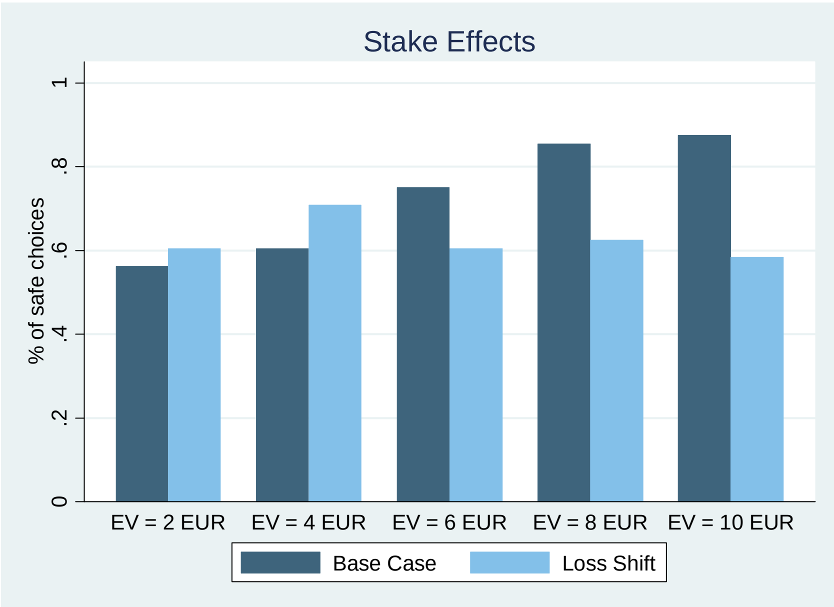


Figure 1-1: Choice frequency of the safe prospect - Stake effects for gains and losses

The stake effect is clearly visible for the gain prospects, with increasing expected values resulting in increased levels of risk aversion. Indeed, we cannot reject risk neutrality for the lowest stakes (p=0.47), with risk aversion increasing with stake levels and being highly significant for the highest stake level (p<0.001). For losses, on the other hand, there is no clear trend and risk aversion has only a very slight (and non-significant: p=0.31 for the highest stake level) tendency to increase with absolute stake values[5.] A parametric analysis of these descriptive results can be found in appendix A3. We next turn to the differences between the individual and the responsibility treatment.

[5]The Spearman correlation coefficient between the stake size b and choice for the safe option in the individual treatment is indeed significantly positive for the base case (p<0.001), but not different from zero for the loss shift (p=0.57).

Individual decisions versus Responsibility

Figure 1-2 shows choice frequencies for the safe prospect by treatment, for males and females respectively[6].

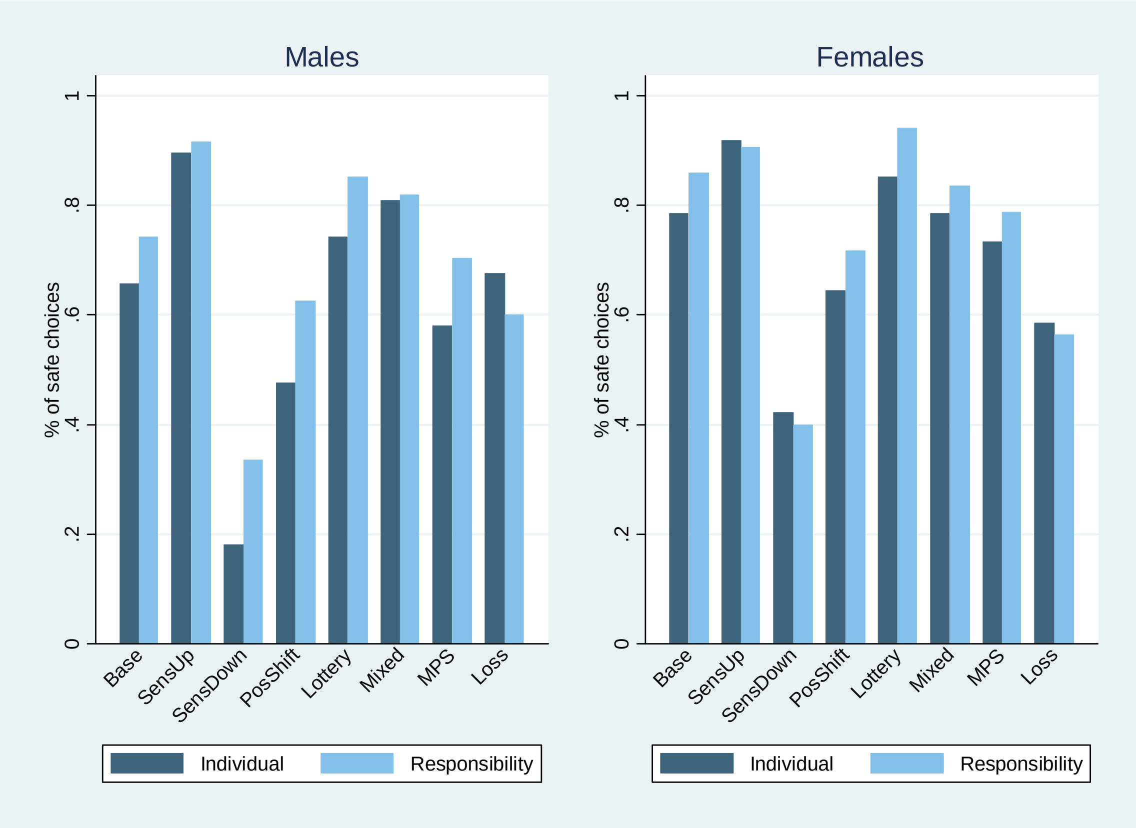


Figure 1-2: Choice frequency of the safe prospect for different prospect pairs, by treatment

One can clearly see how for the base case subjects are more risk averse under responsibility than in the individual decisions - this holds both for males and females. The same tendency is visible in almost all other positive prospect pairs, except for the upward sensitivity prospect pair, in which there is no difference. There is only a very slight indication of responsibility inducing more risk aversion in the mixed prospect pair, while this tendency is again more pronounced for the mean-preserving spread (MPS) pair. For pure loss choices, however, the tendency is inverted, with responsibility decreasing risk aversion.

[6]We display the effects by gender because of the large gender effects in risk taking typically found in the literature (Donkers *et al.*, 2001; Eckel and Grossman, 2008), which are also present in our data.

Table 1-2 presents a random effects probit model regressing choices for the safe prospect on a variety of explanatory variables. Regression I regresses choices on the treatment dummy, a dummy variable indicating the pure loss prospects, a dummy indicating mixed prospects, and two interaction terms between the latter two and the treatment dummy.

Dep. Var.: choice of safe prospect	**I**	**II**	**III**
responsibility	0.070* (0.036)	0.080* (0.037)	0.099* (0.049)
pure loss	–0.043* (0.033)	–0.067* (0.033)	–0.067* (0.033)
responsibility × pure loss	–0.098* (0.049)	–0.106* (0.049)	–0.106* (0.049)
mixed prospect	0.131*** (0.027)	0.112*** (0.027)	0.112*** (0.027)
responsibility × mixed prospect	–0.024 (0.051)	–0.032 (0.051)	–0.032 (0.051)
EV difference		0.196*** (0.012)	0.197*** (0.012)
SD difference		0.022*** (0.002)	0.022*** (0.002)
female	0.083* (0.033)	0.086* (0.036)	0.107* (0.049)
responsibility × female			–0.046 (0.076)
constant	✓	✓	✓
# observations (# subjects)	3840 (96)	3840 (96)	3840 (96)
Wald Chi2	64.43	417.03	417.27

Random effects probit regressions: coefficients show marginal effects relative to choices in the individual treatment; standard errors in parenthesis; ×: interaction; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.

Table 1-2: Experiment 1 – Choice of safe prospect

Being responsible for somebody else's payoffs as well as one's own increases risk aversion relative to affecting only one's own payoffs; the latter is a *simple main effect*, indicating the effect of responsibility for all prospects except the pure loss prospects (i.e., with the pure loss dummy held *constant at zero*) and the mixed prospect (i.e., with the mixed dummy held *constant at zero*). The effect of the pure loss dummy indicates that for pure loss prospects subjects are more risk seeking compared to all other gain prospects. The interaction between the treatment dummy and the one identifying pure loss prospects indicates that for pure loss prospects the effect of responsibility goes in the opposite direction compared to pure gain prospects, and thus shows that subjects in the responsibility treatment are *more risk seeking* (or *less risk averse*) for losses compared to subjects in the individual treatment. The significant effect of the mixed-prospect dummy shows that subjects choose the safe option significantly more often for the mixed prospect than for pure gain prospects. The insignificant interaction between the treatment dummy and the mixed dummy on the other hand indicates that there is no significant treatment effect for mixed prospects, with the effect thus going in the same direction as for gains. Finally, we also find that females are significantly more risk averse than males. Such an effect is commonly found for decision making under risk (Donkers *et al.*, 2001; Eckel and Grossman, 2008).

Regression II keeps the same independent variables as regression I, and adds the difference in expected value (defined as the expected value of the safe prospect minus the expected value of the risky prospect) and the difference in standard deviations (defined as the standard deviation of the risky prospect minus the standard deviation of the safe prospect, which is thus always positive). The higher the difference between the safe prospect and the risky prospect in terms of expected value, the more likely subjects will choose the safe prospect. Also, the larger the difference in terms of standard deviation, the more likely subjects are to choose the safer alternative. The main treatment effects discussed above are stable, indicating increased risk aversion under responsibility in the gain domain, increased risk seeking in the loss domain, and no treatment effect in the mixed domain.

Regression III further adds an interaction term between the gender dummy and the treatment dummy. The effect is not significant, which goes to show that being responsible for somebody else does affect males and females in the same way. Once again, all the effects previously discussed remain stable. We next turn to the analysis of the satisfaction ratings of recipients in the responsibility treatment.

Choice satisfaction of recipients

In the responsibility treatment, recipients saw the decision maker's choice with one period lag and indicated whether they were satisfied with the decision or not. Although this rating was not incentivized, it may nevertheless give an indication of the extent to which decision makers adapted their decision to the commonly acceptable one, or correctly intuited which decision would be deemed more acceptable while doing so. Since satisfaction ratings were not communicated to the decision maker and had no influence on payoffs whatsoever, recipients had indeed no reasons to systematically misrepresent their preferences. Also, the fact that providing such ratings was the only occupation of recipients during the experiments leads us to suspect that they took this task seriously.

Table 1-3 shows a random effects probit model regressing the recipients' satisfaction with each choice on a number of independent variables. The highly significant effect of the safe prospect being chosen by the decision maker shows that safe choices are deemed more satisfactory in the gain domain (this being a simple main effect measuring the effect of safe choices with the pure loss dummy held constant at zero). While the fact that a prospect offers only negative outcomes per se does not affect satisfaction ratings, choosing the safe amount in pure loss prospects is generally not perceived as satisfactory by recipients, as shown by the highly significant interaction effects of the pure loss and safe choice dummies. This finding confirms that risk seeking is deemed more acceptable than safe choices in the loss domain. There is no main gender effect for satisfaction ratings.

Regression II confirms the stability of the findings we have just discussed, and adds some more variables. The significantly negative main effect of the mixed prospect dummy indicates that choices of the prospect are considered even less satisfactory in the mixed domain as compared to the gain domain. In a parallel fashion, satisfaction increases relative to the pure gain domain when a safe amount is chosen, giving again an indication of loss aversion on the side of recipients. Choices are deemed more satisfactory the higher the difference in expected value, providing an indication that higher differences in expected value increase the agreement between decision makers and recipients on which choice is the best one. Finally, in keeping with previous findings on gender effects, women generally deem choices of the safe prospect as more satisfactory than choices of the risky prospect.

Dep. Var.: satisfied with decision	**I**	**II**
safe prospect chosen	0.346*** (0.028)	0.208*** (0.038)
pure loss	0.046 (0.036)	–0.002 (0.040)
pure loss × safe prospect chosen	–0.268*** (0.075)	–0.174* (0.072)
mixed prospect		–0.227** (0.080)
mixed prospect × safe prospect chosen		0.187*** (0.286)
EV difference		0.068*** (0.012)
female	0.034 (0.032)	–0.016 (0.039)
female × safe choice		0.078* (0.037)
constant	✓	✓
# observations (# subjects)	1920 (48)	1920 (48)
Wald Chi2	196.08	230.93

Random effects probit regressions: coefficients indicate marginal changes in satisfaction levels relative to a choice of the risky prospect; standard errors in parenthesis; ×: interaction; ***represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.

Table 1-3: Experiment 1 – Satisfaction rating

At the end of the experiment we asked subjects to rate their degree of risk aversion on a scale from being very risk seeking (1) to being very risk averse (6). This self-declared risk aversion correlates strongly with the number of safe choices taken in non-negative prospect pairs during the experiment itself on the basis of the Spearman correlation coefficient (p=0.01) across both treatments. Self-declared risk attitudes are not significantly different between the two treatments (p=0.26; Mann-Whitney test, two-sided), nor is there a significant difference between decision makers and recipients in the responsibility treatment (p=0.72; Mann-Whitney test, two-sided). Finally, we also asked subjects to rate themselves according to their risk aversion relative to other participants in the experiment. The rating went from 1 (indicating that a subject considered herself to be amongst the four most risk-loving participants in the session of 24) to 6 (indicating that a subject considered herself to be amongst the four most risk averse participants in the session). On average, decision makers in the responsibility treatment had a rating of 4.17, indicating that they considered themselves more risk averse than the median participant in the experiment, and thus ruling out that they may have considered recipients on average to be more risk averse than they are themselves. This finding corresponds to existing evidence according to which subjects generally consider others as more risk loving than themselves (Hsee and Weber, 1997).

1.3.3 Discussion

For gain prospects, we find that responsibility increases risk aversion. An account based on the assumption that decision makers consider others to be more risk averse than they are themselves seems to be ruled out by the answers to the relative risk attitude ranking questions discussed above. Also, Hsee and Weber (1997) found that in a series of different experimental designs subjects systematically predicted others to be less risk averse than themselves. We can thus conclude that subjects do not simply try to adapt their decisions to what they think may be others' risk attitudes.

A different possibility is that subjects comply with an implicit social rule dictating increased caution when responsible for somebody else as well as oneself, thus increasing their risk aversion when responsible for somebody else. This explanation is distinct from the argument discussed in the last paragraph, inasmuch as such a social norm may push subjects to be more risk averse when deciding for others even in cases they expect others to be more risk loving than themselves if left to decide for themselves. Such a *cautious shift* explanation, however, cannot explain our increased risk seeking for loss prospects. Arguably, different social rules dictating a cautious shift for gains and a *risky shift* for losses could well exist, but such a hypothesis does have a distinctly *ad hoc* flavor. Given that individual risk attitudes have been established to be much richer than the simple risk-aversion/risk-seeking dichotomy implicit in such explanations (Abdellaoui, 2000; Abdellaoui *et al.* 2010; Bleichrodt and Pinto, 2000), we rather hypothesize that risk attitudes typically found in individual decision making are accentuated under conditions of responsibility.

Prospect theory would predict risk aversion to prevail both for medium and large probabilities. Thus, a theory based on the amplification of the fourfold pattern of risk attitudes predicted by prospect theory cannot be separated from an account based on a social rule favoring increased risk aversion under responsibility when considering the evidence collected in the gain domain only. Risk seeking, however, seems to appear more acceptable than risk aversion in the loss domain for the medium probabilities used in our experiment. Evidence in this direction comes both from the behavior of decision makers, who under conditions of responsibility in the loss domain are induced to become more risk seeking rather than more risk averse; and from recipients, who are much more likely to be dissatisfied with a decision in the loss domain when the decision maker chose the sure loss rather than the prospect. This, in turn, cannot be explained by a uniform social norm dictating increased caution under conditions of responsibility.

We thus propose as an alternative hypothesis that the fourfold pattern of risk attitudes predicted by prospect theory - risk aversion for medium to large probability gains and small probability losses, risk seeking for medium to large probability losses and small probability gains - is amplified by responsibility. At this point, the hypothesis that responsibility accentuates the fourfold pattern may be no more plausible than the already discussed hypothesis of different social norms for decisions under gains and under losses. Luckily however, there is a possibility to disentangle such different explanations. The hypothesis of an accentuated fourfold pattern of risk attitudes as found in prospect theory and the social norm argument make very different predictions for different probability levels in the gain domain, making it easy to test them against each other. For large probabilities, both prospect theory and the social norm argument predict an increase in risk aversion under conditions of responsibility. For small probabilities, on the other hand, the social norm hypothesis still predicts an increase in risk aversion; quite to the contrary, however, prospect theory and the argument of an amplification of the fourfold pattern laid out above now predict an *increase in risk seeking* under conditions of responsibility.

The same test can also be adopted to rule out yet another alternative explanation that we cannot rule out on the basis of the results from above. When deciding for others as well as themselves - so the objection goes - decision makers effectively decide over twice the amount of money. Given the common finding that risk aversion increases in stake levels, the increased amounts over which decisions are taken may thus well be the factor underlying the finding of increased risk aversion in the responsibility treatment, rather than the responsibility effect itself. This explanation is indeed plausible for the medium probability gains used in experiment 1 (although it cannot account for the findings for loss prospects). Notice, however, how this explanation would again predict increased *risk aversion* for small probability gains under higher stakes, which has been found repeatedly (Kachelmaier and Shehata, 1992; Lefebvre *et al.*, 2010). We thus now proceed to testing the effect of responsibility on decisions for different probability levels in the gain domain.

1.4 Experiment 2: Disentangling Social Norm and Amplification Accounts

1.4.1 Experimental Design

Subjects: 180 subjects were recruited from a subject pool of the experimental laboratory MELESSA at Ludwig-Maximilian's University in Munich, Germany, using ORSEE (Greiner, 2004). The experiment was run together with another, unrelated, experiment. 59% of subjects were female, and the average age was 23.88 years.

Task: This task was run after another, unrelated experiment[7]. Subjects were asked to choose between a safe option and a risky option in a fashion similar to experiment 1. However, we now only looked at choices in the gain domain. The safe option always consisted in a sure amount of money, while the prospect provides a chance of either 10% or 90% to win €10. Overall, subjects had to make 10 choices where the order of presentation was randomized for every subject. Subjects took decisions sequentially and had no opportunity to return to an earlier decision to revise it.

Prospect: The choice was always between a sure amount of money and a prospect. There were two prospects, one providing a 10% chance to win €10 and €0 otherwise; and one providing a 90% chance to win €10 and €0 otherwise. The sure amount could take one of five different amounts for each prospect: €0.8, €1, €1.2, €1.5 and €2 for the 10% prospect, and €7, €8, €8.5 €9, and €9.5 for the 90% prospect.

[7] Although the preceding experiment was unrelated, care was taken to distribute the treatments of this experiment orthogonally to the treatments in the other experiment.

Treatments: Subjects were randomly assigned to one of two treatments that exactly replicated those of experiment 1: an *individual treatment* in which subjects took their decisions only for themselves; or a *responsibility treatment*, in which half of the subjects were randomly assigned the role of decision maker and half the subjects were assigned the role of passive recipient.

Incentives: One decision was randomly extracted to be played for real pay. Since in the unrelated experiment subjects could obtain at least an approximate knowledge about their payoffs, we decided to fully reveal earnings from this experiment in order to be able to control for the exact income effect in a regression (rather than having unknown perceptions of earnings).

1.4.2 Results

Individual decisions versus decisions under responsibility

Figure 1-3 displays the choice frequencies by treatment separately for small and large probabilities. On average we find the typical pattern of risk seeking for small probabilities and risk aversion for large probabilities. Indeed, when the subjects face a choice between a prospect and a sure amount of equal expected value, only about 27% of subjects choose the sure amount for the 10% probability ($p<0.001$, binomial test), while 99% of subjects do so for the 90% probability ($p<0.001$, binomial test). For the 10% probability, subjects who are responsible for somebody else choose the sure amount *less often* for all but the smallest two certain amounts, where choices of the safe amount are generally low. For the 90% probability, responsible subjects always choose the sure amount at least as often as subjects who only decide for themselves.

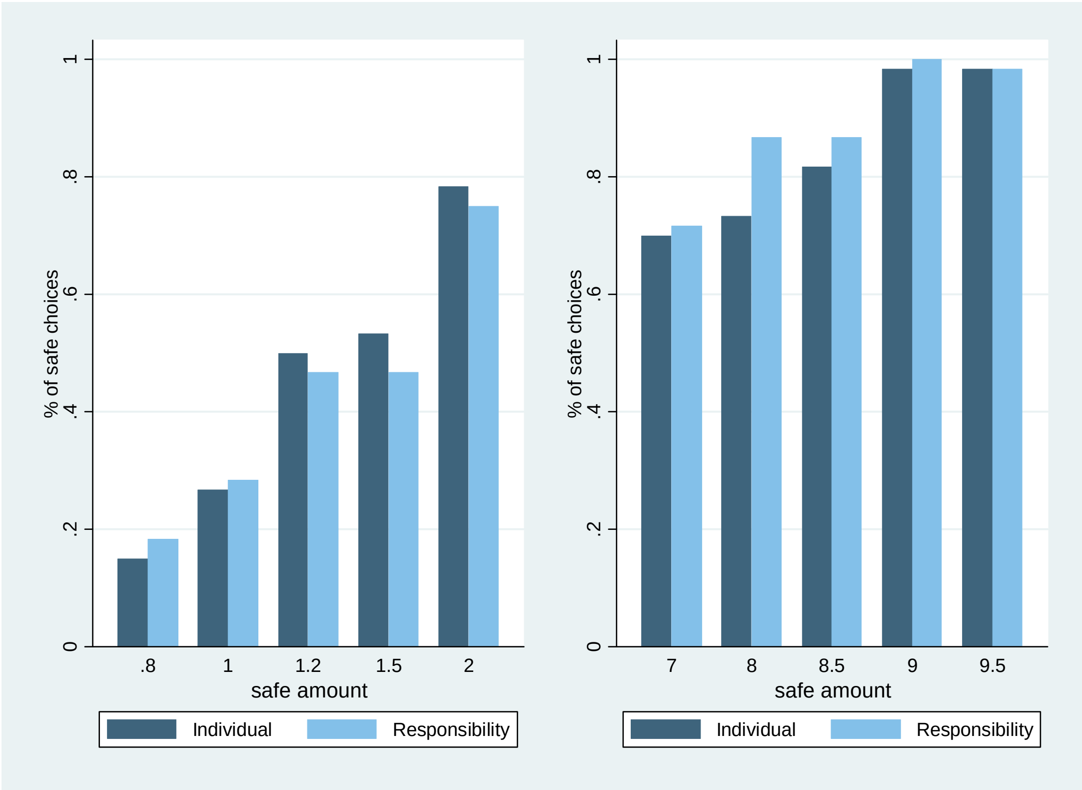


Figure 1-3: Choices of safe amount by treatment for p=0.1 (left) and for p=0.9 (right)

Table 1-4 presents a random effects probit model regressing choices of the safe alternative on a variety of explanatory variables. The effect of the responsibility treatment dummy now indicates the simple main effect of being responsible when probabilities are large (Jaccard and Turrisi, 2003). Subjects are thus more likely to choose the sure amount for a 90% probability of winning when responsible compared to the individual treatment. Under small probabilities, subjects are significantly more risk seeking than under large probabilities, as indicated by the highly significant effect of the small probability dummy. More importantly, the interaction of the small-probability dummy with the treatment dummy indicates that this risk-seeking tendency is further enhanced relative to the individual treatment when subjects are responsible for somebody else. As presumably expected, the difference in expected value between the sure amount and the prospect (defined as in experiment 1) is also highly significant. Finally, we find a significant, if small, income effect, which goes as expected in the direction of increased risk seeking by subjects who have realized higher earnings from the previous experiment.

Dep. Var.: choice of safe prospect	**I**	**II**
responsibility	.107† (0.058)	0.148† (0.086)
small probability	–0.666*** (0.036)	–0.589*** (0.047)
small probability × responsibility	–0.135* (0.068)	–0.133* (0.068)
EV difference	0.295*** (0.025)	0.295*** (0.025)
female	0.071 (0.044)	0.197*** (0.061)
past profit	–0.008*** (0.002)	–0.007* (0.003)
female × small probability		–0.209** (0.068)
past profit × responsibility		–0.003 (0.005)
constant	✓	✓
# observations (# subjects)	1200 (120)	1200 (120)
Wald Chi2	264.55	263.57

Random effects probit regressions: coefficients show marginal effects relative to choices in the individual treatment; standard errors in parenthesis; ×: interaction; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.

Table 1-4: Experiment 2 – Choice of safe prospect

Regression II adds two further interaction terms. Almost all effects can be seen to be stable. The gender effect, which had not been significant in regression I, is now also significant: since this is a simple effect, the positive effect of the female dummy now indicates increased risk aversion by females relative to males for large probability prospects. This effect is qualified by the interaction of the female dummy with the small-probability dummy. The negative effect of that interaction shows that females are significantly *more risk seeking* relative to males for small probabilities. Past profits remain significant, though less so than in regression I. Most importantly, however, there is no significant interaction effect between past profits from the preceding experiment and our treatment manipulation, showing that this is not interfering with our results.

Satisfaction ratings

Exactly as in experiment 1, recipients in experiment 2 saw the decisions of their assigned decision maker with a lag of one period, and had to indicate whether they were satisfied with the decision or not. Table 1-5 reports the results of a random effects probit model, regressing the satisfaction dummy on a number of explanatory variables.

Dep. Var.: satisfied with choice	**I**	**II**
safe choice	0.530*** (0.119)	0.458*** (0.129)
small probability (10%)	0.226* (0.113)	0.238* (0.113)
safe choice × small probability	–0.658*** (0.130)	–0.680*** (0.126)
EV difference	–0.228*** (0.064)	–0.233*** (0.064)
EV difference × safe choice	0.341*** (0.072)	0.345*** (0.071)
female	0.124** (0.044)	0.023 (0.060)
female × safe choice		0.146* (0.064)
past profit		–0.002 (0.003)
constant	✓	✓
# Observation (# subjects)	600 (60)	600 (60)
Wald Chi2	61.70	64.59

Random effects probit regressions: coefficients indicate marginal changes in satisfaction levels relative to a choice of the risky prospect; standard errors in parenthesis; ×: interaction; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.

Table 1-5: Experiment 2 – Satisfaction ratings

The first dummy shows the simple main effect of choosing the safe amount over the large probability prospect: choosing the safe amount for large probability prospects is deemed much more satisfactory in general than choosing the prospect. The dummy indicating the simple main effect of a small probability choice is also positive, indicating considerable agreement with choices of the prospect in this instance.[8] However, choosing the safe amount for small probability prospects is considered to be very dissatisfying, as shown by the large negative coefficient of the interaction effect. Recipients are in general less satisfied with choices of the prospect the closer the safe amount is to the expected value of the prospect, which is indicated by the simple effect of the relative dummy. For large probabilities they are, however, more satisfied with a choice of the safe alternative for relatively small deviations in expected value than for small probabilities. Females tend to be much more satisfied when the safe amount is chosen for the large probability prospects, while past profits of the recipients have no influence on satisfaction ratings.

1.4.3 Discussion

The social norm hypothesis and the amplification of fourfold pattern hypothesis make very different predictions on behavior for small probabilities in the gain domain. While for large probabilities both theories predict an increase in risk aversion under responsibility, for small probabilities the social norm argument predicts a cautious shift towards increased risk aversion (or reduced risk seeking), whereas the amplification argument predicts increased risk seeking. Having directly tested these contradictory predictions in experiment 2, we conclude that the social norm dictating a cautious shift under conditions of responsibility has been discredited as an explanation of the results. In contrast, an increased fourfold pattern of risk attitudes explains our results well. At the same time, this finding also excludes explanations based on which our initial effects could have been due to stake effects rather than responsibility.

While an accentuation of the fourfold pattern of risk attitudes is a good fit for our results, we have not fully proven that such an accentuation takes place. Indeed, we miss results for small probability losses. While such an additional result may seem desirable, our experiment was designed with the explicit purpose of testing two different predictions in the gain domain against each other. While the fourfold-

[8] Indeed, the dummy indicates the satisfaction levels for small probabilities with all interactions which include this dummy held constant *at zero* (Jaccard and Turrisi, 2003). This in turn means that the safe choice dummy must be zero, thus resulting in the interpretation that the effect indicates satisfaction with choices of the prospect; this satisfaction in turn is measured relative to the (much fewer) choices of the prospect for the large probability prospect.

pattern hypothesis finds strong evidence in our data, it is not impossible that a different explanation could exist for our results. Indeed, even if the interpretation of an increase in the fourfold pattern of risk attitudes prevails - or at least an increase in typical risk attitudes found at the individual level - such an interpretation is merely descriptive in nature. The more fundamental question remains *why* we observe such a shift in risk attitudes under responsibility.

We can only speculate about the answer at this point. One possibility is to examine the finding in the light of Wegener and Petty's (1995) flexible self-correction model. The model postulates that people may shift away from their *natural* or spontaneous behavior when motivated to do so. The extent to which they correct their behavior, however, as well as the direction in which they correct it, will fundamentally depend on their naïve theory of the bias. This explanation appears however highly unsatisfying, given that there is no way of determining what such unconsciously determined naïve theories of bias may be - with the consequence that such an account could be used to ex-post justify any kind of behavior that one may find.

The fact that typical individual risk attitudes are accentuated under conditions of responsibility provides an indication that increased responsibility does by no means push decisions closer to expected utility maximization - generally held to be normative - but rather farther away from it. There seems, however, to be general agreement on this tendency, as indicated by our satisfaction rating patterns. Indeed, in experiment 1 we found recipients to be generally satisfied with safe choices in the gain domain, but dissatisfied with such choices in the loss domain. Given that safe choices have already been found to decrease under conditions of responsibility in the loss domain, this is indeed a strong indication for the perceived social acceptance (or at least desirability) of such choices. A similar pattern can be seen in experiment 2, where safe choices were deemed satisfactory for the large probability prospect, but very unsatisfactory for the small probability prospect.

Whatever the psychological reasons behind our findings may be, the mere economic fact of more extreme patterns under responsibility remains. Such factors may have important consequences for economic predictions and for policy design. Probability weighting - from which the fourfold pattern is thought to derive to a large extent - has been used to explain the simultaneous take-up of insurance and lottery play (Wakker, 2010). The fourfold pattern of risk attitudes has also been used to explain reference point effects that have been observed in financial markets (Baucells *et al.*, 2011; Wiseman and Gomez-Mejia, 1998) and for investment behavior by firms (Fiegenbaum, 1990; Fiegenbaum and Thomas, 1988). Our results provide a further indication that typical risk attitudes found for individuals may not only generalize to professional agents or firms, but even be reinforced to some extent. Given that these patterns seem very resilient to debiasing, explicit rules may be needed to rein in excessive risk taking in certain conditions, or special training programs for managers may seem desirable in order for them not to fall prey to automatic decision making patterns that may be suboptimal from the point of view of the company that they manage.

1.5 Conclusion

In this chapter, we systematically explored decision situations in which a decision maker bears responsibility for somebody else's outcomes as well as for her own. In the gain domain, and for medium to large probabilities, we confirmed the intuition that being responsible for somebody else's payoffs increases risk aversion. Looking at risk attitudes in the loss domain, however, we found an increase in risk seeking under conditions of responsibility.

This raises issues about the extent to which changed behavior under responsibility may depend on a social norm of caution in situations of responsibility, or to what extent pre-existing risk attitudes found at the individual level may simply be enhanced under responsibility. To further explore this issue, we designed a second experiment to explore risk-taking behavior for gain prospects offering very small or very large probabilities of winning. For large probabilities, we found increased risk aversion, thus confirming our earlier finding. For small probabilities, on the other hand, we found an increase of risk seeking under conditions of responsibility. The latter finding thus discredits hypotheses of a social rule dictating caution under responsibility, and points towards an amplification in the fourfold pattern of risk attitudes found for individual decisions.

At the present point we can only speculate on what may underlie such an amplification of individual risk attitudes. Additional evidence - possibly from neighboring disciplines such as neuroscience - will probably be needed to fully understand the underlying dynamics. Nevertheless, our findings point out how important and resilient to debiasing these risk attitudes are, and hence the importance of considering them in policy design or for the training and supervising of decision makers.

1.6 Appendix

A1. Instructions *(translated from German)*

Welcome to the experiment and many thanks for your participation!
Please stop talking to other participants of the experiment from now on

General rules concerning the procedure

This experiment serves the investigation of economic decision making. You can earn money which will be paid to you in cash after the experiment.
During the experiment you and all other participants will be asked to make decisions.
In total, the experiment lasts for approximately **1 hour and 30 minutes**. Please raise your hand in case you have any questions during the experiment. One of the experimenters will then come to you and answer your questions in private. In the interest of clarity, we use male terms only in the instructions.

Payment
You receive €4 for arriving in time in addition to your earnings from the experiment. There is a possibility that you suffer losses from specific decisions. Possible losses must be offset with your earnings from other decision situations and/or with your €4 starting balance.
In (the very unlikely) case of an overall loss from the experiment, you may choose between paying it back in cash or by working as an assistant in the laboratory (€5 per half an hour).

Support
You are provided with a pen on your desk.
Please type your decisions into the computer. While making your decisions, there is a clock counting down in the right upper corner of your computer screen. This clock serves as a guide for how much time it should take for you to make your decisions. Of course, you are allowed to exceed the time; particularly in the beginning, this may be happening quite frequently.

Lottery decision making

[IND: You do not interact with other participants of the experiment at any point during the experiment. Your final payment is determined exclusively by your own decisions and according to the rules explained in the following. Other participants do not find out about your decisions and about how much you have earned at any point during or after the experiment. In the same manner, you do not learn about other participants' decisions and their earnings at any point during or after the experiment.]

[RESP: You will be matched with another participant of the experiment. Your decisions <u>or</u> the decisions of the other participant determine your payment according to the rules explained in the following. At no point during or after the experiment other participants in the experiment learn your identity. In the same manner, you do not find out the identity of other participants at any point during or after the experiment.]

Task
[RESP: There are two types of participants, **type A and type B**. The matching is such that a type A person is always matched with a type B person. At the beginning your computer screen will tell you which type you are. **The decision on which type you are is made randomly by the computer**. You will remain the same type throughout the experiment.

Decisions are made by type A only. Participants of type A make their decision **for themselves and at the same time for their matching partner of type B**. This means that **every decision that applies for type A applies to his matching partner of type B in exactly the same way**.]

In total, there are 40 periods. [IND: You] [RESP: Type A persons] have to make **one decision <u>per</u> <u>period</u>** for which [IND: you] [RESP: they] always have to choose between two alternatives.

