Social Influences on Individual Decision Making Processes

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Sociale invloeden op individuele besluitvorming

Thesis

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To my Parents

Preface

It may be considered ironic that what brought me to a PhD in decision making under uncertainty in the first place was, more than anything else, pure chance. In fact, the decision making process that led me to that choice may in all fairness be considered less than sound. So the question whether my decision making has improved thanks to my studies—one I have been asked many different times by as many different people since the start of this endeavor—is certainly a pertinent one. My standard answer to that question is that it has, at least *in theory*. Reality though tends to be complicated, and is an entirely different matter.

Theories are simplifications of reality, and hence wrong by definition. Indeed, if a theory were to explain reality in all its details it would be just as complex as reality itself, and hence useless. As a direct implication of their necessarily limited scope, theories are made to serve certain purposes. Economic theories explain the working of markets. Psychological theories explain the works of the mind or social interactions. A fundamental step in the progress of science is however to combine different fields of knowledge once they have reached a certain maturity. I hope that I can contribute a little part to that effort.

Before I lose even my last reader, thanks are in place. These go first of all to my parents, for supporting me in every which way in my studies and far from linear career choices. Further thanks go to my supervisor Peter P. Wakker, for his infinite patience, his flexibility, and relentless advice. Not to be forgotten is my colleague and office-mate Stefan Trautmann, for many interesting insights, but most of all for restoring my enthusiasm in moments when I did not feel all too happy with my academic career choice. Special thanks also go to Philip Tetlock, who has given and is continuing to give me priceless inputs for my research (and of course for hosting me at Berkeley), and to Han Bleichrodt, my second supervisor. Further thanks go to all my colleagues first at CREED and the University of Amsterdam and later at Erasmus University Rotterdam, especially to Martin Filko, Kirsten Rohde, Arthur Attema, Gijs van de Kuilen, and Aurelien Baillon.

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Chapter 1

Introduction

1.1 Motivation

‘Ο ’ανθρωπως πολιτικον ζωιον—*man is a social animal*, or so Aristotele would have it. Yet strangely, when one looks at economic theories and experimental investigations thereof, man emerges as quite an isolated being. Especially in individual decision making, the decision maker is often portrayed and treated as a separate entity that can be abstracted from her social environment. However, theories that abstract from naturally occurring contexts come at a cost —their conclusions cannot easily be generalized to everyday decisions.

It has long been considered important in experimental economics to test theories in an environment that is as context-free as possible. Experimental tasks have thus often been devised not only to isolate the decision maker from her usual social environment, but also to abstract as much as possible from concrete tasks encountered in the world outside the lab. It has however been shown that adding context can dramatically change decisions (Griggs, 1995; Loewenstein, 1999). The fundamental issue that is raised by such findings is one of external validity of the experimental evidence. In other words, how much does it really matter that people generally fail to give a correct answer to abstract decision problems?

The issue of external validity is a delicate one. At one extreme, adding too much

context means going back to reality itself, and thus loosing the benefits of theoretical simplification. Indeed, if we could understand reality as it is, there would be no need for simplified theoretical representations in the first place. At the other extreme, abstracting too much from reality risks to eliminate some situational variables that are fundamental for a given decision and, hence, to change behavior radically. Alas, inferences are often too quickly drawn from abstract decision problems to decisions in the real world, which may lead to an overestimation of the real-world impact of biased decision making detected in the lab.

Failures of rationality have been the central point of much of the decision making literature in both experimental economics and psychology in the last decades, though often with a very different focus. Bounds on rationality (Simons, 1954) in situations of complexity and/or uncertainty (Williamson, 1975, p.22) have been widely studied and discussed in that literature. Experimental economists have generally focused on showing how limits to rationality tend to lose their importance when disciplined by market mechanisms. Monetary incentives (Camerer & Hogarth, 1999; Hertwig & Ortmann, 2001), repeated play or learning (List, 2004), and feedback as provided by markets (Coursey *et al*., 1987) have all been shown to potentially make a difference for decision making processes. One could thus say that biases found in relatively abstract problems have been shown to be reduced in market contexts. Social constraints on individual actions have on the other hand been largely neglected in the experimental economics literature.

In a parallel fashion, social psychologists have shown how social constraints may discipline actions and, hence, influence decision processes. At the same time, they have generally neglected market mechanisms such as monetary incentives and learning. Both social and economic mechanisms are however present in real life decision making, and abstracting from one or the other thus limits the generalizability of findings in the literature—not even to mention that it misses out on potentially complex interaction effects of the two elements. Taking a step toward integrating market discipline with social bounds on behavior is thus the main aim of this Ph.D. Thesis.

1.2 Social Influences on Individual Decisions

Although widely studied in social psychology, *accountability*—the expectation on the side of the decision maker that she may have to justify her choice in front of somebody else (Lerner

& Tetlock, 1999)—has not received much attention in economics. Given the strong effect that social elements exert in decision making processes, an increased awareness of social influences in economics seems desirable (McFadden, 2006). Indeed, social mechanisms have such relevance in everyday life that they may at times overwhelm economic motives.

Accountability acts as a powerful cognitive motivator. This finding is supported by the fact that accountable subjects have been found to think about options more in depth in order to anticipate potential criticism, a phenomenon that has been called *preemptive self-criticism* (Tetlock, 1983; Tetlock *et al.*, 1989). Whether such critical thinking about one's decisions results in better decision making depends on the complexity of the task at hand (Simonson & Nye, 1992). If situations reach a level of complexity such that the correct answer cannot be calculated or requires some knowledge that subjects may not have, then the goodness of the decision making process comes to depend on whether the most easily justifiable choice is also the correct one (see chapter 2 for a case in which this is not the case). In the latter case, unconscious thought may well outperform conscious thought, as has been shown by Dijksterhuis (2004).

The issue of the differential activation of conscious and unconscious thought processes and their relative merit is best addressed in the framework of *dual processing theories* (Chaiken & Trope, 1999; Epstein, 2003; Evans, 2003; Kahneman, 2003a, b; Sloman, 2002). These theories assume that an emotional or heuristic system that is located in an evolutionarily older part of the brain is activated together with a rational or rule-based system. The final decision will then result from the interaction of those two systems. Accountability is generally thought to act as a motivational activator of the rule-based system (Kirkpatrick & Epstein, 1992; Scholten *et al.*, 2007), and thus to correct the quicker reactions from the heuristic system that are generally activated automatically.

One should not be surprised that self-presentation concerns can act as a powerful cognitive motivator in decision problems that are highly observable, and for which the decision maker feels accountable. Creating a positive impression seems to be one of the most powerful motivators of human actions, and it is thought to be closely connect with self-esteem. According to *sociometer theory* (Leary& Baumeister, 2000; Leary *et al.*, 2001; Leary & Downs, 1995), self-esteem acts as a monitor of social relations. Low self-esteem thereby signals relational devaluations. Self-esteem is recognized to be important for all aspects of

life, including happiness (Baumeister *et al.*, 2003; Furnham & Cheng, 2000) and economic success (Feinstein, 1999; Trzesniewski *et al.*, 2006). Also, not fitting into group dynamics carries heavy punishment, and may lead to stigmatization and ostracism (Kurzban & Leary, 2001). This explains why subjects who are accountable in front of an audience whose views are unknown (Lerner & Tetlock, 1999; Tetlock, 1983) tend to choose the option that they deem most easily justifiable (Simonson, 1992) in order to avoid embarrassment (Miller & Leary, 1992). In this sense, a metaphor that aptly describes the situation is the view of decision makers as intuitive politicians, who try to please their audience or constituency (Tetlock, 1991), as opposed to the traditional economic view of people as intuitive optimizers.

The main focus of this thesis is to combine the multiple findings from social psychology and apply them with an economic approach to decision making. To this purpose, we investigate accountability and its interaction with market mechanisms, more specifically real incentives in experimental settings. This allows us on the one hand to test the relative strength of incentives and of accountability, and whether one may overwhelm the other under certain circumstances. On the other hand, it brings experimental methods in economics closer to real world conditions, thus increasing the external validity of the findings.

1.3 Outline

This PhD thesis is structured as follows. Chapter 2 studies the effect of accountability on *ambiguity aversion*—the preference for known over normatively equivalent unknown probabilities (Ellsberg, 1961). The effect of accountability is tested using an experimental design that makes the decision maker's preference over the outcomes her private information. Accountability is thereby found to strengthen ambiguity aversion even in the presence of real incentives. What is more, accountability is necessary for ambiguity aversion to occur in the experimental task employed. Additional incentives for normative decision making, though improving the general decision pattern, are found to be insufficient to counterbalance the strong accountability effect. Susceptibility to social pressure as measured by the *fear of negative evaluation* personality scale is found to be correlated with ambiguity aversion under accountability, which indicates that susceptibility to social pressure is indeed what makes people shy away from unknown probability processes when processes with more probabilistic information are available.

Chapter 3 follows up on the ambiguity aversion issue by studying preference reversals under ambiguity. Preference reversals are said to occur whenever a decision maker changes her preference depending on how that preference is expressed. We find that subjects who prefer the ambiguous prospect if given a choice between extracting a ball from an urn with ambiguous probabilities of winning and an urn with known probabilities of winning generally are willing to pay a higher price for the known probability urn. Besides shedding light on ambiguity aversion *per se*, this preference reversal is also new in itself. Traditional preference reversals under risk have been explained through the different salience that the probability and outcome dimension have in choice versus pricing tasks. The fact that preference reversals are found within the sole dimension of likelihood perception is instructive, and a theoretical model is presented to explain it. The latter relies on prospect theory and loss aversion relative to a state-dependent reference point—an explanation that has also been recently employed for traditional preference reversals.

Chapter 4 examines the influence of accountability on risk attitude. Risk attitude is thereby decomposed into its three basic components—utility curvature, probability weighting, and loss aversion. While no effect on utility curvature or probability weighting is detected, accountability is found to reduce loss aversion. This effect is explained with the higher cognitive effort induced by accountability. The emotional reactions at the base of loss aversion are thus counterbalanced by more rational thought processes activated by accountability. The latter conclusion is reinforced by evidence from dual processing models.

Chapter 5 is of a methodological nature. By neglecting social influences on individual decisions, economists have not only run into problems of external validity. There is also a problem with internal validity of experiments that vary incentives and try to study their effect on decisions—the very heart of economic experimentation. Indeed, by contrasting hypothetical decisions with decisions played out for real money, most scholars have co-varied accountability with incentives. Thus, accountability is a confound for real incentives, and effects traditionally ascribed to real incentives may be due to accountability. This makes causal attributions of effects problematic. We separate accountability and incentives, and find several effects. Accountability is found to reduce preference reversals between frames, for which incentives have no effect. Incentives on the other hand are found to reduce risk seeking for losses, where accountability has no effect. In a choice task between simple and compound

events, accountability increases the preference for the simple event, while incentives have a weaker effect going in the opposite direction. It is thus shown that the confounding of accountability and incentives is relevant for studies on the effect of the latter, and that existing conclusions on the effect of incentives need to be reconsidered in light of this issue.

Chapter 2

Causes of Ambiguity Aversion: Known versus Unknown Preferences[1]

2.1 Motivation

In decision under uncertainty people have been found to prefer options involving clear probabilities (*risk*) to options with vague probabilities (*ambiguity*), even if normative theory (Savage, 1954) implies indifference. This phenomenon is called *ambiguity aversion* (Ellsberg, 1961). Ambiguity aversion has been shown to be economically relevant and to persist in experimental market settings (Gilboa, 2004; Sarin & Weber, 1993) and among business owners and managers familiar with decisions under uncertainty (Chesson & Viscusi, 2003). People are often willing to spend significant amounts of money to avoid ambiguous processes in favor of normatively equivalent risky processes (Becker & Brownson, 1964; Chow & Sarin, 2001; Keren &Gerritsen, 1999).

Curley, Yates, & Abrams (1986) showed that increasing the number of people watching a decision enhanced ambiguity aversion, and enhanced it more than other factors that they manipulated. The relevance of evaluations by others is supported by Heath and Tversky

[1] This chapter has been adapted from Trautmann, Vieider, & Wakker (2008a).

(1991), Fox & Tversky (1998), and Fox & Weber (2002), showing that ambiguity aversion increases with the perception that others are more competent and more knowledgeable. If people choose an ambiguous option and receive the bad outcome, then they fear criticisms by others. Such criticisms are easier to counter after a risky choice when a bad outcome is more easily explained as bad luck than after an ambiguous choice, which explains the enhanced ambiguity aversion. We will call such social effects *fear of negative evaluation* (*FNE*), borrowing a term from psychology (Watson & Friend, 1969). A detailed review of the literature on FNE for ambiguity will be presented in Section 2.2.

The studies of ambiguity aversion available in the literature so far could not determine the extent to which ambiguity aversion can exist beyond FNE. It was always clear what the preferred outcomes were and this information was public for the experimenter and others, so that subjects could always be criticized if they received a bad outcome. We introduce a design where preferences between outcomes are private information of the subjects that cannot be known to the experimenter or to other people unless the subject explicitly reveals it. Thus, we can completely control the presence or absence of FNE, and we can exactly determine the effect of the corresponding social factors on ambiguity aversion.

In our main experiment, the stimuli are two DVDs that on average are equally popular but between which most individuals have strong preferences. These preferences are unknown to others, in particular to the experimenter. Subjects choose between a risky prospect and an ambiguous prospect to win one of the two DVDs. With unobservable preferences the decision maker cannot be judged negatively by the experimenter or others because only the decision maker knows what the winning and what the losing outcome is. Remarkably, eliminating the possibility of evaluation by others makes ambiguity aversion disappear entirely in our experiment. Introducing the possibility of evaluation by letting subjects announce their preference between the DVDs before they make their choice is sufficient to make ambiguity aversion reemerge as strongly as commonly found. Thus, our finding adds to the aforementioned studies showing how important social factors are for ambiguity aversion.

To provide psychological background for our finding, we did another experiment with the classical Ellsberg urn and with traditional monetary outcomes, where we additionally measured subjects' sensitivity to FNE using Leary's (1983) scale. We indeed found a positive correlation between this scale and ambiguity aversion, confirming our interpretations.

Empirically, many economic phenomena deviating from traditional rational choice theory have been attributed to ambiguity aversion (Camerer & Weber, 1992; Gilboa, 2004; Mukerji & Tallon, 2001). A famous example is the home bias in consumption and financial investment (French & Poterba, 1991). Implications of our findings regarding FNE will be discussed in Section 2.5.

A research question resulting from our study is to what extent ambiguity aversion can at all exist in the absence of FNE, that is, to what extent it at all is a phenomenon of individual decision making. Most of the theories popular today use individual decision models to analyze ambiguity attitudes.

This paper proceeds as follows. The next section discusses the FNE hypothesis and its literature. Section 2.2 presents a replication of the Curley, Yates, & Abrams (1986) result and discusses the role of hypothetical choice for ambiguity. The main experiment and a discussion of its results are in Section 2.3. Section 2.4 considers the role of FNE as a personality trait for ambiguity aversion. Section 2.5 discusses theoretical and empirical implications. The last section concludes.

2.2 Literature on the Fear of Negative Evaluation

A central point in the explanation of ambiguity aversion concerns the perceived informational content of the outcome generating process. People shy away from processes about which they think they have insufficient information (Frisch & Baron, 1988). This happens in particular if an alternative process with a higher perceived informational content is available (Fox & Tversky, 1995; Fox & Weber, 2002). The effect appears to be particularly strong when somebody with a higher knowledge of the outcome generating process may serve as a comparison (Heath & Tversky, 1991; Taylor, 1995) or observes the decision (Chow & Sarin, 2002). In Ellsberg's (1961) example the effect leads to preference for the urn with known probability of winning, for which subjects feel more knowledgeable.