A representative decision scenario may look like the following:

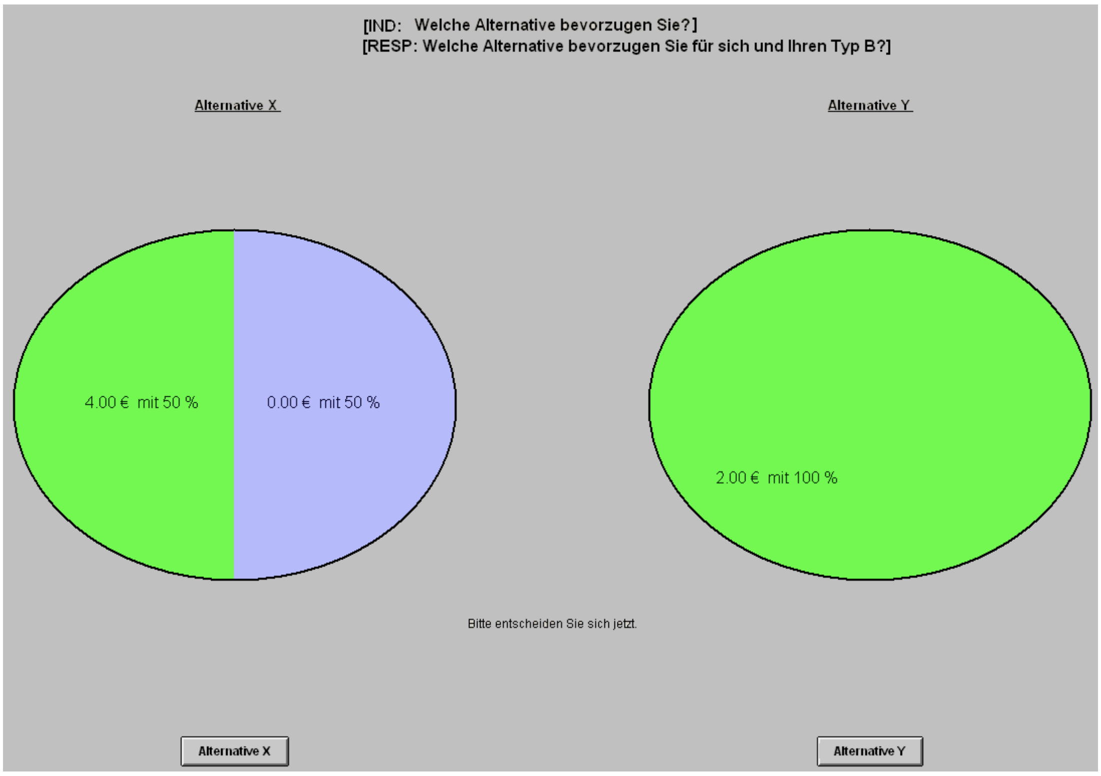


Figure 1-4: Screenshot – Gain domain

In the above example, [IND: you have] [RESP: type A player has] the choice between **<u>alternative X</u>**, that yields **€4** with a probability of **50%** and **€0** with the complementary probability of **50%** [IND: to you] [RESP: to him and to his matching partner of type B], and **<u>alternative Y</u>**, that yields **€2** with a probability of **100%** [IND: to you] [RESP: to him and to his matching partner of type B]. [IND: You decide] [RESP: Type A player decides] on one of the two alternatives by clicking on either the button “Alternative X” or the button “Alternative Y” below the pie charts.

An alternative such as alternative Y from the above example is called a **“certain payment”** since it is paid out with a probability of 100%. An alternative such as alternative X is called **“lottery”** since one amount is paid out with a probability of 50 % and another amount is paid out with a probability of 50%.
The alternatives between which [IND: you have] [RESP: type A has] to choose in each period either represent a **choice between a certain payment and a lottery**, or a **choice between two different lotteries**. In both alternatives there may be **positive as well as negative amounts** involved.

A decision scenario involving negative amounts may look like the following:

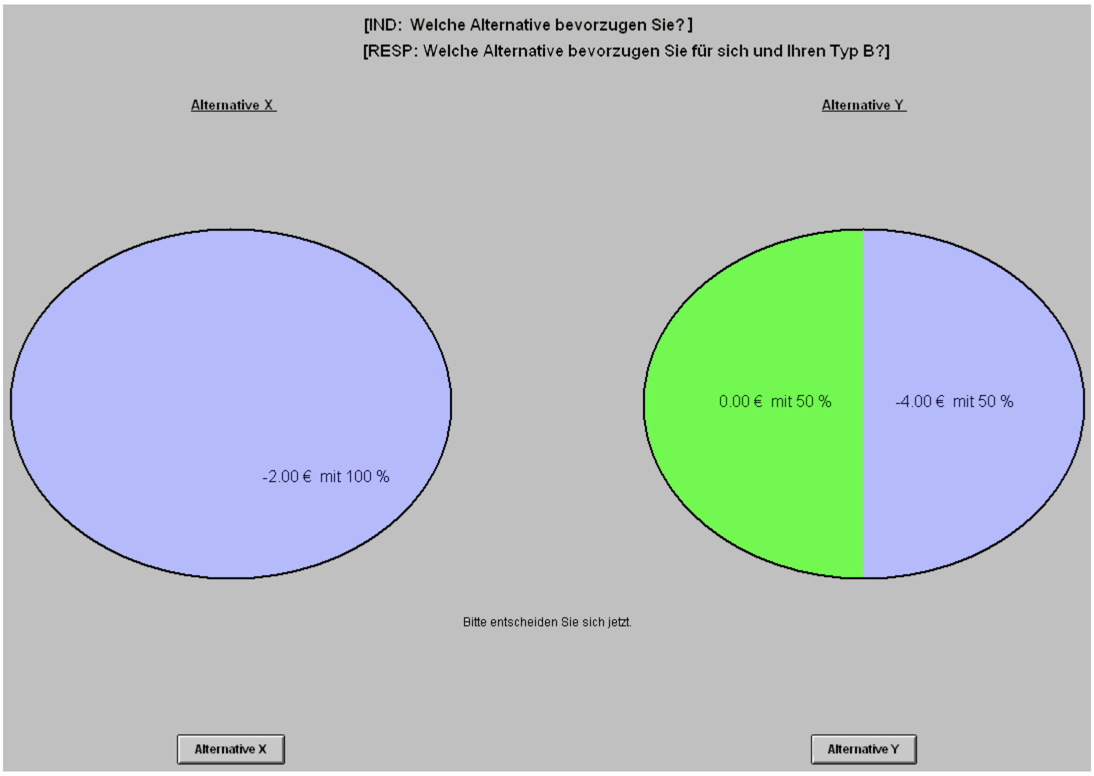


Figure 1-5: Screenshot – Loss domain

In this example, [IND: you have] [RESP: type A player has] a choice between **alternative X**, that yields **€-2** (a loss of €2) with a probability of **100%** [IND: to you] [RESP: to him and to his matching partner of type B], and **alternative Y**, that yields **€0** with a probability of **50%** and **€-4** (a loss of €4) with a complementary probability of **50%** [IND: to you] [RESP: to him and his matching partner of type B].

[RESP: **Type B players are provided with the information on the decisions of their type A partner with a lag of one period**. This means that type B players see the decision scenario on their screens with which their type A partner was confronted in the previous period and are told the alternative which their type A partner chose. Finally, type B players can indicate whether they were "content" with the decision or "not content". The statements of contentment do not influence type B's earnings or the earnings of his type A partner. **The statements of contentment do not get passed on to type A.]**

Please note: Carefully check the alternatives that you can choose from. Pay attention to the corresponding **signs** of the amounts since they can be **negative or positive**.

Payment
[IND: It is in your interest **to think thoroughly about each decision** because **each single decision may determine your payment** at the end of the experiment.]
[RESP: If you are a type A player, it is in your interest **to think thoroughly about each decision** because **each single decision may determine your payment** <u>as well as</u> **the payment of your type B partner** at the end of the experiment.]
This happens as follows:

To determine final payments the computer randomly selects **three different periods that are relevant for the payment** **<u>at the end of the experiment</u>**. Each period is **equally likely to be selected** by the computer. The sum of the earnings from the three selected periods determines [IND: your final payment] [RESP: the final payment for type A as well as for this type B partner].
[IND: On your screen you get told which periods got selected at random and how you chose in these periods.]
[RESP: All participants are told on their screens which periods got selected at random and how type A chose in these periods.]

In case [IND: you] [RESP: type A] chose a **<u>certain payment</u>** in a selected decision period, [IND: you] [RESP: type A and his type B partner] receive the amount of the certain payment as [IND: your] [RESP: their] earning from this selected period.

In case [IND: you] [RESP: type A] chose a **<u>lottery</u>**, the outcome of the lottery has to be determined first. To this end, lottery numbers from 1 to 6 get assigned to the possible earning amounts. As there are only lotteries involving probabilities of 50%, lottery numbers 1, 2 and 3 get assigned to one amount and lottery numbers 4, 5 and 6 get assigned to the other amount. The computer randomly determines which amount gets assigned to the low numbers and which amount gets assigned to the high numbers. Finally, a **randomly chosen participant is asked to roll a 6-sided die in public**. The amount corresponding to the lottery number that was rolled is then paid out for the selected period.

<u>Example 1:</u> *The computer selects a period in which [IND: you] [RESP: type A] chose alternative X which yields **€4** with a probability of 50% and **€0** with a probability of 50%. Lottery numbers **1, 2 and 3 were assigned to the amount of €4** and numbers **4, 5 and 6 were assigned to the amount of €0** by the computer. [IND: You] [RESP: Type A and his type B partner] thus have a 50% chance to receive €4 and a 50% chance to receive €0. If, for example, the lottery number 1 is rolled, the earnings from this period amount to **€4** [IND: for you] [RESP: for type A and for his type B partner]. If, for example, the lottery number 5 is rolled, the earnings from this period amount to **€0** [IND: for you] [RESP: for type A and for his type B partner].*

<u>Example 2:</u> *The computer selects a period in which [IND: you] [RESP: type A] chose alternative Y which yields **€-4** (a loss of €4) with a probability of 50% and **€0** with a probability of 50%. Lottery numbers **1, 2 and 3 were assigned to the amount of €-4** and numbers **4, 5 and 6 were assigned to the amount of €0** by the computer. [IND: You] [RESP: Type A and his type B partner] thus have a 50% chance to receive **€0** and a 50% chance to receive **€-4** (a loss of €4). If, for example, the lottery number 4 is rolled, the earnings from this period amount to **€0** [IND: for you] [RESP: for type A and for his type B partner]. If, for example, the lottery number 3 is rolled, the earnings from this period amount to **€-4** [IND: for you] [RESP: for type A and for his type B partner]. This loss must be offset with earnings from other decisions and/or with your starting balance of €4.*

Your payment is formed by the sum of your earnings in the three selected periods.
[RESP: **Two participants that are matched with each other** (type A and his type B partner) always have **identical earnings and thus final payments.]**
Please note that it is optimal [IND: for you] [RESP: for type A] to choose the alternative that [IND: you prefer for yourself] [RESP: he prefers for himself and for his type B partner].
There is **no possibility to increase the final payment by adopting a different behavior.**

A2. Tables

Lottery Number	Option A ("Safe") Prob. Left	Amount Left	Prob. Right	Amount Right	Option B ("Risky") Prob. Left	Amount Left	Prob. Right	Amount Right	Category
1	1	2	0	0	0.5	4	0.5	0	Base
2	1	2.5	0	0	0.5	4	0.5	0	Sensitivity Up
3	1	1.5	0	0	0.5	4	0.5	0	Sensitivity Down
4	1	3	0	0	0.5	5	0.5	1	Positive Shift
5	0.5	3	0.5	1	0.5	4	0.5	0	Lottery Choice
6	1	0	0	0	0.5	2	0.5	-2	Mixed Prospect
7	1	2	0	0	0.5	5	0.5	-1	MPS
8	0	0	1	-2	0.5	0	0.5	-4	Loss shift
9	1	4	0	0	0.5	8	0.5	0	Base
10	1	5	0	0	0.5	8	0.5	0	Sensitivity Up
11	1	3	0	0	0.5	8	0.5	0	Sensitivity Down
12	1	6	0	0	0.5	10	0.5	2	Positive Shift
13	0.5	6	0.5	2	0.5	8	0.5	0	Lottery Choice
14	1	0	0	0	0.5	4	0.5	-4	Mixed Prospect
15	1	4	0	0	0.5	10	0.5	-2	MPS
16	0	0	1	-4	0.5	0	0.5	-8	Loss shift
17	1	6	0	0	0.5	12	0.5	0	Base
18	1	7.5	0	0	0.5	12	0.5	0	Sensitivity Up
19	1	4.5	0	0	0.5	12	0.5	0	Sensitivity Down
20	1	9	0	0	0.5	15	0.5	3	Positive Shift
21	0.5	9	0.5	3	0.5	12	0.5	0	Lottery Choice
22	1	0	0	0	0.5	6	0.5	-6	Mixed Prospect
23	1	6	0	0	0.5	15	0.5	-3	MPS
24	0	0	1	-6	0.5	0	0.5	-12	Loss shift
25	1	8	0	0	0.5	16	0.5	0	Base
26	1	10	0	0	0.5	16	0.5	0	Sensitivity Up
27	1	6	0	0	0.5	16	0.5	0	Sensitivity Down
28	1	12	0	0	0.5	20	0.5	4	Positive Shift
29	0.5	12	0.5	4	0.5	16	0.5	0	Lottery Choice
30	1	0	0	0	0.5	8	0.5	-8	Mixed Prospect
31	1	8	0	0	0.5	20	0.5	-4	MPS
32	0	0	1	-8	0.5	0	0.5	-16	Loss shift
33	1	10	0	0	0.5	20	0.5	0	Base
34	1	12.5	0	0	0.5	20	0.5	0	Sensitivity Up
35	1	7.5	0	0	0.5	20	0.5	0	Sensitivity Down
36	1	15	0	0	0.5	25	0.5	5	Positive Shift
37	0.5	15	0.5	5	0.5	20	0.5	0	Lottery Choice
38	1	0	0	0	0.5	10	0.5	-10	Mixed Prospect
39	1	10	0	0	0.5	25	0.5	-5	MPS
40	0	0	1	-10	0.5	0	0.5	-20	Loss shift

Table 1-6: Overview of lotteries

A3. Prospect type regression

Dep. Var.: choice of safe prospect	I	II	III
Sensitivity up	0.185*** (0.023)	0.185*** (0.023)	0.185*** (0.023)
Sensitivity down	–0.465*** (0.032)	–0.465*** (0.032)	–0.485*** (0.033)
Positive shift	–0.161*** (0.034)	–0.161*** (0.034)	–0.170*** (0.035)
Lottery choice	0.101*** (0.026)	0.101*** (0.026)	0.098*** (0.026)
Mixed Lottery	0.059* (0.028)	0.059* (0.028)	0.060* (0.028)
Mean-preserving spread	–0.065* (0.032)	–0.65* (0.032)	–0.069* (0.033)
Loss shift	–0.171*** (0.034)	–0.172*** (0.032)	–0.183*** (0.035)
female		0.080* (0.036)	0.080* (0.036)
age		0.010* (0.005)	0.010* (0.005)
stake size			0.064*** (0.006)
# observations (# subjects)	3840 (96)	3840 (96)	3840 (96)
Wald Chi2	510.80	515.91	608.33

Random effects probit regressions: coefficients show marginal effects relative to choices in the basic prospect pair; standard errors in parenthesis; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.

Table 1-7: Choices by prospect type

Table 1-7 shows a random effects probit model with coefficients indicating the deviation of choices with respect to the basic prospect pair. In addition to the effects already discussed in the main text, it shows that females are on average significantly more risk averse than males. Also, risk aversion increases with age. Both findings are commonly found in decision making under risk (Donkers *et al.*, 2001; Eckel and Grossman, 2008). More interestingly, we find an effect of stake size, represented by the expected value of the prospect (taken in absolute terms for the pure loss prospect). The higher the stakes of the decision, the more risk averse subjects become on average. This is in agreement with general findings in the literature (Binswanger, 1980; Kachelmeier and Shehata, 1992; Lefebvre *et al.*, 2010). Controlling for stake effects also makes the effect of the mixed prospects much more significant. This increased effect derives from the fact that the mixed prospects are obtained by adjusting the expected value of the prospect downward from the basic prospect pair. Since subjects tend to be less risk averse for lower stakes, the increased risk aversion found for mixed prospects appears more relevant once one controls for the decreased stakes in those choice pairs.

Chapter 2

Tempus Fugit: Time Pressure in Risky Decisions

2.1 Introduction

Time pressure is common to many economic decisions. Traders make orders in financial markets within seconds after new information becomes available (Busse and Green, 2002). Last-minute bidders in auctions learn about common value components and adjust their valuation in an instant (Roth and Ockenfels, 2002). Negotiators must often reach agreements before a deadline (Roth *et al.*, 1988; Sutter *et al.*, 2003). This chapter studies the effect of time pressure on decision making under risk. Risk attitudes are important for economic policy decisions (Barsky *et al.*, 1997; Borghans *et al.*, 2009; Dohmen *et al.*, 2011; Guiso and Paiella, 2008), and the effects of time pressure on behavior in risky decisions should be considered by regulators of fast-paced markets. If there is a change in behavior under time pressure, existing behavioral predictions and empirical evidence based on data without time pressure may not be valid in these environments. Self-selection of individuals into occupations and a lack of comparable decision making situations with different degrees of time pressure complicate the study of time pressure in the real world. We therefore use a laboratory experiment to identify the effect of time pressure on decision making under risk.

The effects of time pressure on decision making under risk are also important from a methodological perspective when eliciting risk attitudes. Whereas standard incentivized and non-incentivized methods for eliciting risk attitudes do usually not put the decision makers under time pressure, it is sometimes necessary to do so like in the quickly emerging field of neuro-economics (e.g., Glimcher, 2004; Camerer *et al.*, 2005). Because of the technical need to record neural activity in short time windows, subjects are presented stimuli and have to make decisions in only a few seconds (e.g. in Tom *et al.* (2007) the whole process of presentation and decision took a maximum of 3 seconds; Engelmann *et al.* (2009) allow 3.5 seconds decision time). If risky decisions are affected by time pressure, the observed patterns of neural activity may also be specific to this condition. If they are not affected or only partially affected, the results can claim wider applicability. More generally, there is no systematic study so far in economics that compares decision making under risk with time pressure and without time pressure.

In this chapter, we analyze decisions in the gain domain, decisions in the loss domain, and decisions with both gains and losses involved. For gains we observe strong risk aversion; a finding that is very robust under time pressure. For losses we find that subjects become more risk averse under time pressure, turning mild risk seeking into risk aversion. For mixed prospects, the tendency to weigh gains and losses differently can become important. We call such weighting differences *gain-loss attitude*, with loss aversion denoting an overweighting of losses and gain seeking an overweighting of gains. Gain-loss attitude is less robust under time pressure. Both loss aversion and gain seeking can be increased under time pressure depending on simple framing manipulations. In general, we find time pressure to alter choices once we move outside the domain of pure gain prospects.

We also provide half of our subjects with information about the expected value of prospects. For instance, information on average returns is readily available on financial markets, and the impact of this availability on decision making under time pressure seems particularly relevant. We observe that the elicited risk attitudes are not systematically affected by expected value information in pure gain or pure loss decisions, but they are strongly affected for mixed prospects where choices are closer to risk-neutral expected value maximization when the information is available. This holds for decisions with and without time pressure. Interestingly, there are no interaction effects of time pressure and the availability of information. Our result suggests that subjects use information helping them to eliminate the influence of economically irrelevant gain-loss framing on their decisions (Slovic, 1972; Hilton, 2003).

Despite its relevance, time pressure in risky decision has received very little attention in the economics literature.[9] Bollard *et al.* (2007) study the effect of time pressure in an experiment where subjects can buy prospects with different variance and expected payoff in the gain domain. They find more variance aversion for time pressure.

[9] Its impact has been studied in a few other economic contexts such as search behavior (Ibanez *et al.,* 2009), bargaining (Sutter *et al.*, 2003), and in the beauty-contest game (Kocher and Sutter, 2006). Reutskaja *et al.* (2011) investigate search dynamics from choice sets of different size under time pressure using an eye-tracking device.

Given that subjects had to pay for the prospects, however, all their decisions involve gains and losses, and the increased variance aversion could also be explained by the finding of stronger loss aversion under time pressure. There is a psychological literature on time pressure in risky decision (Ben-Zur and Breznitz, 1981; Payne *et al.*, 1993; Payne *et al.*, 1996; Maule *et al.*, 2000). These studies focus on information processing and identify two strategies to cope with time pressure. First, behavior becomes more heuristic. Second, subjects exert more cognitive effort. These findings are consistent with evidence on decision-making costs in economics (Wilcox, 1993; Camerer and Hogarth, 1999; Moffatt, 2005). Ben-Zur and Breznitz (1981) consider risk attitudes for mixed prospects involving both gains and losses. They find more risk aversion under time pressure, and our results suggest that this is due to increased loss aversion.

Our study is the first to consider risk attitude separately for gains, for losses and for mixed gambles and the first to vary the availability of information both without time pressure and under time pressure. We provide evidence suggesting that risk attitude for losses and gain-loss attitude may be less robust with regard to external circumstances than risk attitude for gains. The generality of risk seeking for losses has been questioned by studies using repetition and financial incentives (Myagkov and Plott, 1997), and our results show that time pressure creates yet another environment in which risk aversion for losses may prevail. Under time pressure, gain-loss attitude is strongly affected by economically irrelevant framing effects and loss aversion is not necessarily a valid assumption if gains become more attractive because they give the impression to agents that they can break even.

Different theories of decision making under risk predict different effects of time pressure. Under expected utility, time pressure should have no effects, with the potential exception of increased noise because of errors (Schmidt and Neugebauer, 2007). Prospect theory assumes psychophysical effects in the weighting of probabilities and the weighting of gains versus losses. Such weighting effects would reasonably be expected to change under time pressure, but no clear direction is implied by the original theory. An expected utility model with an aspiration level has recently been formalized (Payne, 2005; Diecidue and van de Ven, 2008). This model can explain similar deviations from expected utility as prospect theory does by allowing utility to increase discontinuously at the aspiration level. The model explicitly assumes that the aspiration level effect derives from a heuristic focus on the likelihood of breaking even (or the total probability to surpass the aspiration level). It therefore predicts that deviations from expected utility become stronger if decisions are made under time pressure. Our results suggest that aspiration-based models may be a useful alternative to prospect theory to model non-expected utility in situations with time pressure. Behavior becomes more heuristic and the probability of breaking even can lead to reversals of typical loss aversion patterns.

The remainder of this chapter is organized as follows. The next section introduces the experimental design and the time pressure conditions. In section 2.3 we discuss our measures of risk attitude. Section 2.4 presents the experimental results, and section 2.5 discusses the results and concludes this chapter.

2.2 Experimental Design

2.2.1 Treatments and Procedures

Our experiment employs a 2×2 between-subject factorial design. The two factors we vary are the degree of *time pressure* and the availability of information about the *expected values* of the risky prospects. The four treatments are summarized in Table 2-1.

	No time pressure	Time pressure
No EV information	NTP-NEV N = 42	TP-NEV N= 41
EV information	NTP-EV N = 45	TP-EV N = 48
EV = expected value; N = number of observations (experimental participants).		

Table 2-1: Treatments

In all four treatments, subjects made choices between risky prospects in three separate experimental parts. Part I consisted of binary choices between pure gain prospects that were individually time constrained. Part II consisted of a choice list of seven choices adapted from Holt and Laury (2002). The whole list had to be completed within a time limit. The third part consisted of two subparts: in Part IIIA choices involving both gains and losses were made, and the choices were individually time constrained. In Part IIIB, subjects made individually time-constrained choices between pure gain prospects with a smallest payoff of €20 to cover possible losses from Part IIIA. When making their choices in Part IIIA subjects did not know how much money they could win in Part IIIB. The order of the three parts was fixed.

At the end of the experiment one part was randomly selected to determine a participant's income with equal probability. If either Part I or Part II was selected, one decision within this part was selected with equal probability to be paid out for real.[10] If Part III was selected, then one randomly selected decision from Part IIIA with possible losses *and* one randomly selected decision from Part IIIB were played for real. The decision in Part IIIB was selected independently of the Part IIIA decision. The random selection of the payoff-relevant task was done independently for each subject. The structure of payments in Part III was chosen to avoid giving experimental participants a clear reference point when making decisions in the loss domain in Part IIIA. Hence, losses were more likely to be experienced as real losses at the time of decision making. The sequence of events in the experiment is summarized in Figure 2-1.

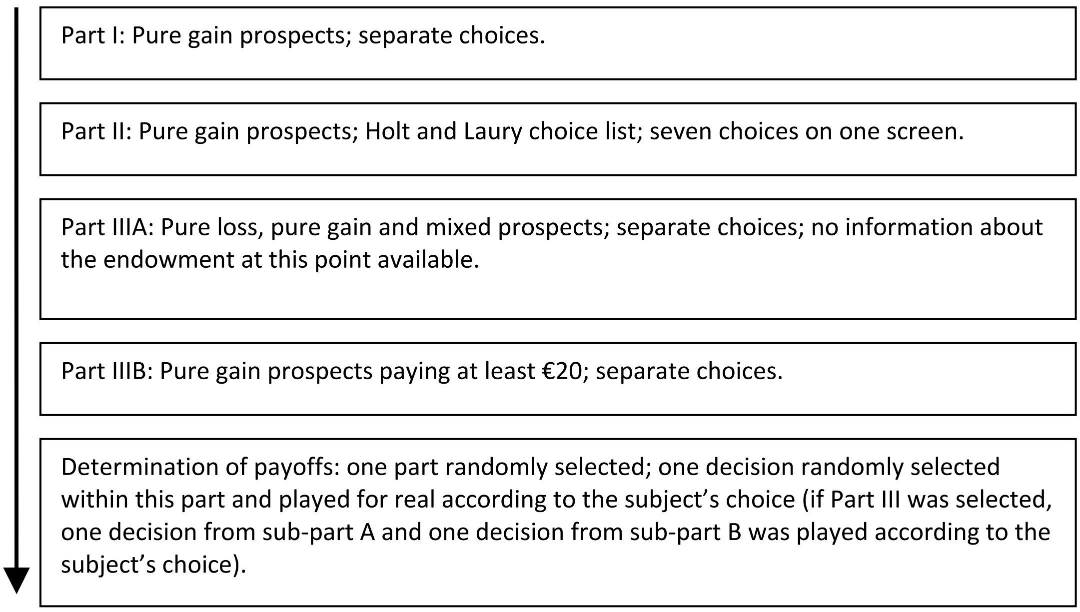


Figure 2-1: Structure of the experiment

[10] In individual decision experiments, this random lottery system is almost exclusively used for financial incentives (Myagkov and Plott, 1997; Holt and Laury, 2002; Harrison *et al.*, 2002). Its equivalence to a single and payoff relevant decision task has been empirically tested and confirmed (Starmer and Sugden, 1991; Hey and Lee, 2005).

All parts of the experiment were computerized using the experimental software z-Tree (Fischbacher, 2007). All randomizations were conducted by throwing a die individually at the subjects' desks.

Figure 2-2 shows how the prospects were presented to the subjects on the screen. Subjects always chose between two prospects A and B represented by the second and third row in Figure 2-2. All prospects had a maximum of two possible outcomes. The first row of the figure shows the faces of a twenty-sided die. The payoffs of the prospects depended on the outcome of a throw of the die. Each face of the die corresponds to a 5% probability. In the example, prospect A therefore pays €20 with probability 50% and zero with probability 50%. Prospect B pays €10 for sure. The procedure was explained in detail to all subjects (the original instructions are provided in appendix A1), and all rules of the experiment were common knowledge among participants.

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

Figure 2-2: Presentation of prospect choices

2.2.2 Time Pressure and Expected Value Manipulation

We manipulated decision time and availability of expected value information. In the treatments without time pressure, we constrained the decisions by introducing a maximum decision time that was very large and then measured actual decision times. Decisions in these treatments were practically unconstrained but we could use identical wording in all instructions by providing some threshold in the treatments without time pressure. Decisions in Part I and Part III were presented and constrained individually. Decisions in Part II had to be made on one screen and were constrained simultaneously, that is, all seven decisions of the choice list had to be made within the time limit. In the time pressure treatments, we restricted the decision times such that there was significant time pressure but subjects would still have sufficient time to make meaningful decisions at the computers.[11] All subjects within a treatment faced identical time constraints because we used a between-subject design. Table 2-2

[11] We conducted a pilot session with different time limits to test the severity of different limits.

summarizes the maximum and the actual decision times for each part of the experiment.

	No time pressure			**Time pressure**		
	max	No EV info actual	EV info actual	max	No EV info actual	EV info actual
Part I	60	5.64	5.95	4	2.38	2.05
Part II[a]	150	59.5	71	30	29	26
Part IIIA[b]	60	5.87	5.95	4	2.42	2.47

Numbers are average medians in Part I and Part III and medians in Part II; EV info = expected value information.

[a] Part II decision time refers to the total time for seven choices of the Holt and Laury (2002) choice list.

[b] Data for Part IIIB were not used to determine decision times under time pressure. The time limit in this part was identical to the time limit in Part I and Part IIIA.

Table 2-2: Maximum and actual median decision times per decision in seconds

For each decision problem subjects had to click a button to make their choice between options A and B, and then, had to click an 'OK'-button to confirm their choice within the time limit. The clock was clearly visible at the top of the screen. An example screenshot is given in appendix A2. If a subject failed to submit and confirm a choice before time runs out, this decision would pay the minimum payoff possible in either of the two prospects, should it be selected randomly for real pay. In decisions involving losses this would be the maximum loss. If a subject failed to submit all seven decision in Part II within the time limit, she would earn zero for each possibly selected decisions in this part of the experiment. Between two decision screens, a waiting screen occurred for 2 seconds in all treatments. This ensured that subjects could properly prepare for the next decision problem, especially under time pressure. Before each part of the experiment, specific instructions were distributed and read aloud. This gave subjects time to rest between parts.[12]

In the treatments with expected value information, the information was provided for each prospect next to the button that had to be clicked for the decision. This location allowed subjects to access the information efficiently (see the screenshot in appendix A2). The meaning of an expected value was explained to the subjects in written

[12] The working of the computer mouse was essential for subjects to enter decisions rapidly into the computer. Before the experiment, we checked the computer mice with each subject for proper working.

instructions before the experiment. We did not refer to the information as “useful” or “helpful” and did not make any suggestions regarding whether subjects should actually use the information in their decisions.

2.2.3 Subjects and Payoffs

176 undergraduate students from the University of Amsterdam in the Netherlands participated in eight laboratory sessions and were randomly assigned to treatments. Students were recruited electronically from a pool of approximately 1,200 potential participants and came from different disciplines. Each subject participated only once.

Subjects received a show-up payment of €5 and could earn between €0 and €200 based on their choices and, when applicable, on the random draws. Average earnings were €17.15, and the experiment took between 30 and 50 minutes depending on the treatment.

2.3 Prospects and Dependent Variables

We analyze the effects of time pressure and expected value information on dependent variables that measure attitudes towards risk under gains and losses, and gain-loss attitude. The prospects used to construct these variables are discussed in the following and summarized in Table 2-3.

RAG (from Part I) measures **r**isk **a**version for **g**ains by the percentage of safe choices a subject makes in six decisions between pure gain prospects each involving one sure gain. Three decisions involve choices between a prospect and its expected value. The other three decision problems are adapted from prospect choices for which a preference of roughly 50% for each option has been found in the literature (Wakker *et al.*, 2007b). These choices are therefore likely to distinguish well between subjects.

RAG=EV (from Part I) uses only the three choices between prospects and their expected value from RAG. This variable is used to calibrate the average risk attitude for our sample of subjects.

RAGHL (from Part II) measures **r**isk **a**version for **g**ains using a **H**olt and **L**aury (2002) choice list with pure gain prospects. We scaled up their low payoff treatment (2002, p. 1645) by a factor of six and used only choices with probabilities between 20% and 80% including. The variable indicates the percentage of safer choices a subject makes if there was a unique point where the subject switched from the safer to the riskier option as the probability of the larger payoff increased. Subjects who switched twice or switched from risky to safe as the probability of the higher payoff increased were excluded from the analysis.

RAL (from Part IIIA) measures **r**isk **a**version for **l**osses by the percentage of safe choices a subject makes in six decisions between pure loss prospects each involving one sure loss. Three decisions involve choices between a prospect and its expected value. The other three decisions have a lower expected value for the risky option to detect possible risk seeking for losses.

RAL=EV (from Part IIIA) uses only the three choices between prospects and their expected value from RAL to calibrate the average risk attitude for losses for our sample of subjects.

RALMPS (from Part IIIA) measures **r**isk **a**version for **l**osses considering two choices between prospects and **m**ean **p**reserving **s**preads of these prospects. The variable indicates the percentage of a subject's choices of the prospect with lower variance. All prospects involved non-zero losses and had positive variance.

PLA (from Part IIIA) measures **a**voidance of prospects with a **p**rominent **l**oss by the percentage of a subject's choices of a pure gain prospect over a mixed prospect with higher expected value (and variance) in three decision problems. We call the loss in these decision problems prominent because gain-loss differences are more prominent here compared to pure loss decisions in RAL, and there is only one loss outcome but three gain outcomes in each decision problem.

PGS (from Part IIIA) measures **s**eeking of prospects with a **p**rominent **g**ain by the percentage of a subject's choices of a mixed prospect over a pure loss prospect with higher expected value (and lower variance) in three decision problems. There is only one gain outcome but three loss outcomes in each decision problem.

<u>*ENDOW*</u> (from Part IIIB) measures the percentage of a subject's safe choices in six decisions between prospects and their expected values used to endow subjects with at least €20 for the part involving losses.

Variable	Short Description	Choices		Expected values
RAG	Percentage of safe choices	(€20, .5) vs. €10 (€52, .25) vs. €13 (€15, .8) vs. €12 (€18, .95) vs. €14 (€32, .5) vs. €13 (€200, .05) vs. €11	} RAG=EV (first three)	€10 vs. €10 €13 vs. €13 €12 vs. €12 €17.10 vs. €14 €18 vs. €13 €10 vs. €11
RAGHL	Percentage of safe choices if there was a unique switching point toward the riskier prospect	(€12, .2; €9.60, .8) vs. (€23.10, .2; €0.60, .8) (€12, .3; €9.60, .7) vs. (€23.10, .3; €0.60, .7) (€12, .4; €9.60, .6) vs. (€23.10, .4; €0.60, .6) (€12, .5; €9.60, .5) vs. (€23.10, .5; €0.60, .5) (€12, .6; €9.60, .4) vs. (€23.10, .6; €0.60, .4) (€12, .7; €9.60, .3) vs. (€23.10, .7; €0.60, .3) (€12, .8; €9.60, .2) vs. (€23.10, .8; €0.60, .2)		€10.08 vs. €5.01 €10.32 vs. €7.35 €10.56 vs. €9.60 €10.80 vs. €11.85 €11.04 vs. €14.10 €11.28 vs. €16.35 €11.52 vs. €18.60
RAL	Percentage of safe choices	(– €20, .5) vs. – €10 (– €15, .8) vs. – €12 (– €20, .1) vs. – €2 (– €20, .8) vs. – €15 (– €10, .95) vs. – €9 (– €19, .85) vs. – €13	} RAL=EV (first three)	– €10 vs. – €10 – €12 vs. – €12 – €2 vs. – €2 – €16 vs. – €15 – €9.5 vs. – €9 – €16.15 vs. – €13
RALMPS	Percentage of choices with lower variance	(– €18, .5; – €10, .5) vs. (– €15, .5; – €13, .5) (– €9, .5; – €1, .5) vs. (– €6, .5; – €4, .5)		– €14 vs. – €14 – €5 vs. – €5
PLA	Percentage of pure gain prospects chosen	(€4, .35; €2, .65) vs. (– €6, .25; €8, .75) (€7, .25; €2, .75) vs. (– €4, .2; €7, .8) (€11, .85; €15, .15) vs. (– €1, .1; €15, .9)		€2.70 vs. €4.50 €3.25 vs. €4.80 €11.60 vs. €13.40
PGS	Percentage of mixed prospects chosen	(– €14, .15; – €11, .85) vs. (– €17, .85; €8, .15) (– €14, .4; – €5, .6) vs. (– €14, .8; €4, .2) (– €6, .45; – €3, .55) vs. (– €19, .35; €2, .65)		– €11.45 vs. – €13.25 – €8.60 vs. – €10.40 – €4.35 vs. – €5.35
ENDOW	Percentage of safe choices	(€20, .5; €24, .5) vs. €22 (€20, .6; €25, .4) vs. €22 (€20, .75; €28, .25) vs. €22 (€20, .8; €30, .2) vs. €22 (€20, .9; €40, .1) vs. €22 (€20, .95; €60, .05) vs. €22		€22 vs. €22 €22 vs. €22 €22 vs. €22 €22 vs. €22 €22 vs. €22 €22 vs. €22

Table 2-3: Dependent variables and prospects

For each variable we have slightly different sample sizes because different numbers of subjects violated the time constraint in different parts of the experiment. More precisely, subjects who violated the time constraint in at least one of the decision problems used to construct a variable were excluded from the analysis of this variable.

2.4 Experimental Results

2.4.1 Time Pressure Manipulation

Table 2-2 in section 2.2.2 shows that median decision times under time pressure were approximately half the size of median decision times under no time pressure. We tested for each decision problem and under both expected value information conditions the null hypothesis that decision times are equal under both decision time conditions, using Mann-Whitney tests. Equality of decision times was rejected for all choice problems. The smallest z-value was z=3.171 (p=0.002),[13] indicating that decision times have been much shorter under time pressure. We lose between four and eight observations per variable in Part I and Part III because of violations of time limits in the separate binary choices. Hence, the time constraints in these decisions have been substantial but not prohibitive. We lose twenty observations in the choice list in Part II; a fact suggesting that decision makers appeared to be seriously constrained with the 30 seconds time limit in Part II.[14]

In a post-experimental questionnaire subjects indicated their stress level and the perceived difficulty of the experiment on a five-point Likert scale. Subjects in the time pressure treatments felt more stressed during the experiment (Mann-Whitney test, z=5.520, p<0.001) and considered it a more difficult experiment than subjects in the unconstrained treatments (Mann-Whitney test, z=2.230, p=0.026).

The correlation between our risk measures and decision times was practically zero for all variables in the treatments *without* time pressure. That is, there were not certain types of subjects in terms of their risk attitudes that were more constrained than others; for instance, more risk averse subjects did not deliberate longer before making a decision. Our choice for a between-subject design is, hence, corroborated by the data.

Note also that in the treatments without time pressure, decisions times for gains in Part I and for losses and mixed prospects in Part IIIA do not differ (Mann Whitney

[13] All tests in this chapter are two-sided tests, and the abbreviation *ns* denotes non-significance.

[14] Another ten subjects were eliminated because they switched more than once in the choice list, nine of them under time pressure. Yet, in comparison to other experiments using the same elicitation method the percentage of consistent choice lists submitted by the subjects is high, despite the time pressure.

tests, z<1.093, ns). This suggests that pure gain, pure loss, and mixed decisions created a similar level of difficulty for the decision makers.

2.4.2 Time Pressure and Risk Attitude

An overview of the means and standard deviations of all variables in our four treatments is given in appendix A3. Here, we first consider results for pure gain and pure loss decisions, and then consider results for decisions involving mixed prospects. For each variable in Table 2-3 we run linear regressions of the general form

$$mra_i = \alpha + \beta_1 \, TP_i + \beta_2 \, EV_i + \beta_3 \, (TP_i \times EV_i) + \beta_4 \, FEMALE_i + \varepsilon_i \qquad [1]$$

where mra_i is the measure of risk attitude for subject i that is considered in the regression (always in percentages), TP_i is a dummy variable that equals 1 if subject i was in the time pressure condition, EV_i is a dummy variable that equals 1 if the subject was given expected value information, and the interaction term $TP_i \times EV_i$ which equals 1 if the subject experiences time pressure and received expected value information. FEMALE controls for the subjects' gender and ε_i is the error term.[15]

2.4.2.1 Pure Gain and Pure Loss Decisions

The linear regressions in Table 2-4 show that risk attitude for pure gains is not affected by time pressure. The variables RAG, RAGHL, and ENDOW involve different payoff ranges and time constraints, and they are measured in different parts of the experiment. There is no direct effect of time pressure on either of these variables. Expected value information does not affect these variables, nor does its interaction with time pressure.

RESULT 1: *Risk attitude for gains is robust under time pressure, and it does not respond to the availability of expected value information.*

[15] We report linear regression results here in order to simplify the interpretation and the comparison between variables in terms of percentage of safe choices. Ordered probit regressions on the *number* of safe choices for each variable give qualitatively the same results for all reported regressions.

For losses, however, subjects become comparably more risk averse under time pressure (Table 2-5). For both variables RAL and RALMPS, the percentage of safe choices under time pressure increases. No significant effect is found for expected value information or for its interaction with time pressure.

Dep. Var.:	RAG	RAG	RAGHL	RAGHL	ENDOW	ENDOW
time pressure	–0.011 (0.035)	–0.001 (0.051)	0.010 (0.038)	–0.054 (0.055)	–0.026 (0.039)	–0.032 (0.057)
EV information	0.069 (0.035)	0.079 (0.05)	0.025 (0.037)	–0.024 (0.048)	0.011 (0.039)	0.006 (0.055)
time pressure × EV information		–0.019 (0.071)		0.121 (0.075)		0.011 (0.079)
female	**0.121**** **(0.037)**	**0.120**** **(0.038)**	0.059 (0.039)	0.062 (0.039)	**0.122**** **(0.042)**	**0.122**** **(0.042)**
# observations	172	172	146	146	170	170

OLS regressions: Standard errors in parenthesis; ×: interaction; ** represents significance at p=0.01 and * at p=0.05.

Table 2-4: Linear regression results for pure gains

Dep. Var.:	RAL	RAL	RALMPS	RALMPS
time pressure	**0.078*** **(0.039)**	**0.110*** **(0.056)**	**0.143*** **(0.057)**	**0.203*** **(0.082)**
EV information	0.000 (0.039)	0.031 (0.054)	0.046 (0.057)	0.103 (0.08)
time pressure × EV information		–0.061 (0.077)		–0.114 (0.113)
female	**0.084*** **(0.041)**	**0.083*** **(0.041)**	0.109 (0.06)	0.106 (0.06)
# observations	171	171	173	173

OLS regressions: Standard errors in parenthesis; ×: interaction; ** represents significance at p=0.01 and * at p=0.05.

Table 2-5: Linear regression results for pure losses

The effect of time pressure for loss prospects is larger for the mean preserving spreads (RALMPS) than for RAL. This is consistent with the fact that RAL contained three decisions that were designed to detect risk seeking for losses. As will be discussed next, there was on average only mild risk seeking for losses in the baseline treatment; that is, without time pressure many subjects chose the safer options already.

RESULT 2: *With losses, time pressure leads to more risk averse choices. The availability of expected value information does not affect behavior in either of the treatment conditions.*

To show the effect of time pressure on average risk attitudes for both gains and losses under time pressure conditions we consider the variables RAG=EV and RAL=EV, pooling the data from both expected value information treatments. These variables involve only choices between prospects and their expected values and the average risk attitude can be determined by testing whether subjects chose on average more than half of the safe options (Table 2-6).

	No time pressure	Time pressure	Mann-Whitney test
RAG=EV	73.9% (z=6.669, p<0.001)[a]	71.7% (z=6.173, p<0.001)[a]	z=0.391, ns
RAL=EV	46.7% (z=0.696, ns)[a]	60.1% (z=4.130, p<0.001)[a]	z= 2.677, p=0.007

[a] Wilcoxon signed-rank test for the average percentage of safe choices being equal to 50%.

Table 2-6: Average percentage of safe choices

In the baseline condition with no time pressure, our data show the common pattern of *partial reflection* (see Wakker *et al.* (2007a) for an extensive review of empirical findings). There is strong risk aversion for gains, but mild and insignificant risk seeking for losses. Under time pressure, we obtain risk aversion for both gains and losses, i.e. risk seeking for losses without time pressure turns into risk aversion for losses under time pressure. We observe that under time pressure, subjects have strong preferences for safer options for both gains and losses, clearly rejecting the conjecture that choices were more random under time pressure.[16]

[16] Risk aversion for gains is quite strong for our variable RAG=EV, and it is conceivable that no treatment effect for risk attitude is observed because risk aversion was high without time pressure already. However, the above regressions also employ the variables RAG and RAGHL to detect changes in risk attitude. These variables include decisions between prospects of unequal expected value, and the mean percentage of safe choices without time pressure was 60% for RAG and 64% for RAGHL (pooling the data from both expected value information conditions). These preferences are not extreme, and they are comparable to the mean percentage of safe choices of 58% without time pressure for the variable RAL for which we detected increased risk aversion for losses under time pressure.

RESULT 3: *With time pressure, choices are risk averse, on average, in the gain domain and in the loss domain.*

2.4.2.2 Decisions Involving Gains and Losses

The variables PLA and PGS study gain-loss attitude. PLA measures the percentage of choices of a pure gain prospect over a mixed prospect with higher expected value. These decisions always involve one prominent loss, and apart from risk aversion also loss aversion can lead subjects to choose the pure gain prospect. PGS measures the percentage of choices of a mixed prospect over a pure loss prospect with higher expected value. These decisions always involve one prominent gain. Loss aversion would lead to fewer choices of the mixed prospect while gain seeking, which is overweighting of gains relative to losses, may lead subjects to choose the mixed prospect.

Dep. Var.:	PLA	PLA	PGS	PGS
time pressure	**0.170**** **(0.055)**	0.115 (0.08)	**0.253**** **(0.043)**	**0.186**** **(0.062)**
EV information	**–0.12*** **(0.056)**	**–0.173*** **(0.078)**	**–0.125**** **(0.043)**	**–0.188**** **(0.06)**
time pressure × EV information		0.106 (0.11)		0.129 (0.086)
female	**0.130*** **(0.059)**	**0.133*** **(0.059)**	–0.006 (0.046)	–0.003 (0.046)
# observations	172	172	168	168
OLS regressions: Standard errors in parenthesis; ×: interaction; ** represents significance at p=0.01 and * at p=0.05.				

Table 2-7: Linear regression results for mixed prospects

The linear regressions in Table 2-7 show that subjects avoid more mixed gambles in PLA and take more mixed gambles in PGS under time pressure.

RESULT 4: *In mixed gambles, decision makers are more likely to avoid the prominent loss and seek the prominent gain under time pressure.*

We also observe an effect of expected value information on both PLA and PGS. If expected values are provided, subjects choose the higher expected value prospect more often, leading to less aversion to the prominent loss and less seeking of the prominent gain. We did not observe an effect of expected value information for the pure gain or pure loss decisions, suggesting that gain-loss attitude is affected by expected value information and plays an important role in PLA and PGS choices. The effect of expected value information occurs under both time pressure conditions and there are no significant interactions between time pressure and expected value information.

RESULT 5: *In mixed gambles, the availability of expected value information leads to decisions that are closer to risk neutrality than without expected value information. This effect occurs with and without time pressure.*

A simultaneous increase in loss aversion and gain seeking under time pressure cannot be explained by a change in gain-loss attitude as typically modeled under prospect theory. An increase in loss aversion implies that losses receive more weight relative to gains under time pressure than without time pressure. An increase in gain seeking implies the opposite effect. However, aspiration level-based expected utility theory (Diecidue and van de Ven, 2008) is consistent with such an effect. The theory predicts people to consider the total probability of surpassing the aspiration level, leading to deviations from expected utility. In the current setting, the aspiration level would naturally be a zero outcome. Consequently, in the PLA decisions, the overall probability of a gain is lower for the mixed gamble, leading to loss aversion; and in the PGS decisions, the overall probability of a gain is higher for the mixed gamble, leading to gain seeking. We find that PLA and PGS are strongly positively correlated on the individual level (Spearman's $\rho=0.32$, $p<0.001$). That is, subjects who avoid the *prominent-loss mixed prospect* in PLA are also more likely to seek the *prominent-gain mixed prospect* in PGS. This observation corroborates the view that gain-loss attitude is driven by the interaction of framing and a subject's susceptibility to aspiration levels.[17]

[17] A similar effect has been found in Isaac and James (2000) and James (2007) in comparisons between risk attitude elicitation procedures. In the two studies the subjects who are most risk averse under one elicitation procedure are most risk seeking under the other procedure, suggesting that differences in elicited risk attitudes depend on differences in the susceptibility towards the specific framing of the procedure.

2.4.2.3 Gender and Risk Attitude

There has been much interest in gender differences in risk attitude (Barsky *et al.*, 1997; Booij and van de Kuilen, 2009; Borghans *et al.*, 2009; Croson and Gneezy, 2009; Dohmen *et al.*, 2011; Fehr-Duda *et al.*, 2006; Schubert *et al.*, 1999). We control for gender in our regressions and find, in line with the exiting literature, that females are more risk averse both under gains and under losses. There were no significant interactions between gender and time pressure or gender and expected value information.

2.5 Conclusion

Decisions under uncertainty that are made in auctions, managerial settings, or financial markets often involve serious decision time constraints. Similarly, neuro-economic studies involving risky decisions let subjects make decisions under a strict timing schema. In contrast, most experimental measurements of risk attitudes provide subjects with ample decision time, and in fact urge subjects to consider their choices carefully.

In this chapter, we study risky decisions under time pressure for gains, losses, and mixed prospects. Our results showed that time pressure affects choices under risk, but only in situation with loss prospects or mixed prospects. In such settings, behavior becomes more heuristic, and the findings support the view that aspiration levels become important. In the gain domain, the patterns of decision making seem very robust, even under severe time pressure.

In particular, we found more risk aversion for losses, more loss aversion, and more gain seeking under time pressure. Our results show that typical non-expected utility patterns as modeled by prospect theory may not provide an appropriate description of choice behavior if time pressure becomes important. We have shown that recently developed models of expected utility with an aspiration level (Diecidue and van de Ven, 2008) may be a useful alternative in such situations.

Our results provide reasserting evidence for the generalizability of risk attitude results from the laboratory to naturally occurring environments that exhibit time pressure in decision making, as long as the gain domain is concerned. If losses and mixed outcomes are involved, results with existing elicitation methods seem to be only partially valid in such environments. Obviously, the same remark applies to results from neuro-economic studies.

Our finding that expected value information reduces both loss averse and gain seeking behavior, irrespective of time pressure, suggests that subjects benefit from such decision aids. Surprisingly, no effect of expected value information was found for pure gain or pure loss gambles, supporting the view that elicited risk attitudes are robust to the availability of summary statistics. For mixed gables, our results suggest that subjects are aware of their sensitivity to framing and aspiration levels, and try to avoid such effects by falling back on presumably objective measures. Given that in many decision situations outside the lab a wide range of decision aid is available to the decision maker, actual behavior may thus be closer to the risk neutral benchmark than laboratory studies sometimes suggest.

2.6 Appendix

A1. Experimental Instructions

In this experiment you make choices between two risky options A and B, which pay some amount of money depending on the outcome of a 20-sided die. See Example 1.

Example 1:

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€11										€9									
B	€20					€21														

Figure 2-3: Presentation of prospect choices (example 1)

A 20-sided die will be thrown. Option A pays €11 if the die shows a 1, 2, 3,…, or 10, and pays €9 if the die shows an 11, 12, 13,…, or 20. Option B on the other hand pays €20 if the outcome of the die is 1, 2, ..., or 5, and pays €21 if the outcome of the die is 6, 7, 8, …, or 20. If this choice was selected to be payoff relevant for you, the experimenter would come to your desk and you would throw a 20-sided die. You would receive the payoff depending on the Option you have chosen before and the number shown by the die.

Recognize that each number of the die represents a probability of 5%. The whole die adds up to 100% therefore. In Example 1 this means that Option A offers a chance to win €11 with probability 50% and to win €9 with probability 50%. Option B on the other hand offers a 25% chance to win €20 and 75% chance to win €21.

Another example:

Example 2:

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

Figure 2-4: Presentation of prospect choices (example 2)

Here, Option A is the same as above: if you choose A, and the die shows any number between 1 and 10 including, you receive €11. If the die shows any number between 11 and 20 including, you receive €9. If you choose Option B, on the other hand, you receive €10 for any number the die might show. Option B pays €10 with probability 100%.

A2. Example Screen Shot

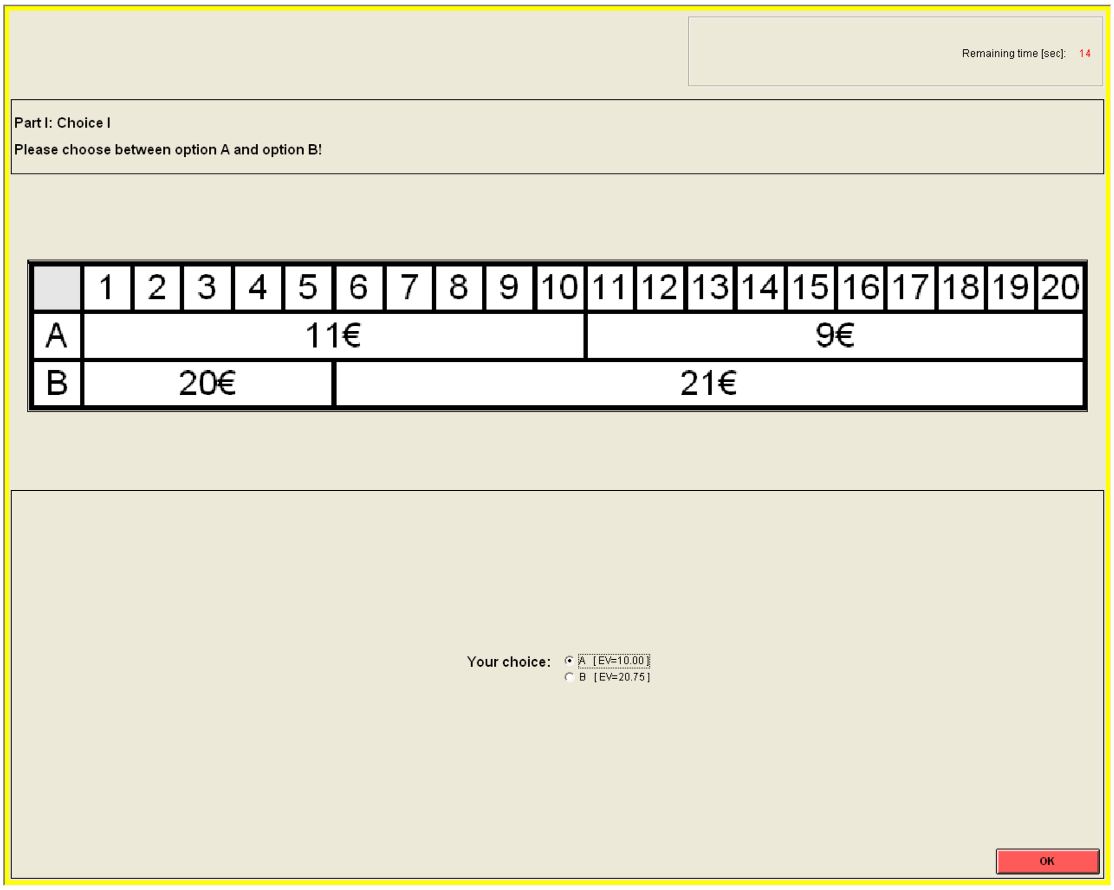


Figure 2-5: Screenshot - Decision screen

A3. Tables

Treatment[a] / Variable	NTP &NEV (I)	TP&NEV (II)	NTP&EV (III)	TP&EV (IV)	Mann-Whitney tests[b]
RAG	0.56 (0.24)	0.56 (0.27)	0.65 (0.21)	0.63 (0.23)	I<III, z=2.15, p=0.0318
RAGHL	0.65 (0.20)	0.60 (0.29)	0.64 (0.16)	0.70 (0.26)	-
RAL	0.56 (0.27)	0.68 (0.25)	0.60 (0.24)	0.65 (0.25)	I<II, z=2.060, p=0.0396
RALMPS	0.44 (0.39)	0.65 (0.36)	0.56 (0.37)	0.64 (0.38)	I<II, z=2.445, p=0.0145
PLA	0.47 (0.38)	0.59 (0.33)	0.31 (0.35)	0.53 (0.39)	III<IV, z=2.605, p=0.0092 I<III, z=1.991, p=0.0464
PGS	0.28 (0.28)	0.47 (0.30)	0.10 (0.18)	0.41 (0.32)	I<II, z=2.723, p=0.0005 III<IV, z=4.867, p=0.0000 I<III, z=3.413, p=0.0006
ENDOW	0.20 (0.27)	0.18 (0.22)	0.22 (0.29)	0.20 (0.26)	-

Standard deviations in parenthesis.
[a] NTP: no time pressure; TP: time pressure; NEV: no expected value information; EV: expected value information.
[b] Only significant test-results (p<0.05) are reported.

Table 2-8: Means and standard deviations of variables by treatment

Chapter 3

An Experimental Test of Precautionary Bidding

3.1 Introduction

Consider an auction with pure *ex-post risk*: The value of the auctioned good is risky, with the risk being independent of private or common value components and signals thereof. The risk is known ex-ante and is common knowledge among buyers. In the language of decision theory, the auctioned good is a *risky lottery*. Esö and White (2004) theoretically study auctions with ex-post risk in the affiliated value model by Milgrom and Weber (1982). They show for the standard first-price auction that bidders exhibiting decreasing absolute risk aversion (DARA) unambiguously reduce their bids by more than the appropriate risk premium, an effect they call *precautionary bidding*.[18] The intuition is that DARA bidders prefer higher income in the case they win the auction and have to bear the ex-post risk involved in the good, and therefore bid more conservatively. This effect is similar to the precautionary saving motive where agents transfer current wealth into future periods with more income uncertainty (Kimball, 1990).

Examples for auctions with ex-post risk are numerous and financially significant. Television rights for Olympic Games are usually auctioned off before the host city is selected from a set of competitors. The winner bears the risk of a more or less attractive host, a risk arising from information unavailable to any bidder at the time the rights are allocated. In procurement auctions, unpredicted events that affect production costs add ex-post risk to the winning bidder's profit. More generally, all goods whose resale value or quality is uncertain ex-ante to all buyers involve some ex-post risk. Precautionary bidding, if empirically relevant, has several important implications for auction design in general, and more specifically for information revelation by sellers and information acquisition by buyers. For instance, according to theory, sellers have an additional incentive to reduce the risk exhibited by the auctioned object as much as possible, and buyers have a strategic incentive to disregard some information. Because of its importance for the prediction of auction

[18] In their article, they provide results for various auction formats. We focus on first-price auctions here.

outcomes and for economic design, it is essential to study the empirical validity of precautionary bidding.

Despite the widespread occurrence of ex-post risk in auctions and its relevance, no empirical study on precautionary bidding has been conducted so far. A direct measurement of precautionary bidding with field data is not easily available, because it requires the independent observation of both the bidders' risk tolerance and the riskiness of the good. In order to provide the first empirical assessment of precautionary bidding, we conduct experimental laboratory auctions where the controlled setting allows us to identify and quantify the precautionary premium directly.

Our main experiment finds strong support for bidding behavior being consistent with precautionary bidding in first-price auctions. Bidders are significantly better off in first-price auctions when a risky object rather than an equally valued sure object is auctioned. Although our empirical hypotheses build on Esö and White's (2004) theoretical framework, the experimental tests that we conduct for the data from the experiment are in fact model-free, relying only on observable certainty equivalents. In addition, we provide results for a parametric expected utility analysis. The latter shows that risk averse Nash equilibrium predicts bidding behavior in deterministic auctions reasonably well, but it fails to predict bidding behavior in auctions with ex-post risk. This strongly corroborates the finding from our model-free analysis. In a control experiment, we address alternative explanations based on behavioral biases in decision-making that might have influenced our results in the main experiment. The data from this control experiment are in line with our conclusions from the main experiment and show the robustness of the precautionary bidding effect.

The remainder of this chapter is laid out as follows. In the next section we introduce the theoretical framework and the predictions of the precautionary bidding model. In section 3.3 we present our experimental design in detail. Section 3.4 reports the results from the main experiment, and section 3.5 provides evidence from the control experiment and from additional robustness checks. In section 3.6 we discuss the interpretation of our results in terms of precautionary bidding, and section 3.7 concludes.

3.2 Theoretical Framework and Predictions

Almost all analyses of bidding behavior in auctions today assume that objects are non-risky, although agents are often assumed to have noisy signals regarding the true value of the object. Risk aversion plays a role in first-price auctions, because it reduces bid shading and, therefore, increases the bid in comparison to the risk-neutral Nash equilibrium (e.g., Cox *et al.*, 1982; 1985; Maskin and Riley, 1984; Kagel *et al.*, 1987). The only study so far that considers pure ex-post risk in common auctions formats is Esö and White's (2004) theoretical analysis of the affiliated value model by Milgrom and Weber (1982).

We follow their setup and assume that there are *n* potential bidders for a given object. Bidder *i* receives a private signal $s_i \in [\underline{s}, \bar{s}]$. The joint distribution of the signal follows the properties of affiliation described in Milgrom and Weber (1982). The risky ex-post monetary value of the object for bidder *i* is $v_i = v(s_i, s_{-i}) + z_i$, where v is strictly increasing in its first argument, s_{-i} denotes the highest signal of all bidders other than bidder *i*, and z_i is the realization of a random variable, $\tilde{z}_i$, with zero mean. The realizations of the random variable come from a symmetric joint distribution and are independent of the signals, but they can be (perfectly) correlated across bidders. By definition, if $\tilde{z}_i$ is non-degenerate, the object is risky. The utility function u is strictly concave and thrice differentiable. For DARA bidders, $-(\partial^2 u/\partial x^2)/(\partial u/\partial x)$ is strictly decreasing.

Esö and White (2004, pp. 84-85) prove in this framework that, holding everything else constant, DARA bidders in the first-price auction have unambiguously higher indirect utilities in a symmetric equilibrium when z_i is non-degenerate, i.e., the object is risky.[19] In the following, we give a brief intuition for the result. We then derive empirical predictions, building on Esö and White's (2004) hypothesis, regarding the buyer's equilibrium bids for a risky good and for her (risk free) certainty equivalent of this risky good. Our identification of precautionary bidding in the experiment will be based on the comparison of bids for risky lotteries and their certainty equivalents on the individual level.

[19] The main result extends immediately to situations where another independent noise is added to make already noisy valuations even riskier (Kihlstrom *et al.*, 1981).

In the first-price auction, for risk averse agents who maximize expected utility with a DARA utility function, the introduction of a mean-preserving ex-post risk has three effects. *First*, the value of the object for the agent is reduced by the risk premium. Agents replace the value of the risky object v_i by its certainty equivalent $CE_i(v_i)$ before making their bids. For risk averse bidders by definition $CE_i(v_i) < E[v_i]$, where $E[.]$ denotes the expected value. *Second*, the riskiness of the object introduces a background risk, making bidders become more risk averse regarding other risks (Eeckhoudt *et al.*, 1996). Hence, in the presence of ex-post risk they will shade their bids less than predicted by the appropriate risk premium, because the possibility of reducing the chance to lose the object in the auction becomes more attractive than the risky gain of a higher payoff by decreasing the bid.

There is a *third* effect, however, the *precautionary effect*. This effect causes bidders to bid less aggressively because each extra unit of income is more valuable to them in the case they win the auction for the risky object as opposed to its certainty equivalent, due to the background risk. In other words, increasing the probability of winning the auction through a higher bid becomes more costly in the presence of ex-post risk. This effect is related to the prudence premium (Gollier, 2001; Eeckhoudt and Schlesinger, 2006).

Taking all three effects together, Esö and White (2004) prove that for DARA bidders in equilibrium the total effect of ex-post risk on the reduction of one's bid is unambiguously larger than just the risk premium.[20] Our empirical strategy to identify precautionary bidding is based on the comparison of bids for risky objects and their certainty equivalents. By construction, both goods are equally valuable. If bids for risky objects were larger than bids for their certainty equivalents, the effect of background risk on bid shading would dominate, in contrast to the theory. If bids for risky objects were smaller than for their certainty equivalents, however, the precautionary effect must dominate, in accordance with the theory. Hence, we can formulate our main hypothesis.

[20] For DARA bidders the prudence premium is larger than the risk premium.

HYPOTHESIS 1: *In the affiliated private value first-price auction, buyers' bids* b_i *for a risky object will be lower than their bids for a risk-free object whose value is equal to their individual certainty equivalent of the risky object, i.e.,*

$$b_i(v_i) < b_i(CE_i(v_i)). \quad [1]$$

The precautionary bidding effect is similar to the precautionary saving motive where agents transfer more wealth into the future when they face a higher future income risk (Kimball, 1990). Compared to precautionary saving, however, in first-price auctions individual risk attitudes and the level of riskiness of the object affect equilibrium bidding through multiple channels that point into different directions (see effects two and three above). In particular, increased risk aversion leads to less bid-shading (effect two). This makes EW's result of an unambiguously negative effect of precautionary bidding on the bid the more remarkable. It also implies, *ceteris paribus*, that buyers are better off bidding for a risky object than for the equivalent risk-free object.

As noted before, in most settings the (perceived) riskiness of the good and the degree of bidders' risk aversion cannot easily be measured independently in the field. Moreover, direct comparisons between bids for risky and risk-free goods with an identical certainty equivalent typically cannot be constructed. Our experimental test of precautionary bidding directly compares bids for independently elicited certainty equivalents with bids for the appropriate risky objects on the individual level.

Hypothesis 1 is derived from the precautionary bidding model under the premises of expected utility theory. Since our empirical test is only based on comparisons of bids for risky prospects and observable certainty equivalents, however, it is in fact model-free, and Hypothesis 1 can be interpreted as a behavioral definition of the precautionary effect in first-price auctions.

3.3 Experimental Design

3.3.1 The Auction

In the experiment, we follow the affiliated private value implementation of Kagel *et al.* (1987), adjusted to the setup in which either sure prospects or risky prospects are sold. In particular, for sure prospects the subjects knew their private valuation, and for risky prospects they knew the prospect they could win in the auction. Their valuations were correlated because of a two-step procedure used to draw valuations and prospects from some interval of the whole payoff range (Kagel *et al.*, 1987, p. 1277). That is, a high private value observed by the agent makes it more likely that the other bidders also have high values.

More specifically, our experimental subjects participated in a series of twelve anonymous first-price auctions. In each of them, they could bid for an object from an endowment of €10 in groups of three bidders. In order to produce matched bids for prospects and their individual certainty equivalents, each participant was bidding for two risky prospects and her two corresponding individual certainty equivalents that were elicited before in four out of the twelve auctions (see section 3.3.2 for details of the elicitation procedure). We call the specific bidder whose certainty equivalent is used the *bidder of interest* and the matched observations for the risky object and the appropriate individual certainty equivalent an *auction pair*. In the remaining eight auctions the private valuation of the bidder was drawn according to the procedure described in the following paragraph, with each of the other two bidders being the bidder of interest four times in turn.

For risk-free prospects, the bidder of interest's certainty equivalent for the matched prospect provided her private valuation v_i. For the other two bidders in the group the valuations were determined as follows: v_i was first reduced by a random number $z_{(-)}$, which was drawn from the interval [€0, €2], and then increased by a random number $z_{(+)}$ from the same interval. The number obtained, $v_0^D = v_i + z_{(+)} - z_{(-)}$, forms the midpoint of a €4-interval in which all three deterministic valuations (hence, superscript D) lie. Subjects did not learn the midpoint of the interval. Hence, the valuations of the other two bidders within a group were drawn from the interval $[€v_0^D - €2, €v_0^D + €2]$. By construction, the value v_i lies in the interval and can assume any position in this interval, like the two other valuations. Figure 3-1 illustrates the procedure for v_i = €4.21, v_0 = €5.11, and Bidder 1 as bidder of interest.

For risky prospects, the procedure was similar. The bidder of interest had to bid for a risky prospect, presented in terms of its expected value plus or minus a fixed and announced amount $K \in \{2,3,4,5\}$ with equal probability.[21] For each prospect, K follows uniquely from the rewriting of the gamble presentation in the preceding risk elicitation stage of the experiment (see section 3.3.2 for details). The risky prospects for the other two bidders were determined as described for sure objects, but taking as v_i the expected value of the risky prospect for the bidder of interest. This gives expected values for the other two bidders in the range $[€v_0^R - €2, €v_0^R + €2]$, to which the same ex-post risk of size K was added for all bidders.

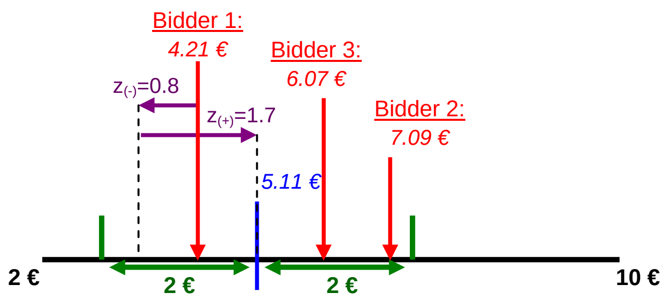


Figure 3-1: Illustration of valuation assignment

For example, assume that Bidder 1 indicated a certainty equivalent of €4.21 for the prospect [0.5: €6.36, 0.5: €2.36] in the risk elicitation task preceding the auction experiment. In the auction experiment, she would face one auction as illustrated in Figure 3-1 and another auction that would be described as offering an object with risky value of €4.36 that will, with a probability of 0.5 each, either be increased or decreased by an amount of K = €2. The two other bidders in the group would face a randomly drawn sure valuation out of the permissible range in the first auction and a randomly drawn risky valuation out of the permissible range (according to the procedure described above) in the second auction.

[21] The presentation is identical to the theoretical formulation of ex-post risk as a noise added to a private valuation. Note that K took on the different values for different auctions but was always clearly announced before bidding.

Subjects were instructed about the general procedure of drawing values and the method of affiliation in great detail (see appendix A1 for the experimental instructions), but they were neither aware of the presence of a bidder of interest, nor of the fact that we took their certainty equivalents and prospects from the preceding risk elicitation stage, nor of the private valuations of the two other bidders. More precisely, they were simply told that the private valuations of the three bidders come from a €4-interval lying within a larger interval and were distributed randomly along this €4-interval (which was true by construction). Everything else was common knowledge among participants. Neither intermediate auction results nor bids were revealed before the end of the experiments, and groups stayed together for all twelve auctions.[22]

After the twelve auctions, one auction was randomly selected for real payment of the full amount in euro. The auction winners paid their bids and received the payoff from the auction, possibly depending on the result of the ex-post risk, and they kept the rest of their €10-endowment that they had not used for bidding. Subjects who did not win the auction kept their endowment of €10. All randomizations concerning risky equal-chance events in the experiment were conducted by throwing dice at the subjects' desks.

Remember that the twelve auction rounds give us, per subject, two matched auction pairs (bids) for identical sure and risky valuations, and four more observations for bids for sure valuations. That is, in total we know individual valuations in eight out of the twelve auctions and can use this information for econometric analyses. We do not observe the valuation for subjects who were not the bidder of interest in the remaining four auctions for risky prospects. Only the bidder of interest submits a bid for a prospect for which we previously elicited her valuation in the risk preference elicitation stage that is described in the next sub-section.

[22] Groups were formed randomly, but subjects who were close to each other in their risk attitude rank (within a session of 15 subjects) from the preceding risk elicitation had a higher chance to end up in the same group. This procedure approximates the assumption of identical risk attitudes for bidders in EW's model and was explained in neutral terms to the participants (see the instructions in appendix A1).

3.3.2 Elicitation of Risk Preferences

At the beginning of the experiment, we elicited subjects' certainty equivalents for eleven risky prospects. All prospects were binary-outcome prospects with a 50% chance of each outcome (see Table 3-1, columns 1-3). Certainty equivalents were elicited using the Becker-DeGroot-Marschak (1963) incentive mechanism (henceforth, BDM). Subjects were asked to state for each prospect their minimum selling price p_s between the low and the high outcome of the prospect. They knew that a random buying price p_b between these two outcomes would be drawn to determine if the prospect is sold to the experimenter if $p_b \geq p_s$, in which case the subject received the randomly drawn buying price, or is not sold otherwise, in which case the subject received the outcome of the prospect.