A preference for the more informative process may be due to fear of negative evaluation, which is driven by the expectation that one's actions or judgments may be difficult to justify in front of others. When the audience's views on an issue are unknown and no prior commitment to one course of action exists, people have been found to make the decision which they deem most easily justifiable to others rather than the one that is intrinsically

optimal (Lerner & Tetlock, 1999; Shafir et al., 1993; Simonson, 1989). In this way they minimize the risk of being judged negatively by others in their quality as decision makers.

Choosing the unfamiliar process entailed by the ambiguous urn may lead to embarrassment in case a losing outcome should obtain (Ellsberg, 1963; Fellner, 1961; Heath & Tversky, 1991; Roberts, 1963; Tetlock, 1991; Toda & Shuffold, 1965). The risky prospect is perceived as more justifiable than the ambiguous one because potentially available probabilistic information is missing from the ambiguous urn (Frisch & Baron, 1988). This is consistent with people's preference for betting on future events rather than on past events, given that information about past events is potentially available whereas the future yet has to materialize (Rothbart & Snyder, 1970; Brun & Teigen, 1990). It is also consistent with people's unwillingness to act on the basis of ambiguous information (van Dijk & Zeelenberg, 2003).

A decision based on more information is generally perceived as better (Tetlock & Boettger, 1989), and it has been shown that a risky prospect is generally considered preferable to an ambiguous one by a majority of people (Keren & Gerritsen, 1999). Kocher & Trautmann (2007) find that people correctly anticipate these negative attitudes towards ambiguity. If a bad outcome should result from a prospect about which an agent had comparatively little knowledge, her failure may be blamed on her incompetence or 'uninformed' choice (Baron & Hershey, 1988). A bad outcome resulting from a risky prospect, on the other hand, cannot be attributed to poor judgment. All possible information about the risky prospect was known, and a failure is simply bad luck (Heath & Tversky, 1991; Toda & Shuford, 1965).

FNE is difficult to eliminate completely, because people naturally expect to make their choices in a social context. This may explain the pervasiveness of ambiguity aversion. Curley, Yates, & Abrams (1986) found that letting more people observe the decision increased ambiguity aversion. To determine to what extent ambiguity aversion can exist beyond FNE, however, FNE should be completely eliminated. This will be achieved in our main experiment (Experiment 2). We first present an experiment that replicates the findings of Curley, Yates, & Abrams (1986) in a slightly different setup, and shows that FNE also can arise with hypothetical choice.

2.3 Experiment 1: Increasing Other-Evaluation

Unless stated otherwise, tests will be one-sided in this paper because there usually is a clear direction of prediction with a one-sided alternative hypothesis. All results in this paper based on *t*-tests do not change if we use non-parametric Fisher tests instead. So as to be comparable to many traditional studies, and to illustrate the role of FNE there, we use hypothetical payoffs in this first experiment. We will make the ambiguous option more desirable so as to make indifferent subjects choose this option. Questionnaires with a simple Ellsberg choice task were distributed to 41 students in a classroom setting. The students were asked to make a simple choice between two hypothetical prospects. One, the risky prospect, gave them a .5 chance to win €15 and nothing otherwise. The second, the ambiguous prospect, gave them an ambiguous chance to win €16 and nothing otherwise. The higher outcome for the ambiguous prospect makes it more desirable than the risky prospect. The choice task was described as a classical Ellsberg two-color bet in which subjects could first choose the color on which they wanted to bet and then the urn from which they wanted to draw (instructions in the appendix).

19 subjects obtained instructions to write down their name and email address prior to taking the decision, with the explanation that they may be contacted by a member of the economics department and asked for explanations regarding their choice (high other-evaluation). 22 subjects were not asked for any personal information before making their choice (low other-evaluation). Of the 19 subjects in the high other-evaluation condition, 15 chose the risky prospect (79%). Of the 22 subjects in the low other-evaluation condition, 11 chose the risky prospect (50%). The difference between the two treatments is significant ($t_{39} = -1.96$, $p = 0.029$).

In general, ambiguity aversion is high in both treatments, especially in view of the higher desirability of the ambiguous option. It should be noted that even with hypothetical questionnaires and low other-evaluation, FNE is still not completely eliminated because people still imagine making a decision in a social situation (announce a color, draw a chip, receive a prize). Imagined social encounters have been shown to be sufficient to induce embarrassment and FNE (Dahl et al., 2001; Miller & Leary, 1992). In this framework, the thought of losing in front of others with the ambiguous urn may thus be enough to produce ambiguity aversion in hypothetical studies as well. Thus, in no experiment on ambiguity

attitude in the literature known to us, FNE could be completely eliminated. In the next experiment we will completely eliminate FNE by explicitly making the subjects' preferences, and therefore the success of their decision, private information.

2.4 Experiment 2 (Main Experiment): Known versus Unknown Preferences

2.4.1 Experimental Design

Subjects. N = 140 subjects participated in individual sessions, 94 from the University of Amsterdam in the Netherlands and 46 from the Erasmus University Rotterdam in the Netherlands. Most students studied economics or business.

Payoffs. Subjects would always win one of two DVDs worth €7. They were not told the price of the DVDs. In two treatments subjects could earn up to €0.80 in addition to the DVD. All payoffs depended on subjects' choices and were paid for real.

The two DVDs were *About a Boy* and *Catch me if you can*. This pair was chosen in a preliminary survey among 50 students at the University of Maastricht because most students had a strong preference between them, but there was no difference in social desirability and no difference by gender, which made preferences unpredictable. On a scale from 3 (strongly prefer *About a boy*) to –3 (strongly prefer *Catch me if you can*), 70% of the subjects indicated a preference larger than or equal to 2 in absolute value. Twenty percent had a preference of 1 or –1, and 10% were indifferent. The mean absolute preference was 1.74. *Catch me if you can* was slightly preferred overall (mean = –0.82).

Procedure. We offered subjects a choice between a risky and an ambiguous lottery to win one of the two DVDs. A detailed description of the lottery mechanism is given below. We conducted four treatments that differed with respect to the experimenter's knowledge of the subjects' preference between the two DVDs and in whether there was a price difference between the risky and the ambiguous lottery (ambiguous card was 50 cent cheaper). Table 1 shows the organization of the four treatments. It also indicates the total number of subjects in each treatment and in brackets the number of students from the Erasmus University Rotterdam.

Treatment KS replicates the classic Ellsberg (1961) example with known preference and

a simple choice between the risky and the ambiguous lottery. At the beginning of the instructions subjects were asked to decide which movie they wanted to win and to write down the name of the movie in front of the experimenter. Treatment US introduces unobserved preferences between the two prizes, which is the essence of our design. It also requires a simple choice of the lottery. At the beginning of the instructions subjects were asked to decide which movie they wanted to win but not to tell the experimenter about their preference. The instructions can be found in the appendix. The remainder of the instructions was identical for both treatments.

	Same price	Ambiguous card 50c **C**heaper
Known Preference	Treatment KS (N=40(21))	Treatment KC (N=30(2))
Unknown Preference	Treatment US (N=40(20))	Treatment UC (N=30(3))

Table 2.1: Treatments

KS: Known preference with Same price; KC, US, and UC are defined similarly.

In Treatment KC we endowed subjects with €10 from which they had to buy either the risky lottery for €9.70 or the ambiguous lottery for €9.20, making the ambiguous choice 50 cent cheaper. They kept the rest of the money. Preferences were known (same instructions as in Treatment KS). In Treatment UC the ambiguous lottery was again 50 cent cheaper (same instructions here as in Treatment KC) and preferences were unknown (same instructions here as in Treatment US). These two treatments were included to measure the economic significance of the ambiguity aversion, and to exclude the possibility that many subjects had been indifferent between all prospects and had chosen on the basis of minor psychological cues.

After deciding which DVD they wanted to win and writing it down or keeping the information to themselves depending on the treatment, subjects chose the lottery (paying for it in Treatments KC and UC) and played it at once. They immediately received the DVD they won. They always received one DVD. Then they filled out a background questionnaire and were dismissed.

The questionnaire contained demographic background questions, asked about the ex-post preferred movie (in Treatments US and UC with ex-ante unknown preference), some questions about the subject's perception of the game, and the valuation difference between the two DVDs. The *valuation difference* was elicited as the subject's maximum willingness-to-

pay to exchange her less preferred DVD for her more preferred DVD, assuming she had won the less preferred one. It served again to verify that subjects had clear preferences between the DVDs.

Lottery Mechanism. The lotteries were conducted as follows. First, subjects assigned a symbol X to one DVD and a symbol O to the other at their own discretion. Then they chose to draw a card from one of two stacks, one representing the risky lottery and the other one the ambiguous lottery. Each stack consisted of about 50 cards. Each card had six numbers on its back, corresponding to the sides of a six-sided die. Next to each number there was either a symbol X or O. In the risky lottery the subjects knew that there were exactly three Xs and three Os on the back of the card. In the ambiguous lottery they did not know the number of Xs and Os, only that there were between zero and six Xs and a complementary number of Os.

Within each stack, cards differed with respect to the actual location of the symbols over the six numbers, and the cards of the ambiguous lottery differed also in the number of Xs and Os. After having drawn a card from either the risky stack with exactly three Xs and three Os, or from the ambiguous stack with an unknown composition of symbols according to their choice, they observed the back of their card and threw a six-sided die to determine which DVD they won. They always got one DVD.

The above mechanism was chosen to make the process as transparent to the subjects as possible and to make clear that the experimenter had no influence on the outcome of either lottery. The latter holds the more so as subjects attached the two symbols to the two DVDs at their own discretion.

2.4.2 Results

In an experiment where both prizes are DVDs, indifference between the two outcomes of the lottery is possible and did occur for some subjects (details on the measurement of indifference are given in the appendix). This section presents results including all data. Excluding indifferences from the analysis does not qualitatively change the results (see appendix).

The following table summarizes the results of the four treatments. It shows the percentage of subjects choosing the unambiguous prospect.

In Treatment KS significantly more than half of the subjects chose the risky prospect over

the ambiguous prospect and we thus find ambiguity aversion, in agreement with common findings. Making preference private information in Treatment US eliminates ambiguity aversion. We find that significantly less than half of the subjects chose the risky prospect. The difference in risky choices between Treatment KS and Treatment US is significant ($t_{78}=3.04$, $p=0.0016$).

	Same price	Ambiguous Card 50c Cheaper
Known Preference	Treatment KS 65% risky card (>50%, p=0.04)	Treatment KC 43% risky card (not significant)
Unknown Preference	Treatment US 33% risky card	Treatment UC 17% risky card

Table 2.2Percentage of Risky Choices

Tables are binomial. KS: Known preference with same price; KC, US, and UC are defined similarly.

In Treatment KC subjects were on average indifferent between the risky prospect and the ambiguous prospect plus 50 cent. The number of subjects who chose the risky prospect is not significantly different from 50%. In Treatment UC with a cheaper ambiguous card and unknown preference only 17% chose the risky prospect. The difference in risky choices between Treatment KC and Treatment UC is significant ($t_{58}=2.32$, $p=0.0121$).

The average valuation difference between the two DVDs was €2.19. There was no significant effect of known versus unknown preference on valuation differences.

Running a probit regression of the effect of unknown preference and price difference on the probability that subjects choose the risky prospect shows that the effect of known versus unknown preference is highly significant (regression I in Table 2.3).

The marginal effect of a (discrete) change from known to unknown preference is an approximate 31 percentage-point reduction in the probability of choosing the risky card. The marginal effect of a 50 cent price reduction for the ambiguous card is an approximate 20 percentage-point reduction in the probability of choosing the risky card. Regressions II and III in Table 3 show that the size and the significance of the effect of unknown preference is stable if we control for gender, age and valuation difference. Valuation differences do not affect ambiguity attitude. Regressions IV and V show that the interaction of unknown preference

and price and the interaction of indifference between the DVDs and price are insignificant.

Probit	Dependent variable: **choice of risky prospect**				
	I	II	III	IV	V
Unknown	–0.3091 (0.0798)**	–0.3204 (0.0806)**	–0.3218 (0.0924)**	–0.3401 (0.1046)**	–0.3160 (0.0808)**
price	–0.2019 (0.0832)*	–0.2077 (0.084)*	–0.1548 (0.1064)	–0.23 (0.1131)*	–0.1899 (0.0871)*
valuation difference (ex-post)			0.0254 (0.0215)		
unknown×price				0.0531 (0.184)	
indifferent×price					–0.1861 (0.2034)
controls (gender, age)		yes	yes	Yes	yes
# observations	140	139	110	139	139

Table 2.3: Probit Regression over all Four Treatments

The table reports marginal effects; standard errors in brackets; ×: interaction; * significant at 5% level, ** significant at 1% level, two-sided; one subject did not indicate age; 15 subjects in Treatments KC and UC had no valuation question.

Analyses of the questionnaire that the subjects filled out after the experiment corroborate our findings. Subjects in the unknown preference condition were asked ex-post about their preference between the two DVDs. Of those who had chosen the ambiguous prospect and were not indifferent between the DVDs, significantly more than half claimed to have won the DVD they preferred ($p = 0.04$, binomial test). No such effect was found for those who had chosen the risky prospect. See part a) of Table 2.4.

Subjects in the unknown preference condition were also asked ex-post whether the experimenter could have correctly guessed which movie they preferred. Those who had chosen the risky prospect were significantly more likely to think that the experimenter could have guessed their preference than those who had chosen the ambiguous prospect ($t_{66} = -2.33$,

p=0.0115). See part b) of Table 4.

a)

		won movie			
		ambiguous chosen		risky chosen	
		A	C	A	C
preferred	A	13	5	4	2
Movie	C	9	13	4	4

A: about a boy; C: catch me if you can.

b)

		ambiguous	risky
think that experimenter could guess preference	no	47	12
	yes	4	5

Table 2.4: Analysis of Ex-Post Questions

Entries refer to numbers of subjects.

2.3.3 Discussion of the Experimental Results of the Main Experiment

The Relevance of Fear of Negative Evaluation

The experimental results show that making preferences unknown to the experimenter leads to a 30 percentage-point reduction of ambiguity averse choices and makes ambiguity aversion disappear. In the current framework with valuation differences between the two prizes of about €2.20, this effect is stronger than the effect of making the ambiguous option 50 cent cheaper. This finding demonstrates that FNE has not only statistical but also economic significance.

In Treatment US we find a majority of subjects choosing the ambiguous option. With other-evaluation eliminated there may be no clear reason to choose either of the two stacks of cards and subjects may look for other minor psychological cues. Curiosity about the symbol distribution of the question mark card or utility of gambling may lead to the preference for the ambiguous prospect. In Treatments KC and UC, however, the price difference provides a clear cue for how to choose in the case of ambiguity neutrality. There is a significant effect of unknown preference in the comparison of these two treatments. Significantly more subjects

were willing to incur the monetary cost to avoid the ambiguous prospect if preferences were known than if they were not known to the experimenter. In Treatment KC with known preferences, a considerable proportion of the subjects were ready to pay 50 cents, or about 23% of the average valuation difference, in order to use the risky prospect instead of the ambiguous one. In Treatment UC with unknown preference the proportion of subjects ready to forego 50 cent for the risky prospect was considerably smaller.

The probit regression results show that the effect of making preferences private information is stable if we introduce other covariates. Including valuation differences, gender or age does not have an effect on the size or significance of the parameter for unknown preference.