The BDM mechanism has been used extensively in preference elicitation and is valid in our expected utility framework (e.g., Karni and Safra, 1987; Halevy, 2007). However, no BDM randomizations or risky prospects were resolved at this stage in order to prevent wealth differences between subjects in the auctions to come. Subjects were instructed that at the end of the experiment they would be paid on the basis of the outcome of one of the risky prospects or receive the random buying price, depending on the outcome of the BDM procedure.

Prospect no.	High payoff (prob. 50%)	Low payoff (prob. 50%)	Expected value	Average CE with BDM[a]	Average CE with choice list[b]
1	12.76	4.76	8.76	7.82	8.18
2	8.30	2.30	5.30	5.00	5.18
3	10.70	2.70	6.70	6.03	6.23
4	6.52	2.52	4.52	4.10	4.41
5	13.22	5.22	9.22	8.54	8.61
6	8.06	2.06	5.06	4.70	5.04
7	6.36	2.36	4.36	3.94	4.38
8	13.20	3.20	8.20	7.83	7.67
9	9.76	5.76	7.76	7.22	7.47
10	12.76	6.76	9.76	8.93	9.21
11	8.01	2.01	5.01	4.68	5.00

Numbers in columns 2-6 show amounts in euro. [a] CE = Certainty equivalent; BDM = Becker-DeGroot-Marschak mechanism. [b] For an explanation of the right-most column, see section 3.5.

Table 3-1: Risky prospects used in the experiment

3.3.3 Laboratory Protocol and Subjects

Computer-based experiments were conducted at the experimental laboratory MELESSA of the University of Munich, using the experimental software z-Tree (Fischbacher, 2007) and the organizational software Orsee (Greiner, 2004). 75 undergraduate students without experience in auction experiments participated in 5 sessions with 15 subjects each. Sessions lasted up to 2 hours, and the average final payoff was €23.75, including a show-up fee of €4. Subjects received written instructions which were read aloud and had the opportunity to ask questions in private. Examples and/or test questions were given for each stage of the experiment, and a stage only began when all subjects correctly understood the procedures.

The experiment started with the risk elicitation stage. Subjects received instructions for this stage, but knew that there would be further stages in the experiment. Upon completion, subjects received instructions for the second stage of the experiment. This stage was purely instructional, i.e., it was intended to make subjects acquainted with bidding in first-price auctions with and without ex-post risk. Subjects participated in twelve affiliated private value first-price sealed-bid auctions for six risky and six sure prospects. Auctions were held anonymously in groups of three people, with new groups formed in every auction. Again, subjects with a similar risk rank from the elicitation stage had a higher chance to end up in the same group in each auction. Subjects could bid from an endowment of €10 in each auction. All bids within a group, the winning bid and the winner were announced immediately after each auction to acquaint subjects with the affiliated value model and with bidding for uncertain prospects. Only at the end of the entire experiment, one auction from this training stage was randomly selected and subjects were paid according to the outcome. Furthermore, payments were scaled down by a factor of 1/10 (compared to the main auction experiment in stage 3). With the exception of the size of earnings and the specific procedure of taking prospects and certainty equivalents from the risk elicitation stage, this second stage of the experiment was identical to the main auction stage that was to follow. After stage 2 had ended, subjects received instructions for the main stage of the experiment, the twelve auctions as described in section 3.3.1.

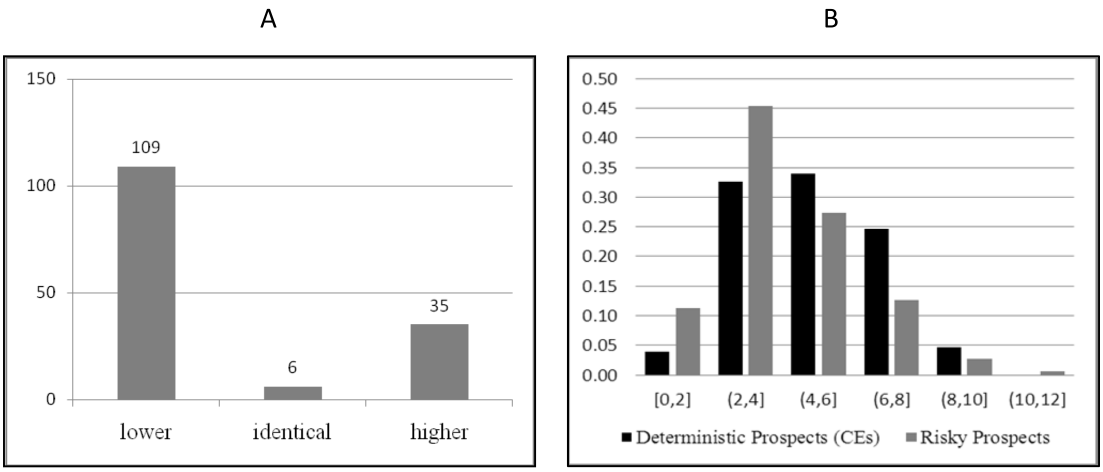
Remember that subjects were not informed about the matched structure and the bidder of interest in the main stage of the experiment. Note that, while prospects were identical to the ones in the risk elicitation for the bidder of interest, the presentation of the prospects was quite different. They were framed in terms of ex-post risk instead of binary gambles and were not easily recognized as the same prospects as in the first stage[23], also because the training stage introduced a significant time lag.

At the end of the experiment, all random aspects of the experiment were resolved, and subjects learned what they had earned in each of the three stages of the experiment. They were paid privately and in cash and then dismissed from the laboratory.

3.4 Results of the Main Experiment

Column 5 in Table 3-1 shows the average certainty equivalents for the lotteries elicited in the first stage of the experiment. On average, subjects exhibit risk aversion, with CEs being smaller than expected values for all prospects (Wilcoxon signed-rank tests; $p < 0.01$).

Figure 3-2 provides clear evidence consistent with the hypothesized precautionary bidding effect $b_i(v_i) < b_i(CE_i(v_i))$.



Notes:
Panel A: Number of pairs in which the bid for a risky prospect was (lower/identical/higher) than the bid for its certainty equivalent (within-person comparisons).
Panel B: Distribution of bids (in €) for risky prospects and for their deterministic certainty equivalents; % of subjects.

Figure 3-2: Comparison of bids for risky prospects with bids for their certainty equivalents (BDM)

[23] If so, it would make our results even stronger. Details are provided in the next section.

In Panel A, for the 150 matched auction pairs (2 auction pairs for each of the 75 subjects), we show the number of pairs in which a buyer made a lower, identical, or higher bid for the risky prospect than for its CE. Clearly, risky prospects elicit lower bids than their certainty equivalents from the same individual (Wilcoxon signed-rank test; $p < 0.01$). In 109 matched auction pairs, a lower bid was submitted for the risky prospect than for its certainty equivalent, in comparison to only 35 pairs with higher bids for the risky prospect. There were virtually no identical bids, suggesting that subjects did not simply remember prospects and their certainty equivalents from stage 1 and tried to be consistent.[24] Panel B of Figure 3-2 shows that for risky prospects the whole distribution of bids shifts toward the left compared with bids for the matched certainty equivalents. Increased bid shading for risky prospects is also consistent within person, with 50 subjects always shading more for risky, 14 always shading less, and only 11 with mixed behaviour.

Alternatively, one can perform a parametric utility analysis to assess precautionary bidding. The first benchmark measure for the evaluation of sure and risky prospects in our experiment are risk-neutral Nash equilibrium bids (Kagel *et al.*, 1987), given in equation [2]:

$$b(v_i) = v_i - \frac{2\varepsilon}{n} + \frac{1}{n} \cdot \frac{2\varepsilon}{(n+1)} \cdot \exp[-\frac{n}{2\varepsilon} \cdot (v_i - (\underline{s} + \varepsilon))]. \qquad [2]$$

In the case of our experiment, $n = 3$ (number of bidders), $\varepsilon = 2$ (radius of the smaller interval), and $\underline{s} = 0$ (lower bound of the larger interval). For each auction and each bidder we calculate the risk-neutral Nash equilibrium bids from individual valuations. We find significant overbidding for the sure prospects, consistent with previous findings in the experimental literature for risk averse subjects (Cox *et al.*, 1988).[25] The actual bids are significantly higher than the risk-neutral Nash bids

[24] Note that even if some bidders of interest had recognized the prospects from the risk elicitation stage in the later auction stage and understood the construction of our matched auction pairs, they had no more relevant information on values and intervals than the other bidders in the group. In the data, there is no evidence whatsoever of bidders of interest bidding differently than the other bidders.

[25] Overbidding is a common empirical phenomenon in first-price auctions. Explanations fall roughly into three categories: risk aversion, inter-personal comparisons, and non-equilibrium behavior or learning. Surveys of the literature are, for instance, provided in Crawford and Iriberri (2007) and Engelbrecht-Wiggans and Katok (2007).

(Wilcoxon signed-rank test, $p < 0.01$). For risky prospects we find significant underbidding with respect to the risk-neutral Nash equilibrium bids (Wicoxon signed-ranks test, $p < 0.01$). As a robustness check, one can aggregate relative overbidding and underbidding on the individual level. The basic result for the 75 subjects does not change, and both tests are still significant ($p < 0.01$ for sure prospects, and $p < 0.05$ for risky prospects).

In a next step, we estimate an individual utility function for each subject based on the data we have from the certainty equivalent elicitation stage (see Table 3-1). This allows us to calculate *risk averse Nash equilibrium bids* for each bidder and auction based on the individual risk aversion parameters and the (expected) value of the risky or deterministic prospects. These Nash bids are then again compared to the actual bids.

More specifically, we use nonlinear least squares estimations to fit a constant relative risk aversion utility function (CRRA), $u(x) = x^{1-r}/(1-r)$, with risk aversion parameter *r*, for all 75 subjects individually.[26] For each risky and for each risk-free auction, we can then calculate risk averse Nash equilibrium bids and compare them to the actual bids. Nash bids are calculated according to the equilibrium bidding formula in Kagel *et al.* (1987).

$$b(v_i, \rho) = v_i - \frac{2\varepsilon \cdot (1-\rho)}{n} + \frac{1}{n} \cdot \frac{(1-\rho)^2 \cdot 2\varepsilon}{(n+1-\rho) \cdot n} \cdot \exp[-n \cdot [v_i - (\underline{s} + \varepsilon)] / 2\varepsilon \cdot (1-\rho)] \quad [3]$$

with $\rho = r$ (risk parameter of the utility function).

Equation [3] can only be applied if $r < 1$. Several subjects in our experiment are more risk averse than that. We therefore truncate their risk aversion parameter *r* at 0.99, which underestimates the actual level of their risk aversion. Nash bids are virtually identical to actual bids for sure prospects (Wilcoxon signed-rank test, $p = 0.83$). However, we observe strong underbidding compared to the benchmark solution for risky prospects (Wilcoxon signed-rank test, $p < 0.01$). The robustness check of using

[26] We selected a CRRA utility function because it has been widely applied in the literature on risk aversion and first-price auctions. It obviously has the DARA property.

relative bids, aggregated on the individual level, gives the same general picture (p = 0.55 for sure prospects; $p < 0.01$ for risky prospects).

In order to avoid arbitrary parametric utility assumptions and to fully exploit the model-free nature of our test of precautionary bidding, we estimate the quantitative effect of ex-post risk on bids by using regression analyses, controlling for the panel structure with eight observations per subject.[27] Plotting valuations and bids suggested a linear specification.[28] We include a dummy variable for bids made for risky prospects and a coefficient that captures the interaction of valuations with the presence of the ex-post risk. Model I in Table 3-2 shows that for sure prospects buyers shade their bids by 15 cents per euro valuation. In the presence of risk, bids are reduced by another 18 cents per euro valuation for the prospect. That is, the precautionary bidding effect is observed, because *equally valuable* risky and sure prospects elicit significantly different bids. Bidders shade their bid approximately twice as much when the good is affected by ex-post risk than when it is not.

Dep. Var: bid	I (BDM)	II (BDM, excl. bids with low risk rank[a])	III (BDM, excl. bidders with low risk rank[b])	IV (BDM)
valuation	0.849**	0.874**	0.831**	0.854**
	(0.024)	(0.026)	(0.026)	(0.024)
risk	0.030	0.322	-0.351	
	(0.281)	(0.301)	(0.330)	
risk ×valuation	-0.181**	-0.229**	-0.133**	
	(0.045)	(0.051)	(0.051)	
risk size K[c]				-0.116
				(0.092)
risk size K × valuation				-0.029*
				(0.014)
constant	0.073	0.007	0.127	0.032
	(0.155)	(0.163)	(0.178)	(0.154)
# observations (bids)	600	461	480	600
(# bidders)	(75)	(74)	(60)	(75)
R^2	0.67	0.69	0.69	0.66

Fixed effects panel regressions: standard errors in parenthesis; ×: interaction; ** represents significance at p=0.01 and * at p=0.05. BDM: Becker-DeGroot-Marschak mechanism; Risk rank: discrete variable ranging from 1 (least risk averse) to 15 (most risk averse).
[a] Bids with lowest risk ranks excluded (ranks 1 to 3 out of 15).
[b] Bidders with lowest risk rank in their session excluded (ranks 1 to 3 out of 15).
[c] Risk size K: margin between lowest and highest outcome of the prospect (€2, €3, or €4).

Table 3-2: Determinants of bidding behavior

[27] Remember that for each subject, we know private valuations for two risky and six sure prospects.

[28] Models with non-linear specifications as a robustness check are provided in the appendix A2.

In models II and III in Table 3-2 we address the robustness of the effect with respect to the *risk aversion rank* of a specific certainty equivalent or of a specific bidder. The least risk averse certainty equivalent for a given prospect has rank 1 and the most risk averse certainty equivalent has rank 15 within an experimental session. Similarly, the subject in each session with the lowest average risk-aversion rank over all eleven prospects has rank 1 etc. From a psychological perspective, it could be argued that the effect found in the auction is driven by subjects who do not exhibit a stable risk attitude or who reveal too large certainty equivalents by mistake in the first stage of the experiment, and successively make very low bids in the auction (regression to the mean). Notice that buyers who provided relatively high certainty equivalents will have low risk ranks. We distinguish between individual certainty equivalents that are high for a certain prospect and may come from different bidders for different prospects (model II) and bidders who generally state high certainty equivalents (model III). In Table 3-2, we show the regression results when we exclude observations or bidders with the lowest three risk ranks. In the two alternative specifications the precautionary effect stays both economically and statistically significant. Standard errors increase due to the loss of more than 100 observations in each model, but the estimates are very robust. Note that while we have chosen to exclude the lowest three ranks, our results do not change when we exclude fewer or some more of the high certainty equivalents. Further, a standard regression-to-the-mean explanation would equivalently imply that low-certainty-equivalent bidders should have higher actual valuations, and, therefore, increase their bids for the risky prospects compared to the matched certainty equivalents. This effect, were it present, would reduce the observed precautionary bidding effect.

The right-most column in Table 3-2 considers the comparative static effect of increases in risk. Since the size of the ex-post risk varied among lotteries, we can test whether the bid-shading effect is correlated with the size of the risk. Indeed, we observe that the median increase in bid shading for risky prospects over the bid shading for the paired CEs equals €0.20, €1.21, and €1.30 for risk of size €2, €3, and €4, respectively.[29] That is, an increase in bid shading is obtained for increases in ex-post risk. Regression IV in Table 3-2 confirms the effect. For each euro increase in the risk-level K, bid shading increases by about 3 cents per euro valuation.

[29] As we only employed prospects 1 to 6 in Table 3-1 to determine the valuations for the auctions, we were not able to observe bid shading for risk level K=5.

3.5 Control Experiment

The results of the main experiment support the precautionary bidding hypothesis. However, several alternative, though ad-hoc, explanations of our data are conceivable. One could, for instance, claim that all subjects consistently reveal too high certainty equivalents in the elicitation stage. Although the BDM mechanism is widely used for preference elicitation (Halevy, 2007, p. 507), an upward bias for BDM selling prices has sometimes been reported (Isaac and James, 2000; Plott and Zeiler, 2005). If all subjects reported too large certainty equivalents, the negative effect of risk on bids could be explained by downward revision of the valuations of risky prospects in the auctions.

Another possible, non-expected-utility explanation builds on the behavioral concept of loss aversion. If outcomes are described in terms of gains and losses from some reference point, subjects hold lower valuations of a prospect compared to a description in terms of gains only. In the risk elicitation stage of the experiment, prospects were described as binary gambles with two positive outcomes. Because of the affiliated value structure with sure and risky prospects, it was more natural to describe prospects in terms of a valuation plus ex-post risk in the auctions. The natural ex-post risk description, however, may have led subjects to frame the prospects in terms of an equal-chance gain or loss from the reference point of the sure valuation. This might have made the risky prospects less attractive than in the binary presentation in the risk elicitation, and, therefore, appear less valuable than the matched certainty equivalents.

Although these behavioral biases provide more ad-hoc explanations than the precautionary bidding model, they have been shown to be descriptively relevant in other situations and may provide a psychologically convincing alternative explanation to the equilibrium model. We therefore conducted a control experiment that is able to assess potential effects of a selling price bias and loss aversion.

3.5.1 Design and Hypotheses

The control experiment (conducted with 75 new subjects in five sessions) was completely identical to the main experiment, except for the following two features. First, the subjects' certainty equivalents were elicited by a choice list. For each prospect, subjects made 21 choices between the risky prospect and a sure payoff, with all choices shown simultaneously on the screen (see screenshot in appendix A1). The lowest sure payoff was equal to the low outcome of the prospect, and the highest sure payoff was equal to the high payoff of the prospect, and these two choices were actually pre-determined for the subjects on the screen in order to enforce stochastic dominance. The 19 choices between the high and the low sure payoff were equally spaced in monetary units. These choices had to be filled in by the subjects, and the certainty equivalent was calculated as the midpoint between the highest sure amount for which the subject prefers the risky prospect, HS_i, and the lowest sure amount for which she prefers the sure payoff LS_i, i.e. $CE_i = (HS_i + LS_i)/2$. Because we needed a unique switching point to calculate individual certainty equivalents for the subsequent auction stages of the experiment, we only allowed a single switching point for each individual in the choice list. As in the main experiment, at the end of the experiment one prospect was randomly selected for real pay. For this prospect, one of the 21 choices was randomly selected, and subjects were paid for this stage according to their decisions for the selected choices.

The second design change regards the inclusion of another choice list at the end of the experiment that has been interpreted as a measure of loss aversion and has been widely used recently (Fehr and Götte, 2007, p. 316; Gächter *et al.*, 2007; Fehr *et al.*, 2008). Subjects are offered a series of prospects, giving an equal chance of either a gain or a loss that they could choose to play or not to play (Table 3-3). They were free to accept or reject any prospect, that is, we did not require single switching from acceptance to rejection as the loss increases along the list.[30] Payments for this choice list were according to decision in *all* six choices, depending on the outcome of the risky prospects.

Prospect (50%–50%)	Accept to play?
Lose €2 or win €6	Yes O No O
Lose €3 or win €6	Yes O No O
Lose €4 or win €6	Yes O No O
Lose €5 or win €6	Yes O No O
Lose €6 or win €6	Yes O No O
Lose €7 or win €6	Yes O No O

Table 3-3: Choice list measure of loss aversion[31]

[30] In fact, all our subjects had a single switching point.

[31] Adapted from Gächter *et al.* (2007).

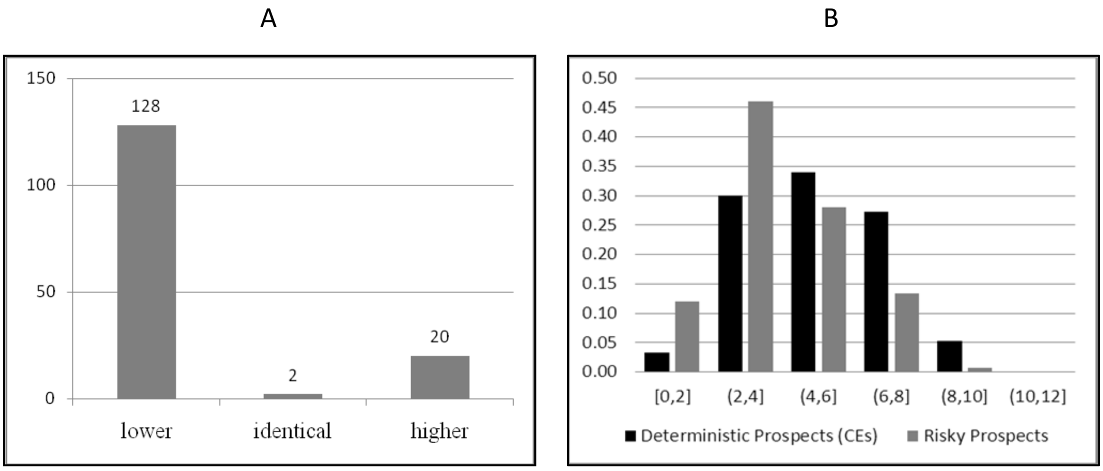
For losses smaller than €6, rejecting to play the prospect implies a significant loss in expected value that may be explained more easily by a gain-loss framing and a kinked utility function of wealth changes than by a concave utility of wealth. It has also been shown that the predictions of reference-dependent utility models hold mainly for people who reject most of the prospects in this choice list (Fehr and Götte, 2007). While we do not aim to add to the debate regarding utility curvature versus loss aversion, we call subjects who reject more prospects in this task *more loss averse,* in line with the alternative behavioral hypothesis we aim to test. Assuming the loss-aversion explanation for the choice list clearly implies that the precautionary effect should be driven by the most loss-averse subjects. Loss-averse subjects could value the prospects lower if presented in terms of ex-post risk in the auction rather than as a binary lottery in the initial risk elicitation stage, leading to a reduction of bids for risky prospects compared to their elicited certainty equivalents. This leads to the following two hypotheses originating from behavioral considerations.

HYPOTHESIS 2 *(BDM Selling): Hypothesis 1 holds only for certainty equivalents elicited through BDM selling prices.*

HYPOTHESIS 3 *(Loss Aversion): Hypothesis 1 holds only for loss averse bidders.*

3.5.2 Results of the Control Experiment

Table 3-1 shows in the right-most column that under the choice list procedure the elicited certainty equivalents were not smaller than under the BDM selling price procedure. In fact, certainty equivalents for prospects 4, 7, 9, and 11 were even significantly larger for the choice list procedure (Mann-Whitney tests (two sided), $p < 0.05$); all other certainty equivalents were not significantly different for the two methods. These first results indicate already that there was no upward bias through the BDM elicitation of certainty equivalents. Further, Figure 3-3 shows an identical pattern as for the main experiment, with the precautionary effect being even stronger on average. Of the 150 matched pairs of auctions the risky prospect elicited lower bids than its certainty equivalent in the large majority of cases (Wilcoxon signed-rank test, $p < 0.01$). Again, the whole distribution of bids shifts towards the left, compared with bids for the matched certainty equivalents. We can therefore clearly reject behavioral Hypothesis 2.



Notes:
Panel A: Number of pairs in which the bid for a risky prospect was (lower/identical/higher) than the bid for its certainty equivalent (within-person comparisons).
Panel B: Distribution of bids (in €) for risky prospects and for their deterministic certainty equivalents; % of subjects.

Figure 3-3: Comparison of bids for risky prospects with bids for their certain equivalents (CL)

As before, conducting a parametric utility analysis for risk-free prospects, we observe significant overbidding in comparison to the risk-neutral Nash equilibrium bids (Wilcoxon signed-rank test, $p < 0.01$), and for risky prospects we observe risk-neutral Nash bids that are much larger than actual bids (Wilcoxon signed-rank test, $p < 0.01$). Overbidding now does not vanish when one compares actual bids to the *risk averse equilibrium Nash bids* based on CRRA utility functions (Wilcoxon signed-rank test, $p < 0.01$) for sure prospects. However, underbidding is nevertheless highly significant for risky prospects (Wilcoxon signed-rank test, $p < 0.01$). Not surprisingly, given the similarity of the descriptive results from the main experiment and the control experiment, all our conclusions regarding precautionary bidding from the utility analysis for the main experiment remain valid for the choice list procedure. This is also true for taking average bids, aggregated on the individual level, as the basis for the statistical comparison.

As in the main experiment we estimate the quantitative effect of ex-post risk on bids using fixed effects panel regressions, shown in Table 3-4. Model I in the table shows our basic regression, now for the choice list (CL) experiment. The results are very similar to the main experiment, with bid shading of about 15 cents per euro valuation and a significant precautionary effect of another 28 cents per euro reduction under ex-post risk. In models II to IV we test for differences between the precautionary effect jointly for BDM- and CL-elicitation stages, using the complete data from both experiments. Model II uses all observations and includes an interaction dummy taking up the difference between the BDM and the CL experiment. The precautionary effect remains significant and its magnitude (a 20 cent reduction in the bids per euro valuation) is considerable.

Figure 3-4 shows the distribution of the number of prospects rejected in the loss aversion measurement task. Note that all subjects switched at most once, and all switched from accepting the first prospects (see Table 3-3, small losses) to rejecting the later prospects (larger losses).

Dep. Var.: bid	I (CL)	II (CL & BDM)	III (CL, LA dummy)	IV (CL, LA continuous)
valuation	0.853**	0.851**	0.857**	0.856**
	(0.020)	(0.016)	(0.020)	(0.020)
risk	0.371	0.190	-0.430	0.447
	(0.256)	(0.190)	(0.254)	(0.257)
risk×valuation	-0.276**	-0.205**	-0.238**	-0.212**
	(0.039)	(0.032)	(0.040)	(0.046)
risk×valuation×CL	-	-0.044*	-	-
		(0.017)		
risk×valuation×LAd	-	-	-0.084**	-
			(0.022)	
risk×valuation×LAc	-	-	-	-0.022**
				(0.008)
constant	-0.159	-0.042	0.187	-0.181
	(0.137)	(0.103)	(0.136)	(0.137)
# observations (bids)	600	1200	600	600
(# bidders)	(75)	(150)	(75)	(75)
R^2	0.74	0.71	0.75	0.74

Fixed Effects Panel Regressions: standard errors in parenthesis; ×: interaction; ** represents significance at p=0.01 and * at p=0.05. BDM: Becker-DeGroot-Marschak mechanism; CL: choice list mechanism; LAd: loss aversion dummy;Lac: loss aversion continuous.

Table 3-4: Determinants of bidding behavior

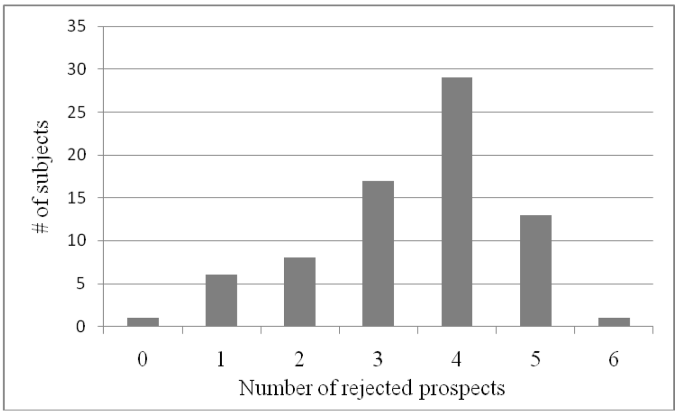


Figure 3-4: Distribution of loss aversion

The median number of rejected prospects is 4, replicating the findings in Fehr *et al.* (2008) and Gächter *et al.* (2007). In regression model III we include the loss aversion measure as a dummy for those bidders who reject four or more prospects (median split), and in regression model IV we include the raw number of rejected prospects. Both regressions show that loss aversion does increase the precautionary bidding effect. The loss aversion measures increase the model fit and add significantly to the precautionary effect. However, the coefficients for the precautionary bidding effect stay at around 20 cents per euro valuation and remain highly significant. Thus, our results clearly reject hypothesis 3. The precautionary bidding effect is robust and cannot be explained solely by the two behavioral effects. Finally, the comparative static effect of increases in risk (the level of K) on the level of bid shading for risky prospects emerges strong also in the choice list based experiment as it did in the main experiment.[32]

3.6 Discussion

Esö and White (2004) showed theoretically that ex-post risk in affiliated value auctions has an unambiguous effect for bidders with decreasing absolute risk aversion: bids for risky prospects in the first-price auction are discounted by more than the appropriate risk premium. This is a strong result given the several simultaneous effects of risk aversion on bid shading in the first-price auction. If precautionary bidding is descriptively relevant, the theoretical result has implications for optimal information collection and revelation by sellers, strategic information acquisition by buyers and, more generally, auction design. An empirical assessment of precautionary motives in auction bidding cannot be obtained easily, however, because it requires independent knowledge of risk and risk attitudes, both difficult to measure precisely in the field.

We designed an experimental auction for risky and sure prospects that aims to provide a first empirical assessment of precautionary bidding. Our study directly compared bids for risky prospects with bids for their relevant certainty equivalents on the individual level. It thus allows for a model-free measurement and gives a

[32] Median increases in bid shading equal €1.00, €1.48, and €1.57 for risk size of €2, €3, and €4. A regression analogously to model IV in Table 3-2 reveals an additional shading of about 6 cents per euro valuation for each euro increase in risk ($p<0.001$).

behavioral definition of the precautionary premium. We find robust evidence that is consistent with the predicted effect. Bids are significantly lower for risky prospects than for the appropriate certainty equivalent for a large majority of our experimental subjects. That is, *bidders are significantly better off bidding for a risky object than for an equally valued risk-free object*. Consistent with the experimental auction literature, we find on average overbidding with respect to the risk-neutral Nash equilibrium for sure objects and that the risk averse Nash equilibrium under expected utility describes bidding behavior for sure objects reasonably well. Corroborating the precautionary bidding effect, in the presence of ex-post risk, there is significant underbidding with respect to the risk-neutral *and* the risk averse Nash equilibrium bids.

Although the empirically observed effect in our experiments is consistent with the idea of a precautionary premium, alternative interpretations are possible. Two behavioral interpretations, based on the elicitation of certainty equivalents and on loss aversion, were rejected in the control experiment. Another potential interpretation involves subjects' beliefs about their competitors' bidding behavior (Costa-Gomes and Weizsäcker, 2008; Heinemann *et al.*, 2009). Subjects may simply believe that other subjects shade the bids for risky prospects more strongly than for sure prospects. It is not clear, however, why they would expect a discount that is larger than the expected risk premium. Thus, a belief-based explanation would rather depend on subjects systematically overestimating the risk premia of other subjects. The existing literature provides little support for such an effect (Ball *et al.*, 2010; Faro and Rottenstreich, 2006). We therefore think that systematically biased believes alone cannot provide a convincing explanation for the observed behavior in the auctions. Another concern about the interpretation of our result as a precautionary effect relates to the unobserved degree of prudence. As Kimball (1990, p. 54) argued, the precautionary effect relates to the propensity of people to "prepare and forearm oneself on the face of uncertainty." In terms of the current analysis, if a buyer wins the auction and has to carry the risk, she wants to be prepared by holding more wealth and will bid less aggressively for the risky good. Although the precautionary effect is quite intuitive in risky auction settings, it may come as a surprise to observe it in a laboratory experiment. The finding is less surprising, however, given that a considerable level of risk aversion in auction experiments are a standard finding, and, in particular, that also behavior consistent with decreasing absolute risk aversion

(DARA) has been observed on experimental markets in the laboratory. For instance, Levy (1994) conducted a dynamic portfolio choice experiment where subjects made investment decisions under changing wealth levels. Payoffs were given in terms of a few thousand experimental euros, but they translated into typical laboratory payoffs, since the market earnings were divided by a factor of 1000. Levy found clear evidence for decreasing absolute risk aversion in terms of experimental wealth. No effect of real wealth on risk taking in the experiment has been observed, however. Levy suggested that subjects make their decision within the frame of payoffs relevant in the experiment and therefore show sensitivity to otherwise rather small changes in payoffs.

Similar findings are provided in Deck and Schlesinger (2010) and Noussair *et al.* (2011), who measure prudence directly using methods in Eeckhoudt and Schlesinger (2006). Both papers show clear evidence of strong prudence for typical experimental stakes, like the ones used in this chapter. Nousair *et al.* (2011) explicitly test for CARA and reject it in favor of DARA. These findings lend support to the interpretation of our results in terms of precautionary bidding. For bidders with decreasing absolute risk aversion, absolute prudence is larger than absolute risk aversion, leading to the additional precautionary premium.

3.7 Conclusion

This chapter demonstrated increased bid shading in experimental first-price auctions for risky prospects, over and above the size of the risk premium. Thinking of, e.g., auctions for consumer products at online platforms, auctions for art items, and auctions for licenses or procurement contracts, stakes and risks are much larger in the real world than in our experiment. We therefore expect precautionary bidding to be an important factor, affecting bids and prices on non-laboratory markets as well.

In some settings, however, precautionary bidding effects may be mitigated by other influences that lead to an upward bias in bidding. Goeree and Offerman (2003) test explanations of the winner’s curse using auctions with noisy signals of an uncertain private value. In the context of the current study, this would be similar to resolving the risky prospects ex-ante and providing subjects with a noisy signal of the outcome. Goeree and Offerman observe too optimistic bidding for these private value auctions, similar to the winner's curse for common values, an effect they call the *news curse*. In situations where buyers receive noisy signals of the value of a risky good, the precautionary effect may therefore be counterbalanced by the news curse.

Related settings where ex-post risk may not necessarily lead to precautionary effects involve auctions with resale opportunities (Haile, 2003), and license auctions with aftermarkets (Janssen and Karamychev, 2009). With potentially countervailing effects on bids, it seems a fruitful direction for future research to study the comparative influence of ex-post risk deriving from different market structure on auction outcomes in controlled experimental settings. In a similar vein, within the precautionary bidding framework, a direct empirical test of the predicted market structure effects, including the buyers' selection into auctions for risky or risk-free goods and the incentives for sellers to invest in the reduction of ex-post risk, would be desirable.

3.8 Appendix

A1. Experimental instructions *(translated from German)*

Welcome to the experiment and many thanks for your participation!

From now on please do not speak with other participants

General Procedure

This experiment serves the investigation of economic decision making You can earn money which will be paid to you in cash after the experiment.

During the experiment you and the other participants are requested to make decisions. Your decisions, as well as the decisions of the other participants, will determine your monetary payoff according to the rules explained below. The whole experiment will take about two hours. If you have any questions during the experiment, please raise your hand. One of the experimenters will come to answer your questions at your desk.
In the interest of clarity, we use male terms only in our instructions.

Anonymity
In some parts of the experiment you will be grouped with other participants. Neither during the experiment nor afterwards you or the other participants will learn about the identity of other group members. Neither during the experiment nor afterwards the other participants will learn about your experimental earnings. We will never connect names with experimental results. At the end of the experiment you will have to sign a receipt about your personal earnings which only serves for accounting purposes. The sponsor of this experiment does not receive any experimental data.

Auxiliaries
You are provided with a pen on your desk. For calculations you will find a link to the *Windows calculator* on the screen.

The Experiment
The experiment consists of three parts. You will receive detailed instructions for each part after finishing the previous. In each part you can earn money. The sum of earnings will determine your final income.

Part 1[33]

Part 1 consists of a sequence of lotteries. Such a lottery could be structured as follows.

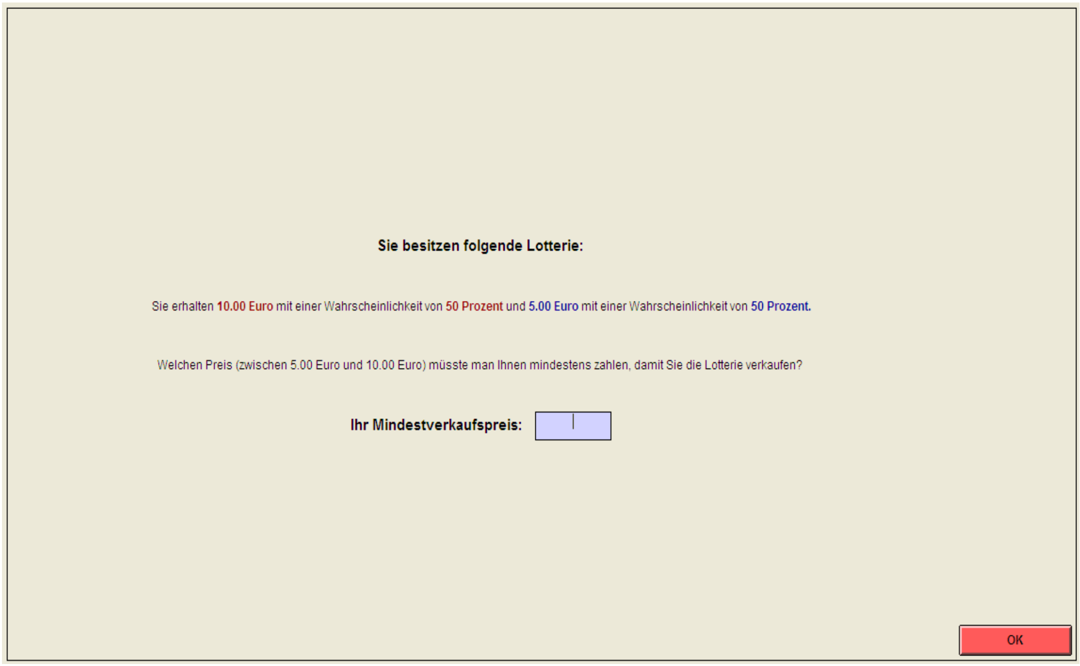


Figure 3-5: Screenshot – Bidding decision

In the above example you would earn €10 with 50% probability and €5 with 50% probability.

For each lottery you have **two possibilities**:

1. you can **gamble** or
2. you can **sell the lottery**.

Proceedings are as follows: You are asked to state a **minimal selling price** for the presented lottery. Minimal selling price denotes the price for which you are willing to sell the lottery. This price has to be within a predetermined range. For the above example the range would be from €5 to €10.

After stating a minimal selling price (an amount within the given range with two digits behind the comma) the **computer randomly generates a buying offer**. The offer is drawn from the same interval which predetermines the range of your choice – in the above example, between €5 and €10. Each two-digit number within this interval can be drawn with same chance. The computer's buying offer is purely random and totally independent from your chosen minimal selling price.

Afterwards the computer's buying offer and your chosen minimal selling price will be matched. **If the computer's buying offer is higher or equal to your minimal selling price**, you sell the lottery to the computer and receive an **amount equal to the computer's buying offer**. **If the computer's buying offer is smaller than your minimal selling price**, **no sale** takes place. You gamble and receive the lottery outcome. The procedure of the "gamble" will be explained in detail below.

Example 1: Let's assume for the lottery shown above you choose a minimal selling price of €7. Let's further assume the computer randomly generates a buying offer of €9.50. In this case the computer's buying offer is at least as high as your minimal selling price. You sell the lottery to the computer and receive an amount equal to the computer's buying offer, namely €9.50.

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[33] For the main experiment, i.e., the Becker-deGroot-Marschak mechanism.

Example 2: Let's once more assume you choose a minimal selling price of €7 for the lottery shown above. This time the computer randomly generates a buying offer of €6.50. Then the computer's buying offer is lower than your minimal selling price. You do not sell the lottery to the computer. You keep the lottery and gamble. Hence, you either receive €5 with 50% probability or €10 with 50% probability.

Please note:
The randomly generated computer offer is independent of your decision about your minimal selling price. Since in case of a purchase your earnings are not determined by your minimal selling price but by the computer's buying offer you should truly state the **minimal price for which you are just willing to sell the lottery**.

Altogether you will state minimal selling prices for **11 lotteries**. At the end of the experiment the **computer randomly picks one lottery**. Since you don't know which one, it is in your own interest to consider all your decisions for all the lotteries carefully. Then, the computer randomly generates a buying offer.

If the buying offer is higher or equal to your minimal selling price, you sell the lottery to the computer and receive an amount equal to the buying offer. If the buying offer is smaller than your minimal selling price, no sale takes place. In this case you gamble and receive the outcome of the lottery. More precisely, the experimenter comes to your desk and you **roll a six-sided die**. For the example above you would receive €5 if you roll the numbers 1, 2 or 3 or €10 if you roll the numbers 4, 5 or 6.

At the top right corner you will find a timer which gives you some temporal orientation for your decision. You can exceed this time limit (especially for the initial decisions, this might most likely be the case).

Part 2[34]

In part 2 in each round (in each auction) all participants will be matched in **groups of three**. The group composition may chance from auction to auction. However, you will always be matched with **participants who have a similar risk attitude**. (As a measure of risk attitude we use your decisions of part 1. From now on, none of your decisions will influence subsequent parts of the experiment).

You and both other group members will take part in an **auction** for fictitious goods. For such a good you receive a **private valuation (V)**. This private valuation may deviate from valuations that the two other members of your group receive. **Private valuations are determined as follows:** In a first step the computer will draw a random number out of a **larger interval**. Let's assume that the computer randomly chooses €9.00. This amount subsequently serves as the midpoint of a **smaller interval**. Later the private valuations will be drawn from this smaller interval. The smaller interval always has a **width of four**, meaning that in our example your private valuation as well as the private valuations of both other group members will be drawn from an interval between €7.00 and €11.00. Let's assume the computer randomly allocates you a private valuation of €8.50. You will learn about your private valuation before the auction starts. In this case you know that this number is drawn from a smaller interval with width 4, and you also know that the midpoint of the smaller interval is drawn from a larger interval. But **you do not know the midpoint of the smaller interval**.

After all group members learned about their private valuations, each group member bids for the good [**bid=(B)**]. Each group member receives an **endowment (E) of €10**. Bids above the endowment are allowed. Please note that this may possibly cause losses which will be subtracted from gains from

[34] Handed out after completion of Part 1.

other parts of the experiment. A group's highest bidder acquires the good and pays her bid. Outbid group members do not have to pay their bids. In case of a tie, a coin toss decides. Earnings are determined as follows:

Earnings:

- Highest bidder: E – B + V
- Outbid group members: E

Some auctioned goods, however, exhibit **risk**. The risk structure is always the same. **With 50% probability** your private valuation **increases by** a certain **amount (R)** and **with 50% probability** your private valuation **decreases by** the **same amount**. Let's assume an amount (R) of €3. In this case the earnings of the highest bidder will be either reduced or increased by €3 both with a probability of 50%. The amount **(R) is identical for all group members** (of course, only the winner has to bear the risk). Prior to each auction you will always learn if the auctioned good exhibits risk and if so by which amount (R) the winner's earnings will be increased or reduced.

Altogether, you will participate in **12 auctions**. Subsequent to each auction you will learn whether or not you have purchased the good. In addition, you learn about the other group members' bids. In case the auctioned good exhibit some risk, the resolution of the risk will take place at the end of the experiment.

For each of the 12 auctions you have an endowment of €10. At the end of the experiment, **one auction will be randomly selected** and the results of this auction will be paid out in cash. Since you don't know which one, it is in your own interest to consider all your decisions for all 12 auctions carefully. Each group member receives her earnings from this auction. Since this part is supposed to make you familiar with bidding in an auction and to give you a better understanding of the auction mechanism **all earnings will be divided by a factor of 10**.

Thus, an outbid player in the selected auction receives €10 * 0.1 = €1. A player who submitted the highest bid in the selected auction will receive her endowment minus her bid plus her valuation (if the good exhibits some risk: plus/minus (R)) divided by 10.

If in the selected auction you have purchased a good exhibiting a risk, the resolution of the risk takes place at the end of the experiment. More precisely, the experimenter will come to your desk and you roll a six-sided die. For the numbers 1, 2 or 3 your earnings will be reduced by the amount (R), and for the numbers 4, 5 or 6 your earnings will be increased by the same amount.

Part 3[35]

This part is very similar to part 2. Again, all participants will be matched in **groups of three** to participate in a number of auctions. As you already know from part 2, you will always be matched with participants exhibiting a **similar risk attitude** (as a measure of risk attitude we use again your decisions in part 1). Prior to each auction you will learn about your **private valuation** which will be determined **similarly to part 2**. Unlike in part 2, in this part your earnings **will NOT be divided by the factor 10**.

As in part 2, you bid either for goods with a certain value or for goods with a risky value depending on the auction. For each auction you receive an **endowment of €10**. Bids above the endowment of €10 are allowed, but in case you make a loss, it will be subtracted from gains stemming from other parts of the experiment. Earnings are determined as described in the instructions for part 2.

[35] Handed out after completion of Part 2

Altogether, you will participate in **12 auctions**. Unlike in part 2, subsequently to each auction you will neither learn whether you have purchased the good, nor what others have bid. Instead, after submitting your bid next auction starts.

If the auctioned good exhibit some risk, the resolution of this risk takes place at the end of the experiment. At the end of the experiment **one auction** will be **randomly selected** and the results of this auction will be **paid out** in cash. Since you do not know which one will be chosen, it is in your own interest to consider all your decisions for all 12 auctions carefully. Each group member receives her earnings from this auction.

Examples:

Example 1 (non-risky good):

Players A, B and C have been grouped together. For a **non-risky good** they receive the following **valuations**: **A:** €4.50; **B:** €8.10; **C:** €6.50

---

a) Let's assume knowing their valuations players submit the following bids:
A: €4.00; **B:** €6.00; **C:** €5.00

Player B submitted the highest bid and thus purchased the good. He has to pay a price equal to his bid, namely €6.00. This results in the following **earnings** in this period:

A: €10.00 (E); **B:** €10.00 (E) – €6.00 (B) + €8.10 (V) = €12.10; **C:** €10.00 (E)

In case this auction is selected to determine payoffs, players A and C receive €10.00, and player B receives €12.10.

---

b) Let's assume knowing their valuations players submit the following bids:
A: €4.00; **B:** €8.10; **C:** €5.00

Player B submitted the highest bid and thus bought the good. He has to pay a price equal to his bid, namely €8.10. This results in the following **earnings** in this period:

A: €10.00 (E); **B:** €10.00 (E) - €8.10 (B) + €8.10 (V) = €10.00; **C:** €10.00

In case this auction is selected to determine payoffs, all players receive €10.00.

---

c) Let's assume knowing their valuations players submit the following bids:
A: €3.00; **B:** €6.00; **C:** €9.00

Player C submitted the highest bid and thus bought the good. He has to pay a price equal to his bid, namely €9.00. This results in the following **earnings** in this period:

A: €10.00 (E); **B:** €10.00 (E); **C:** €10.00 (E) - €9.00 (B) + €6.50 (V) = €7.50

In case this auction is selected to determine payoffs, players A and B receive €10.00, and player C receives €7.50.

d) Let's assume knowing their valuations players submit the following bids:
A: €15.00; **B:** €8.00; **C:** €4.00

Player A submitted the highest bid and thus bought the good. He has to pay a price equal to his bid, namely €15.00. This results in the following **earnings** in this period:

A: €10.00 (E) - €15.00 (B) + €4.50 (V) = - €0.50; **B:** €10.00 (E); **C:** €10.00 (E)

In case this auction is selected to determine payoffs, players B and C receive €10.00 and player A makes a loss of €0.50. This loss will be deducted from gains he made in other parts of the experiment.

Example 2 (risky good):

Players A, B and C have been grouped together. For a **risky good** they receive the following **valuations**: **A:** €11.70; **B:** €9.10; **C:** €8.30

The good exhibits a risk. Its value will either increase by €3 (R) with 50% chance or decrease by €3 with 50% chance.

---

a) Let's assume knowing their valuations players submit the following **bids**:
A: €11.00; **B:** €5.00; **C:** €4.00

Player A submitted the highest bid and thus bought the good. He has to pay a price equal to his bid, namely €11.00. Due to the risk he has to gamble at the end of the experiment (in case this auction is selected to be payoff-relevant). Let's assume he is rolling a two with the die. Hence, his valuation for the purchased good is reduced by €3. This results in the following **earnings** in this period:

A: €10.00 (E) - €11.00 (B) + €11.70 (V) - €3.00 (R) = €7.70; **B:** €10.00 (E); **C:** €10.00 (E)

In case this auction is selected to determine payoffs, players B and C receive €10.00, and player A receives €7.70.

---

b) Let's assume players submit the same bids as in a) but this time player A rolls a four at the end of the experiment. Hence, his valuation for the purchased good is increased by €3. This results in the following **earnings** in this period:
A: €10.00 (E) - €11.00 (B) + €11.70 (V) + €3.00 (R) = €13.70; **B:** €10.00 (E); **C:** €10.00 (E)

In case this auction is drawn to determine payoffs, players B and C receive €10.00, and player A receives €13.70.

1) **Questions:**

Please choose "True" or "False":

- A player, who did not purchase a good, has zero earnings:
  ☐ True ☐ False

- A player bidding exactly his valuation for a non-risky good will earn €10 at most.
  ☐ True ☐ False

- A player who bids more than his valuation for a non-risky good and wins the auction will earn less than he would have earned in case of not bidding at all.
  ☐ True ☐ False

- Altogether, you can't make losses in this part.
  ☐ True ☐ False

- If I submit a bid below my own valuation, I will earn €10 in case of not winning and the difference between my valuation and my bid in case of winning.
  ☐ True ☐ False

- The lower my bid, the lower my chance of winning the auction.
  ☐ True ☐ False

- The higher my bid, the higher my earnings in case of winning.
  ☐ True ☐ False

---

2) **Exercises**

Players A, B and C have been grouped together. For a **non-risky good** they receive the following **valuations**: **A:** €5.50; **B:** €2.70; **C:** €5.60

Knowing their valuations the players submit the following **bids**:

A: €3.00; **B:** €2.00; **C:** €1.00

- Which player purchases the good?
  Your answer: _______________

- What are the earnings of player A?
  Your answer: _______________

- What are the earnings of player B?
  Your answer: _______________

- What are the earnings of player C?
  Your answer: _______________

Part 1[36]

In this part you have to go through a number of lists. You can always choose between two alternatives in these lists: with **option X** you receive a **lottery,** and with **option Y** you receive a **sure payment**. On a given list, option X always represents the same lottery. Sure payments of option Y vary from decision to decision. Such a choice list could look as follows:

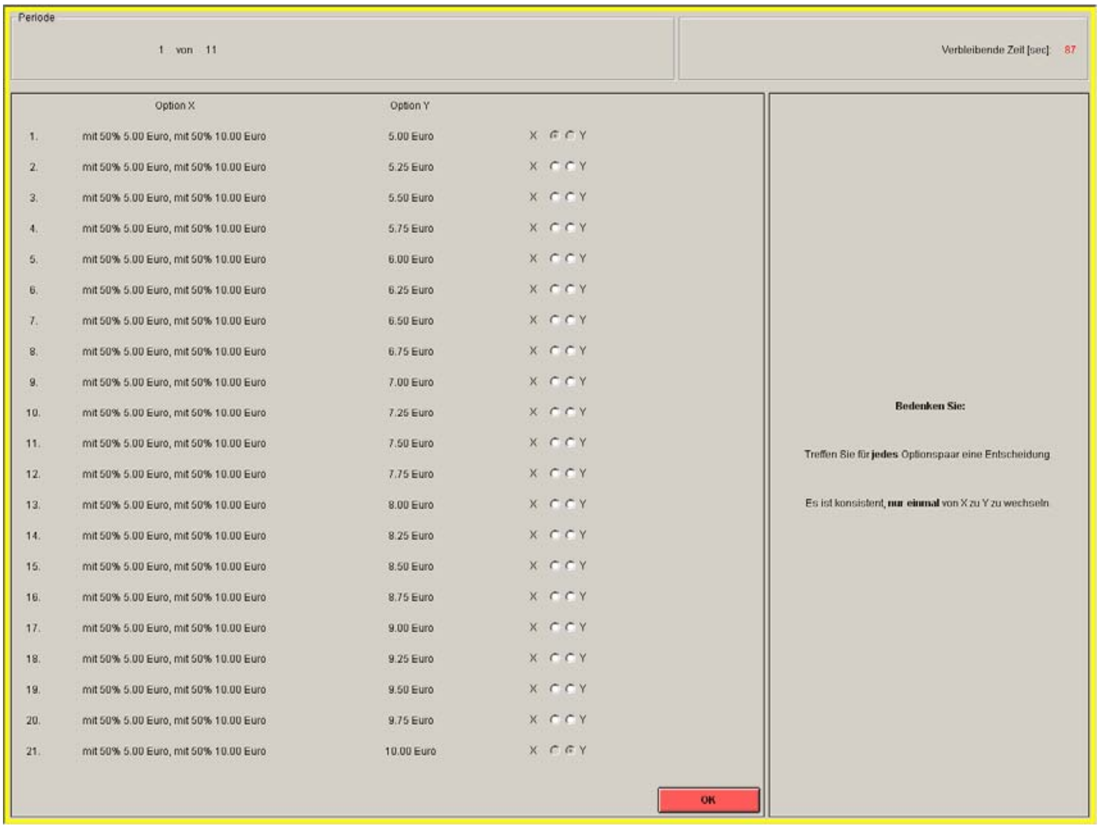

Figure 3-6: Screenshot – Choice list

In the example above option X always represents a lottery which results in earnings of either €5 with 50% chance or €10 with 50% chance. Option Y starts with a sure payment of €5 and ends with a sure payment of €10.

For each row you have to choose between option X and option Y. The first decision in a list is always preselected: Instead of getting a sure payment of €5 with certainty it is always better to receive a lottery with an outcome of either €5 or €10. Thus, **Option X is always preselected for the first decision**. The last decision in a list is also preselected. Instead of getting a lottery with an outcome of either €5 or €10 it is always better to receive a sure payment of €10. Thus, **Option Y is always preselected for the last decision**.

Between these two extremes you have to make choices for 19 option pairs. Since sure payments of option Y are continuously increasing downwards the list, it is **consistent to switch** from option X to option Y **only once**.

Altogether, you will have to fill in **11 choice lists**. The lotteries of option X and the range of the sure amounts will differ between lists. At the end of the experiment, one choice list is randomly selected by the computer. From this list the computer **randomly** selects **one decision to determine your payoffs** in this part. If you chose option X, you will gamble and receive the outcome of the chosen lottery. More precisely, the experimenter comes to your desk at the end of the experiment and you **roll a six-sided die**. For the example above you would receive €5 if you roll the numbers 1, 2 or 3 or €10 if you

[36] For the control experiment, i.e, the choice list.

roll the numbers 4, 5 or 6. If for this option pair you chose option Y, you receive the sure amount of option Y.

Since you don't know which choice will be payoff relevant, it is in your own interest to **consider all your decisions carefully**.

Example 1: Let's assume the computer randomly selects the choice list shown above. From this list choice 2 is randomly selected to determine payoffs. Let's further assume in this decision task you picked option X. In this case you have to gamble. More precisely, you have to roll the dice. With numbers 1, 2 or 3 you receive €5, and with numbers 4, 5 and 6 you receive €10.

Example 2: Let's again assume the computer randomly selects the choice list above. From this list choice 20 is randomly selected to determine payoffs. Let's assume for this decision task you picked option Y. In this case you receive the sure amount of option Y in decision 20, namely €9.75.

At the top right corner you will find a timer which gives you some temporal orientation for your decision. You can exceed this time limit (especially for the initial decisions, this might most likely be the case).

A2. Non-linear specifications

Dep. Var.: bid	I (BDM)		II (CL)		III (CL, LA dummy)	
valuation	1.059**		1.076**		1.089**	
	(0.123)	F(2,520) =635.23**	(0.110)	F(2,520) =904.35**	(0.108)	F(2,518) =939.96**
(valuation)²	-0.016		-0.016*		-0.016*	
	(0.009)		(0.008)		(0.007)	
risk	1.51		0.087		0.30	
	(0.792)		(0.743)		(0.732)	
risk×valuation	-0.181**	F(2,520) =10.44**	-0.182	F(2,520) =26.49**	-0.202	F(2,518)= 5.17**
	(0.045)		(0.222)		(0.220)	
risk×(valuation)²	0.039		-0.007)		0.003	
	(0.020)		(0.016)		(0.016)	
risk×valuation×LAd	-		-		0.079	F(2,518)= 10.13**
					(0.076)	
risk×(valuation)²×LAd	-		-		-0.022*	
					(0.010)	
constant	-0.558		-0.885		-0.938*	
	(0.396)		(0.377)		(0.371)	
# observations (bids)	600		600		600	
(# bidders)	(75)		(75)		(75)	
R^2	0.68		0.74		0.75	

Fixed Effect Panel Regressions: standard errors in parenthesis; ×: interaction; ** represents significance at p=0.01 and * at p=0.05. BDM: Becker-DeGroot-Marschak mechanism; CL: choice list mechanism; LAd: loss aversion dummy.

Table 3-5: Non-linear specifications

Chapter 4

Outcome Risk and Prevention Framing in Social Dilemmas

4.1 Introduction

Social dilemmas are characterized by individual incentives to free-ride, whereas from a social perspective it would be optimal to cooperate. However, the zero-contribution thesis deducted from a purely selfish Nash equilibrium contradicts observations from everyday life where cooperative behavior can be observed frequently (Ostrom, 2000). One of the key challenges of economics is to provide well-funded knowledge on how this cooperative behavior can be fostered and which characteristics of public goods dilemmas help to achieve cooperation. Experimental economic research has contributed greatly to better understand human behavior in social dilemmas identifying numerous institutions to foster or hamper cooperative behavior (Fehr and Gächter, 2000; Masclet *et al.*, 2003; Falk *et al.*, 2005; Nikiforakis, 2008; Rege and Telle, 2004; Güth *et al.*, 2007; Potters *et al.*, 2005; Sutter *et al.*, 2010).[37]

In this experimental study we systematically vary a public goods game in two dimensions and examine how this variations influence individuals' willingness to cooperate.

In the first dimension, we explore whether cooperation is more likely if the goal of cooperation is to create a common value or if it is to prevent the loss of a common value. Henceforth, we will call social dilemmas of the former type *creation frame social dilemmas* and those of the latter type *prevention frame social dilemmas*.

In the second dimension, we analyze whether contributions are higher when deterministically increasing the value of the public good as modeled in the standard voluntary contribution mechanism (VCM) or when increasing the probability of a successful provision. Henceforth, we will denote social dilemmas of the former type as *no outcome risk social dilemmas* and those of the latter type as *outcome risk social dilemmas*.

Distinguishing within these two dimensions allows capturing crucial characteristics of various real world social dilemmas. The *provision problem of public TV and*

[37] For overviews see Ledyard (1995), Zelmer (2003), or Chaudhuri (2011).

radio, for instance, can most likely be classified as a creation frame social dilemma exhibiting no outcome risk. Providing these public good creates a value and the provision has an infinite number of possible provision levels, with each invested monetary unit deterministically increasing the quality of provision. The *provision of public safety*, in contrast, can better be described as a prevention frame social dilemma exhibiting no outcome risk. As in the public TV and radio example an infinite number of provision levels exists, but the goal of provision has rather a preventive than a creative character. The *funding of research and development projects* can be characterized as a creation frame social dilemma exhibiting outcome risk. Here, the provision goal is to create a value but usually only two provision levels exist: the scientific breakthrough is either achieved, or it is not. A contribution to such a project does not deterministically increase the provision quality but rather increases the probability of success. Another social dilemma exhibiting these characteristics would be the *funding of lobbying efforts*. Like the former two examples the *construction of a dyke* exhibits outcome risk as its probability not to burst with the next flood increases with the efforts spent in its construction but has undeniably a preventive character.

How variations in these two dimensions affect cooperativeness has not been analyzed in depth.[38] Furthermore, these two dimensions have to our best knowledge never been explored systematically together. Running such an analysis with field data appears hardly feasible due to the impossibility of finding examples of respective dilemma types with a comparable marginal impact of cooperation. By contrast, a lab experiment allows us to keep the expected value of a marginal contribution constant in all four treatments resulting from the above mentioned variations. In four different treatments we let subjects contribute over ten rounds in a stranger matching design and, additionally, elicit subjects' expectations about others' contributions in each round. In a one-shot interaction we also elicit subjects' conditional contribution

[38] Keser and Montmarquette (2008) investigate cooperation in a prevention frame social dilemma with outcome risk varying initial wealth, initial loss-probabilities, and the degree of information but do neither provide a comparison to the no-risk domain nor to the creation frame domain. Brown and Stewart (1999) vary initial endowments in public bad games to examine how implicit cooperation-thresholds necessary to avoid perceived losses affect cooperativeness. Iturbe-Ormaetxe *et al.* (2011) investigate the impact of prevention- and creation-framing on cooperativeness in a step-level public goods game. An in-depth literature overview will be provided in section 4.3.

functions, i.e. the amount an individual is willing to contribute conditionally on what other individuals contribute. We find both prevention framing and outcome risk to increase contributions as well as expectations about the cooperativeness of others. Consequently, we find highest contributions when combining both outcome risk and prevention framing. Here, a constant contribution level can be established while cooperation collapses over time in all other treatments.

Finding treatment differences when framing a game either as creation- or as prevention-task cannot be explained by expected utility theory (EUT) as same actions result in same outcomes, and thus EUT would predict no treatment differences. Even when assuming participants to adopt reference points in the way intended by the framing, i.e. to adopt initial endowments as reference points and view further changes as either gains or losses, the hypotheses are not clear-cut. Assuming loss aversion, cooperation is more desirable in the prevention frame as preventing a loss is more preferable than enabling a gain of the same size. However, from a selfish perspective contributing is still a dominated strategy as it reduces the own payoff. Given loss aversion, contributing is even more harmful in the prevention frame as it increases the lost amount a contributor suffers with certainty. We find the higher desirability of cooperation in the prevention frame to outweigh the higher individual incentives to free ride. This result is mostly driven by the fact that participants are conditional cooperators on average. We find subjects to expect others to be more cooperative under a prevention frame and consequently reciprocate with higher contributions, establishing higher cooperation in the prevention frame treatments.

Finding higher contributions under outcome risk is more puzzling. Risk aversion in the sense of a concave shaped utility function cannot explain our findings but would rather predict lower contributions in the outcome risk social dilemma. One feasible explanation for our findings would be that probability weighting makes subjects to overweight their marginal impact which mitigates the perceived social dilemma property of the game. This explanation approach is supported by the finding of a significant lower share of free-riders in treatments exhibiting outcome risk. Another explanation would base on the assumption that individuals tend to think themselves as pivotal – a behavioral pattern which has been reported in the psychological literature on voting behavior.

Our findings contribute to a better understanding of why some real world social dilemmas are more easily overcome than others. They help to determine which global social dilemmas need supranational institutions to ensure a successful internalization and which dilemmas may be overcome by non-institutionalized international cooperation. Furthermore, our results suggest interest groups, concerned with the provision of a specific public good containing a preventive character and/or outcome risk, to emphasize these characteristics as much as possible.

The remainder of this chapter is laid out as follows. In the next section we present our experimental design and our hypotheses. In section 4.3 we link our research to the related literature. Section 4.4 presents our experimental results. In section 4.5 we discuss alternative explanations for our findings and finally conclude in section 4.6.

4.2 Experimental Design

4.2.1 Treatments

In order to systematically study the effect of outcome risk and prevention framing on cooperativeness in social dilemmas, we vary two factors: no outcome risk vs. outcome risk and creation framing vs. prevention framing, resulting in a 2x2-design depicted in Table 4-1.

	No Outcome Risk	*Outcome Risk*
Creation Framing	Creation frame no Risk (CnR)	Creation frame Risk (CR)
Prevention Framing	Prevention frame no Risk (PnR)	Prevention frame Risk (PR)

Table 4-1: Treatments

The mechanism used in all treatments is a voluntary contribution mechanism as introduced by Isaac *et al.* (1985). Let I={1, 2, …, n} denote a group of n subjects interacting in a public goods game. Individual $i \in I$ receives an endowment of w with $w > 0$ which can be allocated either to a private account and/or to a public account. The voluntary contribution of individual *i* to the public account is denoted by c_i and must satisfy $0 \le c_i \le w$. Let C denote the sum of all group members' contributions to the public account (i.e. $C = \sum_{j=1}^{n} c_j$). Subject *i's* return for each group contribution is denoted by γ with $0 < \gamma < 1 < n \cdot \gamma$. Depending on the treatment, the mechanism is either modified keeping expected payoffs equivalent (in the risk dimension) or just presented differently (in the framing dimension).

In the ***C**reation frame **n**o **R**isk treatment (CnR),* the mechanism is presented in the way which is usually found in the literature starting with Isaac *et al.* (1985): By contributing to the public account, subjects deterministically increase the amount each group member receives additionally to her private account. Individual *i's* expected payoff is thus given by

$$E(\Pi_i) = \Pi_i = w - c_i + \gamma \cdot C \quad [1].$$

In the ***P**revention frame **n**o **R**isk treatment (PnR),* the mechanism is mathematically identical but presented differently to subjects: They receive an endowment of $w + f$ with $f = \gamma \cdot n \cdot w$. However, each player is about to lose f . By contributing to the public account, this loss is deterministically reduced for each group member by the factor γ . The payoff function as presented to the subjects can be written as

$$E(\Pi_i) = \Pi_i = w + f - c_i - (f - \gamma \cdot C) \quad [2].$$ [39]

In the ***C**reation frame **R**isk treatment (CR),* contributions to the public account do not result in payments for all group members in a deterministic way. Instead, each contribution increases the group's probability of gaining a fixed prize f by $\frac{\gamma}{f}$ percent. The payoff function as presented to the subjects can be written as

$$E(\Pi_i) = w - c_i + (\frac{\gamma}{f} \cdot C) \cdot f \quad [3].$$

The mechanism in the ***P**revention frame **R**isk treatment (PR)* is mathematically identical to the one described for the CR-treatment but presented differently: Subjects receive an endowment of $w + f$ but face the threat of suffering a loss of f .

[39] It is easy to see that [1] and [2] are equivalent.

Contributing to the public good reduces the groups' probability of suffering this loss. The payoff function as presented to the subjects can be written as

$$E(\Pi_i) = w + f - c_i - (1 - \frac{\gamma}{f} \cdot C) \cdot f \quad [4]^{40}.$$

The parameters in our experimental session were set up as follows: group size *n*=4, endowment $w = €5$ and parameter $\gamma = 0.5$. All treatments were presented to participants in a neutral and context-free fashion. We avoided terms like "cooperation", "social dilemma", or "public good". We do deliberately use the terms "gain" in our creation frame treatments and "loss" in our prevention frame treatments in order to induce the framing.

4.2.2 Equilibria and Predictions

As mechanisms in the CnR- and PnR-treatments are purely deterministic, subject *i's* actual payoff Π_i in these treatments is equivalent to the expected payoff $E(\Pi_i)$. The actual marginal per capita return, further denoted as MPCR, i.e. a player's return resulting from a one unit contribution, is equivalent to the expected MPCR denoted by E(MPCR). More precisely:

$$E(MPCR)_{CnR,PnR} = MPCR_{CnR,PnR} = -E(\partial \Pi_i / \partial C) / (\partial \Pi_i / \partial c_i) = \gamma . \quad [5]$$

Given that γ satisfies $0 < \gamma < 1 < n \cdot \gamma$, each unit invested into the private account provides a subject with a higher payoff than an investment into the public account, thus, making free-riding in both the CnR- and the PnR-treatment the unique dominant strategy, while the social optimum would be to fully contribute.

In contrast, mechanisms in the CR- and PR-treatments are stochastic. Hence, the MPCR in these treatments is a mean preserving spread of the MPCR present in the CnR- and PnR-treatments. More precisely:

$$E(MPCR)_{CR,PR} = -E(\partial \Pi_i / \partial C) / (\partial \Pi_i / \partial c_i) = E(\gamma) . \quad [6]$$

[40] It is easy to see that [3] and [4] are equivalent.

Assuming risk neutrality for all players, free-riding is a unique dominant strategy as it maximizes a subject's expected payoff while full contribution would be the social optimum. Given the assumption of individuals being risk averse in the sense of exhibiting a concave utility function, free-riding is not necessarily the unique Nash-equilibrium. A contribution´s marginal impact on a risk averse individual's utility rather depends on the contributions made by other group members, with others' contributions increasing the marginal utility of one's own contribution.

Figures 4-1 and 4-2 illustrate the motivation behind this insight.

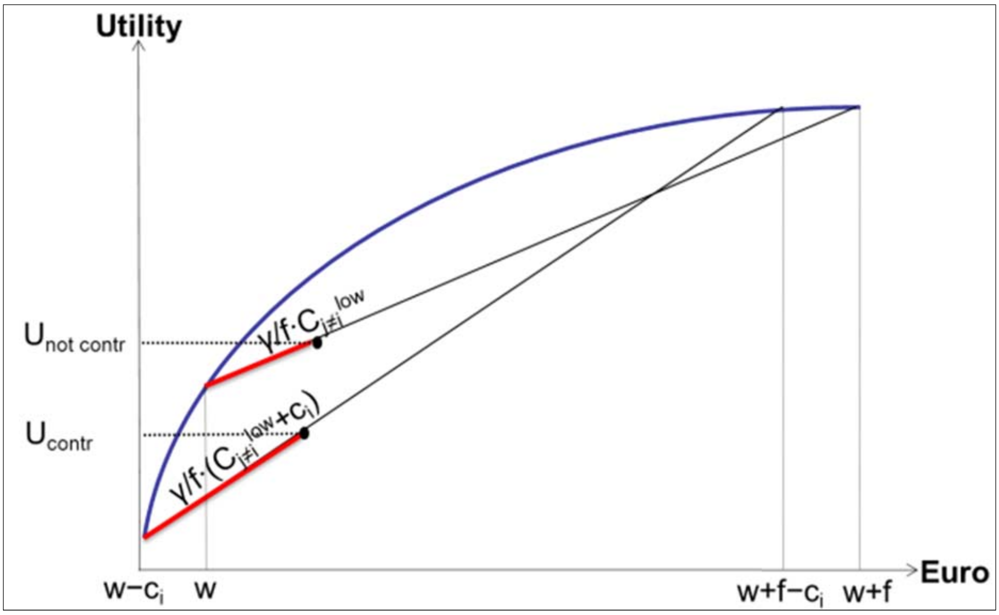

Figure 4-1: Utility of a contribution if C is low

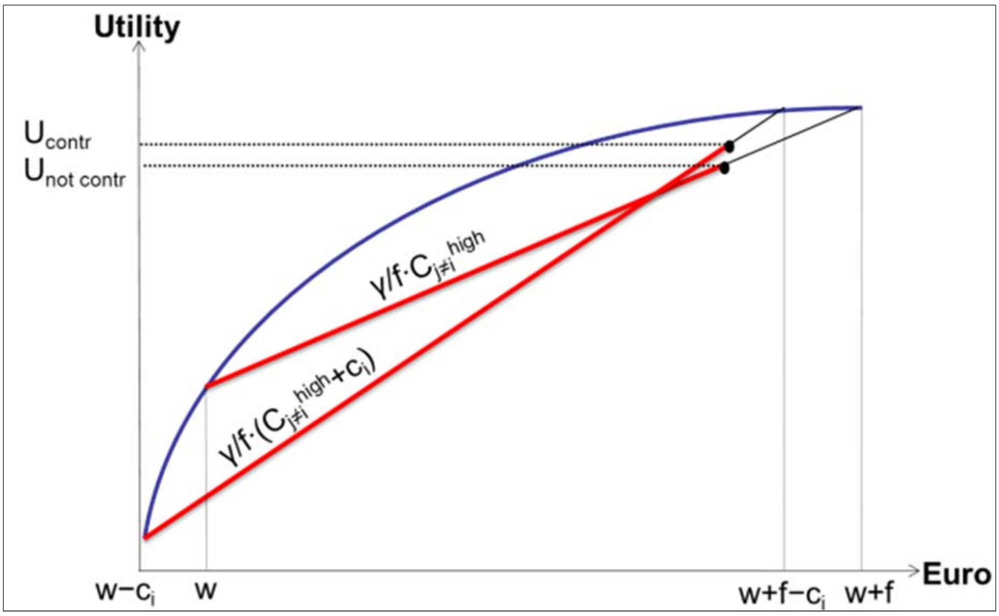

Figure 4-2: Utility of a contribution if C is high

Making no contributions to the public account in the CR- and PR-treatments results in an outcome of either w or $w+f$, with probabilities of either outcome depending on the contributions of others. By contributing c_i, the player receives a prospect with an outcome of either $w-c_i$ or $w+f-c_i$. Compared to the no-contribution-case the probability of the higher outcome increased by $\gamma/f \cdot c_i$. If others' contributions are low as in Figure 4-1, a contribution results in a lower utility for the contributing player. In contrast, if others' contributions are very high as in Figure 4-2 and the player's concave utility function is sufficiently curved, a contribution results in a higher utility, even for a selfish decision maker.