Further evidence supporting the importance of FNE comes from the ex-post behavior of the subjects in the unknown preference condition. If they had chosen the ambiguous prospect they afterwards claimed that they were successful in winning their preferred DVD much more often than would be expected in a prospect with equal chances to win either DVD. This is not the case for those who had chosen the risky option. This finding suggests that losing after playing the ambiguous prospect is more embarrassing than after playing a fifty-fifty prospect. Kitayama *et al.* (2004) suggested that such ex-post justifications are motivated primarily by social evaluations. Such phenomena are known as cognitive bolstering in studies on the effects of accountability on decision making (Tetlock, 1983). The ex-post behavior, therefore, further supports the FNE hypothesis.

We also find that subjects who had chosen the risky option were more likely to think that the experimenter could have guessed their preference. This indicates once more that there is a relation between ambiguity avoidance and the presumed possibility to be evaluated by others, again supporting FNE.

Given the overall evidence for the importance of known versus unknown preference in our experiment and the ex-post behavioral differences between subjects who chose the ambiguous and the risky prospect, FNE appears to be a major cause of ambiguity aversion, and in our experiment it even seems to be a necessary condition. We next discuss some alternative explanations and argue that they are less convincing as an explanation of the data than FNE.

Alternative Explanations

Indifference. It could be suggested that subjects were mostly indifferent between prospects, and that majority choices resulted from minor psychological cues. This suggestion can be ruled out in our experiment because of the price differences between the Treatments KS and US versus KC and UC. In particular, indifference between the DVDs must imply a clear preference for the ambiguous prospect in the treatments where the latter is made cheaper.

It could be suggested that writing down the preferred DVD in Treatments KS and KC reinforced subjects' preference for that DVD. Then subjects in Treatments US and UC, who were not asked to write down their preference, might have had weaker preferences, closer to indifference. This could then have led to less ambiguity aversion. This suggestion can be ruled out for our experiment. First, we find that the valuation difference is not different for unknown or known preference, indicating no difference in strength of preference. Second, the insignificant effect of valuation differences in the probit indicates that there is no effect of strength of preference on ambiguity attitude. Also, inclusion of valuation differences does not affect the strong effect of unknown preference either in size or in significance. These results hold both for the data with and without indifferences.

Additional evidence against weaker preferences in the unknown preference treatments comes from the interaction of the preference and price manipulation (probit regression Table 3.IV). If subjects in the unknown preference conditions have weaker preferences between the DVDs than those in the known preference conditions, introducing the monetary incentive to choose the ambiguous prospect should have a stronger effect on choice in the unknown preference conditions. Subjects without a clear preference do not face a trade-off between ambiguity and money. The indifference explanation therefore predicts a negative effect of the interaction of 'unknown' and 'price' on the probability to choose the risky prospect in regression IV. We observe that the interaction effect is slightly positive and insignificant. As a control, including the interaction of indifferent subjects with 'price' in regression V, we do find a negative effect on the probability of the risky choice as expected. Owing to the small number of indifferent subjects the effect is not significant however. We conclude that the indifference hypotheses cannot hold.

Fear of Manipulation. Fear of manipulation can be a reason for subjects to avoid the

ambiguous prospect if they think the experimenter has an interest to reduce their probability of winning (Ellsberg, 1961; Viscusi & Magat, 1992; Zeckhauser, 1986). Morris (1997) suggested that experimental subjects mistakenly apply strategic considerations appropriate in the real world and reduce their willingness to bet against the experimenter if probabilities are ambiguous. In footnote 24 he wrote: "It would be interesting to test how sensitive Ellsberg-paradox-type phenomena are to varying emphasis in the experimental designs on the experimenter's incentives." This paper presents such a test. In our experiment subjects knew they would always win a DVD, and there was no gain from manipulation for the experimenter. The lottery mechanism provided subjects with a choice of how to attach symbols to DVDs and subjects always had to throw a die to determine the winning outcome. This made it very transparent that the experimenter had no interest and no possibility to influence the outcome.

Self-Evaluation. It might be argued that self-evaluation and anticipated cognitive dissonance or regret are the reason for the observed effect. In other words, the negative evaluation to be feared is not the evaluation by others but the evaluation by oneself. Self-evaluation was tested by Curley, Yates, & Abrams (1986) and was found not to be significant. In our experiment self-evaluation should be the same in the known and the unknown preference treatments. The subject always knows whether she lost or won the prospect and feedback was the same in all treatments. Hence, no difference between the treatments should then have been found. We conclude that self-evaluation cannot account for our findings.

2.4 Experiment 3: Ambiguity Aversion and Fear of Negative Evaluation as a Personality Trait

The results presented so far suggest that FNE makes subjects shy away from the ambiguous option when a risky option is available. This interpretation implies that people who are more sensitive to negative evaluation by others (Leary, 1983; Watson & Friend, 1969) should show stronger ambiguity aversion. In order to test this assumption, we invited 63 subjects for a paid experiment. In the first part of the study subjects filled out an unrelated questionnaire on health insurance and food safety for which they were paid €10. At the end of the questionnaire we included Leary's (1983) 12-item FNE scale.

After completion of the questionnaire the subjects were given an Ellsberg two-color choice task, which they would play for real money with the possibility of winning another €15 (instructions in the appendix). This choice task was framed as a second, distinct experiment. Subjects were invited in groups between 4 and 6 people, and were told that their decisions would be read aloud by the experimenter and played out in front of the group. Subjects made both a straight choice between the risky and the ambiguous option and gave their maximum willingness-to-pay (WTP) for both options.

Of the 63 subjects who took part in the experiment, 46 (73%) chose the risky urn, resulting in high ambiguity version (>50%, $p=0.0002$, binomial test). The median of the Leary FNE score was 37 on a scale from 12 (low) to 60 (high), Cronbach's alpha was 0.87. The average WTP difference between the risky and the ambiguous urn (WTP risky option minus WTP ambiguous option) was €2.11.

A probit regression of choices on the FNE score and demographic controls gives an average marginal increase in the probability of an ambiguity averse choice of 1.1 percentage points per unit of the score, which is marginally significant ($p=0.076$). A linear regression of the WTP difference on the FNE score and demographic controls gives an average increase of 7.3 Eurocent per unit of the score ($p=0.026$).

Table 5 illustrates the effect of the median split. The group that is more sensitive to negative evaluation with an average FNE score of 41.97 has an average WTP difference of €2.91. The less sensitive group with an average FNE score of 29 has an average WTP difference of €1.28. This difference is both statistically and economically significant for two prospects with expected value of €7.50 ($t_{61}=-3.04$, $p=0.0018$). The percentage of ambiguity averse choices is 10.4 percentage points higher in the high-FNE-sensitivity group, but this difference is not significant ($t_{61}=-0.92$, $p=0.1807$) .

	Number of observations	Average FNE score (min 12, max 60)	Average WTP difference	Percentage of ambiguity averse choices
Low FNE sensitivity	31	29	€1.28	67.7%
High FNE sensitivity	32	41.97	€2.91	78.1%

Table 2.5: Median Split

For the low FNE group we observe a moderate but positive WTP difference and a majority of ambiguity averse choices. With score 29 this group is still far from being immune to other-evaluation, however, and they were facing the possibility of missing the €15 prize in front of a group of other students. We would therefore expect FNE to matter for this group as well in the experiment. Taken together the results show that people who are less sensitive to evaluation by others are less ambiguity averse. This finding supports the FNE hypothesis.

2.5 Implications of FNE

Empirically, the role of FNE has implications for economic phenomena that are affected by ambiguity aversion. A well-known example is the home bias in consumption and finance (French & Poterba, 1991; Obstfeld & Rogoff, 2000): people tend to invest and trade more in their own country than should be expected given the gains from international diversification. Transportation costs, capital controls, or other tangible institutional factors cannot explain the empirically observed size of the home bias. A number of authors have argued that the home bias can be explained by ambiguity aversion (Huang, 2007; Kasa, 2000; Kilka & Weber, 2000; Uppal & Wang, 2003). Geographically remote trade or investment opportunities are more unfamiliar to people and involve more ambiguity than local opportunities. People feel less knowledgeable about the more distant option.

FNE theory predicts different long-term stability of the bias in trade than in finance. Success or failure in trade will remain highly observable in the future, and the home bias in entrepreneurial decisions is therefore likely to be persistent. On the other hand, the propagation of technology generates a more anonymous and impersonal decision environment in finance (online brokerage etc.). This is likely to reduce ambiguity aversion, and therefore the home bias, in the long run. The differential prediction for goods and equity markets is consistent with empirical evidence (Huang, 2007; Tesar & Werner, 1998). Additionally, we would expect that highly observable investments of otherwise large and sophisticated investors are more prone to home bias. Obstfeld & Rogoff (2000, p. 359) cite some evidence for this effect.

In our experiments we manipulated other-evaluation in simple laboratory decision tasks. It will be interesting to study the effect in naturally occurring environments. Online brokerage provides such an environment because it offers investors more anonymity than a traditional

human broker. Data on online investors suggest that they more heavily invest in growth stocks and high tech companies than do investors with traditional brokerage accounts (Barber & Odean, 2001; 2002). Such stocks are often associated with higher ambiguity in the finance literature. Konana & Balasubramanian (2005) find that many investors use both traditional and online brokerage accounts, and hold more speculative online portfolios. One of the investors they interviewed noted in the context of online trading (p.518): “I don’t have to explain why I want to buy the stock.”

2.6 Conclusion

Fear of negative evaluation (FNE) has been proposed in the literature as a factor that increases ambiguity aversion. It was, however, not known to what extent ambiguity aversion can exist beyond FNE. We have introduced an experimental design where preferences between outcomes are private information, so that others cannot judge on the goodness of decisions and outcomes. Thus, we can completely control the presence or absence of FNE and investigate its role. In our experiment, ambiguity aversion completely disappears if FNE disappears. It shows that FNE is more important than has commonly been thought and that it may even be necessary for ambiguity aversion to arise.

Chapter 3

Preference Reversals under Ambiguity[2]

3.1 Motivation

One of the greatest challenges for the classical paradigm of rational choice was generated by preference reversals, first found by Lichtenstein & Slovic (1971): strategically irrelevant details of framing can lead to a complete reversal of preference. Grether & Plott (1979) confirmed preference reversals while using real incentives and while removing many potential biases. Preference reversals raise the question what true preferences are, if they exist at all. This paper shows that preference reversals also occur in one of the most important domains of decision theory today: choice under uncertainty when probabilities are unknown (ambiguity).

The preference reversals that we find are of a fundamentally different nature than the preference reversals found in the literature on decision under risk and, in general, on choices between multiattribute objects. Those preference reversals have been found when the tradeoffs between different attributes (such as probability and gain in decision under risk) are different in different decision modes (Lichtenstein & Slovic, 1971; Tversky et al., 1988; Tversky *et al*., 1990). Our preference reversals concern a complete reversal of ordering within one attribute, i.e. the (likelihood) weighting of ambiguous events. It can be contrasted

[2] This chapter has been adapted from Trautmann, Vieider, & Wakker (2008b).

with preference reversals found for risky choice. There a more favorable gain is to be traded against a better probability. This trading is done differently in different contexts. In our design there will be only one fixed gain, so that the reversal must entirely take place within the likelihood attribute.

We investigate two commonly used formats for measuring ambiguity attitudes. The first is to offer subjects a straight choice between an ambiguous and a risky prospect, and the second is to elicit subjects' willingness to pay (WTP) for each of the prospects. We compare the two approaches in simple Ellsberg two-color problems. In four experiments, WTP generates a very strong ambiguity aversion, with almost no subject expressing higher WTP for the ambiguous urn than for the risky urn. Remarkably, however, this finding also holds for the subjects who in straight choice prefer the ambiguous urn. Hence, in this group the majority assigns a higher WTP to the not-chosen risky urn, entailing a preference reversal. There are virtually no reversed preference reversals of subjects choosing the risky urn but assigning a higher WTP to the ambiguous urn. This asymmetry between choice and WTP shows that either WTP finds too much ambiguity aversion, or straight choice finds too little (or both).

Using Sugden's (2003) and Schmidt, Starmer, & Sugden's (2005) generalization of prospect theory with a random reference point, we develop a quantitative model that explains the preference reversals found: a distorting loss aversion effect in willingness to pay leads to an overestimation of loss aversion there. In interviews conducted after one of the experiments, we made subjects aware of the preference reversals if occurring. No subject wanted to change behavior, suggesting that the preference reversals are not due to choice errors. The explanations that subjects gave suggested reference dependence and loss aversion in WTP, which led to our theoretical explanation. Differences between WTP measurements and another measurement, using certainty equivalents, further supports our theory that WTP overestimates ambiguity aversion. It does so not only for the subjects for whom it leads to a preference reversal but also for the other subjects.

It is well known that changes in psychological and informational circumstances can affect ambiguity attitudes. Examples of such circumstances are accountability (being evaluated by others or not; Curley, Yates, & Abrams 1986), relative competence (whether or not there are others knowing more; Fox & Weber, 2002; Heath & Tversky, 1991; Tversky & Fox, 1995), gain-loss framings (Du & Budescu, 2005), and order effects (Fox & Weber, 2002). Closer to

the preference reversals reported in our paper is a discovery by Fox & Tversky (1995), that ambiguity aversion is reduced if choice options are evaluated separately rather than jointly (Du & Budescu, 2005, Table 5; Fox & Weber, 2002). From this finding, preference reversals can be generated. The preference reversals reported in our paper are more fundamental. We compare two evaluation methods while keeping psychological and informational circumstances constant. For example, all evaluations will be joint and not separate. Thus, the preference reversals cannot be ascribed to changes in information or to extraneous framing effects. They must concern an intrinsic aspect of evaluation.

We present a theoretical model to explain the preference reversals found, based on loss aversion for willingness to pay. Recent studies demonstrating the importance of loss aversion are Fehr & Götte (2007) and Myagkov & Plott (1997). That loss aversion may not only be the strongest component of risk attitude, but also the most volatile, can be inferred from Plott & Zeiler (2005). That it plays an important role in willingness-to-pay questions was demonstrated by Morrison (1997).

There is much interest today in relations between risk/ambiguity attitudes and demographic variables. We find that females and older students are more risk averse and more ambiguity averse.

The organization of the paper is as follows. Section 2 presents our basic experiment, and our preference reversals. Section 3 presents a control experiment where no preference reversals are found, supporting our theoretical explanation. Whereas the WTP was not incentivized in our basic experiment so as to avoid income effects, it is incentivized in Section 4, showing that this aspect does not affect our findings. Section 5 considers a modification of the random lottery incentive system used and shows that this modification does not affect our basic finding either. Section 6 discusses the effect of gender and age for the pooled data of all three experiments. A theoretical explanation of our empirical findings is in Section 7. Section 8 discusses implications, and Section 9 concludes.

3.2 Experiment 1; Basic Experiment

3.2.1 Method

Subjects. N = 59 econometrics students participated in this experiment, carried out in a classroom.

Stimuli. At the beginning of the experiment, two urns were presented to the subjects, so that when evaluating one urn they knew about the existence of the other. The known urn[3] contained 20 red and 20 black balls and the unknown urn contained 40 red and black balls in an unknown proportion. Subjects would select a color at their discretion (red or black), announce their choice, and then make a simple Ellsberg choice. This choice was between betting on the color selected for the (ball to be drawn from the) known urn, or betting on the color selected from the unknown urn. Next they themselves randomly drew a ball from the urn chosen. If the drawn color matched the announced color they won €50; otherwise they won nothing.