Hence, given a sufficiently concave utility function, it is feasible to receive Nash-equilibria of non-zero contributions for selfish but risk averse individuals. However, assuming individuals to exhibit a Constant Relative Risk Aversion (CRRA) utility function of the form $U(x) = x^{1-r}/(1-r)$ with x denoting a subject's payoff and r her degree of risk aversion, our simulation results in appendix A3 show that with the values used in our setup the slope of the contribution function is not sufficiently bent to turn the marginal utility of a contribution positive, even when assuming an r substantially higher than elicited for our participants. In our setup, zero-contribution remains a unique Nash-equilibrium for selfish decision makers independently of their degree of risk aversion. This leads us to our first hypothesis.

HYPOTHESIS 1: *Assuming purely selfish decision makers, zero contributions will be observed in all four treatments no matter whether decision makers are either risk neural or risk averse.*[41]

Experimental evidence on social dilemmas, however, reports decision makers not to act purely selfish but finds average contributions to lie significantly above the zero-contribution Nash-equilibrium (see Ledyard (1995) for an overview). Positive contributions are driven by social preferences such as altruism, warm-glow (Andreoni, 1989), reciprocal preferences (Sugden, 1984; Falk and Fischbacher, 2006), interaction of heterogeneous players (Ambrus and Pathak, 2011) or inequality

[41] We did not derive hypotheses for risk loving individuals as risk aversion is a commonly found behavioral pattern in laboratory experiments. The results of our risk test (see section 4.2.4) show that risk loving preferences do indeed not matter for our sample.

aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002).

When formulating further hypothesis, we assume the distribution of social preferences to be constant over treatments. We further assume subjects to form no reference points but to decide according to expected utility theory.[42] When assuming subjects not to adapt reference points but to evaluate absolute outcomes only, the presentation of a decision problem either as creation- or as prevention-task should not affect decisions as same actions result in same final payoffs regardless of social preferences. This leads us to our second hypothesis.

HYPOTHESIS 2: *Assuming other regarding decision makers and no reference dependency, contributions do not differ between prevention- and creation frame both under outcome risk and no outcome risk.*

Varying the game in the risk dimension means to determine whether contributions either deterministically determine the provision level or just the probability of provision. However, treatments are designed in a way that same actions always result in same expected payoffs. Thus, assuming subjects to be risk neutral, contributions should not differ between outcome risk treatments and no outcome risk treatments.

HYPOTHESIS 3: *Assuming other regarding and risk neutral decision makers, contributions do not differ between outcome risk and no outcome risk both under prevention- and creation-framing.*

As shown above, for a risk averse decision maker the marginal utility of a contribution increases in the outcome risk treatments with the sum of contributions collected in the public account but remains negative even when assuming other group members to fully contribute. Thus, for selfish individuals it does not matter in our setup whether marginal utility of a contribution differs between outcome risk and no outcome risk treatments, as it is negative in both cases and thus free-riding is always the unique dominant strategy. In contrast, if we assume subjects to be other regarding and thus, contributions to result in an additional utility gain, e.g. warm glow, altruism etc., differences in marginal utility stemming from the evaluation of own payoffs according to the convex utility function can become crucial as the

[42] How predictions change when assuming reference dependence will be discussed in section 4.5.

marginal utility gain resulting from other regarding preferences adds to the former one and thus, might turn a contribution into a utility-maximizing strategy. In appendix A4 we compute the marginal utility of a contribution (without the additional gain resulting from social preferences) for both outcome risk and no outcome risk treatments for a slightly risk averse as well as for an extremely risk averse individual (with respect to the convexity of the utility function). We can show both for slightly risk averse and for extremely risk averse decision makers that marginal utility of contributions are lower in the risk treatments than in the no risk treatments for all but one case: Only when assuming all other group members to fully contribute, marginal utility of a contribution is higher in the outcome risk treatments. This leads us to our last hypothesis.

HYPOTHESIS 4: *Assuming other regarding and risk averse decision makers, contributions in the outcome risk treatments will not exceed contributions in the no outcome risk treatments, except for the case that all group members fully contribute.*

4.2.3 Laboratory Protocol

Computerized experiments were conducted in the experimental laboratory MELESSA at the University of Munich, using the experimental software z-Tree (Fischbacher, 2007) and the organizational software ORSEE (Greiner, 2004). Subjects received written instructions which were read aloud. After reading the instructions subjects had to solve a couple of control questions to ensure that everybody understood the task. Control questions were designed ensuring that results were equivalent in all four treatments.[43] Once every subject had solved her control questions successfully, the experiment started.

192 undergraduate students without experience in public goods experiments participated in eight sessions with 24 subjects each. 66 % subjects were female, and average age was 24.52 years. In all four treatments sessions lasted about 1.5 hours with average earnings of €21.85.

[43] A complete presentation of control questions can be found in the appendix A2.

4.2.4 Procedure

An experimental session consisted of three parts: In part 1, participants had to state one shot unconditional as well as conditional contribution decisions filling out a contribution table equivalent to the one used by Fischbacher *et al.* (2001). In part 2, unconditional contributions as well as individual expectations about others' contributions were elicited in ten consecutive rounds with feedback in between. In part 3, subjects' risk preferences were elicited using a choice list mechanism equivalent to the one introduced by Holt and Laury (2002).

Part 1: Subjects were arranged in groups of four and were faced with one of the social dilemmas presented in section 4.2.1, depending on the treatment. They were asked to decide about two types of contributions to a public account: an *unconditional contribution* and a *contribution table*. While in the former type only one single contribution decision was requested, in the latter a participant had to indicate for each average contribution level of her group members (rounded to integers) how much she was willing to contribute to the public account. While for one randomly assigned group member the actual contributions were determined by her conditional contribution decisions, all other group members contributed according to their unconditional contribution decisions. Part 1 consisted of only one round. The result of this random draw was revealed only at the end of the experiment. Subjects knew that at the end of the experiment they would further learn about average contributions of their group members and their final payoff in this part. Subsequently to this part (before starting part 2), no information was revealed.

Part 2: Subjects were faced with the same social dilemma situation as in part 1 and had to make two types of decision in ten consecutive rounds: a *contribution decision* where they had to state their unconditional contribution, c_i, and an *estimation decision*. In the latter, subjects had to estimate average contributions of their group members providing a vector of probabilistic forecasts. Thus, for each possible average contribution level k ($0 \leq k \leq 5$) of their group members (rounded to integers) subjects had to state an estimated probability p_k (with $\sum_{k=0}^{5} p_k = 1$). Subjects were rewarded for the correctness of their stated estimations, applying a quadratic scoring rule as suggested by Murphy and Winkler (1970).[44] Player *i's* reward (in euro) in round τ was given by

$$E_i^\tau(p) = 5 - 2.5 \cdot \sum_{k=0}^{5} (I_k^\tau - p_k^\tau)^2 \text{ , [5]}$$

where I_k^τ was an indicator function that took the value 1 if the realized event was k and 0 otherwise. The procedure ensured earnings between €0 and €5 depending on the correctness of player *i's* estimation. After each round, subjects were informed about average contributions of their group members, their earnings from the contribution task (Π_i^t), as well as their earnings from the estimation task (E_i^τ). At the end of the experiment, only one Π_i^t and one E_i^τ were randomly selected and paid out for real. t and τ were separately selected by two independent die throws in order to disallow for hedging opportunities. By using a stranger-matching approach to allocate subjects into groups, we elicited pure preferences without strategic considerations. Each round three groups of four were randomly composed from a pool of twelve. Thus, for each session we received two statistically independent observations and, hence, four independent observations per treatment.

Part 3: Subjects faced a set of ten choices between two binary lotteries, one with a higher and one with a lower payoff-variance. Probabilities varied systematically over all ten choices in such a way that a subject should switch from the lower variance lottery to the higher variance lottery only once and a more risk averse subject should switch later than a more risk loving one. The payoffs replicate the low payoff condition of Holt and Laury (2002) and are presented in appendix A5.

The experiment ended after a short socioeconomic questionnaire.

[44] See also Palfrey and Wang (2009), Croson (2007), and Gächter and Renner (2010).

4.3 Related Literature

4.3.1 Prevention vs. Creation Framing

In order to compare cooperativeness when the goal of cooperation is either to create a value or to prevent the loss of a value, we endow participants either with a high amount of money and present them with a loss scenario or with a low amount presenting them with a win scenario. Parameters are chosen in a way ensuring equal decisions in both treatments to result in equal outcomes. Thus, treatments are payoff-equivalent but *framed* differently.

"A framing effect is said to be present when different ways of describing the same choice problem change the choices people make, even though the underlying information and choice options remain essentially the same" (Cookson, 2000, p. 55).

The impact of framing on economic decision making has been intensely studied. Kühberger (1998) and Levin *et al.* (1998) provide overviews on the relevant framing literature. Research on social dilemmas identified several determinants where framing matters, e.g. ingroup-outgroup framing, restart effects, decomposed game-presentation, provision of rank information, or context framing (Andreoni, 1988; Cookson, 2000; Brandts and Schwieren, 2009; Dufwenberg *et al.*, 2011).

Building on psychological studies by Brewer and Kramer (1986), Komorita (1987), and McCusker and Carnevale (1995) among others, Andreoni (1995) analyzed the effect of *give- vs. take-framing* in social dilemmas. He discovers whether cooperativeness differs between public goods problems and public bad problems (commons dilemmas). In the former type of social dilemma games, cooperative behavior is determined by the *act of giving,* since giving causes positive external effects on others (give-frame). In the latter type, cooperative behavior is determined by the *act of not taking,* since taking causes negative external effects on others (take-frame). Andreoni (1995) finds contributions to be significantly higher in the give-frame. He concludes that *"people are significantly more willing to cooperate in a public goods experiment when the problem is posed as a positive externality rather than a negative externality"* (Andreoni, 1995, p. 13). His explanation: the perceived warm-glow of the act of giving must be stronger than the cold-prickle of taking. Subsequent studies find largely similar results (Sonnemans *et al.*, 1998; Willinger and Ziegelmeyer, 1999; Cookson, 2000; Park, 2000; Cubitt *et al.*, 2011).

Brown and Stewart (1999) examine in an environmental dilemma, i.e. a public bad game, whether the fact that collective egoistic behavior results in a loss outcome motivates group members to deviate from the selfish Nash-strategy and to behave more cooperatively instead. In their experiment, a decision maker has to divide a certain number of tokens between an individual account resulting in private costs only and a public account resulting in lower personal costs than in the individual account but generating negative external effects by reducing the initial endowment of other group members by the same amount. By varying initial endowments between treatments, a certain degree of cooperation becomes crucial for the treatment group in order to avoid negative outcomes for the group members. The authors examine whether loss avoidance motivates these group members to increase their internalization rates although they risk incurring even greater losses if others do not follow. They find subjects of the treatment group facing the threat of a loss to be more cooperative on average than subjects in a base treatment where no loss is possible. However, they find the same subjects to be more cooperative even when facing identical decision problems as the control group, raising questions concerning the comparability of treatments.

In a step level public goods game, further denoted as *provision point mechanism* (PPM), Iturbe-Ormaetxe *et al.* (2011) investigate both theoretically and empirically whether cooperation is higher if the task is framed as an opportunity to provide a public good or as an opportunity to prevent the deterioration of a public good. They find higher contributions in the prevention frame if the threshold necessary to provide the public good is high, but higher contributions in the creation frame if the threshold is low.

In contrast to Iturbe-Ormaetxe *et al.* (2011), we investigate cooperativeness not in a PPM but in a VCM. Both games differ crucially with respect to their strategic characteristics, as the former has multiple and even pareto efficient equilibria and is thus a coordination game while the latter has only one equilibrium which deviates from the social optimum, thus describing a social dilemma. Like Brown and Stewart (1999), we do not vary our treatments in the give- vs. take-framing dimension examined by Andreoni (1995). But while Brown and Stewart examine cooperation in a public bad game (taking task) where the goal of cooperation does not vary across treatments but an implicit minimum contribution threshold necessary to avoid losses is introduced by reducing initial endowments, we explore cooperation in a public goods game (giving task) and vary cooperation as either exhibiting a creative- or a preventive character.

4.3.2 Outcome Risk vs. No Outcome Risk

While in our no outcome risk treatments the provision of the public good is deterministic, it is uncertain under outcome risk. Messick *et al.* (1988) refer to uncertainties arising from uncertain external factors which might influence the quality of a provided public good or its provision itself as *environmental uncertainty* and oppose it to *strategic uncertainty* resulting from the fact that contributions of other group members are unknown.

Wit and Wilke (1998) introduce environmental risk in a PPM by drawing the required provision threshold from a uniform distribution. They find lower contributions under high environmental uncertainty. Au (2004) and Gustafsson *et al.* (2000) find similar results. Van Dijk *et al.* (1999) compare contributions in an ordinary PPM with those of a step-level game where the prize is not fixed but can take any value of a certain interval with uniformly distributed probabilities. Keeping the prize's expected value identical in both treatments, they do not find a significant effect and therefore conclude that uncertainty about the value of a public good has no influence of contribution behavior in PPMs. McCarter *et al.* (2010) reproduce this experiment but let the lower bound of the interval fall below the provision threshold. They find significantly lower cooperativeness under outcome risk. They argue that the possibility of a perceived loss discourages participants to contribute.

Dickinson (1998) introduces environmental risk into the VCM. He compares the findings of a deterministic VCM where contributions to a public account deterministically increase payoffs of all group members with two treatments exhibiting environmental risk. In the so called *uncertainty treatment* the return each group member receives from the provision of the public good linearly increases with each contribution but the provision takes place only with a 75% chance. In another treatment referred to as *incentive treatment* contributions not only linearly increase the return of a public goods provision but the provision-probability as well. Dickinson finds the introduction of environmental risk to weakly reduce contributions. However, increasing the return and the probability of a public goods provision at the same time, as done in the incentive treatment, results in a non-constant marginal per capita return making a proper comparison with the base treatment impossible.

Building on the study of Dickinson (1998), Gangadharan and Nemes (2009) systematically varied the provision probability of either the pubic or the private good and additionally introduced a treatment where the provision probability is unknown to subjects. The authors refer to treatments with known probability as *risk treatments* and to those with unknown probability as *uncertainty treatments*. They find subjects to significantly decrease public goods contributions when the provision of the public good is either risky or uncertain but to increase contributions to the public good when either risk or uncertainty is involved in the provision of the private good.

Keser and Montmarquette (2008) explore cooperativeness when voluntary contributions reduce the probability of a fixed public loss. By varying initial loss-probabilities, initial wealth, and the degree of information, they find an increase in loss probability as well as an introduction of ambiguity to reduce the level of voluntary contributions. By revealing the outcome of the loss-lottery after each of their 100 rounds, they show that the recent occurrence of a loss decreases aggregate contribution levels in the subsequent periods. However, the authors exclusively investigate cooperative behavior in the prevention frame risk domain but do neither provide a comparison in the gain- nor in the risk-dimension.[45]

By varying a VCM both in the risk and in the framing dimension keeping marginal incentives of a contribution equal we are to our best knowledge the first to provide an experimental setting that allows us to systematically identify the impact of the above mentioned variations.

[45] Altogether, they find efficiency levels between 22% and 32% and argue that this is lower than efficiency levels between 40% and 60% usually found in the standard VCM (creation frame no risk). Due to the specific design (100 repetitions in a partner matching), such a comparison has, however, limited validity.

4.4 Results

We start with presenting our main results found in part 2 of our experiment. Subsequently, our findings resulting from the conditional cooperation task carried out in part 1 are exposed.

4.4.1 Unconditional Cooperation (Part 2)

Figure 4-3 shows our main finding in Part 2.

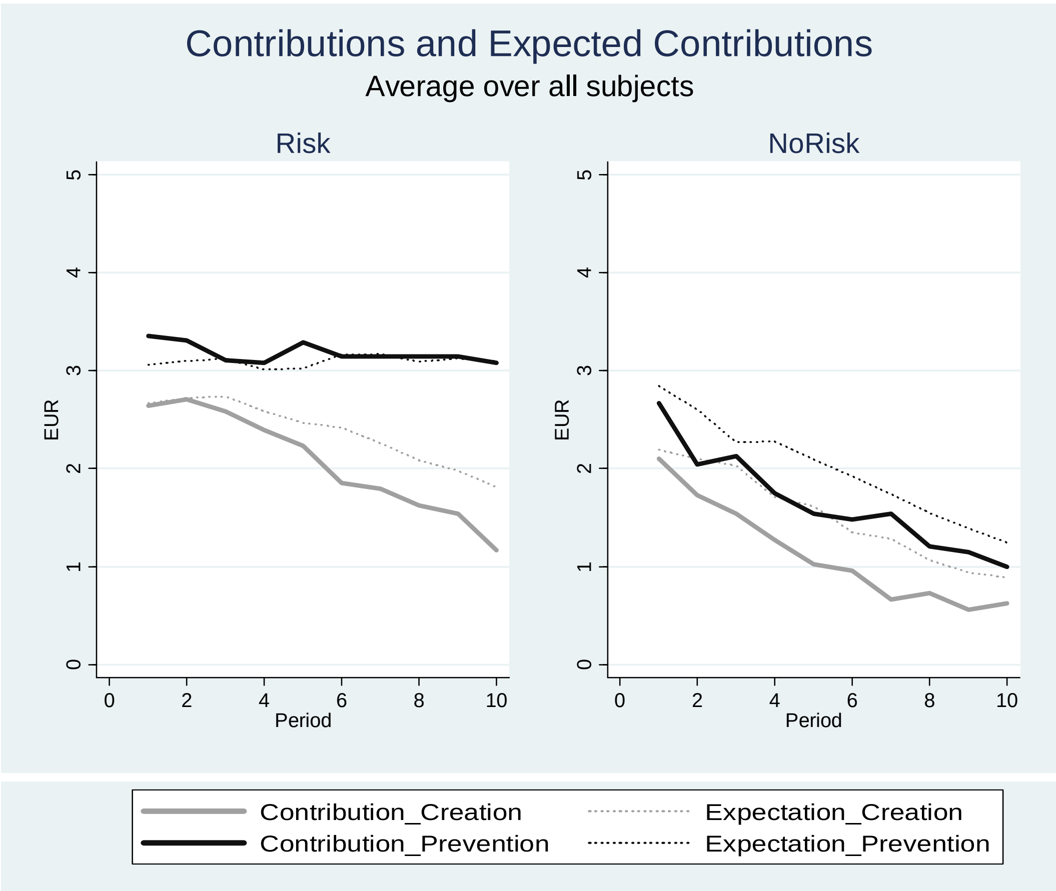

Figure 4-3: Contributions and expected contributions (creation vs. prevention)

Both under outcome risk and under no outcome risk average contributions (depicted by solid lines) are higher in the prevention frame than in the creation frame in every round. Moreover, both under creation- and prevention-framing average contributions are higher in treatments with outcome risk than in treatments without outcome risk in every round.[46] Figures 4-11 and 4-12 in appendix A6 show average contributions

[46] See also Figure 4-10 in appendix A6.

separately for each of our 16 independent observation. Expectations about contributions made by group members (depicted by dashed lines) are found to be higher under prevention framing as well as under outcome risk already in the first round. In order to validate the significance of these observations, we run several non-parametric tests. P-values reported below show test-statistics of two sided Mann-Whitney tests.

PR vs. CR: When comparing average amounts contributed in all ten rounds (further denoted as *average contributions*) for each independent observation (matching pool of 12 subjects), contributions are significantly higher in the PR-treatment compared to the CR-treatment (p=0.043, n=4[47]). This holds also true when comparing contributions collected in the first round (p=0.021, n=48). Likewise, expectations about average contributions of group members[48], which subjects revealed in the first round, are significantly higher in the PR- than in the CR-treatment (p=0.022, n=48).

PnR vs. CnR: Neither average contributions (p=0.149, n=4) nor first round contributions (p=0.119, n=48) are significantly higher in the PnR- than in the CnR-treatment. However, the difference in first round expectations about others´ contributions is found to be highly significant (p=0.004, n=48).

PR vs. PnR: Average contributions are significantly higher in the PR- than in the PnR-treatment (p=0.021, n=4) as are first round contributions (p=0.070, n=48). In contrast, the difference in first round expectations is not found to be significant (p=0.194, n=48).

CR vs. CnR: The contributions observed in the CR-treatment are higher than in the CnR-treatment, a difference which is weakly significant both for average contributions (p=0.100, n=4) and for first round contributions only (p=0.083, n=48). In the same line, first round expectations are significantly higher in the CR-treatment (p=0.025, n=48).

Figure 4-4 summarizes our non-parametrical test-results.

[47] n denotes the number of independent observations per treatment.

[48] We used the stated vector of probabilistic forecasts to calculate for each participant the average amount of money she expects her group members to contribute.

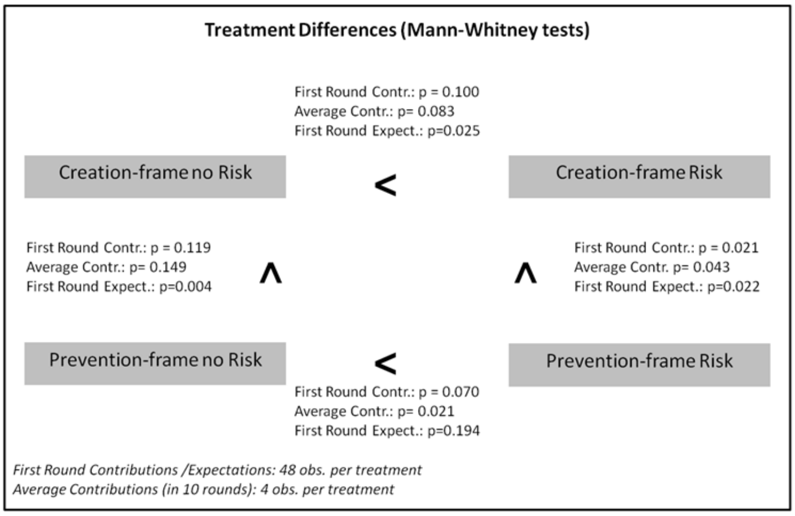

Figure 4-4: Treatment differences with Wilcoxon rank sum tests

Table 4-2 presents different specifications of a tobit model regressing average contributions on a variety of explanatory variables.

Dep. Var.: Average contr.	I	II	III
prevention	**0.609**† (0.328)	**0.609**† (0.334)	**0.610**† (0.333)
risk	**1.013*** (0.414)	**1.023*** (0.416)	**0.759*** (0.354)
prevention × risk	0.598 (0.502)	0.589 (0.508)	0.588 (0.507)
risk aversion[a]		0.002 (0.028)	-0.019 (0.017)
risk aversion × risk			0.041 (0.055)
constant	**1.041***** (0.256)	**1.018***** (0.297)	**1.152***** (0.225)
# observations	192	179[49]	179
(# groups)	(16)	(16)	(16)
Pseudo R^2	0.49	0.48	0.48
Log Pseudolikelihood	-135.58	-127.62	-127.33

Tobit regressions with errors clustered for independent observations (groups); standard errors in parenthesis; ×: interaction; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.
[a] Discrete variable ranging from 0 (extremely risk seeking) to 10 (extremely risk averse).

Table 4-2: Sum of contributions over ten rounds

[49] Thirteen players were excluded as no risk aversion parameter could be determined due to multiple switching in the choice list task.

Errors are clustered for our 16 independent observations (matching pools of 12 subjects each). Regressions confirm the previous finding that both prevention framing and outcome risk have a significant impact on total contributions (outcome risk at a 5% level, prevention framing at a 10% level). The interaction effect of both, however, is not found to be significant. The level of risk aversion elicited in part 3[50] is found to have a non-significant impact on contributions – neither in the outcome risk treatments nor in the treatments without outcome risk. [51]

This appears surprising as we have shown in section 4.2.2 that risk aversion resulting from utility curvature should have an impact on contributions in the treatments exhibiting outcome risk. It is, however, important to note that Holt and Laury's (2002) mechanism cannot distinguish between risk aversion arising from concavity of the utility function and risk aversion resulting from probability weighting. In fact, for the payoffs involved in laboratory experiments subjects are often assumed not to exhibit a concave but a linear utility function, implying observed risk aversion to stem from probability weighting (Abdellaoui, 2011; Wakker 2010).

As shown in Figure 4-3, average contributions lie consistently below average expectations about others´ contributions in the CnR-, PnR-, and CR-treatment. Only in the PR-treatment average contributions match or even exceed average expectations. This finding is confirmed by a random effects tobit model regressing contributions to the public account on expectations, which is presented in Table 4-3. Each matching pool (containing 12 subjects) is treated as an independent observation, resulting in 16 independent observations in total. In all treatments participants' contributions are significantly affected by their expectations on others' contributions indicating that subjects are on average conditional co-operators. However, in the CnR-treatment, participants contribute only €0.87 on average for

[50] Risk aversion is determined by the decision number where a decision maker switches from the lower variance lottery to the higher variance lottery in part 3 – resulting in a categorized risk measure ranging from 0 (extremely risk seeking) to 10 (extremely risk averse).

[51] Risk aversion elicited in part 3 did not differ significantly between treatments. Figure 4-13 in appendix A6 shows distributions of save choices per subject separately for each treatment. On average, subjects chose in 6.42 out of 10 cases the low variance lottery (safe choice) in the CnR-treament compared to 6.44 in the PnR-, 6.42 in the CR-, and 6.39 in the PR-treatment.

each euro they expect others to contribute.[52] This relation is not significantly different for the CR- and PnR-treatments. Solely in the PR-treatment participants are on average willing to contribute an amount at least as high as the average they expect others to contribute.[53]

Dep. Var.: contribution	
expectation	**0.865****** (0.059)
expectation × CR	0.065 (0.065)
expectation × PR	**0.181**** (0.064)
expectation × PnR	-0.112 (0.090)
constant	no
# observations	1920
(# groups)	(16)
# rounds	10
Wald chi2	1993.31
Log Likelihood	-3276.69
Random effects tobit regressions: standard errors in parenthesis; ×: interaction; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05, and † at p=0.10.	

Table 4-3: Contributions as a function of expectations

Looking at single observations, in the PR-treatment subjects chose a contribution lower than their average expectation (on others' contributions) only in 34.6 % of all contribution decisions, compared to 52.0 % in the CR-treatment, 66.9 % in the CnR-treatment, and 59.0 % in the PnR-treatment. This *self-serving bias* (Fischbacher *et al.*, 2001) is confirmed when looking at conditional contribution patterns further below. Fischbacher and Gächter (2010) argue that the self-serving bias in conditional contributions itself leads to a decay in contributions even if an entire group of participants consists solely of conditional cooperators. Indeed, while contributions decrease over time in all other treatments they remain fairly constant in the PR-treatment – the treatment with no self-serving bias on average.

[52] Applying a chi^2-test we can reject the null hypothesis that this coefficient is not different from one (p=0.0291).

[53] A chi^2-test cannot reject the null hypothesis that $\beta_{expectation} + \beta_{expectation*PR}$ is not different from one (p=0.1230).

4.4.2 Conditional Cooperation (Part 1)

Figure 4-5 shows average conditional contribution functions elicited in our four treatments.

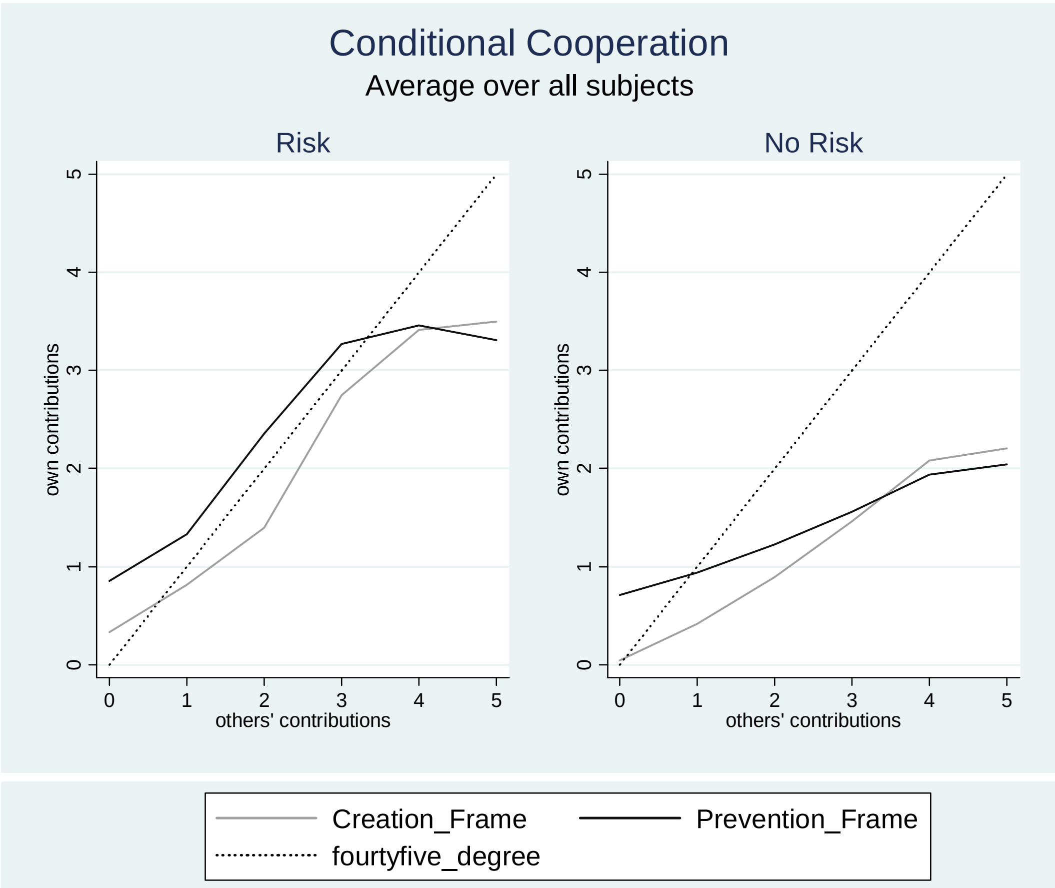

Figure 4-5: Conditional Cooperation (creation vs. prevention)

In all four treatments conditional contribution is an increasing function of the contributions made by others. Thus, in all four treatments subjects display conditional cooperation on average, confirming the findings presented in Table 4-3.[54] Noticeably, the slope of the conditional contribution line is steeper in the PR- and CR- than the PnR- and CnR-treatments, meaning that subjects exhibit a stronger tendency to conditionally cooperate when facing outcome risk than when facing no

[54] While in treatments CnR, CR, and PnR the average conditional contribution line is below the 45°-line, thus indicating the above mentioned self-serving bias in conditional cooperation behavior, the conditional contribution line for the PR-treatment lies in large parts even above the 45°-line, confirming our finding in section 4.4.1.

outcome risk on average.[55] Using a two sided Mann-Whitney test we find this difference to be highly significant both for the creation frame and for the prevention frame comparing the sum of contributions over all six levels (creation frame: z=3.784, p=0.0002; prevention frame: z=3.751, p=0.0002). Comparing conditional contribution behavior under either creation- or prevention-framing in treatments without outcome risk, no significant difference can be found. Conditional contributions in the PnR-treatment start on a slightly higher level than in the CnR-treatment. However, contribution levels converge with higher average contributions of other group members. Taking average contributions aggregated over all six levels, conditional contribution behavior is not significantly different between the CnR- and the PnR-treatment (Mann-Whitney (two sided): z=0.605, p=0.545). Facing outcome risk subjects' average conditional contributions are higher in the PR-treatment than in the CR-treatment in 5 of 6 cases. Nevertheless, comparing average aggregate contributions over all six levels, a two sided Mann-Whitney test does not yield a significant difference (z=0.1237, p=0.216).[56]

We thus find average conditional contribution decisions to differ significantly between outcome risk and no outcome risk while no difference is found in the framing dimension. In order to better understand the differences found in the risk dimension we compare distributions of cooperation types using the classification introduced by Fischbacher *et al.* (2001)[57].

	Free-rider	**Cond. Cooperator**	**Hump-shaped**	**others**
CnR	**38%** *(18)*	**50%** *(24)*	**10%** *(5)*	**2%** *(1)*
PnR	**27%** *(13)*	**35%** *(17)*	**19%** *(9)*	**19%** *(9)*
CR	**2%** *(1)*	**73%** *(35)*	**19%** *(9)*	**6%** *(3)*
PR	**2%** *(1)*	**52%** *(25)*	**29%** *(14)*	**17%** *(8)*
(Absolute numbers in brackets.)				

Table 4-4: Cooperation types per treatment

[55] Figure 4-14 in appendix A6 provides a better graphical comparison of conditional cooperation under outcome risk and under no outcome risk.

[56] Table 4-8 in appendix A5 reports all test statistics separately for each contribution level.

[57] We apply the same classifications used in Fischbacher *et al.* (2001). Thus, subjects are classified as *conditional cooperators* not only if their responses are monotonic in a strict sense but also if their contributions positively and significantly (at the 1% level) correlate with others' contributions according to a Spearman rank correlation coefficient.

As shown in Table 4-4, we find a strikingly lower number of *free riders* in the outcome risk treatments compared to treatments with no outcome risk involved. In the CR- as well as in the PR-treatment only 1 out of 48 participants can be classified as free riders compared to 18 in the CnR- and 13 in the PnR-treatment. Differences are highly significant using a Fisher's exact test ($p<0.001$). Additionally, we find contributors classified as *others* to be more frequent in the prevention treatments compared to treatments framed as creation-task.[58] Conditional cooperation patterns of the four classified types do not differ considerably across treatments as shown in Figures 4-13 to 4-16 in appendix A6.[59] Thus, the steeper slope of conditional contribution lines under outcome risk appears to be mostly driven by the differing distribution of types, specifically the lower share of free-riders.

4.5 Discussion

We find that both prevention framing as well as outcome risk tend to increase cooperativeness in social dilemmas.