Subjects were also asked to specify their maximum WTP for both urns (Appendix A). In this basic experiment, the WTP questions were hypothetical to prevent possible house money effects arising from the significant endowment that would have been necessary to enable subjects to pay for prospects with a prize of €50. Subjects first made their choice and then answered the WTP questions.

All choices and questions were on the same sheet of paper and could be answered immediately after each other, or in the order that the subject preferred. We also asked for the age and the gender of the subjects.

*Incentive*s. Two subjects were randomly selected and played for real. The subjects were paid according to their choices and could win up to €50 in cash.

Analysis. In this experiment as in the other experiments in this paper, usually a clear direction

[3] This term is used in this paper. In the experiment, we did not use this term. We used bags instead of urns, and the unknown bag was designated through its darker color without using the term "unknown." We did not use balls but chips, and the colors used were red and green instead of red and black. For consistency of terminology in the field, we use the same terms and colors in our paper as the original Ellsberg (1961) paper did.

of effects can be expected, because of which we use one-sided tests unless stated otherwise throughout this paper. Further, tests are *t*-tests unless stated otherwise. The abbreviation ns designates nonsignificance. The *WTP-implied choice* is the choice for the prospect with the higher WTP value. The *WTP difference* is the WTP for the risky prospect minus the WTP for the ambiguous prospect. It is an index of ambiguity aversion, and it is positive if and only if the WTP-implied choice is for the risky prospect.

3.2.2 Results

In straight choice, 22 of 59 chose ambiguous, which entails ambiguity aversion ($p < 0.05$, binomial). The following table shows the average WTP separately for subjects who chose ambiguous and those who chose risky.

	WTP risky	WTP ambiguous	WTP difference	t-test
Ambiguous chosen	12.25	9.50	2.75	$t_{21}=2.72$, $p < 0.01$
Risky chosen	11.64	6.27	5.37	$t_{36}=6.7$, $p < 0.01$
Two-sided *t*-test	$t_{57} = 0.33$, ns	$t_{57} = 2.14$, $p < 0.05$	$t_{57} = 2.01$, $p < 0.05$	

Table 3.6: Willingness to Pay in €

The subjects who chose the ambiguous prospect, the *ambiguous choosers* for short, are in general more risk seeking, although their WTP for the risky prospect is not significantly higher than for the risky choosers. Their WTP for the ambiguous prospects is obviously much higher than for the risky choosers. Risky choosers value the risky prospect on average €5.37 higher than the ambiguous one ($p < 0.01$). Surprisingly, ambiguous choosers also value the risky prospect €2.75 *higher* than the ambiguous one ($p < 0.01$), which entails the preference reversal. The following table gives frequencies of WTP-implied choices and straight choices.

WTP-implied / straight	Ambiguous	Indifferent	Risky	Binomial test
Ambiguous	2	9	11	$p = 0.01$
Risky	0	6	31	$p < 0.01$

Table 3.7: Frequencies of WTP-Implied Choice versus Straight Choices

Almost no WTP-implied choice is for ambiguous, not only for the risky choosers but also for the ambiguous choosers. Thus, for 11 of 59 subjects the WTP-implied choice and the straight choice are inconsistent. For all these subjects, the WTP-implied choice is for risky and the straight choice is for ambiguous. No reversed inconsistency was found. The number of the reversals found is large enough to depress the positive correlation between straight and implied choices to 0.34 (Spearman's ρ, $p < 0.05$ two-sided), excluding indifferences. We find significant WTP-implied ambiguity aversion for the straight ambiguity choosers ($p=0.01$, binomial). For subjects with straight choice of risky this is clearly true as well ($p < 0.01$, binomial).

3.2.3 Discussion.

We find ambiguity aversion in straight choice, but still 22 out of 59 subjects choose ambiguous. For WTP there is considerably more ambiguity aversion and virtually everyone prefers ambiguous, leading to preference reversals for 11 subjects. Only 2 ambiguous choosers also have an ambiguous WTP-implied choice. This result is particularly striking because straight choice and WTP had to be made just one after the other on the same sheet. No preference reversal occurs for the risky choosers.

An explanation of the preference reversal found can be that during their WTP task subjects take the risky prospect as a reference point for their valuation of the ambiguous prospect. Valuating the risky prospect is comparatively easy so that it is a natural starting point. Then, because of loss aversion, the cons of the ambiguous prospect relative to the risky prospect weigh more heavily than the pros, leading to a systematic dislike of the ambiguous prospect. Section 7 gives a more detailed explanation. Experiment 2 serves to test for this explanation because there no similar choice of reference point is plausible.

An alternative explanation instead of genuine preference reversal could be suggested to explain our data, an error-conjecture. The *error conjecture* entails that WTP best measures true preferences, which supposedly are almost unanimously ambiguity averse, and that straight choice is simply subject to more errors. The 11 risky WTP-implied preferences would then be errors (occurring less frequently for WTP but still occurring) and they would not entail genuine preference reversals. One argument against this hypothesis is that straight choices constitute the simplest value-elicitations conceivable, and that the literature gives no reason to suppose that straight choice is more prone to error than WTP. This holds the more so as straight choices were carried out with real incentives. Other arguments against the error hypothesis are provided in Experiments 2 and 4 that test and reject the hypothesis.

The preference reversal in Experiment 1 were observed without incentivized WTP and in a classroom setting. WTP with real incentives may differ from hypothetical WTP (Cummins, Harrison, & Rutström, 1995; Hogarth & Einhorn, 1990). To test the stability of our finding in the presence of monetary incentives and in controlled circumstances in a laboratory we conducted Experiments 3 and 4.

3.3 Experiment 2; Certainty Equivalents from Choices to Control for Loss Aversion

Experiment 2 tests a loss-aversion explanation (with details in Section 7) of the preference reversal found in the basic experiment. It also tests the error conjecture described in the preceding section. It further shows that the WTP bias detected by the preference reversal holds in general, that is, also for subjects for whom it does not lead to a preference reversal.

3.3.1 Method

Subjects. N = 79 subjects participated as in Experiment 1.

Stimuli. All stimuli were the same as in Experiment 1, starting with a simple Ellsberg choice, with one modification. Subjects were not asked to give a WTP judgment. Instead, they were asked to make 9 choices between playing the risky prospect and receiving a sure amount, and 9 choices between playing the ambiguous prospect and receiving a sure amount (Appendix

A). Thus, there was no direct comparison of the risky and ambiguous prospects' values. The choices served to elicit the subjects' certainty equivalents, as explained later.

Incentives. The prizes were as in Experiment 1. Subjects first made all 19 decisions. Then two subjects were selected randomly. For both, one of their choices was randomly selected to be played for real by them throwing a 20-sided die, where the straight choice had probability 2/20 and each of the 18 CE choices had probability 1/20.

Analysis. For each prospect, the CE was the midpoint of the two sure amounts for which the subject switched from preferring the prospect to preferring the sure money. All subjects were consistent in the sense of specifying a unique switching point. The *CE-implied choice* is the choice for the prospect with the higher CE value. The *CE difference* is the CE of the risky prospect minus the CE of the ambiguous prospect.

3.3.2 Results

In straight choice, 26 of 79 chose ambiguous, which entails ambiguity aversion ($p < 0.01$, binomial). The following table gives average CE values.

	CE risky	CE ambiguous	CE difference	t-test
Ambiguous chosen	16.73	17.60	–0.86	t_{25}=1.61, p=0.06
Risky chosen	14.84	11.90	2.94	t_{52}=4.84, $p < 0.01$
Two-sided *t*-test	t_{77} = 1.53, ns	t_{77} = 4.75, $p < 0.01$	t_{77} = 4.02, $p =< 0.01$	

Table 3.8: CEs in €

The ambiguous choosers are again more risk seeking with higher CE values. Their CE for the risky prospect is not significantly higher than for the risky choosers, but is very significantly higher for the ambiguous prospect. Now, however, the ambiguous choosers evaluate the ambiguous prospect higher, reaching marginal significance and entailing choice consistency. The following table compares the CE-implied choices with straight choices.

CE-implied straight	Ambiguous	Indifferent	Risky	Binomial test
Ambiguous	8	16	2	$p = 0.05$
Risky	4	18	31	$p < 0.01$

Table 3.9: Frequencies of CE-Implied Choice versus Straight Choices

There is considerable consistency between CE-implied preferences and straight preferences, with only few and insignificant inconsistencies. Hence, we do not find preference reversals here. There is a strong positive correlation of 0.64 between straight and implied choices (Spearman's ρ, $p < 0.01$ two-sided), excluding indifferences. We reject the hypothesis of CE-implied ambiguous preference for the risky straight choosers ($p < 0.01$, binomial), and we reject the hypothesis of CE-implied risky preference for the ambiguous straight choosers ($p = 0.05$). Subjects who are indifferent in the CE task distribute evenly between risky and ambiguous straight choice.

Results Comparing Experiments 1 and 2. For both prospects, CE values in Experiment 2 are significantly higher than the WTP values in Experiment 1 ($p < 0.01$). The CE differences in Experiment 2 are smaller than the WTP differences in Experiment 1 ($p < 0.01$), suggesting smaller ambiguity aversion in Experiment 2.

3.3.3 Discussion

The results of Experiment 2 are in many respects similar to those in Experiment 1. Only, the CE values are generally higher than the WTP values whereas the differences between risky and ambiguous are smaller. They are so both for the ambiguous choosers, who exhibit preference reversals, but are so also for risky choosers. This suggests that there may be a general overestimation of ambiguity aversion in WTP. Because the CE differences are negative for ambiguous choosers, no preference reversals are found here. The error-conjecture that ambiguous straight choice be due to error is rejected because there is significant CE-implied ambiguous choice among the ambiguous straight choosers.

3.4 Experiment 3; Real Incentives for WTP

3.4.1 Method

N = 74 subjects participated similarly as in Experiment 1. Everything else was identical to Experiment 1, except the incentives.

Incentives. At the end of the experiment, four subjects were randomly selected for real play. They were endowed with €30. Then a die was thrown to determine whether a subject played his or her straight choice to win €50, or would play the Becker-DeGroot-Marschak (1964) (*BDM*) mechanism (both events had equal probability). In the latter case, the die was thrown again to determine which prospect was sold (both prospects had an equal chance to be sold). Then, following the BDM mechanism, we randomly chose a prize between €0 and €50. If the random prize was below the expressed WTP, the subject paid the random prize to receive the prospect considered and played this prospect for real. If the random prize exceeded the expressed WTP, no further transaction was carried out and the subject kept the endowment (Appendix B).

3.4.2 Results

In straight choice, 15 of 74 chose ambiguous, which entails ambiguity aversion ($p < 0.01$, binomial). The following table gives average WTP.

	WTP risky	WTP ambiguous	WTP difference	*t*-test
Ambiguous chosen	13.44	11.21	2.23	t_{14}=2.58, p = 0.01
Risky chosen	13.46	7.14	6.31	t_{58}=6.21, p < 0.01
Two-sided *t*-test	t_{72} = 0.01, ns	t_{72} = 1.99, p = 0.05	t_{72} = 1.97, p = 0.05	

Table 3.10: Willingness to Pay (BDM) in €

The WTPs for both groups and both prospects are slightly (but not significantly) higher than the WTPs in experiment 1 ($p>0.5$, two-sided). Also the WTP differences are not significantly different from Experiment 1 ($p>0.5$, two-sided). All patterns of Experiment 1 are confirmed. In particular, the ambiguous choosers have a higher WTP for the risky prospect. The

following table compares choices implied by WTP with subjects' straight choices.

WTP-implied straight	Ambiguous	Indifferent	Risky	Binomial test
Ambiguous	0	9	6	$p < 0.05$
Risky	1	13	45	$p < 0.01$

Table 3.11: Frequencies of WTP-Implied Choice (BDM) versus Straight Choices

Here 6 out of 15 ambiguous choosers were inconsistent in having a WTP-implied preference for risky. All other ambiguous choosers exhibited WTP-implied indifference, and not even one of them had a WTP-implied preference for ambiguous. Of 59 risky choosers 1 was inconsistent and had a WTP-implied preference for ambiguous. Clearly, there is no positive correlation between straight and implied choices (Spearman's $\rho = -0.051$, ns two-sided) excluding indifferences. We find significant WTP-implied ambiguity aversion for the straight ambiguity choosers ($p < 0.05$, binomial). The same holds for the risky choosers ($p < 0.01$, binomial).

The distribution of bids in experiment 3 is very similar to that in experiment 1. There is no systematic over- or underbidding (WTP > 25 or WTP = 0) that would suggest that subjects misunderstood the BDM mechanism. The subjects who reversed their preference did so over a large range of buying prices[4].

3.4.3 Discussion

With all parts of the experiment, including WTP, incentivized, this experiment confirms the findings of Experiment 1.

3.5 Experiment 4; Real Incentives for Each Subject in the Laboratory

3.5.1 Method

This experiment was identical to Experiment 1 except for the following aspects.

[4] The subjects who reversed their preference from ambiguous in choice to risky in valuation had the following pairs of WTPs (WTP risky/WTP ambiguous): (25/20), (20/15), (20/10), (12.5/5), (10/5), and (3/2).

Subjects. N = 63 students participated in groups of 4 to 6 in the laboratory. Now about 25% were from other fields than economics.

Incentives. The experiment was part of a larger session with an unrelated task. Every subject would receive €10 from the other task and up to €15 from the Ellsberg task. Each subject played his or her choice for real. Subjects were paid in cash. Now the nonzero prize was €15 instead of €50.

3.5.2 Results

In straight choice, 17 of 63 chose ambiguous, which entails ambiguity aversion ($p < 0.01$). The following table gives average WTP values. Note that the prize of the prospects was €15 now.

	WTP risky	WTP ambiguous	WTP difference	*t*-test
Ambiguous chosen	5.63	4.65	0.99	t_{16}=1.56,p = 0.07
Risky chosen	5.23	2.71	2.53	t_{45}=8.53,p < 0.01
Two-sided *t*-test	t_{61} = 0.53, ns	t_{61} = 2.90, p < 0.01	t_{61} = 2.49, p = 0.01	

Table 3.12: Willingness to Pay in € when the Nonzero Prize is €15

The pattern is identical to previous results. The following table compares WTP-implied choices with straight choices.

WTP-implied straight	Ambiguous	Indifferent	Risky	Binomial test
Ambiguous	2	6	9	p < 0.05
Risky	0	6	40	p < 0.01

Table 3.13: Frequencies of WTP-Implied Choice (Lab) versus Straight Choices

The positive correlation between straight and implied choices is 0.39 (Spearman's ρ, $p < 0.01$ two-sided), excluding indifferences. The hypothesis of WTP-implied ambiguous preference can be rejected for the ambiguous straight choosers ($p < 0.05$, binomial). The same holds for the risky straight choosers ($p < 0.01$, binomial). After the experiment we approached the 9 subjects who exhibited inconsistencies, pointing out the inconsistency and asking them if they wanted to change any experimental choice. None of them wanted to change a choice and they confirmed that they preferred to take the ambiguous prospect in a straight choice but nevertheless would not be willing to pay as much for this prospect as they did for the risky one.

3.5.3 Discussion

This experiment replicates the findings of experiment 1 in the laboratory and with real incentives for every subject. This shows that the preference reversal is not due to low motivation in the classroom. The interviews reject the error-conjecture that suggested that ambiguous straight choice be due to error.

3.6 Pooled Data: Gender and Age Effects

The four experiments conducted for this study provide comparable choice and valuation data and can therefore be pooled into a large data set with 275 subjects. This allows us to consider the effects of age and gender. There is much interest into the role of such personal characteristics (Barsky et al. 1997; Booij & van de Kuilen 2006; Cohen & Einav 2007; Donkers et al. 2001; Hartog, Ferrer, & Jonker 2002; Schubert et al. 1999).