Finding higher cooperation in treatments framed as preventive tasks than in treatments framed as creative tasks cannot be explained by expected utility theory as framing does not affect final payoffs. Alternatively, participants can be assumed to adopt reference points in the way intended by the framing, i.e. to adopt initial endowments as reference points and view further changes as either gains or losses, and to exhibit loss aversion as described by Kahnemann and Tversky's Prospect Theory (Kahnemann and Tversky, 1979). But even under these assumptions, predictions are still not clear-cut. On the one hand, if losses loom larger than gains, preventing the loss of some amount of money generates a higher utility than creating the same amount. Hence, cooperation would be perceived as more desirable under a prevention frame. On the other hand, from a selfish perspective contributing remains a dominated strategy as it reduces the subject´s own payoffs (either deterministically

[58] Following Fischbacher *et al.* (2001), the class of 'others' consists of individuals who cannot be classified into any of the remaining classes. In our sample these are mostly subjects who indicated to contribute the same non-zero amount independently of what others do.

[59] See also Table 4-9 in appendix A5.

or in expectations). Given loss aversion, contributing is even more harmful in the prevention frame as it increases the lost amount a contributor suffers with certainty. Our findings suggest that higher contributions in the prevention frame are mostly driven by higher expectations about others' contributions. We find first round expectations about others' contributions to be significantly higher in the prevention frame than in the creation frame both under outcome risk and under no outcome risk. Thus, subjects expect others to be more cooperative when the goal of cooperation has a preventive character. As subjects are conditionally cooperative on average they consequently respond with higher contributions resulting in higher average contributions in the prevention frame treatments. Cachon and Camerer (1996) find loss avoidance to help individuals establishing more desirable equilibria in coordination games. Brown and Stewart (1999) find participants in a public bad game to raise their internalization rates if a minimum cooperation level is necessary to avoid losses for the group. Despite the absence of implicit cooperation thresholds, higher expectations about the cooperativeness of others help conditional cooperative subjects to coordinate on higher contribution levels in our prevention frame treatments (although a self-serving bias in the CnR-treatment still leads to a decay in contributions over time).

The finding of higher contributions in the outcome risk treatments compared to the treatments without outcome risk is more puzzling. As shown in section 4.2.2 higher contributions under outcome risk cannot be explained by the curvature of the utility function - at least not with the curvature we elicit on average for our participants. Eliciting risk preferences with a choice list mechanism (Holt and Laury, 2002) we find subjects to be risk averse on average exhibiting a level of risk aversion that would rather predict lower contributions under risk. Instead, our elicited risk measure does not explain variances in contribution decisions at all, indicating that the risk aversion measured with this elicitation method might not stem from utility curvature but rather from probability weighting.

One possible explanation for our findings could base on the assumption of betrayal aversion. In a modified trust game Bohnet *et al.* (2008) find subjects to be more averse to a risk deriving from being betrayed by one's partner than to an equivalent risk deriving from a chance device. In a public goods game exhibiting no outcome risk by contributing a decision maker puts her fate completely in the hand of others as she can only hope that others will do the same. In an outcome risk social dilemma the profitability of a contribution does not necessarily depend on the contributions made by others. A contribution can be beneficial even if group members do not reciprocate appropriately as each contributed euro can provide the pivotal percentage points to enable the desired event and hence, provide an outcome larger than without contribution. However, betrayal aversion cannot explain our finding of differing conditional cooperation behavior as the strategy method eliminates any risk of betrayal, thus discrediting this explanation.

An alternative explanation for higher contributions under outcome risk could be that it is less obvious for decision makers under outcome risk than under no outcome risk that contributing is a dominated strategy. Individuals are usually found to have difficulties with the evaluation of probabilities. They tend to overweight small probabilities while underweighting medium and large probabilities (Kahnemann and Tversky, 1979). In our case, the overweighting of small probabilities might induce subjects to overweight the marginal impact of a contribution. Assuming a probability weighting function of the functional form $w(p) = \frac{p^{\gamma}}{(p^{\gamma} + (1-p)^{\gamma})^{1/\gamma}}$ introduced by Tversky and Kahnemann (1992) with p denoting the actual probability, $w(p)$ the perceived probability, and $\gamma \in (0,1]$ the intensity of the bias (with $\gamma = 1$ implying no probability weighting) a $\gamma \leq 0.735$ would be sufficient to evaluate a contribution as beneficial (as $w(p = 0.05) > 0.1$).[60] Empirical studies proof a weighting bias of this size to be a quite realistic assumption. Employing maximum-likelihood estimation techniques, Tversky and Kahnemann (1992) find a $\gamma = 0.61$, Wu and Gonzalez (1996) $\gamma = 0.71$, and Camerer and Ho (1994) $\gamma = 0.56$. Thus, probability weighting might cause subjects to perceive contributions as more beneficial in the risk treatments than in the no risk treatments as it makes them overweighting the marginal impact of a contribution and thus mitigates the social dilemma structure of

[60] With w(p)=0.1, one euro contribution to the public account increases the pubic good's expected value by the same amount. Hence, a risk-neutral decision maker (with respect to utility curvature) is indifferent between contributing and not contributing. Solving the inequation $0.1 > \frac{0.05^{\gamma}}{(0.05^{\gamma} + (1-0.05)^{\gamma})^{1/\gamma}}$ for γ yields the weighting parameter necessary for a risk neutral agent to perceive a contribution as beneficial (blanking out any potential contributions of other participants).

the game. This hypothesis is supported by the finding of a significantly lower share of free-riders in the treatments exhibiting outcome risk compared to the ones without outcome risk. However, if subjects are assumed to aggregate probabilities, probability weighting would predict them to contribute particularly when they expect others to contribute either a lot or very little since the perceived marginal impact of an additional percentage point (slope of the weighting function) is largest at these points. We do not find such pattern. Yet, it is a frequently observed behavioral pattern of boundedly rational individuals to evaluate new gambles to some extent in isolation instead of merging them with other existing risks. Kahneman and Lovallo (1993) introduce the term *narrow framing* to describe this phenomenon. It is hence conceivable that participants do not use their expectations on other's contributions to aggregate probabilities. They might rather consider each euro contribution as an independent small probability prospect and thus, overweight the impact of each contribution, no matter what others do.

A further explanatory approach for our finding of higher contributions under outcome risk builds on the assumption that subjects contribute more in these treatments in order not to feel responsible for the desired event not to occur. Not contributing in a treatment without outcome risk means to refuse other group members an amount of €0.50 for each euro not contributed to the public account, thus €2.50 per group member at most. In contrast, if non-contributors in the outcome risk treatments feel responsible for the occurrence of the undesired event the feeling of guilt should be much stronger ex-post as their behavior refused all group members (including the non-contributor herself) earnings of €10 each. In fact, evidence for individuals to feel ex-post responsible for an event not to occur despite their very low marginal impact can be found in the literature on voting behavior. Kanazawa (1998; 2000) finds individuals who preferred the losing presidential candidate but abstained in the election to become more likely to vote in subsequent elections as they feel responsible for the defeat. His results can hardly be explained by an overweighting of small probabilities but rather by a tendency of individuals to feel as pivotal. This tendency could likewise explain our finding of higher contributions under outcome risk when assuming participants to anticipate the guilt they would feel ex-post in case of not contributing.

4.6 Conclusion

In this chapter, we systematically varied a public goods game in two dimensions exploring on the one hand whether cooperation is more likely if the goal of cooperation is either to create a common value (creation frame) or to prevent the loss of a common value (prevention frame) and on the other hand whether contributions are higher when deterministically increasing the value of the public good (no outcome risk) or when increasing the probability of successful provision (outcome risk). Combining both dimensions, we obtained four different treatments which were designed in a way that kept the expected value of a marginal contribution constant over treatments. Matching subjects over ten rounds in groups of four in a stranger design we elicited subjects' contributions as well as their expectations about others' contributions in each round, incentivizing the estimation decision with a quadratic scoring rule. Additionally, we elicited subject's conditional cooperation functions employing a strategy method introduced by Fischbacher *et al.* (2001).

We found both prevention framing and outcome risk to increase cooperation. Consequently, highest contribution levels could be found when combining both features. Here, the self-serving bias usually found in conditional cooperation (Fischbacher and Gächter, 2010) disappeared, allowing the establishment of a constant contribution level in this treatment while cooperation collapsed over time in all other treatments.

Although we found a higher share of individuals contributing a constant non-zero amount independently of what others do if the game was framed as a preventive task, average conditional cooperation behavior did not differ significantly between preventive and creative tasks. Instead, higher contribution levels in the prevention frame were mostly driven by higher expectations about the contributions made by others. Our participants who we found to be conditionally cooperative on average expected group members to be more cooperative under a prevention frame and consequently responded with higher contributions.

Higher contributions under outcome risk were also partly driven by higher expectations about others' contributions but mostly by a significantly lower share of free-riders in treatments exhibiting outcome risk.

Finding no self-serving bias on average in our treatment exhibiting both outcome risk and prevention framing was driven by a combination of the near disappearance of free-riders under outcome risk and an increase in the share of constant contributors, i.e. participants constantly contributing a non-zero amount independently of what others did, which we generally found under prevention framing. The combination of both occurrences made the self-serving bias to disappear on average. However, the latter finding should not be generalized as it might depend on the specific configuration of our task.

Expected utility theory cannot explain our finding of higher cooperation under prevention framing as the framing does not affect final outcomes. But even the assumption of subjects to adopt reference points in the way intended by the framing, i.e. to adopt initial endowments as reference points and view further changes as either gains or losses while exhibiting loss-aversion, does not provide a clear-cut hypothesis. On the one hand, as losses loom larger than gains, cooperation in order to prevent a loss would be more desirable than cooperation with a creative character. On the other hand, contributing remains a dominated strategy under prevention framing and might even be more harmful here, as it increases the lost amount a contributor suffers with certainty. We argue that the higher desirability of cooperation in preventive tasks may drive the higher expectations found under prevention framing and, hereby, helps conditional contributors to establish higher contribution levels. Our finding of higher contributions under outcome risk is more puzzling as it cannot be explained by a concave shaped utility function which would rather predict lower contributions under risk. We argue that probability weighting may lead subjects to overweight their marginal impact which mitigates the perceived social dilemma property of the game, an assumption which is supported by the significantly lower share of free-riders in treatments exhibiting outcome risk. Another explanation we discussed is that subjects contributed in the outcome risk treatment in order to avoid the burden of feeling responsible for an occurrence of the undesired event ex-post. Further research is necessary to better understand the mechanisms behind these findings.

Our results contribute to better understand why some real world social dilemmas are more easily overcome than others. It may help to explain the record-setting election turnout in France’s presidential election 2002 where voters contributed to a public good by going to the polls in order to prevent the election of radical right-wing candidate Jean-Marie Le Pen, who made it surprisingly into the runoff election due to a fragmentation of the left. It may help to explain the unprecedented success of the global community in abandoning chlorofluorocarbon (CFC) which was ratified in the Montreal Protocol 1989 in order to prevent a further depletion of the ozone layer. And it may explain why global aid agencies have such a hard time to attract money for their day to day business in developing regions, including mostly creative tasks exhibiting no outcome risk such as the funding of education and the construction of infrastructure, while donations abound in case of disasters when money is needed to prevent further damage.

Furthermore, our results suggest interest groups concerned with the provision of a specific public good containing a preventive character and/or outcome risk to emphasize these characteristics as much as possible. In fact, there may often exist some leeway in how to present a social dilemma. The construction of an airport, for instance, can be portrayed as an opportunity to create access to new markets and more convenient travelling but it can also be presented as a necessary investment in order to prevent a region from falling behind in global competition. Such leeway may also exists when justifying a military intervention either as a measure to create free access to markets and commodities or as a measure to prevent the expansion of international terrorism. It would be interesting to know to what extent interest groups are aware of these effects and whether they understand to use it the right way.

Further research is, however, necessary to better understand the mechanisms behind our findings. Moreover, various extensions are conceivable. Instead of modeling probability of success as a strictly linear function of contributions - a simplification which we made to enable a clean comparison between outcome risk and no outcome risk conditions - it would be interesting to disclose how non-linear functions may affect our findings. This is particularly true for an S-shaped relationship with contributions having an increasing marginal impact on probability of success up to a certain level and a decreasing marginal impact afterwards thus, never reaching certainty. Additionally, a systematic investigation on the effects of ambiguity concerning the probability of success could be a valuable extension. We leave these issues for future research.

4.7 Appendix

A1. Instructions *(translated from German)*[61]

Welcome to the experiment and many thanks for your participation!
Please stop talking to other participants of the experiment from now on

General rules concerning the procedure
This experiment serves the investigation of economic decision making. You can earn money which will be paid to you in cash after the experiment. During the experiment you and all other participants will be asked to make decisions. Your decisions, as well as the decisions of the other participants, will determine your monetary payoff according to the rules explained below. Additionally, you receive a **show-up fee** of **€4**. The whole experiment will take about **one and a half hours** and consists of **three parts**. In all parts you will make your decisions at the computer. You will receive detailed instructions for each part after finishing the previous. **Parts** are **completely unrelated**. Decisions in one part don't have any consequences for other parts. If you have any questions during the experiment, please raise your hand. One of the experimenters will come to answer your questions in private. In the interest of clarity, we use male terms only in the instructions.

Anonymity
In some parts of the experiment you will be grouped with other participants. Neither during the experiment nor afterwards you or the other participants will learn about the identity of other group members. Neither during the experiment nor afterwards will other participants learn about your experimental earnings. We will analyse the data only in aggregate form and never connect names with experimental results.

Support
You are provided with a pen on your desk. For calculations you will find a link to the Windows calculator on the screen.

Part 1

Procedure
In this part you will be matched in a group of four. All group members receive an endowment of [CnR, CR: €5] [PnR, PR: €15]. You have to split this endowment between a public and a private account. You can allocate each integer between €0 and €5 to the public account. The amount allocated to the public account affects all group members in the same way as you will see below. Each amount which you don't allocate to the public account will automatically be allocated to your private account. For instance, if you put €3 to the public account, [CnR, CR: €2] [PnR, PR: €12] will be allocated to your private account. The amount allocated to your private account affects exclusively your own payoff.
You have to make **two types of decisions** which will be explained in detail more below. In the next section you will learn the consequences of your decisions:

Consequences of your decision
Dependent on your decisions as well as the decisions of the other group members…

-- CnR, PnR --

… your payoff will be determined as follows:

[61] Parts exclusively used in a certain treatment are enclosed in square brackets. Larger treatment-specific text passages are marked with horizontal lines. CnR denote *creation frame no risk*, PnR *prevention frame no risk*, CR *creation frame risk*, and PR *prevention frame risk*.

- If no one allocates money to the public account [CnR: each group member will receive the **amount allocated to his private account**] [PnR: **€10** will be **deducted** from each group members' private account. This money will be lost].
- For each euro allocated to the public account [CnR: you as well as all other group members will **earn €0.50**] [PnR: the **loss** which you and your group members suffer will be **reduced by €0.50**].

--- CR, PR ---

…there are two possible outcomes:

Outcome 1: [CR: Each player receives the amount of **€10 additionally** to the amount located on his private account.] [PR: Each player receives the **full amount** allocated to his **private account**.]

Outcome 2: [CR: Each player receives the **amount** located on his **private account**.] [PR: **€10** are **deducted** from each player's private account. This money is lost. Each player receives the money remaining on his private account.]

Please note:

- One of the two possible outcomes will definitely occur. This outcome occurs **for all four group members**. It is not possible that different outcomes occur for different group members.

The **probability** for **outcome 1** to occur is further denoted with $\mathbf{W_1}$. As one of the two outcomes will definitely occur the probability for the occurrence of **outcome 2** (denoted as $\mathbf{W_2}$) is always the converse probability of W_1 ($100-W_1$). For instance, if $W_1=70$ → $W_2=30$; if $W_1=30$ → $W_2=70$.

Influencing Outcome probabilities
By contributing to the public account, probabilities W_1 and W_2 can be altered:
- **If no group member contributes** to the public account, outcome one will certainly not occur ($W_1=0\%$) for the group and therefore, outcome two will occur with certainty ($W_2=100\%$).
- **For each euro** allocated to the public account by any group member, probability $\mathbf{W_1}$ **rises by 5%** and thus $\mathbf{W_2}$ **drops by 5%**.

--- all ---

Example 1: All four group members allocate nothing to the public account (i.e. [CnR, CR: €5] [PnR, PR: €15] to their private accounts). In total €0 are allocated to the public account.

[CnR: Thus, each group member solely receives €5 (the amount allocated to his private account).]

[PnR: Thus, each group member suffers a loss of €10 (deducted from his private account).]

[CR: Thus, $W_1=0\%$ ($W_1=0\cdot5=0$) and $W_2=100\%$ ($W_2=100-W_1$). Outcome 2 occurs with certainty. Each group member receives the amount allocated to his private account.]

[PR: Thus, $W_1=0\%$ ($W_1=0\cdot5=0$) and $W_2=100\%$ ($W_2=100-W_1$). Outcome 2 occurs with certainty. Each group member suffers a loss of €10 (deducted from his private account).]

Consequently, each group member receives €5.

[CnR: $\underbrace{\underbrace{5}_{endowment} - \underbrace{0}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} + \underbrace{\underbrace{0}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{income\ group\ account} = \underbrace{5}_{total\ income}$]

[PnR: $\underbrace{\underbrace{15}_{endowment} - \underbrace{0}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} - \underbrace{\left(\underbrace{10}_{\max.loss} - \underbrace{\underbrace{0}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{loss\ reduction\ due\ to\ group\ contributions} \right)}_{loss} = \underbrace{5}_{total\ income}$]

[CR: $\underbrace{\underbrace{5}_{endowment} - \underbrace{0}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} + \underbrace{0}_{no\ bonus\ (outcome\ 2)} = \underbrace{5}_{total\ income}$]

[PR: $\underbrace{\underbrace{15}_{endowment} - \underbrace{0}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} - \underbrace{10}_{loss\ (outcome\ 2)} = \underbrace{5}_{total\ income}$]

Example 2: All four group members allocate €5 each to the public account (i.e. [CnR, CR: €0] [PnR, PR: €10] to their private accounts). In total, €20 are allocated to the public account (4·5=20).

[CnR: Thus, each group member receives €10 from the public account additionally to the amount of €0 from his private account.]

[PnR: Thus, €0 will be deducted from each group member’s private account.]

[CR: Thus, W_1=100% (W_1=20·5=100) and W_2=0% (W_2=100-W_1). Outcome 1 occurs with certainty. Additionally to the amount allocated on the private account, each group member receives €10 from the public account.]

[PR: Thus, W_1=100% (W_1=20·5=100) and W_2=0% (W_2=100-W_1). Outcome 1 occurs with certainty. Each group member receives the full amount located on his private account.]

Consequently, each group member receives €10.

[CnR: $\underbrace{\underbrace{5}_{endowment} - \underbrace{5}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} + \underbrace{\underbrace{20}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{income\ group\ account} = \underbrace{10}_{total\ income}$]

[PnR: $\underbrace{\underbrace{15}_{endowment} - \underbrace{5}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} - \underbrace{\left(\underbrace{10}_{\max.loss} - \underbrace{\underbrace{20}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{loss\ reduction\ due\ to\ group\ contributions} \right)}_{loss} = \underbrace{10}_{total\ income}$]

[CR: $\underbrace{\underbrace{5}_{endowment} - \underbrace{5}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} + \underbrace{10}_{bonus\ (outcome\ 1)} = \underbrace{10}_{total\ income}$]

[PR: $\underbrace{\underbrace{15}_{endowment} - \underbrace{5}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} - \underbrace{0}_{no\ loss\ (outcome\ 1)} = \underbrace{10}_{total\ income}$]

Example 3: All four group members allocate €3 each to the public account (i.e. [CnR, CR: €2] [PnR, PR: €12] to their private accounts). In total €12 are allocated to the public account (4·3=12).

[CnR: Thus, each group member receives €6 from the public account additionally to the amount of €2 from his private account. Consequently, each group member receives €8.]

[PnR: Thus, €4 will be deducted from each group member’s private account. Consequently, each group member receives €8.]

[CR, PR: W_1=60% (W_1=12·5=60) and W_2=40% (W_2=100-W_1).]

[CnR: $$\underbrace{\underbrace{5}_{\text{endowment}} - \underbrace{3}_{\text{own contribution to public account}}}_{\text{income private account}} + \underbrace{\underbrace{12}_{\text{sum of contributions to public account}} \cdot \underbrace{0{,}5}_{\text{multiplier}}}_{\text{income group account}} = \underbrace{8}_{\text{total income}}$$]

[PnR: $$\underbrace{\underbrace{15}_{\text{endowment}} - \underbrace{3}_{\text{own contribution to public account}}}_{\text{income private account}} - \underbrace{\left(\underbrace{10}_{\text{max. loss}} - \underbrace{\underbrace{12}_{\text{sum of contributions to public account}} \cdot \underbrace{0{,}5}_{\text{multiplier}}}_{\text{loss reduction due to group contributions}}\right)}_{\text{loss}} = \underbrace{8}_{\text{total income}}$$]

[CR:

With 60% probability each group member receives €12

$$\left(\underbrace{\underbrace{5}_{\text{endowment}} - \underbrace{3}_{\text{own contribution to public account}}}_{\text{income private account}} + \underbrace{10}_{\text{bonus (outcome 1)}} = \underbrace{12}_{\text{total income}}\right)$$

and with 40% probability each group member receives €2

$$\left(\underbrace{\underbrace{5}_{\text{endowment}} - \underbrace{3}_{\text{own contribution to public account}}}_{\text{income private account}} + \underbrace{0}_{\text{no bonus (outcome 2)}} = \underbrace{2}_{\text{total income}}\right).]$$

[PR:

With 60% probability each group member receives €12

$$\left(\underbrace{\underbrace{15}_{\text{endowment}} - \underbrace{3}_{\text{own contribution to public account}}}_{\text{income private account}} - \underbrace{0}_{\text{no loss (outcome 1)}} = \underbrace{12}_{\text{total income}}\right)$$

and with 40% probability each group member receives €2

$$\left(\underbrace{\underbrace{15}_{\text{endowment}} - \underbrace{3}_{\text{own contribution to public account}}}_{\text{income private account}} - \underbrace{10}_{\text{loss (outcome 2)}} = \underbrace{2}_{\text{total income}}\right).]$$

-- CR, PR --

The following table provides a detailed overview of the amounts jointly allocated to the public account by all four group members and the resulting outcome probabilities W_1 and W_2.

Sum of group contributions	W_1	W_2	Sum of group contributions	W_1	W_2
0	0%	100%	11	55%	45%
1	5%	95%	12	60%	40%
2	10%	90%	13	65%	35%
3	15%	85%	14	70%	30%
4	20%	80%	15	75%	25%
5	25%	75%	16	80%	20%
6	30%	70%	17	85%	15%
7	35%	65%	18	90%	10%
8	40%	60%	19	95%	5%
9	45%	55%	20	100%	0%
10	50%	50%			

Table 4-5: Instructions - Probabilities

Please note: At the end of the experiment, you will only receive the amount located on your private account. Your contributions to the public account will not be paid out. Their only purpose is to [CR:

raise the probability for outcome 1 for you and your group] [PR: reduce the probability for outcome 2 for you and your group].

Determining payoffs for part 1
The actual probabilities for outcome 1 and 2 depend on your as well as your group members' decisions. In order to determine your payoff the definite occurrence of outcome 1 or 2 will be played out as follows:
Visible for all, a randomly selected participant throws a ten-sided-die (with digits 0 to 9) twice for each group **at the end of the experiment** (subsequently to part 3). By this **two die throws,** a random number for part 1 will be found for each group with the first throw determining the first digit and the second throw the second digit of this random number.

<u>Example 1:</u> First throw: 7; second throw: 2 → random number: 72.

<u>Example 2:</u> First throw: 0; second throw: 5 → random number: 5.

<u>Please note:</u> This procedure guarantees the occurrence of all **integers between 0 and 99** with equal chances.

This way, a random number is determined which will be typed in the computer by the participant, supervised by the experimenter. Four each group, the computer compares their random number with the probability for outcome 1 (W_1) determined by group members' contributions to the public account. **If** the **random number** is **smaller** than W_1, **outcome 1** will occur for the group. **If** the **random number** is **larger or equal** than W_1, **outcome 2** will occur.

<u>Example 1:</u> Group members have allocated €10 to the public account. Thus,$W_1=10\cdot5=50$. Outcome 1 will occur for random numbers smaller than 50, i.e. 0, 1, 2, ..., 49. These are 50 out of 100 possible numbers. Therefore, the probability for outcome 1 to occur is exactly 50%.

<u>Example 2:</u> Group members have allocated €0 to the public account. Thus,$W_1=0\cdot5=0$. Outcome 1 will occur for random numbers smaller than 0. This will never be the case as the random number is limited to values between 0 and 99. Therefore, the probability for outcome 1 to occur is exactly 0%.

<u>Example 3:</u> Group members have allocated €20 to the public account. Thus,$W_1=20\cdot5=100$. Outcome 1 will occur for random numbers smaller than 100. This will always be the case as the random number is limited to values between 0 and 99. Therefore, the probability for outcome 1 to occur is exactly 100%.

-- all --

Before continuing with the different types of decisions, you are requested to solve a couple of exercises on the computer screen. If you have any questions, please raise your hand. The experimenter will come to answer your questions in private.

CONTROL QUESTIONS[62]

Decisions

As mentioned before you will have to make two types of decisions, which we will call "unconditional contribution" and "contribution table" from now on.

- On the first screen you have to select your **unconditional contribution** to the public account by typing in the amount you want to allocate to the public account. Only integers are possible. After choosing your amount, click "OK".
- On the second screen you have to fill in a **contribution table**. For each possible rounded contribution-average of the other group members you have to choose your contribution to the public account. The contribution table looks as follows:

[62] See appendix A2.

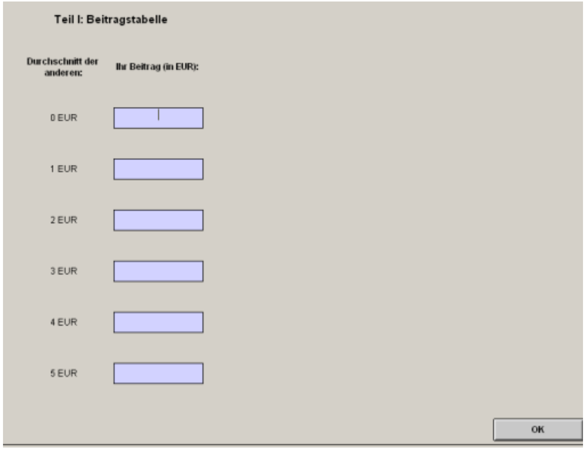

Figure 4-6: Screenshot – Conditional cooperation

Numbers is the left column represent the possible rounded average contributions of other group members to the public account, i.e. the average contribution of each other group member.

In the right column you indicate how much you are willing to contribute to the public account, given the average contribution of your group members.

For instance, you have to choose your contribution to the public account given that others contribute on average €0, €1, €2, €3 etc. In each box you can type in any integer between 0 and 5. **You have to make an entry in each box.** After filling in all boxes, please click "OK".

When all entries for the unconditional contribution and the contribution table are made, the computer will randomly select **one group member** for whom only his **contribution table** will determine his contribution. For **all other group members** the **unconditional contribution** decisions determine their contributions. Since you don't know if you will be randomly selected or not, you should **consider both types of contributions thoroughly**.

Example 1: Suppose you are randomly selected by the computer. Thus, the entries in the contribution table determine your contribution to the public good. The unconditional contributions of the other group members are €0, €2 and €5. Thus, the rounded average is €2 ((0+2+5)/3=2.33). Let's assume you declared in your contribution table to contribute €1 to the public account if the other group members contribute €2 on average. In total 0+2+5+1=€8 are allocated to the public account.

[CnR: Your income would therefore be as follows:

$$\underbrace{\underbrace{5}_{endowment} - \underbrace{1}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} + \underbrace{\underbrace{8}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{income\ group\ account} = \underbrace{8}_{total\ income}$$]

[PnR: Your income would therefore be as follows:

$$\underbrace{\underbrace{15}_{endowment} - \underbrace{1}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} - \underbrace{\left(\underbrace{10}_{\max.\,loss} - \underbrace{\underbrace{8}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{loss\ reduction\ due\ to\ group\ contributions}\right)}_{loss} = \underbrace{8}_{total\ income}$$]

[CR, PR: Probability for outcome 1 will therefore be W_1=40% (W_1=8·5=40) and probability for outcome 2 W_2=60% (W_2=100-W_1).]

Example 2: Suppose you are not randomly selected by the computer. Thus, your entry in the unconditional contribution screen determines your contribution to the public good. Assume you chose an unconditional contribution of €5. Two other group members who aren't selected either chose unconditional contributions of €4 and €5 respectively. Thus, the rounded average is €5 ((5+4+5)/3=4.66)). Let's assume the randomly selected group member indicated in his contribution table to contribute €4 if the other group members contribute €5 on average. In total 5+4+5=€18 are allocated to the public account.

[CnR: Your income would therefore be as follows:

$$\underbrace{\underbrace{5}_{endowment} - \underbrace{5}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} + \underbrace{\underbrace{18}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{income\ group\ account} = \underbrace{9}_{total\ income}$$]

[PnR: Your income would therefore be as follows:

$$\underbrace{\underbrace{15}_{endowment} - \underbrace{5}_{own\ contribution\ to\ public\ account}}_{income\ private\ account} - \underbrace{\left(\underbrace{10}_{max.\ loss} - \underbrace{\underbrace{18}_{sum\ of\ contributions\ to\ public\ account} \cdot \underbrace{0{,}5}_{multiplier}}_{loss\ reduction\ due\ to\ group\ contributions} \right)}_{loss} = \underbrace{9}_{total\ income}$$]

[CR, PR: Probability for outcome 1 will therefore be W_1=90% (W_1=18·5=90) and probability for outcome 2 W_2=10% (W_2=100-W_1).]

The **results of this part** will be revealed **at the end of the experiment**. Then, you will learn the average contribution of others… [CnR, PnR: …as well as your income of this part.] [CR,PR: ... ,the resulting probabilities W_1 and W_2 for your group and (after determining the random number by die throw) your income.]

Part 2[63]

Procedure
In **10 identical rounds** you are matched in **groups of four**. Groups are randomly **re-matched** by the computer **for each round**. Thus, in each round you are matched **with other group members**. Each round you have to make a "**contribution decision**" as well as an "**estimation decision**". The contribution decision is equivalent to your selection of unconditional contributions in part I. In your estimation decision you have to estimate the contributions of other group members to the public account. Both types of decisions determine your income in this part in the following manner: At the end of the experiment one round is randomly selected to be payoff relevant for your contribution decision and one round in randomly selected to be payoff relevant for your estimation decision. Thus, you should consider all your decisions carefully.

Decisions

Contribution decision
This decision problem is **identical** to the one you already know from **part 1**. Endowed with an amount of [CnR, CR: €5] [PnR, PR: €15] you have to decide how much to allocate to a public account (minimum €0, maximum €5) and how much to a private account. Your decisions have identical consequences as in part 1. However, you only have to make an **unconditional contribution decision**. You do **not** have to fill in a **contribution table**. After making ten decisions in ten rounds, **one decision is randomly selected by die throw** to be payoff relevant.

Estimation decision
After choosing your unconditional contribution, you are asked to **estimate** the **average contribution of your three group members** on the next screen. You state how likely you think it is that the three other group members allocate €0, €1, €2, €3, €4 or €5 to the public account on average. The six probabilities must **sum up to 100%**. Depending on the correctness of your estimation, **you can earn**

[63] Handed out after completion of part 1.

an **additional amount between €0 and €5** - provided this round is randomly selected to be payoff relevant.
The next table shows four examples of estimation decisions. The examples will help you to understand the mechanism. Generally, you should consider the following:

- **More probable events** should receive **higher numbers**.
- **Less probable events** should receive **lower numbers**.

Average contribution of group members	Example 1: Estimation	Example 2: Estimation	Example 3: Estimation	Example 4: Estimation
€0	0 %	15 %	0%	70%
€1	0 %	20 %	0%	0%
€2	100 %	30 %	0%	0%
€3	0 %	20 %	50%	5%
€4	0 %	15 %	50%	5%
€5	0 %	0 %	0%	20%

Table 4-6: Instructions – Examples for estimation decisions

In **example 1** the decision maker is absolutely sure that other group members will allocate exactly an average of €2 to the public account. In **example 2** the decision maker does not believe in an average contribution of more than €4, but beliefs average contributions between €0 and €4 to be possible with different probabilities (most probably a contribution of €2). Examples 3 and 4 are further examples for estimation decisions.

Please note: All examples are randomly selected. Your decisions may look completely differently.

The correctness of your estimation determines your income. We use a so called "quadratic deviation rule" to calculate your income. This method ensures that you can earn between €0 and €5, depending on the correctness of your estimation. Let's assume your estimation decision is similar to the one shown in example 1. If average contributions of your group members have been €2, you would earn €5. However, if average contributions have been higher or lower than €2, you would earn €0. With an estimation as shown in example 2, you would earn less than in example 1 in case of average contributions of €2 (more precisely €3.46). However, you would earn a positive amount also in case of average contributions of €0, €1, €3 or €4. And even in case of average contributions of €5 you would still earn a non-zero amount (more precisely €1.96).

This means in general:

- If you are **certain** about the occurring event(s), you should allocate **higher probabilities** to this (these) event(s).
- If you are **uncertain** about the occurring events, it is advisable to **split probabilities** evenly between different events.

In any case you should always **allocate** the **probabilities according to your belief**. The quadratic deviation rule ensures that there is no other way to maximize your earnings.

Information between rounds

After each round your contribution to the public account and the **average contributions of your group members** are displayed on your computer screen. [CnR, PnR: Additionally, the resulting amount that will be paid out at the end of the experiment, provided this round will be randomly selected as payoff relevant, is displayed.] [CR, PR: Additionally, the resulting probabilities for outcome 1 (W_1) and outcome 2 (W_2) are displayed. Whether outcome 1 or outcome 2 finally occur will only be determined at the end of the experiment for the round selected as payoff relevant by die throw.]

Determination of income in part 2

At the end of the experiment **payoff relevant rounds** will be determined for all participants. To determine the round that is payoff relevant for **estimation decisions**, a randomly selected participant throws a **ten-sided-die** (with digits 0 to 9) once and types in the resulting number supervised by the experimenter. This number determines the payoff relevant round for your estimation decision (0 represents round 10). For instance, a digit of one means that you will receive the amount that you earned with the correctness of your decision in round one.

Subsequently, the round relevant for your contribution decision will be determined. The same randomly selected participant throws the **ten-sided-die** again to determine the **relevant round** for your **contribution decision**. Your and your group members' (of this round) decisions determine [CnR, PnR: your income from your contribution decision.] [CR, PR: the probabilities for outcome 1 or outcome 2. Whether outcome 1 or outcome 2 definitely occur will be determined afterwards. This is done exactly as described in part 1. Visible for all, a randomly selected participant throws a ten-sided-die (with digits 0 to 9) twice for each group existent in the payoff relevant round. By this two die throws a random number for part 2 will be found for each group. As in part 1, this random number will be compared to the outcome probabilities of the payoff relevant round. This determines whether outcome 1 or outcome 2 occur for your group in the payoff relevant round.]

This amount will be paid out additionally to your earnings from parts 1 and 3.

Part 3[64]

Task

You face **10 decision problems**. In each decision problem you have to **choose one out of two lotteries**. Once you have made all your decisions, please click the "OK"-button. You should consider all your decisions carefully since each decision can potentially determine your income in this part.

Here is an example for such a decision problem:

Lottery X	**Lottery Y**	**Your Choice**
You receive **€2.00** with probability **8/10** or **€1.60** with probability **2/10**	You receive **€3.85** with probability **8/10** or **€0.10** with probability **2/10**	..Lottery X ..Lottery Y

Your income in this part will be determined as follows: The **computer chooses randomly and with equal probability one decision problem to be payoff relevant**. The lottery chosen by you in this decision problem will be played and paid out in cash.

<u>Example:</u> Suppose the computer randomly selects the decision problem given in the example above and suppose you preferred lottery X. The computer will then simulate lottery X and you will either receive €2 (with probability 8/10 = 80%) or €1.60 (with probability 2/10 = 20%).

[64] Handed out after completion of part 2.

A2. Control Questions *(translated from German)*[65]

Problem 1

From their individual endowment of [CnR, CR: €5] [PnR, PR: €15], the four members of a group decide to allocate the following amounts to the public account: Player 1 → **€0**, Player 2 → **€2**, Player 3 → **€1**, Player 4 → **€0**.

-- CnR, PnR --

a) Which amount do the respective players allocate to their private account?
b) [CnR: Which amounts are added through the contributions made to the public account?] [PnR: By how much will the amount on their private account be reduced because of the contributions to the public account?
c) What payoffs do the players receive?

-- CR, PR --

a) What are the final payoffs in case one or two respectively?
b) With which probabilities do case one (W_1) and case two (W_2) occur?
c) Let´s assume 58 results from the dice throw to determine the random number at the end of the experiment. Which case will occur for the group?

Problem 2

From their individual endowment of [CnR, CR: €5] [PnR, PR: €15], the four members of a group decide to allocate the following amounts to the public account: Player 1 → **€3**, Player 2 → **€4**, Player 3 → **€5**, Player 4 → **€4**.

-- CnR, PnR --

a) Which amount do the respective players allocate to their private account?
b) [CnR: Which amounts are added through the contributions made to the public account?] [PnR: By how much will the amount on their private account be reduced because of the contributions to the public account?
c) What payoffs do the players receive?

-- CR, PR --

a) What are the final payoffs in case one or two respectively?
b) With which probabilities do case one (W_1) and case two (W_2) occur?
c) Let´s assume 58 results from the dice throw to determine the random number at the end of the experiment. Which case will occur for the group?

[65] Parts exclusively used in a certain treatment are enclosed in square brackets. Larger treatment-specific text passages are marked with horizontal lines. CnR denote *creation frame no risk*, PnR *prevention frame no risk*, CR *creation frame risk*, and PR *prevention frame risk*.

A3. Simulation: Marginal contribution-utility for different degrees of risk aversion

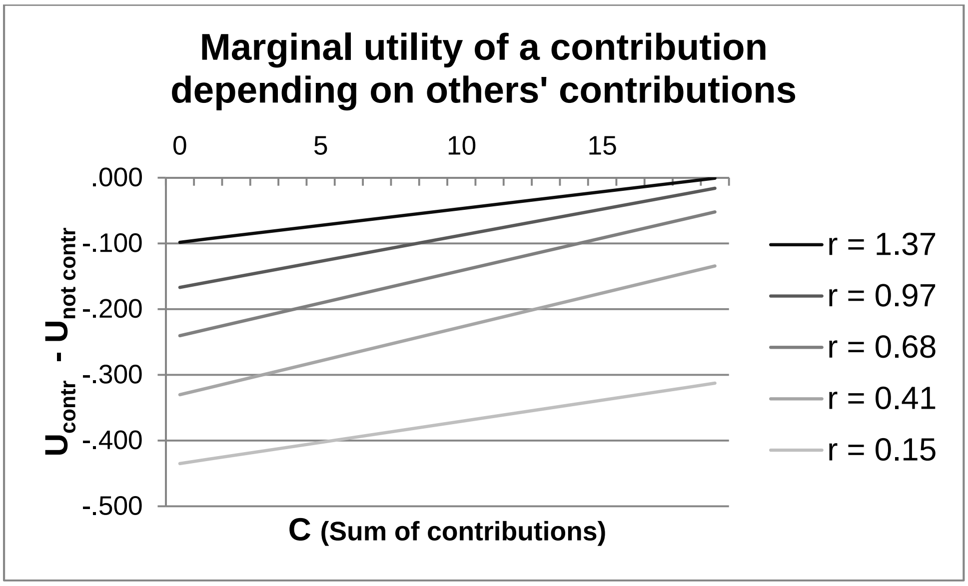

Figure 4-7: Marginal contribution-utility depending on others' contributions

Figure 4-7 shows subject i's marginal utility-gain of a contribution in the CR- and PR-treatments depending on the sum of contributions made by her as well as other group members. Results are computed using the same parameters we apply in our experiment, i.e. $n=4$, $w=5$ and $\gamma=0.5$. Thus, the maximum sum of contributions collected is €20. Utility is determined by a CRRA utility function of the form $U(x)=x^{1-r}/(1-r)$, varying the risk aversion parameter r. The values applied for r are the margins of risk aversion intervals which can be individually determined by a choice list elicitation mechanism introduced by Holt and Laury (2002).[66] Holt and Laury classify subjects obtaining a risk aversion parameter r within the interval between 0.15 and 0.41 as *slightly risk averse* and subjects with $0.97<r<1.37$ as *highly risk averse*. While the vast majority of our participants exhibits an r between 0.41 and 0.97 (see section 4.4), Figure 4-7 shows that even for an extremely high risk aversion parameter of $r=1.37$ marginal utility of a contribution is always negative, although increasing in the sum of contributions. Thus, given the parameters used in our experiment and a realistically sloped utility function, free-riding remains a dominant strategy for selfish individuals.

[66] The mechanism is elaborately described in section 4.2.4.

A4. Simulation: Marginal contribution-utility under risk and no risk condition

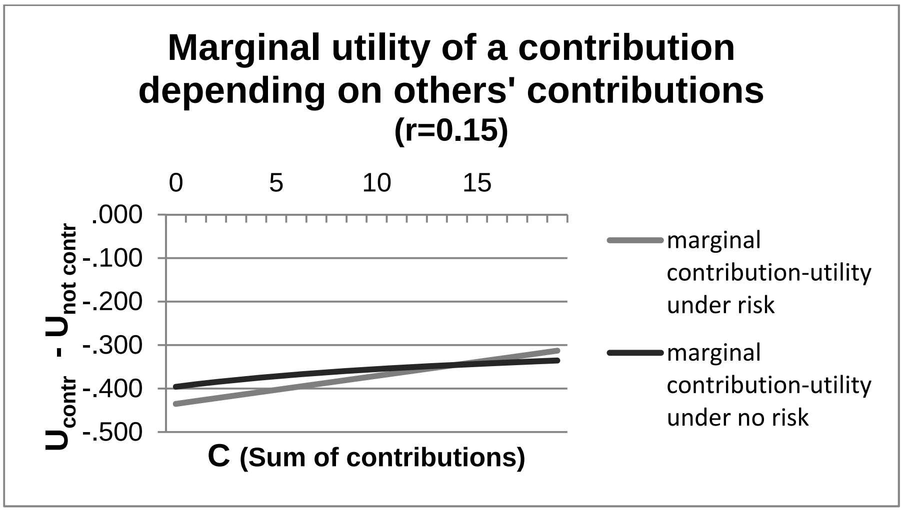

Figure 4-8: Marginal contribution-utility depending on others' contributions (low risk aversion)

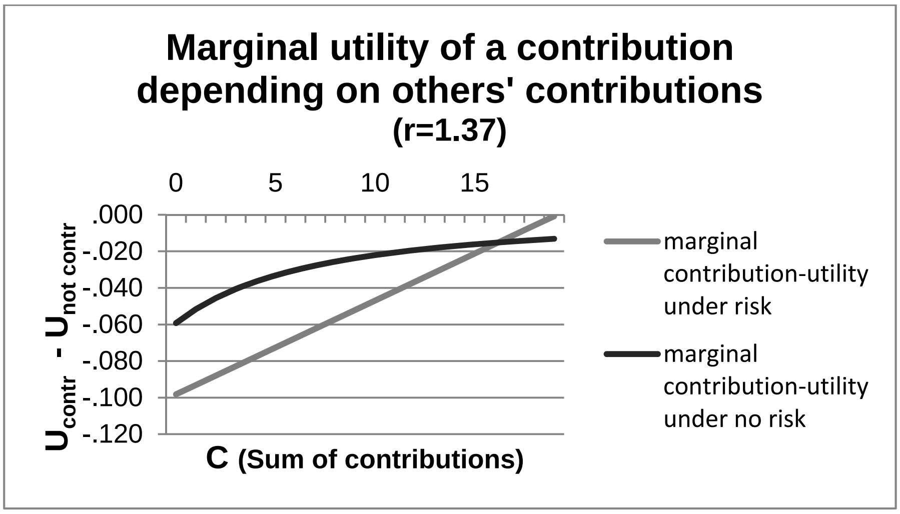

Figure 4-9: Marginal contribution-utility depending on others' contributions (high risk aversion)

Figures 4-8 and 4-9 show the marginal utility of a contribution dependent on the sum of group contributions both under outcome risk and under no outcome risk. It is important to note that marginal utility of a contribution is not only increasing with the sum of contributions under outcome risk but also with no outcome risk in place. The intuition behind this finding is as follows: Given a concave utility function, the marginal utility of money decreases. Given generous contributions of other group members, a decision maker's earnings are already high and thus, the marginal costs of a contribution are lower. Figure 4-8 shows utility curves intercepting at $C = 14$ for slightly risk averse individuals ($r = 0.15$) applying an identically sloped CRRA-utility function as in appendix A3. For highly risk averse individuals ($r = 1.37$)

marginal contribution-utility is higher under the outcome risk condition only if the sum of contributions amount to at least 17 (Figure 4-9).

A5. Tables

Option X	Option Y	Expected payoff difference
1/10 of €2.00, 9/10 of €1.60	1/10 of €3.85, 9/10 of €0.10	€1.17
2/10 of €2.00, 8/10 of €1.60	2/10 of €3.85, 8/10 of €0.10	€0.83
3/10 of €2.00, 7/10 of €1.60	3/10 of €3.85, 7/10 of €0.10	€0.50
4/10 of €2.00, 6/10 of €1.60	4/10 of €3.85, 6/10 of €0.10	€0.16
5/10 of €2.00, 5/10 of €1.60	5/10 of €3.85, 5/10 of €0.10	-€0.18
6/10 of €2.00, 4/10 of €1.60	6/10 of €3.85, 4/10 of €0.10	-€0.51
7/10 of €2.00, 3/10 of €1.60	7/10 of €3.85, 3/10 of €0.10	-€0.85
8/10 of €2.00, 2/10 of €1.60	8/10 of €3.85, 2/10 of €0.10	-€1.18
9/10 of €2.00, 1/10 of €1.60	9/10 of €3.85, 1/10 of €0.10	-€1.52
10/10 of €2.00, 0/10 of €1.60	10/10 of €3.85, 0/10 of €0.10	-€1.85

Table 4-7: Choice list (Holt and Laury 2002)

others' contribution	Framing dimension		Risk dimension	
	CR vs. PR	CnR vs. PnR	CnR vs. CR	PnR vs. PR
0	z= 1.439	**z= 3.272***	**z= -2.194***	z= -0.125
1	z= 1.563	z= 1.942	z= -1.411	z= -1.001
2	**z= 2.662****	z= 1.032	z= -1.516	**z= -3.149****
3	**z= 1.978***	z= 0.157	**z= -4.207*****	**z= -4.999*****
4	z= 0.396	z= -0.262	**z= -3.514*****	**z= -3.712*****
5	z= -0.114	z= -0.114	**z= -2.674****	**z= -2.547***
sum[a]	z= 1.237	z= 0.605	**z= -3.784 *****	**z= -3.751*****

Mann-Whitney tests comparing contributions conditional on what others contribute between treatments; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05.
[a] sum of contributions over all six levels.

Table 4-8: Conditional contribution (by treatment) - Mann-Whitney tests

others' contribution	CnR vs. CR			PnR vs. PR		
	Cond. Contr.[67]	Humpshaped	others	Cond. Contr.	Humpshaped	others
0	z=-1.459	z=0.745	z= -0.447	z= -0.672	z= 1.247	z= -1.514
1	z= 1.053	z= -1.091	z= -1.000	z= -0.472	z= 1.185	z= -1.805
2	z= 0.914	z= -0.416	z= 0.000	z= -1.406	z= -1.298	**z= -1.998***
3	z= -1.037	z= 1.561	z= -1.414	z= -0.772	**z= -3.684*****	**z= -2.307***
4	z= -0.616	z= 0.750	z= 0.000	z= -0.331	**z= -2.344***	z= -1.585
5	z= 0.484	z= -1.644	z= -0.471	z= 0.075	**z= -1.965***	z= -0.717
sum[a]	z= -0.249	z= -1.269	z= -0.447	z= -0.862	**z= -2.978****	z= -1.560

Mann-Whitney tests comparing contributions conditional on what others contribute between treatments separately for each contribution type; *** represents significance at p=0.001, ** at p=0.01, * at p=0.05.
[a] sum of contributions over all six levels.

Table 4-9: Conditional contribution (by contribution type) – Mann-Whitney test

[67] We apply the same classifications used in Fischbacher *et al.* (2001). Thus, subjects are classified as *conditional cooperators* not only if their responses are monotonic in a strict sense but also if their contributions positively and significantly (at the 1% level) correlate with others' contributions according to a Spearman rank correlation coefficient.

A6. Figures

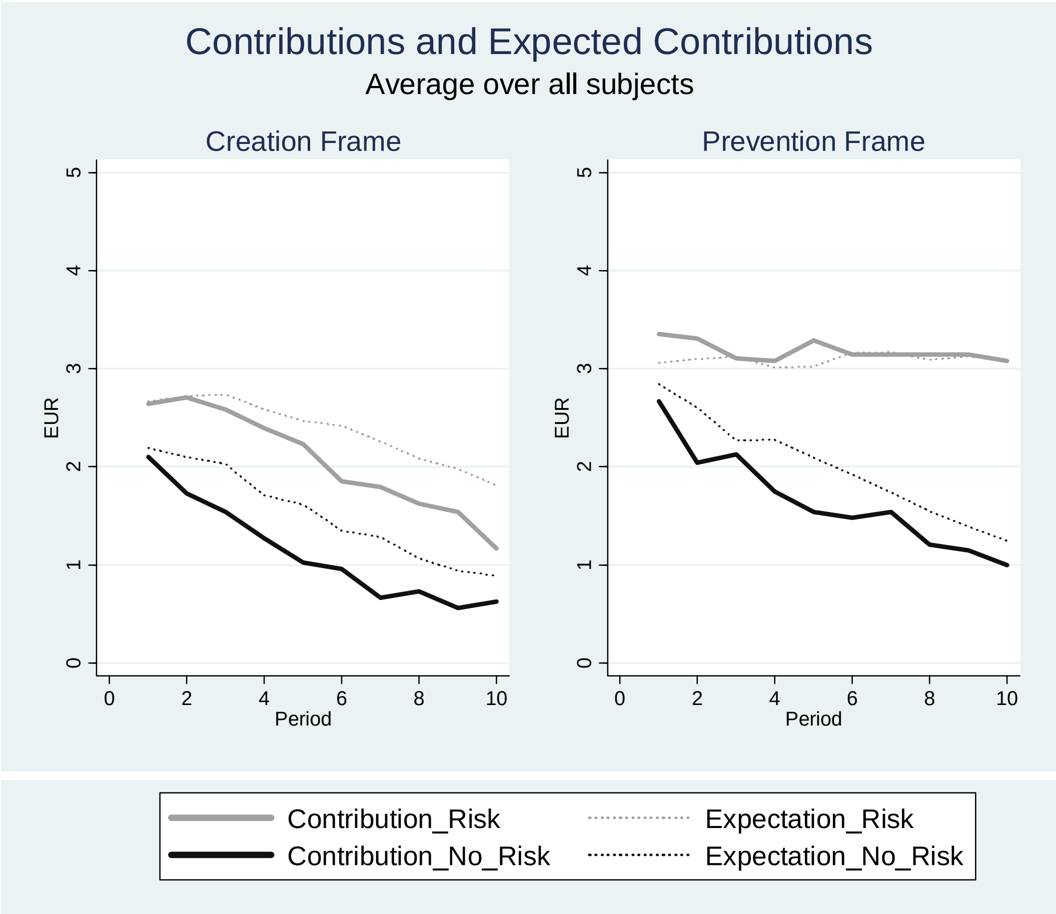

Figure 4-10: Contributions and expected contributions (risk vs. no risk)

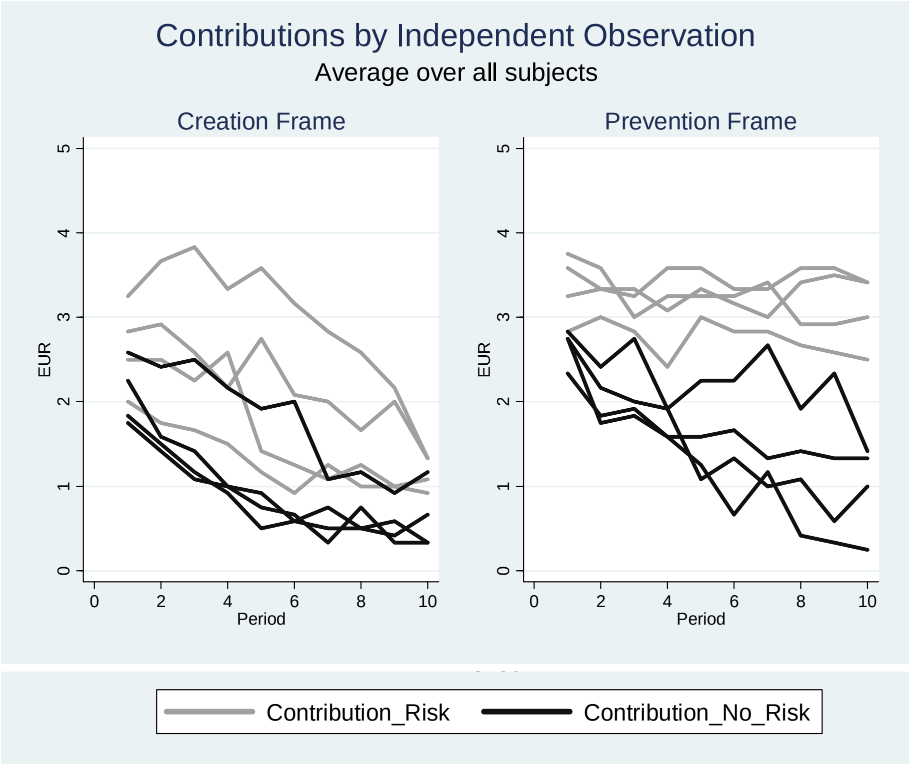

Figure 4-11: Contributions by independent observation (creation vs. prevention)

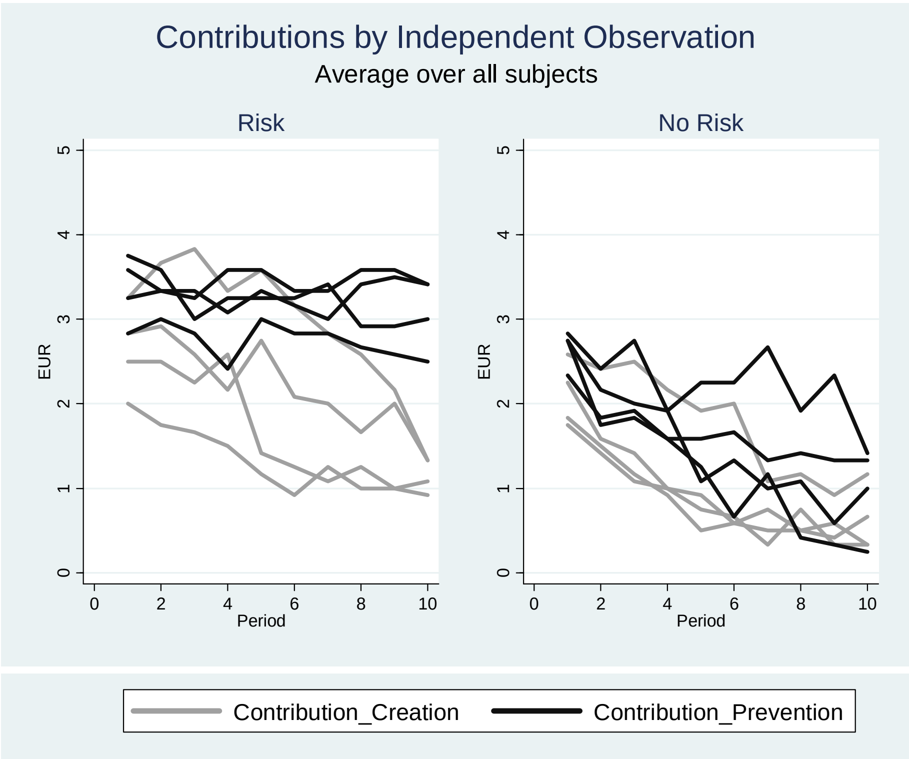

Figure 4-12: Contributions by independent observation (risk vs. no risk)

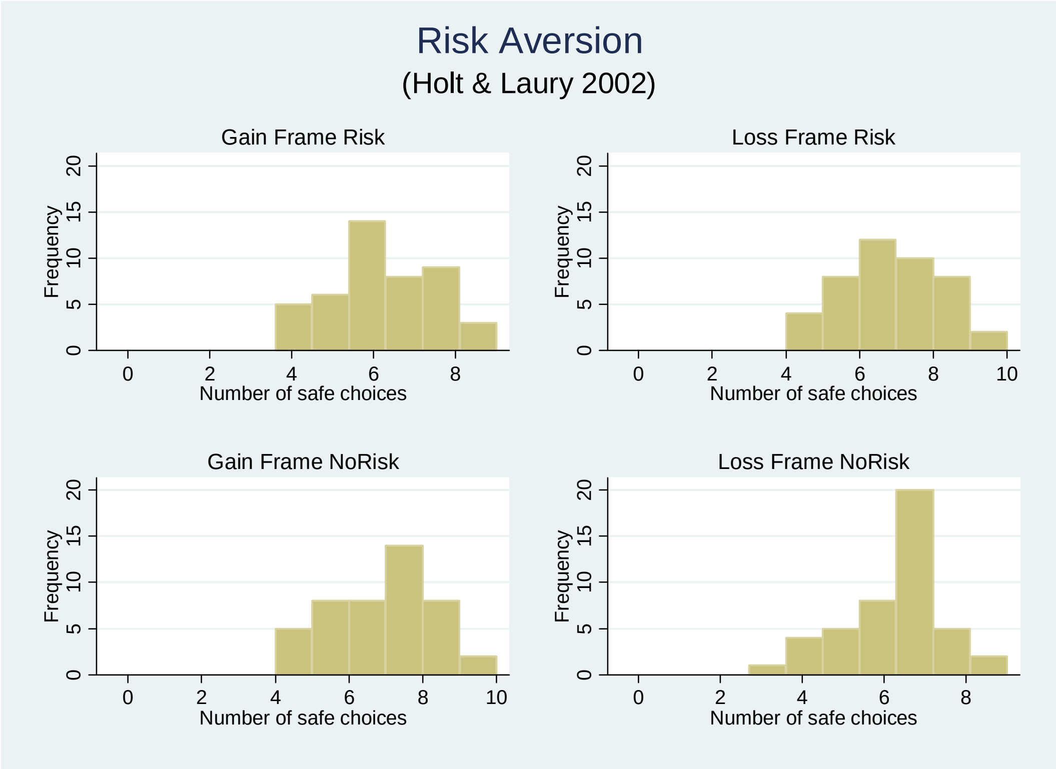

Figure 4-13: Distribution of risk aversion elicited by a choice list mechanism (Holt & Laury 2002)

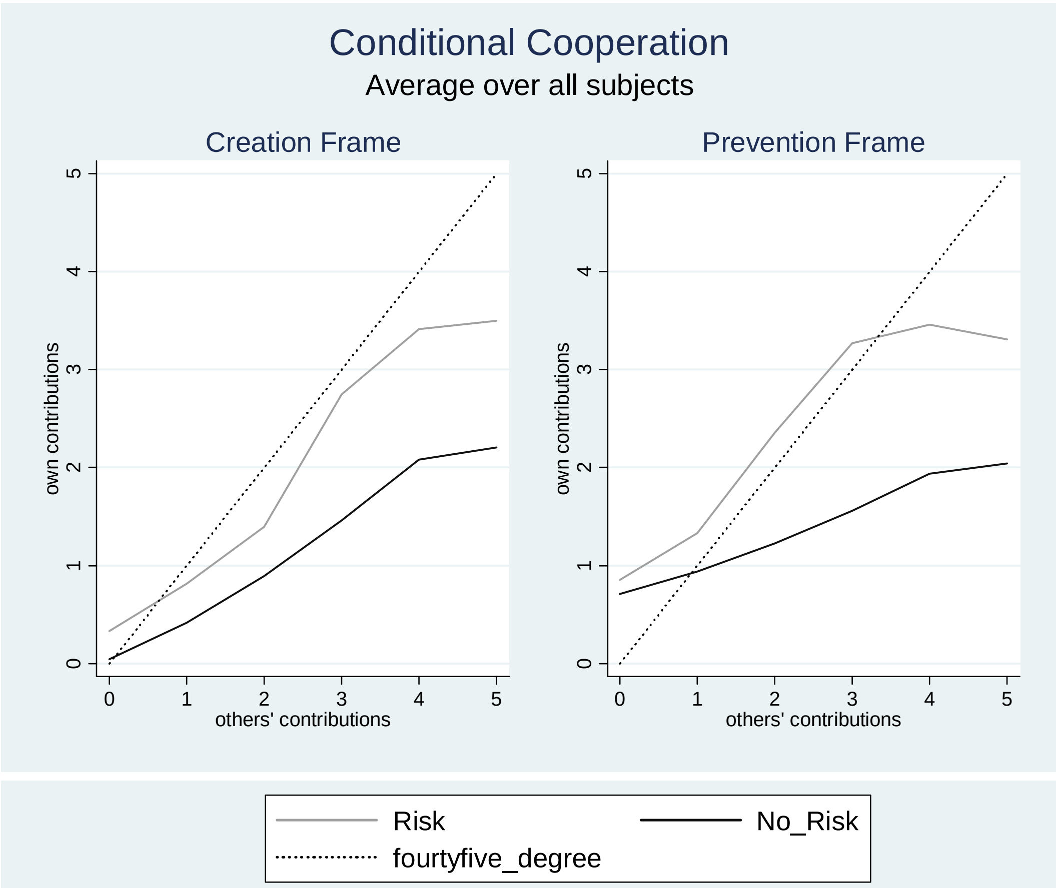

Figure 4-14: Conditional Cooperation (risk vs. no risk)

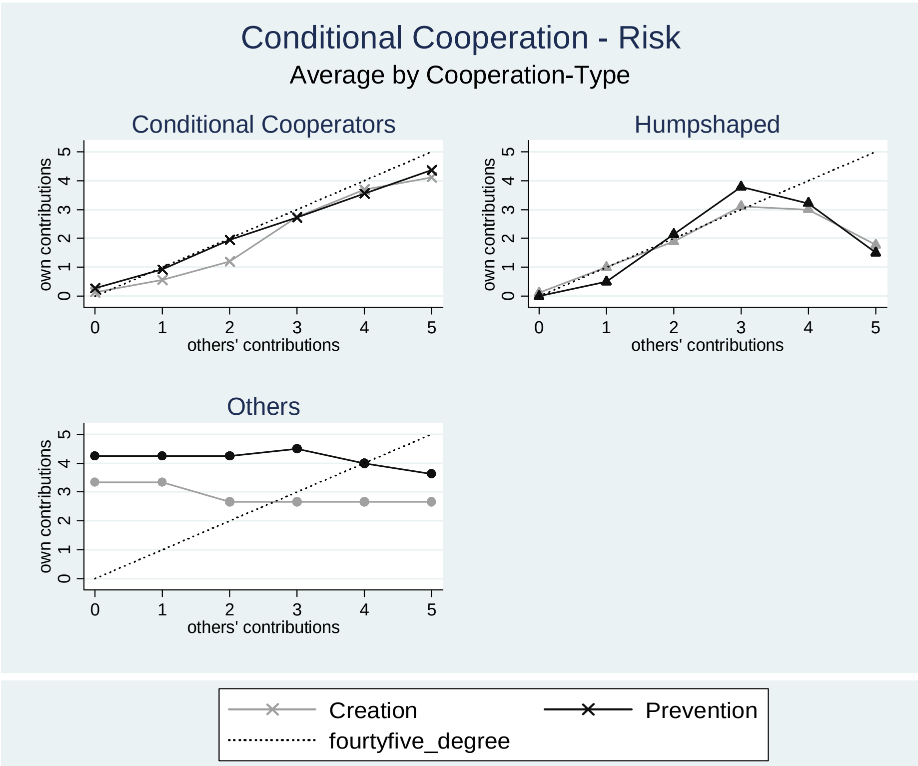

Figure 4-15: Conditional Cooperation by type (creation vs. prevention under risk)

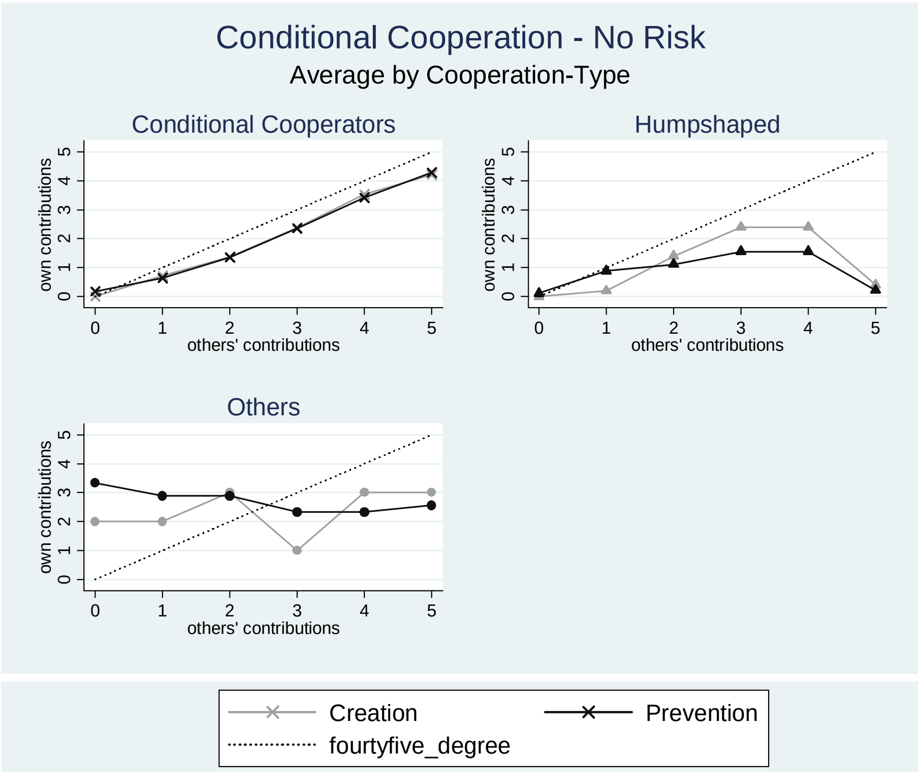

Figure 4-16: Conditional Cooperation by type (creation vs. prevention under no risk)

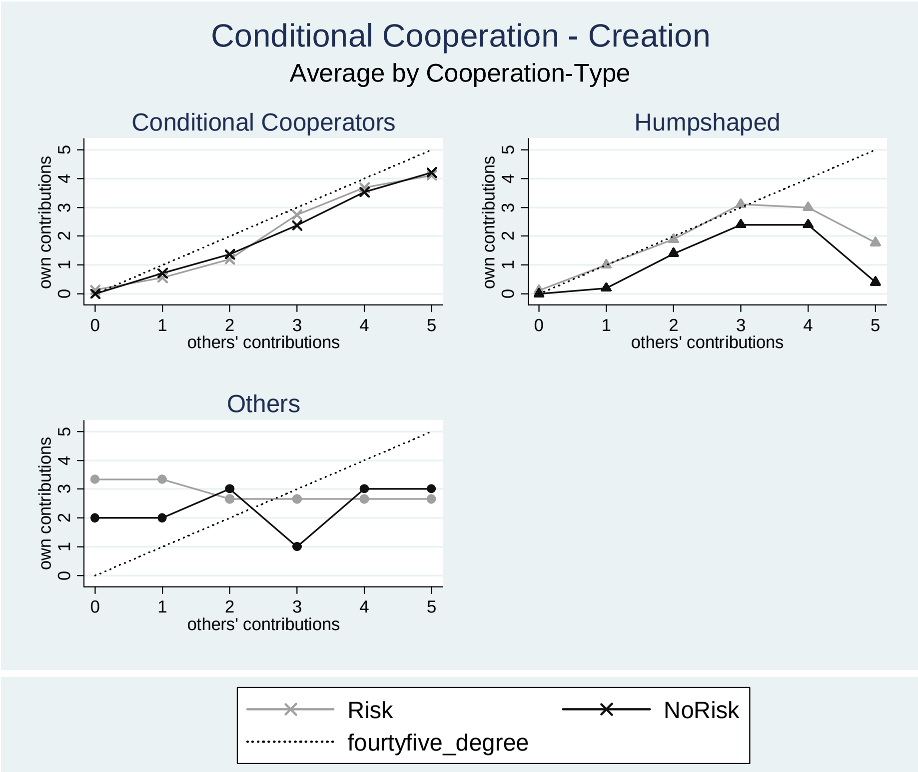

Figure 4-17: Conditional Cooperation by type (risk vs. no risk under creation)

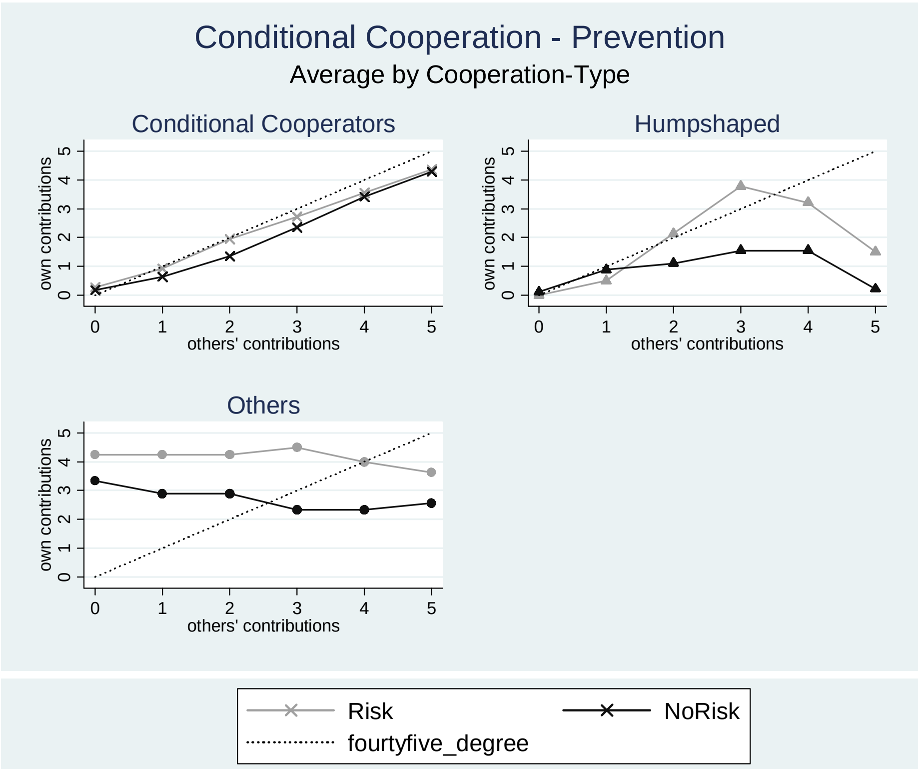

Figure 4-18: Conditional Cooperation by type (risk vs. no risk under prevention)

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Eidesstattliche Versicherung

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