Table 9 shows the valuations for risky and ambiguous prospects, valuation differences, and actual choices, separated by age and gender. Valuations are calculated here as the percentage of the monetary prize of the prospect. For example, a WTP of €15 for an ambiguous prospect with a prize of €50 gives a percentage valuation of 30.00.

The table shows that females hold significantly lower valuations for both the risky and the ambiguous prospect than do males. Their valuation differences are not significantly smaller though. Our finding is consistent with the evidence in the literature that women are more risk averse than men (Cohen & Einav, 2007). Booij & van de Kuilen (2006) argued that

females' stronger risk aversion can be explained by stronger loss aversion in a prospect theory framework. The last column in the table shows that women are significantly more ambiguity averse than men in a straight choice between the prospects. This has also been found by Schubert et al. (2000) for the gain domain.

Although there is relatively little variation in age in our sample, we find that young students give lower valuations for both the risky and the ambiguous prospect, but are not more ambiguity averse than older students. This is confirmed by correlational analysis, where age has a positive correlation with risky evaluation ($\rho = 0.15$, $t(273) = 2.55$, $p = 0.01$) and with the ambiguous evaluation ($\rho = 0.11$, $t(273) = 1.86$, $p = 0.06$) but not with value difference ($\rho = 0.06$, $t(273) = 0.97$, ns) or with the percentage of straight risky choices ($\rho = -0.07$, $t(273) = 1.10$, ns).

	Percentage Valuation of Risky Prospect	Percentage Valuation of Ambiguous Prospect	Valuation Difference	Choice of Risky prospect (%)
Females (N=79)	24.77	14.64	10.13	79.7
Males (N = 196)	31.23	22.64	8.59	63.3
Two-sided t-test	$p < 0.01$	$p < 0.01$	ns	$p < 0.05$
Age≤19 (N=153)	26.48	18.39	8.09	73.9
Age>19 (N=122)	33.00	22.79	10.21	67.2
Two-sided t-test	$p < 0.01$	$p = 0.01$	ns	ns

Table 3.14: Age and Gender Effects in the Pooled Data

Age ranged from 17 to 31 with median age 19. There is no correlation between age and gender in the data.

3.7 Modeling Preference Reversals through Loss Aversion in Comparative WTP

Butler & Loomes (2007) wrote about preference reversals that they are " … easy to produce, but much harder to explain." This section presents a theoretical deterministic model that explains our data, building upon theories that have been employed to explain preference reversals under risk (Schmidt et al., 2005; Sugden, 2003). Incorporating imprecision of preference is a topic for future research. That the preference reversals found here cannot be ascribed exclusively to error was demonstrated in Experiments 2 and 4.

3.7.1 Definitions

Let f and g be uncertain prospects over monetary *outcomes* x, and let a constant prospect be denoted by its outcome. We assume that preferences are reference dependent, and that reference points can depend on states of nature, following Schmidt, Starmer, & Sugden (2005). The latter paper extended Sugden (2003) to incorporate probability weighting. We extend this model to uncertainty with unknown probabilities.

Let $V(f|g)$ denote the value of prospect f with prospect g as reference point. This value will be based on: (a) an event-weighting function W; (b) a utility function $U(x|r)$ of outcome x if the reference outcome on the relevant event is r, where U satisfies $U(r|r) = 0$ for all r; and (c) a loss aversion parameter λ, with furter details provided below. Sugden (2003) derived the case where $U(x|r)$ is of the form $\varphi(U(x) - U(r))$. Our analysis can be seen to agree with the multiple priors model, with the weighting function W assigning minimal probabilities to events (Gilboa & Schmeidler, 1989; Mukerji, 1998).

Let ρ represent the *risky prospect* and α the *ambiguous prospect* of guessing a color drawn from an urn with a known and unknown proportion of black and red balls, respectively. We consider four atomic events ("states of nature") that combine results of (potential) drawings from urns—a black ball is/would be extracted from both the risky and the ambiguous urn (*Event 1;* E_1); a black ball from the risky urn and a red one from the ambiguous urn (*Event 2;* E_2); a red ball from the risky urn and a black ball from the ambiguous urn (*Event 3;* E_3); a red ball from both the risky and the ambiguous urn (*Event 4;* E_4). Let us assume that the announced color to be gambled on is black; for red the problem is

exactly equivalent. Let *x* be the prize to be won in case the announced color matches the color of the ball extracted from the chosen urn.

3.7.2 Straight Choice

We first consider straight choice. In later analyses we will consider subtracting a constant c from all paymnents, and for convenience we have written c already in Table 10. For the current analysis, c can be ignored, i.e., c=0. The following payoffs result under the four events.

	E_1 ($B_R B_A$)	E_2 ($B_R R_A$)	E_3 ($R_R B_A$)	E_4 ($R_R R_A$)
α	x−c	−c	x−c	−c
ρ	x−c	x−c	−c	−c

Table 3.15: Payoffs for the Risky and the Ambiguous Prospect

Because $P(E_1 \cup E_2) = 0.5$, the event $E_1 \cup E_2$ is unambiguous and ρ is risky. $P(E_1 \cup E_3)$ is unknown so that event $E_1 \cup E_3$, and α, are ambiguous. The reference point at the time of making the choice can be assumed to be zero (previous wealth). Then

$$V(\alpha|0) = W(E_1 \cup E_3)U(x|0) \qquad (1)$$

and

$$V(\rho|0) = W(E_1 \cup E_2)U(x|0) \qquad (2)$$

where we dropped terms with $U(0|0) = 0$.[5] In Ellsberg-type choice tasks a minority of individuals prefer the ambiguous prospect over the risky prospect, with $V(\alpha|0) > V(\rho|0)$. Then event $E_1 \cup E_3$, the receipt of the good outcome x under α, receives more weight than event $E_1 \cup E_2$, the receipt of the good outcome x under ρ:

$$\text{Ambiguity seeking in straight choice} \Leftrightarrow W(E_1 \cup E_3) > W(E_1 \cup E_2). \qquad (3)$$

Most people exhibit the reversed inequality of ambiguity aversion with more weight for the

[5] Thus, we need not specify the (rank-dependent) weights of the corresponding events in our analysis.

known-probability event $E_1 \cup E_2$, but nevertheless several people exhibit ambiguity seeking as in Eq. 3. Note that each single event $E_1,\ldots,E_4$ will be weighted the same because each has the same perceived likelihood and the same perceived ambiguity, because of symmetry of colors. The unambiguity of $E_1 \cup E_2$ versus the ambiguity of $E_1 \cup E_3$, and the different weightings of these events depending on ambiguity attitudes, are generated through the unions with E_1, with different likelihood interactions between E_3 and E_1 than between E_2 and E_1.

3.7.3 Willingness to Pay and Loss Aversion

We next turn to the WTP evaluation task. Consider Table 10 with a value c that may be positive. Such cases are relevant for WTP. We will take the WTP of ρ as given and equal to c without need to analyze how c has been determined. In particular, we need not specify the reference prospect relevant for the WTP of ρ. We now show that the value of the upper row regarding α is lower, which will imply that its WTP must be smaller than c. The following analysis is in fact valid for any value of c. In particular, it is conceivable that some subjects, when evaluating the ambiguous prospect α for WTP, do not incorporate the values of c as should be under rational choice theories, but ignore c ($c = 0$) in their mind, then come up with a lower preference value of α than of ρ along the lines analyzed hereafter, and then derive a smaller WTP value for α from that in intuitive manners.

Because subjects have to come up with a value for the two prospects, it is natural to start from the one for which probabilities are given and for which it is thus easier to produce a quantitative evaluation. This way of thinking for WTP is natural irrespective of the actual straight choice made between these prospects. It was also suggested by the interviews we conducted after Experiment 4 with subjects who committed preference reversals. For their WTP evaluation of α they would refer to the WTP of ρ and then would emphasize the drawbacks of α relative to ρ.

We will, therefore, assume that the risky prospect ρ in the lower row in Table 10 is the reference point for the determination of the WTP for α. Consider the prospect in the upper row of Table 10, α with the WTP of ρ, c, subtracted. According to the theory of Schmidt, Starmer, & Sugden (2005), events E_1 and E_4 are taken as neutral (utility 0) and they do not contribute to the evaluation, which is why they do not appear in the equation below. Thus, we

need not specify their rank-dependent weights. E_2 is now a loss event and E_3 a gain event. Although the nonadditive decision weights of loss events can in principle be different than for gain events, many studies do not distinguish between such events, and empirical studies have not found big differences so far (Tversky & Kahneman, 1992). We will therefore simplify the analysis and use the same weighting function for losses as for gains. For ambiguity aversion we have to establish negativity of the following evaluation

$$\text{Ambiguity aversion in WTP} \iff W(E_3)U(x-c|-c) + \lambda W(E_2)U(-c|x-c) < 0. \quad (4)$$

Thus, losses are extra overweighted through the loss aversion parameter $\lambda>1$. It is plausible that utility U is approximately linear for the moderate stakes considered here, so that $U(-c|x-c) \approx -U(x-c|-c)$. For the data of Tversky & Kahneman (1992), utility for gains and losses was found to have the same power (0.88) so that $U(-c|x-c) = -U(x-c|-c)$ held there exactly and even for large outcomes. A similar assumption was central in Fishburn & LaValle (1988). We divide by $U(-c|x-c)$, giving

$$\text{Ambiguity aversion in WTP} \iff W(E_3) - \lambda W(E_2) < 0. \quad (5)$$

In the above analysis, given symmetry of colors, events E_2 and E_3 will have similar perceived likelihood and ambiguity. In Eqs. 4 and 5, they are weighted in isolation and not when joint with another event. Hence it is plausible that they have the same weights, $W(E_2) = W(E_3)$. Then Eq. 5 reduces to:

$$\text{Ambiguity aversion in WTP} \iff 1 < \lambda. \quad (6)$$

This inequality is exactly what defines loss aversion. Because only single events play a role in Eq. 5 and no unions as in Eq. 3, ambiguity attitudes did not play a role in establishing Eq. 6. By this equation we can expect a higher WTP of the risky prospect as soon as loss aversion holds ($\lambda > 1$), irrespective of ambiguity attitude. Empirical studies have suggested that loss aversion is very widespread and strong. Hence virtually all subjects will evaluate the risky prospect higher than the ambiguous prospect, in agreement with our data.

The conclusion just established, with WTP for the ambiguous prospect entirely driven by loss aversion with no role for attitude towards ambiguity, has been derived under the theory of Schmidt, Starmer, & Sugden (2005). This result should not be expected to apply

exactly to all subjects. There will be many subjects who entirely, or partly, are driven by other considerations in which also ambiguity aversion affects a negative WTP of α. We believe, however, that the phenomenon just established is prevailing and that much of the ambiguity aversion ascribed to WTP observations is in fact explained by loss aversion.

3.7.4 Discussion

Summarizing, prospect theory predicts that our preference reversals appear whenever a subject is ambiguity seeking and loss averse. Given that there is a nonnegligible minority of subjects exhibiting ambiguity seeking and given that virtually all of them will be loss averse, preference reversals as we found can be expected to arise for a nonnegligible minority indeed. Reversed preference reversals would arise among those subjects who are ambiguity averse and who are not loss averse but rather the opposite, gain seeking ($\lambda < 1$). In view of the strength of loss aversion this can be expected to be a rare phenomenon, as was confirmed by our data.

Systematic preference reversals as modeled above cannot be expected to occur for CE valuations. Whereas for the WTP assessment of the ambiguous prospect the subjects will resort for reference to the risky prospect that is easier to evaluate, for the CE measurements the subjects are involved in comparing the ambiguous prospect to a sure outcome for the purpose of choosing, which will not encourage them to search for other anchors. The CE tasks are similar to the straight choices and can be expected to generate similar weightings and perceptions of reference points. That the differences between ambiguous and risky CE evaluations are smaller than the corresponding WTP differences for both ambiguous and risky choosers further supports the theory of this section. It also underscores that the bias for WTP that we discovered at first through the observed preference reversals does not apply only to the subjects, a minority, for whom this preference reversal arises, but that it concerns all subjects.

An interesting question is what happens if the reference point is changed extraneously. Roca, Hogarth, & Maule (2006) found that when subjects are endowed with the ambiguous prospect they indeed become reluctant to switch to the risky prospect if offered such an opportunity. The authors explain such reluctance through loss aversion where the ambiguous

prospect constitutes the reference prospect. This finding supports our theory.

Many studies have used willingness to accept (WTA) to measure ambiguity attitudes. Here subjects are first endowed with a prospect and are then asked for how much money they are willing to sell it. This procedure will encourage some subjects, as in the study of Roca, Hogarth, & Maule (2006), to take the ambiguous prospect as reference point when determining its WTA. Other subjects may, however, take the risky prospect as reference point, and then an analysis as in this section will apply. Therefore, it can be expected that for WTA there will be biases as in our WTP but possibly to a less pronounced degree. Eisenberger & Weber (1995) found similar ambiguity aversion for WTA as for WTP.

Fox & Weber (2002) considered evaluations of ambiguous prospect both if preceded by risky prospects and if not. In the former case, their evaluations were considerable lower than in the latter case. This finding is consistent with our analysis based on loss aversion.

3.8 General Discussion

It is common in individual choice experiments not to pay for every choice made because this would generate distorting income effects. Hence, random payment is used (Harrison et al., 2002; Holt & Laury, 2002; Myagkov & Plott, 1997). Its equivalence to a single and payoff relevant decision task has been empirically tested and confirmed (Hey & Lee, 2005; Starmer & Sugden, 1991). Some papers explicitly tested whether it matters if for each subject one choice is played for real as in our experiment 4, or if this is done only for some randomly selected subjects as in our other experiments (Armantier 2006, Harrison et al. 2007). These studies found no difference, and our study confirms this finding.

We have found preference reversals in choice under ambiguity. The reversals are not due to errors, as appeared from Experiment 2 where straight choice and CE-implied choice were consistent, and from the interviews after Experiment 4. They are neither due to extraneous manipulations in framing. All evaluations and choices were joint in the sense that the subjects were first presented with all choice options and all choices to be made before they made their first choice. Further, the subjects could always carry out all choices in any order they liked and compare them all with each other; all choices were on one page. Thus, there was no psychological or informational difference between the different choice situations considered.

As preference reversals have had far-reaching implications for the domains where they

have been discovered, their discovery in ambiguous choice sheds new light on previous findings. Many studies in the literature have measured ambiguity aversion through WTP, where ambiguity aversion will be strongest. Our empirical findings and theoretical model suggest that this ambiguity aversion may in fact be driven primarily by loss aversion with reference points following Sugden (2003) and Schmidt et al. (2005). That the WTP differences exceed the CE differences for all groups suggests that the WTP bias affects all subjects, also the straight-risky choosers for whom the bias could not lead to a preference reversal. Binary choice may give more unbiased assessments of ambiguity aversion. There ambiguity aversion still is a pronounced phenomenon.

The occurrence of preference reversals when two lotteries have to be evaluated jointly and the absence of such reversals when the lotteries are compared to different options, such as given certain amounts of money, support theories of comparative ignorance (Fox & Tversky, 1995; Fox & Weber, 2002). Fox & Tversky (1995) similarly found strong ambiguity aversion under joint evaluation, with ambiguity aversion even disappearing under separate evaluation. Du & Budescu (2005, Table 5) replicated this result in a finance setting and investigated a number of other factors influencing ambiguity attitudes. It will be useful to develop a taxonomy of situations that generate more or less ambiguity aversion, and our paper has contributed here.

3.9 Conclusion

Preference reversals have affected many domains in decision theory. We found that they also affect choice under ambiguity, even if psychological and informational circumstances are kept fixed. All results were obtained within subjects, with the willingness to pay task on the same sheet as the choice task. The results are stable under real incentives, different experimental conditions, and concern deliberate choices that were not made by mistake. Our results support recent theories explaining preference reversals through reference dependence and loss aversion for willingness to pay (Schmidt et al., 2005; Sugden, 2003). Our study suggests that the often used willingness to pay measurements overestimate ambiguity aversion.

Chapter 4

The Effect of Accountability on Risk Attitude: Probability Weighting, Utility Curvature, and Loss Aversion

4.1 Motivation

Social factors affect many types of human behavior. Possible evaluation by others has been found to be relevant for racist attitudes (Warner & DeFleur, 1969) and for alleged aggression differences by gender (Lightdale & Prentice, 1994). According to Tetlock (1985), the potential evaluation by others is one of the most important factors influencing human decision making processes. Curley, Yates & Abrams (1986) found that other-evaluation can increase ambiguity aversion when several people observe the decision maker's choice. Trautmann, Vieider, & Wakker (2007a) found that eliminating the possibility of other-evaluation by making the subject's preferences her own private knowledge causes ambiguity aversion to disappear. McFadden (2006) calls for a more general role of social influences in the explanation of economic behavior.

A substantial literature in social psychology shows the effects that *accountability*, the expectation by a decision maker that she may be called upon to justify her behavior in front of others, has on human decision making processes. Accountability in front of an audience with

unknown views generally results in more cognitive effort. This phenomenon has been called pre-emptive self-criticism (Lerner & Tetlock, 1999; Tetlock, 1983; Tetlock & Kim, 1987), consisting in more options being considered more in depth, thereby anticipating possible criticisms others might bring against one's choice.

Accountability to an unknown audience has been found to lead to less biased decisions in cases where the normatively correct decision was either known by the subjects, or could be arrived at by higher cognitive effort (Simonson & Nye, 1992). When on the other hand no solution is easily arrived at, people tend to choose the option that appears more easily justifiable. This may be explained by the fact that people have been found to often rely on reasons instead of indices such as expected value when making choices (Shafir et al., 1993). When called upon to make a risky choice they may need to justify in front of an audience with unknown views, we would thus expect that the decision maker picks the decision which she will deem most easily justifiable (Simonson, 1989).

An additional complication encountered in the realm of choices under risk lies in the different components of risk attitude. Indeed, risk attitude is commonly taken to consist of three elements—curvature of utility, probability weighting, and loss aversion (Kahneman & Tversky, 1979; Tversky & Kahneman, 1992). The aim of this study is to investigate whether accountability may have an effect on choices under risk, and how such an effect may influence the different components of risk attitude. Whereas no effect of accountability is found for utility curvature or probability weighting, accountability is found to strongly influence loss aversion.

This finding is explained recurring to *Dual-Processing Theories* (Chaiken & Trope, 1999), according to which an evolutionarily older *associative* or experiential system that humans share with other animals is in conflict with a rational system unique to humans (Chen et al., 2006; Loewenstein et al., 2001). The different origin and original purpose of the two systems leads to maladaptive functioning for some modern decision tasks. Implications of accountability for the activation of different mental processes in dual processing models are discussed.

4.2 Risk attitudes

It is by now commonly accepted that risk attitude should be subdivided into different

components. The original prospect theory paper that proposed such a conceptual division (Kahneman & Tversky, 1979) is the second most cited in economics between 1990 and 2000 (Coupé, 2003). It has also earned one of its authors a Nobel Prize and is proving increasingly popular and influential in all of the social sciences. Given that differential findings of an effect of accountability on the three components of risk attitude may lead to different issues for empirical research, they will henceforth be discussed separately.

4.2.1 Risk attitudes in the gain domain: Probability Weighting and Utility Curvature

Prospect theory predicts different risk attitudes in the gain domain according to whether probabilities are small or medium to large, thus proposing a probability weighting function given by an inverse-S shape (Gonzalez & Wu, 1999; Tversky & Kahneman, 1992; Wu & Gonzalez 1996). Tversky & Wakker (1995) give a characterization of the probability weighting function in terms of preference conditions. Two properties of the probability weighting function that are mostly satisfied are called lower and upper subadditivity.

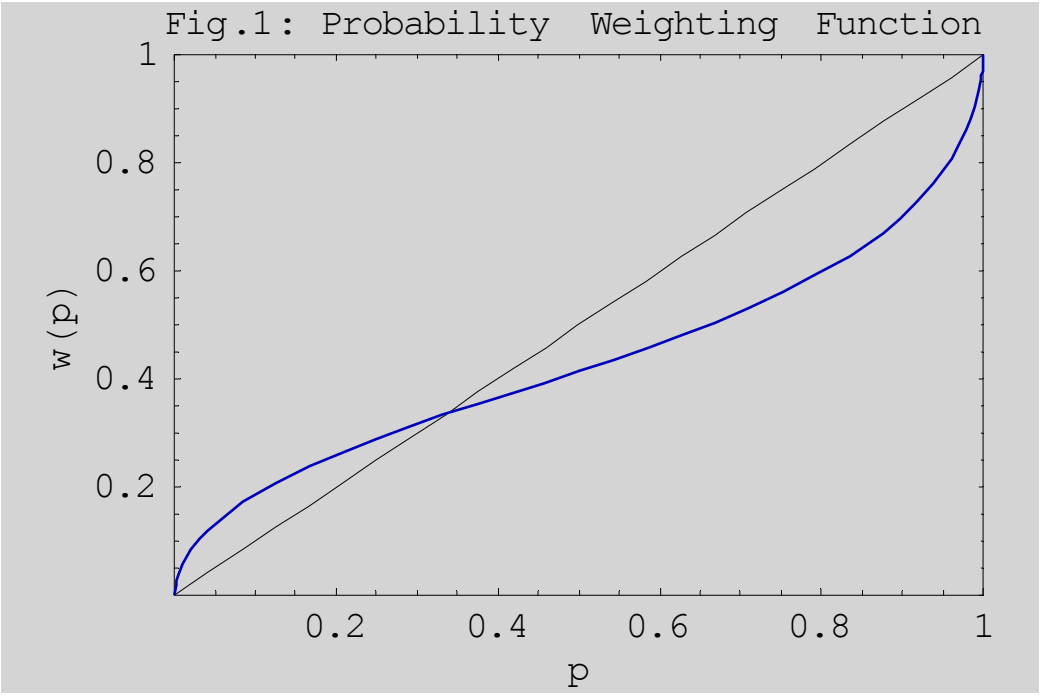


Figure 4.1: The Probability Weighting Function

Lower subadditivity describes the empirical phenomenon that a given probability

increase is given more weight when it makes the impossible possible than when it merely increases a given probability (*possibility effect*). In a parallel fashion, upper subadditivity describes the *certainty effect*, by which a probability increase is given more weight if it leads from possibility to certainty than when it merely increases a given probability. These two properties lead to the steep segments of the probability weighting function near its endpoints at 0 and 1 (see figure 4.1).

Risk attitudes reflected in probability weighting need to be disentangled from attitudes towards the value of money, which are reflected in the curvature of the utility function. Traditional theories such as expected utility used to model all of risk attitude as deriving from the curvature of the utility function, so that no proper separation of attitudes towards probabilities and money was possible (Lopes, 1987).

Weigold & Schlenker (1991) investigated the effect of accountability on risk attitudes for gains. After dividing subjects into risk seekers and risk averters based on a personality question, they elicited their risk attitudes a few weeks later manipulating accountability conditions. Although they found that risk averters did indeed become significantly more risk averse when accountable, there was no significant effect for risk seekers (who however became slightly more risk seeking). The paper however studies the effect of accountability on risk aversion only in very special circumstances. Indeed, it employs multi-outcome prospects taken from Lopes (1984) with constant expected values across prospects. Though rarely tested, the applicability of prospect theory to choices between multi-outcome lotteries seems to be limited (Bernstein et al., 1997).

4.2.2 Risk Attitude and Loss Aversion

Loss aversion is generally thought to be responsible for the greatest part of risk aversion (Köbberling & Wakker, 2005). It thus seems particularly interesting to see whether accountability has an effect on loss aversion. Loss aversion reflects the fact that people weigh losses more heavily than gains of the same size. In prospect theory this is modeled through a utility function that presents a kink at the status quo, resulting in a steeper utility function for losses than for gains (see figure 4.2). The prospect theory utility functions over monetary *outcomes* $z \in \mathbb{R}$ can be characterized as $U(z) = z^{\alpha}$ if $z \geq 0$ and $U(z) = -\lambda |z|^{\alpha}$ if $z < 0$, where

U: $\mathbb{R} \longrightarrow \mathbb{R}$ is a strictly increasing utility function, λ is the loss aversion index, and α determines the curvature of utility. Loss aversion can then be estimated by means of simple two-outcome prospects, each involving a gain and a loss. A subject is thereby asked to indicate a positive amount such as to make her indifferent between the prospect and the status quo (not playing). The loss aversion parameter is generally found to be between 1 and 2.5 (Abdellaoui et al., 2007, Booij & van de Kuilen, 2006; Tversky & Kahneman, 1992).

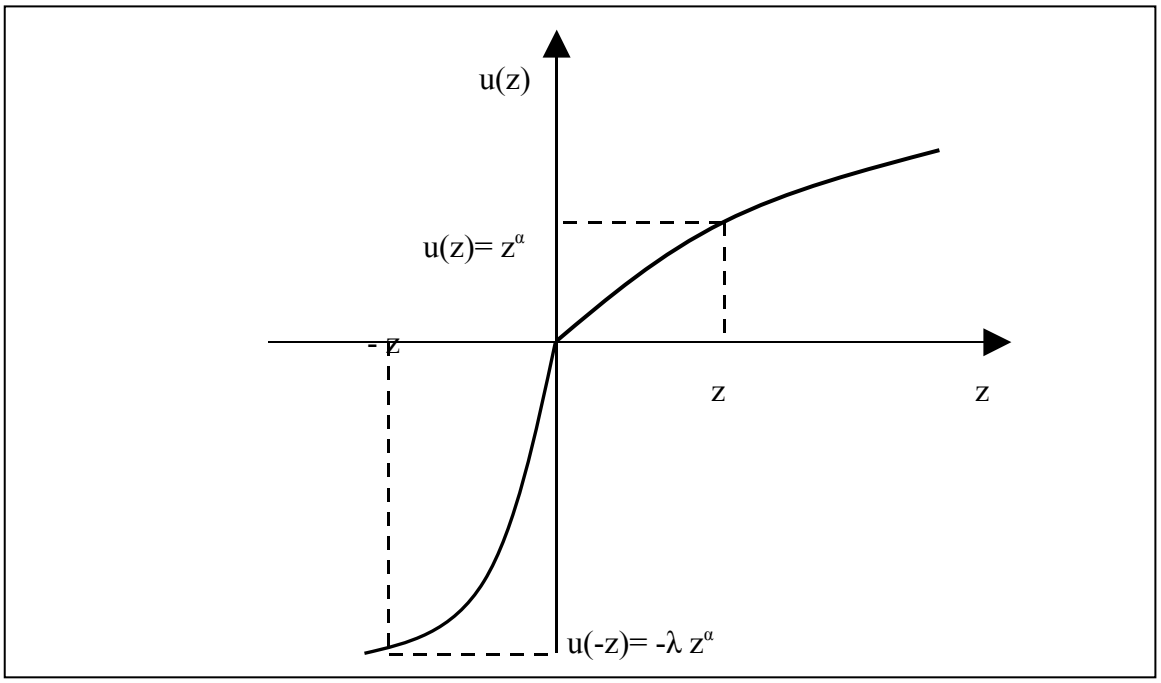


Figure 4.2: Utility of a gain of z relative to a loss of z

Loss aversion is frequently used to explain phenomena that had long been known empirically but for which sound explanations were still missing (Camerer, 2000). Loss aversion is generally thought to be the cause of the endowment effect and of the status quo bias (Kahneman et al., 1991; Thaler, 1980; Tversky & Kahneman, 1991), or in any case the WTA-WTP gap (Brown, 2005). It has also been employed to explain the equity premium puzzle (Benartzi & Thaler, 1995; Thaler et al., 1997), disposition effects (Shefrin & Statman, 1985; Weber & Camerer, 1998) and the labor supply of cab drivers (Camerer et al., 2000). Sugden (2003) and Schmidt et al. (2006) use loss aversion in relation to a reference point that is itself a prospect to explain preference reversals. Trautmann, Vieider, & Wakker (2007b) use the same principle to explain preference reversals under ambiguity. Loss aversion is also

increasingly employed to explain phenomena outside the strictly economic realm, ranging from international relations (e.g. Levy, 1996) to explanations of differential perception of the progress made by racial minorities dependent on group membership (Eibach & Keegan, 2006).

No studies exist to the best of the author's knowledge about the effect that accountability may have on loss aversion. Finding such an effect may thus lead to differential predictions about the phenomena listed above according to whether a decision is observable or not. Although loss aversion is commonly recognized to be empirically strong (Abdellaoui et al., 2007; Bleichrodt et al., 2001; Booij & van de Kuilen 2006; Fishburn & Kochenberger, 1979; Johnson et al., 2006), it has also been found to be subject to subtle framing effects and to be dependent on subjects' experience (List, 2004; Plott & Zeiler, 2005a; Plott & Zeiler 2005b).

4.3 Study 1 – risk aversion for gains

The aim of study one was to investigate the effect of accountability on general risk attitudes in the gain domain. To this end it employed commonly used measures of risk attitude consisting in two-outcome prospects. Two-outcome prospects have the advantage of being easy to understand, which generally results in less noise in the data compared to multi-outcome lotteries (Bernstein et al., 1997).

4.3.1 Method

Subjects. 48 subjects participated in the experiment. The average age of the subjects was 21.83 years, and 65% of the subjects were male. The subjects were recruited from a mailing list at Erasmus University Rotterdam, the Netherlands. Subjects completed a questionnaire with choices under risk together with other experiments. They were compensated with a flat payment of €15.

Stimuli. The stimuli consisted of 15 choice pairs in which subjects were called upon to choose between a two-outcome prospect and a certain amount of money. The choice pairs included 11 prospects from Tversky & Kahneman (1992)—extreme probabilities of 1% and 99% were

excluded. The task consisted in choosing between the prospect and the median certainty equivalent elicited for that prospect by Tversky & Kahneman. The remaining four choice pairs were either taken from Birnbaum et al. (1992) or served to distinguish extreme risk aversion. Instructions appear in the appendix.

Manipulation. The manipulation between the two treatments consisted in varying the accountability level of the subjects. In the *low accountability* treatment, subjects were told that their answers were confidential and could not be traced back to them. They were told that after the experiment they should put their completed questionnaire in a cardboard box by the exit of the room upon which they would be paid and could leave. In the *high accountability* treatment subjects were asked to write down their name and email address at the beginning of the sheet. They were told that upon completion of the task they would be asked to take their questionnaire with them to another room, where an experimenter would interview them about their choices. Several subjects participated at the same time, so as to give an additional impression of anonymity in the low accountability condition.

Analysis. Choices for the certain amount of money were encoded as 1 and choices for the prospect as 0. A general risk-attitude index was then constructed by adding up the choice indicators in the 15 choice pairs. Separate indices were constructed in the same manner for low, medium and high probabilities by adding up the choice indicators for the probability subgroups. A normal distribution of the results was not rejected, hence two-tailed t-tests are used throughout. Non-parametric tests do not change the results.

4.3.2 Results

The general risk index ranged from 2 at the low end of risk aversion to 13 at the high end, with a median of 7. No difference in general risk attitude could be found between treatments (t_{46}= - 0.128, p=0.899). Also division of probabilities into sub-categories brought no results (low probabilities: t_{46}=0.012, p=0.99; medium probabilities: t_{46}=0.951, p=0.994; high probabilities: t_{46}= - 1.218, p=0.356).

These results may be driven by the finding of Weigold & Schlenker (1991) that risk attitudes become more extreme under accountability but do not go in one predetermined

direction. In order to test this hypothesis, a difference in variance test was performed. The null hypothesis that variances were the same for both treatments could not be rejected by a variance ratio test ($f_{21,25}$=0.799, p=0.606). Also for the probability-subdivisions mentioned above no difference in variances could be found.

4.3.3 Discussion

Accountability seems to have no effect on risk attitude measured through choices between a simple two-outcome prospect and a certain amount of money. Weigold & Schlenker (1991) employ prospects with an expected value of $100 and tell subjects that all prospects have the same expected value. This may have led to a different perception of those prospects. Also, the complex nature of the prospects employed by Weigold & Schlenker diverts the attention from actual probability levels and focuses attention on the general probability distributions and the size of the prizes (Bernstein et al., 1997). The fact that we cannot find any effect on general probability attitude may well be caused by this difference in the prospects employed.

Arguably, the choice tasks employed in this experiment are somewhat crude and may not be apt to distinguish fine shades in risk attitudes. Also, they cannot be employed to divide attitudes towards probabilities from attitudes towards money, i.e. they cannot distinguish probability weighting and utility curvature. Given the predominance of theories that separate attitudes towards money from attitudes toward probabilities it seems desirable to separately study the influence of accountability on these two components of risk attitude.

4.4 Study 2 – separation of utility and probability attitudes

Study two was carried out with two major purposes in mind. The first one was to permit more graded attitudes towards probabilities than can be obtained by simple choices between a prospect and one sure amount of money, and hence to permit finer measurement of risk attitudes. The second aim was to permit for the separation of attitudes towards utility and probabilities.

4.4.1 Method

Subjects. 62 subjects participated under the same conditions as in study 1. The average age of

the subjects was 21.40 years and 50% of the subjects were male.

Stimuli. The stimuli consisted of 15 choice lists in which subjects were called upon to repeatedly choose between a decreasing certain amount of money and a given prospect (an example appears in the appendix). The 15 prospects employed were taken from Tversky & Kahneman (1992).

Manipulation. Same as in study 1.

Analysis. The middle point between the values of the certain amount for which a subject would switch between the certain amount and the prospect was taken to be the certainty equivalent of the prospect. A risk-attitude index was then constructed for each choice-list by dividing the certainty equivalent by the non-zero outcome of the prospect (*prize*). These normalized indices were then averaged separately for each probability level, for low, medium and high probabilities, and for all probabilities together to build several indices. A normal distribution of the results could not be rejected, hence two-tailed t-tests are used throughout. Non-parametric tests do not change the results.

4.4.2 Results

No difference in general risk attitude could be found between treatments (t_{60}= -1.206, p=0.233), and contrary to expectation subjects were found to become slightly less risk averse under accountability on average. Also subdivision into different probability levels brought no results (low probabilities: t_{60}= - 0.335, p=0.739; medium probabilities: t_{60}= - 1.191, p=0.238; high probabilities: t_{60}= - 0.663, p=0.51).

Additional tests carried out to control for differences in the curvature of the utility functions also brought no results. The utility function was found to be the same between treatments for all four outcomes involved (€50, €100, €200, €400), with t_{60} = -1.434, p=0.157, t_{60} = -0.986, p=0.328, t_{60} = - 0.856, p=0.395, and t_{60} = - 0.707, p=0.483 respectively.

Again, the null hypothesis that variances were the same for both treatments could not be rejected by a variance ratio test ($f_{31,29}$ = 1.666, p=0.171, with a slightly higher variance in the low accountability condition). For the probability-subdivisions mentioned above results were similar.

4.4.3 Discussion

The reason that no effects are found may very well lie in the nature of the prospects employed. In addition to the issues already discussed above, this experiment employs choices between certain amounts of money and prospects, whereas Weigold & Schlenker employ choices between (multi-outcome) prospects. The latter may be seen as a different type of decision, thus leading to different processes being activated.

Other problems may also have occurred. Weigold & Schlenker classified their subjects beforehand as risk seekers or risk averters based on a personality question, and subsequently invited those subjects to study their risk attitudes. It was however not reported what proportion of their subjects is classified as risk seekers or risk averters. Furthermore, the risk-classification of the multi-outcome prospects seems to have been derived from the results in Lopes (1984). It is, however, not clear whether the latter results had themselves been obtained under conditions of low or high accountability. In other words, the classification of the prospects as more or less risky may itself depend on the accountability level, thus making the classification of the risk level itself dependent on the main experimental control.

4.5 Study 3 – loss aversion

Loss aversion is commonly thought to express the greatest part of risk aversion (Köbberling & Wakker, 2005). The aim of study three thus was to explore possible effects of accountability on loss aversion.

4.5.1 Method

Subjects. 109 subjects were recruited under the same conditions as in studies 1 and 2. The average age of the subjects was 21.6 years, and 56% were male.

Stimuli. The stimuli from Tversky & Kahneman (1992) were used employing a straight matching task. This methodology involves eliciting indifference between the status quo (an outcome of 0) and a two-outcome prospect. The prospect involved a given loss that would obtain with a .5 probability and a gain with a .5 probability that subjects were supposed to fill in so as to make them indifferent between the status quo and the prospect itself (*simple*

prospects). These simple prospects were then used to elicit the loss attitude of subjects, and some additional prospects were included to test for consistency and curvature of utility for gains (instructions in appendix). The loss aversion index λ for the simple prospects is given by the gain divided by the loss it needs to compensate (Tversky & Kahneman, 1992).

Since no complete utility functions are elicited and probability weighting is not considered in the calculations described above, only an approximation of the loss aversion index is obtained (Abdellaoui et al. 2007, Schmidt & Zank 2005). To the extent that the probability weighting function for gains and losses is however generally found to be very similar (Tversky & Kahneman 1992), this definition seems good enough, and does not influence the main issue at stake—the comparison of the index between treatments.

Manipulation. Same as in studies 1 and 2.

Analysis. Of the 109 subjects that participated, 7 were excluded from the analysis for violation of stochastic dominance. Of the remaining 102 subjects, 5 were classified as gain seeking and the remainder as loss neutral or loss averse. Preliminary tests strongly rejected a normal distribution of the data (p=0.000, skewness-kurtosis test for normality), hence two-sided non-parametric tests are used throughout.

4.5.3 Results

A two-sample Wilcoxon rank-sum test rejected the hypothesis that the two samples were drawn from the same population (p=0.030). Loss aversion was found to be significantly lower under conditions of high accountability. The median value of the average lambda from the simple gambles was 1.95 under high accountability and 2.38 under low accountability. Table 4.1 shows the different medians of lambda for the different prospects (means are given in parentheses). Testing on the other hand for effects on curvature of utility for gains, no difference between treatments was found. This indicates that accountability does indeed influence loss aversion and not curvature of the utility function for gains.

	low accountability	high accountability
lambda 1	2 (2.51)	1.8 (2.10)
lambda 2	2 (3.18)	2 (2.31)
lambda 3	2.5 (3.59)	2 (2.98)
lambda 4	2.67 (3.80)	2 (2.86)
average lambda	2.38 (3.27)	1.95 (2.56)

Table 4.16: median values of lambda (means in parentheses)

4.5.4 Discussion

Accountability is found to reduce loss aversion and, thus, to enhance rationality and the quality of decisions. Accountability theory can explain this if one accepts that loss aversion is recognized as a bias, and thus reduced when subjects have to justify their behavior (Simonson & Nye 1992). In particular, this makes sense inasmuch as the accountability in this case is procedural rather than outcome-related (Siegel-Jacobs & Yates, 1996). This seems indeed intuitive for the simple prospects employed, because it is easy to see that the value of the gain that makes the tradeoff "fair" is equal to the absolute value of the loss. This leads subjects to indicate gains that are close to the absolute value of the loss in the high accountability condition. When on the other hand subjects are not accountable, they follow their instinct and demand higher compensation for a given potential loss, an effect which seems to increase with the amount to be lost (see table 1).

Decisions in general often appear to be the outcome of a conflict between a quicker emotional mechanism and more reflective and rational mechanisms that are activated more slowly (Kahneman, 2003a; Loewenstein et al., 2001; Sloman 2002; Sanfey et al., 2006), as described in dual-process models (Chaiken& Trope, 1999; Sloman, 2002). Loss aversion appears to be caused mainly by emotional mechanisms. This interpretation also finds support in studies about loss aversion displayed by Capuchin monkeys (Chen et al., 2006) and by young children (Harbaugh et al., 2001), which both point in the direction of an instinctive origin of loss aversion.

Loss aversion thus seems to stem from adaptive mechanisms that have developed in the very early stages of human evolution to cope with basic environmental challenges (Chen et al., 2006; Rayo & Becker, 2005). This interpretation is corroborated by recent evidence from neuro-economics (Breiter et al., 2001; Sanfey et al., 2006), which with proper caution

constitutes a promising way to test social psychological models (Willingham & Dunn, 2003).

When there is the need to justify one's behavior in front of somebody else, however, the higher cognitive effort activated by this need reduces the bias (Tetlock, 1983; for a case of how rationality can impair decisions, see Dijksterhuis, 2004). Indeed, "an answer provided by the associative system just 'pops' into the head so the perceiver may be unable to provide any justification for it other than intuition"(Smith & DeCoster, 2000, p. 115). They will however "go beyond heuristic processing when circumstances […] make them feel an unusually great need to be accurate, defend an attitude or create a positive impression" (p. 119). This interpretation thus raises the issue of the motivational activation of the more rational, rule-based system by accountability that warrants further investigation.

The activation of more rational thought processes seems to be driven by the typical desire of being favorably evaluated and avoiding criticism generally displayed by accountable subjects (Chen & Chaiken, 1999; Simonson & Nye, 1992, Smith & DeCoster 2000; see also Baumeister & Leary, 1995). Indeed, accountability can be seen as increasing the social and reputation cost of relying on a simple decision heuristic. It thus becomes beneficial to engage in more complex deliberative behavior which results in a reduction of the decision bias, if the higher cost of the more complex deliberation is outweighed by the reputation costs of a bad decision (Arkes, 1991). This conclusion is also consistent with recent findings of the disappearance of loss aversion when subjects think more deeply about the decision at hand either because they are more experienced in market transactions (List, 2004) or because they are encouraged to properly learn and understand the incentive mechanism in repeated trials (Plott & Zeiler, 2005a).

Several reasons for the fact that the result of Weigold & Schlenker could not be replicated have been discussed above. An additional possible reason for the result of Weigold & Schlenker is that in the choice task with multiple outcome prospects subjects develop state-dependent reference points (Schmidt et al., 2006; Sugden, 2003), so that the results are actually driven by loss aversion with respect to that reference point. Because the prospects employed vary both probabilities and the number and relative size of the outcomes involved, it is however difficult to make any assumption about what subjects may see as a reference point in the different situations.

Finally there remains a methodological point to be made. Traditional experiments in

economics and psychology tend to isolate subjects as much as possible from outside influences and to guarantee subjects as much anonymity as possible, thereby keeping accountability artificially low. Plott & Zeiler (2005a) explicitly state that they want subjects to be anonymous in order to study their "real" preferences. Such a procedure does however not accurately reflect circumstances as encountered in the real world and thus threatens to jeopardize the external validity of experimental results. Any effect accountability is found to have on loss aversion may thus change the interpretation of results obtained in lab experiments according to the particular circumstances under which they were conducted.

4.6 Conclusion

Risk attitude is a complex phenomenon that is driven by attitudes towards probabilities, attitudes towards money, and by gain versus loss frames. This paper investigated the effect of accountability on the different components of risk attitude. Whereas no effect was found for either probability weighting or utility curvature for gains, accountability was found to reduce loss aversion. This result is consistent with recent studies that find beneficial effects of learning on decision making, in the sense that they reduce or even eliminate loss aversion. Additional cognitive effort induced by accountability is hereby found to improve decisions, even though it falls clearly short of completely eliminating the bias of loss aversion.

This activation of cognitive effort is linked to recent dual-processing models of the human mind, and a connection between the existing accountability literature and those models is established. This study thus touches upon the interesting question of accountability-driven motivational effects for the differential activation of mental processes. This finding also has important implications for traditional laboratory studies of loss aversion. To the extent that such studies have kept accountability low as is common practice in psychology and economic experiments, they may systematically overestimate the size of the bias. Future investigation of loss aversion will thus need to carefully control for the accountability variable in order to maximize the external validity of their results.

Chapter 5

Separating Real Incentives and Accountability

5.1 Motivation

Experimental economists have demonstrated the importance of real incentives for inducing cognitive effort in experimental tasks (Davis & Holt, 1993; Harless & Camerer, 1994; Harrison, 2007; Smith, 1982; Smith & Walker, 1993), although the actual effect of incentives in different situations is sometimes still debated (Camerer & Hogarth, 1999; Hertwig & Ortmann, 2001; Loewenstein, 1999). Social influences on individual decisions studied in social psychology on the other hand have not received much attention from experimental economists, or from psychologists studying the effects of incentives. Accountability—the implicit or explicit expectation by a decision maker of having to justify her decisions in front of others—has however been found to influence numerous decision making processes (Lerner & Tetlock, 1999).

When investigating the effects of incentives, accountability is often a confound. Hypothetical conditions provide for more anonymity than games that are actually played out in front of the experimenter, so that accountability is varied together with incentives in studies concerning the latter. An unaccountable hypothetical treatment is thus generally compared to a

treatment in which outcomes are really paid out and in which accountability is high. While both accountability and real incentives usually trigger higher levels of attention towards the decision making process, confounding the effects of the two makes causal attributions problematic. It may thus be, in principle, that effects traditionally ascribed to real incentives are in fact due to accountability.

To investigate this issue, we separate accountability and incentive variations in typical experimental tasks. Studying choices between sure amounts and prospects framed as either gains or losses, we find accountability to reduce preference reversals between frames, whereas incentives do not affect the incidence of preference reversals. Incentives are however found to reduce risk seeking for losses, for which accountability shows no effect. In a choice task between simple and conjunctive prospects (Bar-Hillel, 1973), we find accountability to increase the frequency of choice for the simple prospects. Incentives on the other hand result in more frequent choices of the conjunctive prospects. When accountability and incentives are confounded, no significant effect is observed relative to the control treatment because the two effects cancel out. While the particular results obtained are specific to the tasks employed, these two examples illustrate the general desirability to disentangle real incentives and accountability.

The paper proceeds as follows. Section 5.2 discusses accountability and its effects. Section 5.3 presents evidence for the confounding of real incentives and accountability. In section 5.4 dual processing models are discussed as a possible interpretative framework. Section 5.5 presents the experiment and discusses results for the different tasks employed. Overall results and their implications are discussed in section 5.6. Section 5.7 concludes.

5.2 Accountability

A substantial literature in social psychology shows that *accountability*—the expectation by a decision maker that she may be called upon to justify her behavior in front of others—can substantially affect human decision making processes (Lerner & Tetlock, 1999). Accountability in front of an audience with unknown views generally results in more cognitive effort. More options are considered in greater depth, thereby anticipating possible criticisms others might raise against one's choice, a phenomenon that has been called *pre-emptive self-criticism* (Tetlock, 1983; Tetlock & Kim, 1987).

Accountability to an unknown audience has been found to lead to less biased decisions in cases where the normatively correct decision was either known by the subjects, or could be arrived at by higher cognitive effort (Simonson & Nye, 1992). Accountability has thus been found among other things to reduce the fundamental attribution error (Tetlock, 1985), to improve coherence between gain and loss frames (Miller & Fagley, 1991; Takemura, 1993; Takemura, 1994), and to reduce overconfidence (Arkes *et al.*, 1987).

When on the other hand no solution is easily arrived at, people tend to choose the option that appears more easily justifiable (Simonson, 1989). This may be explained by the fact that people often rely on reasons when making choices (Shafir *et al.*, 1993). In such cases, accountability has been shown to impair decisions e.g. for ambiguity aversion (Curley *et al.*, 1986; Trautmann *et al.*, 2008), for the dilution effect (Tetlock & Boettger, 1989), and for the attraction and compromise effects (Simonson, 1989).

5.3 Separating Incentives from Accountability

In investigations of the effects of real incentives, manipulations of incentives need to be clearly set apart from other external influences, in order to permit unequivocal causal attributions of any effect that may be observed. Unfortunately, many studies investigating the effects of real incentives on decisions co-vary accountability with incentives. The undetected manipulation of accountability in incentive studies may cast doubt on the results obtained, all the more so since it is not clear whether accountability might reinforce any potential effects of incentives or attenuate them. In other words, this co-variation implies a loss of control over the experimental conditions (Harrison, 1994; Smith, 1982).

Sometimes the confounding of accountability and incentives can be clearly deduced from the letter of the paper (e.g. in Epley & Gilovich, 2005; Simmons *et al.*, 2006; Wright & Anderson, 1989). This covariation probably occurs for many more studies that vary incentives, and one may assume that it occurs in the majority of cases where it does not emerge clearly from the text that accountability variations have been controlled for. This suspicion is justified by the fact that controlling for accountability generally calls for special experimental procedures to be implemented—procedures that, if applied, could be reasonably expected to emerge from the description of the experimental method. Examples where such controls are implemented and can be deduced from the description of the experimental

method are Wilson *et al.* (1996) and Wiseman & Levin (1996).

5.4 Dual Processing Theories: An Interpretative Framework

Recent theorizing in psychology points in an interesting direction regarding mental processes. According to so-called *dual processing theories* (Chaiken & Trope, 1999; Epstein, 2003; Evans, 2003; Kahneman, 2003a,b; Sloman, 2002) different mental processes may be activated in a given decision problem. An emotional or associative system that is located in an evolutionarily older part of the brain is activated together with a rational or rule-based system. The final decision will then result from the interaction of those two systems.

Dual processing theory assumes that different stimuli may activate different mental processes, which in turn may lead to different outcomes of a decision process. While incentives have generally been found to increase motivation and improve decision making (Davis & Holt, 1993; Harless & Camerer, 1994; Harrison, 2007; Smith, 1982; Smith & Walker, 1993), there is some evidence that high monetary incentives may under certain circumstances trigger emotional reactions which activate the associative reasoning system (Camerer, 1992; Loewenstein *et al.*, 2001; Rottenstreich & Hsee, 2001). Accountability is thought to mostly activate rational mechanisms (Kirkpatrick & Epstein, 1992; Scholten *et al.*, 2007; Vieider, 2007). The latter may however not always result in better decisions being taken (Dijksterhuis, 2004; Simonson & Nye, 1992; Wilson & Schooler, 1991).

Additional evidence in favor of dual processing theories can be gathered from recent studies in neuroeconomics (Breiter *et al.*, 2001; Fehr & Camerer, 2007; Sanfey *et al.*, 2006). Indeed, susceptibility to framing effects has been found to be associated with increased activity in parts of the brain that are associated with emotional processes (the *amygdala*), while decreased susceptibility to framing effects has been found to be associated with activity in parts of the brain thought responsible for rational processing (De Martino *et al.*, 2006; McElroy & Seta, 2003; McElroy & Seta, 2004). Indications of an increased role of the amygdala in emotional reactions also come from the absence of skin conductance responses in patients whose amygdala is damaged (Bechara *et al.*, 1999).

5.5 Experiments

5.5.1 General Structure

Two-sided non-parametric tests are used throughout, unless specified otherwise.

Subjects: 166 subjects were recruited from a list of volunteers at Erasmus University Rotterdam. The average age of the subjects was 21.8 years, and 58% were male. All subjects were paid a flat fee of €15 ($23) for their participation. No additional earning possibilities were mentioned in the recruitment process in order to avoid a possible selection bias into the real-incentive treatments.

Treatments: The design is 2x2, with accountability and incentives varied in an orthogonal fashion. Subjects were divided as indicated in table 5.1:

	Hypothetical	**R**eal Incentives
Unaccountable	Treatment UH (43)	Treatment UR (42)
Accountable	Treatment AH (43)	Treatment AR (38)

Table 5.17: Experimental Design

Treatments are designated by first letters of manipulations—UH: Unaccountable Hypothetical; AH: Accountable Hypothetical, etc. Numbers of subjects are indicated in parentheses.

Accountability Manipulation: In the *unaccountable* treatment, subjects were told that their answers were confidential and could not be traced back to them. They were told that after the experiment they should put their completed questionnaire in a cardboard box by the exit of the room upon which they would be paid the flat fee of €15 for their participation. They would then either be dismissed or told to return to their seats, depending on the incentive manipulation (see below). Also, all sessions were held with groups of approximately 15 subjects, so as to reassure subjects that their answers could not be traced back to them.

In the *accountable* treatment subjects were told that upon completion of the task they would be asked to take their questionnaire with them to another room, where an experimenter would interview them about their choices. After the interview, subjects were paid the flat fee of €15 for their participation. They would then either be dismissed or told to return to their seats, depending on the incentive manipulation (see below).

Following conventions in the literature, and to be sure that subjects understood the instructions, a manipulation check was included at the end of the experimental questionnaire. Subjects in the high accountability treatments had a higher expectation than unaccountable subjects that they would have to justify their decisions ($Z=3.396$, $p=0.0007$). Also, the time it took subjects to complete the questionnaire was measured. Although instructions for accountable and unaccountable subjects were of the same length, accountable subjects took on average almost 7 minutes more to complete the questionnaire ($Z=5.839$, $p=0.0000$).

Incentive manipulation: In hypothetical treatments subjects were paid the flat fee and dismissed once they had completed the questionnaire (and the interview in the accountable treatment). In the real incentives treatments they were told to return to their seats after they had been paid their participation fee (and after they had been interviewed in the accountable treatment).

Monetary incentives were implemented using a random incentive mechanism (Abdellaoui *et al.*, 2007; Harrison *et al.*, 2002; Holt & Laury, 2002; Myagkov & Plott, 1997). Its equivalence to a single and payoff relevant decision task has been empirically tested and confirmed (Hey & Lee, 2005; Lee, 2008; Starmer & Sugden, 1991). This manipulation did allow us to use high monetary incentives to test for potential emotional reactions. One out of five subjects was selected for real play, and then one of the tasks was randomly selected for real play. Some papers explicitly tested whether it matters if for each subject one choice is played for real or if this is done only for some randomly selected subjects and found no difference (Armantier, 2006; Harrison *et al.*, 2007).

In order to be able to manipulate accountability, a careful procedure was implemented to assure subjects of their anonymity and to convince them that winnings could not be traced to them. This procedure was devised to avoid accountability in the real incentive unaccountable treatment. Also, the procedure was kept intact for the accountable and real incentives treatment in order not to introduce any confounds. Subjects detached a randomly generated four digit number from their questionnaires at the beginning of the experiment. Three numbers for each group of 15 were then randomly selected by the experimenter, so that winners would remain anonymous. The experimenter then played out the selected choice in front of the whole group. Prizes were finally put in envelopes with the corresponding number

and handed to a secretary on a different floor, who was unrelated to both the subjects and the experimenter. Subjects could then pick up their winnings in a sealed envelope by presenting their number as soon as the experiment was over.

Tasks: Different tasks were selected to test the separate effects of accountability and incentives. These tasks are described next.

5.5.2 The Framing Effect

Introduction

Different but normatively identical formulations of decision problems have consistently been found to influence choice patterns in a variety of situations. The most famous such situation is the Asian disease problem (Tversky & Kahneman, 1981). Subject are asked to prepare for the outbreak of a new Asian flue from which 600 people are expected to die. In the gain formulation, they can choose between a) saving 200 people for sure, and b) a probability of 1/3 of saving all 600 people, or else nobody. In the loss formulation a normatively equivalent choice is presented to them, only the two options are now presented as losses: a) 400 people will die for sure, and b) a 2/3 probability that all 600 people will die, or else nobody. While in the gain formulation the typical majority choice is the sure option *a*, in the loss formulation a majority of subjects typically chooses option *b* (Kühberger, 1998).

The Asian disease problem as described has been shown to have a number of confounds that may reinforce the observed decision pattern. Similar results have however also been obtained with equally structured monetary prospects that avoid most of those issues. The latter furthermore have the advantage of permitting the use of real incentives.

Method

Task. A within subjects design is employed. Both gain and loss formulations were presented on the same page so as to encourage comparison of the two. Monetary prospects were employed to make incentives possible. Subjects could win €25 in expected value. The following choice pairs were proposed:

Positive Frame: You are now given a cash gift of €20. Those €20 are yours to dispose of.

Additionally, you are given a choice between obtaining €5 for sure and playing a prospect with a 25% probability of winning €20 and a 75% probability of winning nothing.

Negative Frame: You are now given a cash gift of €40. Additionally, you are given a choice between giving up €15 for sure and playing a prospect with a 75% probability of losing €20 and a 25% probability of losing nothing.

Results

Accountability reduced the incidence of preference reversals, and this holds true for both typical (sure amount in gain frame, prospect in loss frame) and opposite (prospect in gain frame, sure amount in loss frame) preference reversals (see table 5.2). The difference between accountable and unaccountable subjects overall (aggregated across incentive levels) is significant ($Z=2.04$, $p=0.041$). The effect size found (Pearson's $r=0.16$[6]) is similar to other effect sizes found for accountability on within subject framing (Takemura, 1993). Incentives on the other hand do not influence the incidence of preference reversals overall ($Z=0.344$, $p=0.732$).

	Hypothetical	**R**eal Incentives
Unaccountable	24 (56%) [18,6]	22 (52%) [15,7]
Accountable	17 (40%) [14,3]	14 (37%) [6,8]

Table 5.18: Incentive and Accountability Influences on the Framing Effect

Numbers reported refer to overall number of preference reversals. Percentages refer to the percentage of subjects committing preference reversals. Numbers in square brackets represent typical reversals (sure amount in gain frame, prospect in loss frame), and opposite reversals (prospect in gain frame, sure amount in loss frame) according to the scheme: [typical/opposite].

Table 5.3 presents statistical comparisons treatment by treatment, which permit some additional insights into what is driving the results. Under hypothetical conditions, unaccountable subjects commit more preference reversals than accountable subjects, an effect that is marginally significant. A similar marginally significant result of accountability is

[6] Pearson's r is used as a measure of effect size throughout the paper. Effect sizes have the advantage to permit immediate comparison between findings from different studies independently of sample sizes or test statistics used, and thus facilitate comparison and integration of findings from different studies (Rosenthal, 1991). According to Cohen's (1988) classification, effect sizes of approximately r=0.10 can be seen as small, r=0.30 as medium, and r=0.50 as large, even though this scale should be used with caution.

obtained under real incentives. Incentives on the other hand are not found to affect preference reversals between frames.

Treatments	Statistics	Treatments	Statistics
UH > AH:	p=0.066, Z=1.502 r=0.16	AH =UR:	p=0.119, Z=−1.181 r=0.13
UH = UR:	p=0.377, Z=0.316 r=0.03	AH = AR:	p=0.403, Z=0.247 r=0.03
UH > AR:	p=0.045, Z=1.697 r=0.2	UR > AR:	p=0.083, Z=1.386 r=0.16

Table 5.19: Treatment by Treatment Comparison of Framing Effects

The inequality signs are used to indicate that there are more (>) or fewer (<) preference reversals in the first treatment than in the second; the equality sign (=) stands for no statistically significant difference; p-values reported are all one-sided.

An interesting insight is gained by considering the gain and loss frame separately and treating them as between subject data. Overall, subjects were indifferent between the sure amount and the prospect in the gain frame (p=0.938, two-sided binomial test), but displayed a strong preference for the prospect in the loss frame (p=0.0000, two-sided binomial test). For gains there is no main effect of either accountability (Z=−0.616, p=0.54) or incentives (Z=0.708, p=0.44). In the loss frame on the other hand, incentives have a strong effect (Z=3.607, p=0.0003; r=0.28). The effect of incentives goes in the direction of reducing risk seeking for losses. Indeed, while under hypothetical conditions risk seeking predominates (p=0.0000, two-sided binomial test), with real incentives risk neutrality cannot be rejected (p=0.7376, two-sided binomial test). There is no main effect of accountability in the loss frame (Z=1.203, p=0.23). However there is an interaction effect, inasmuch as accountability reduces risk seeking under real incentives, an effect that is marginally significant (Z=1.760, p=0.078).

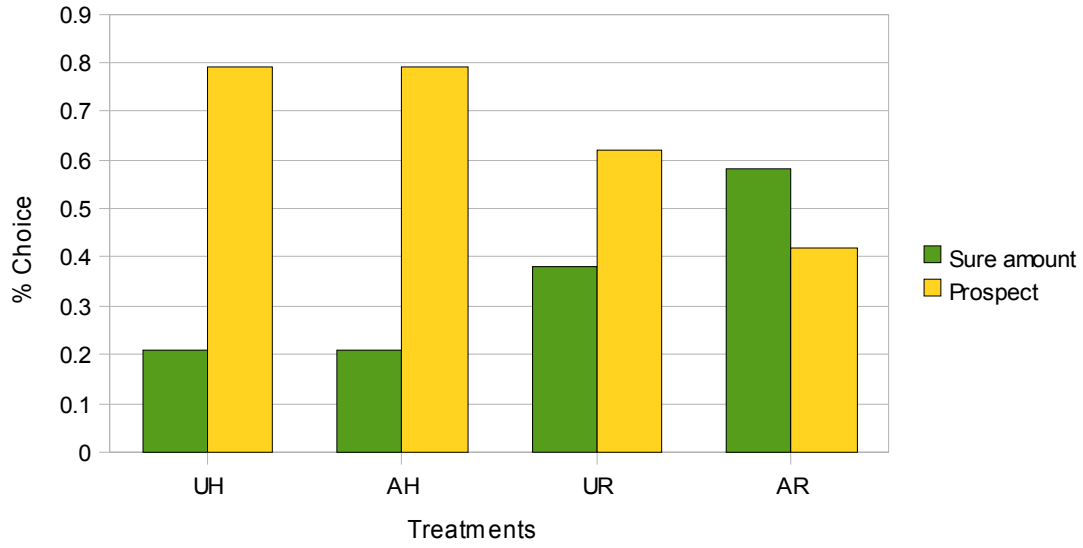


Figure 5.3: Preferences for the Sure Amount versus Prospect in the Loss Frame

Discussion

Accountability pressures improve the rationality of subjects and make them strive for coherence, thus leading to a significant reduction in preference reversals. Incentives on the other hand do not impact the incidence of preference reversals. These findings are consistent with some previous studies (Kühberger *et al.*, 2002, Takemura, 1993). Since incentives *per se* do not have an effect on the occurrence of preference reversals between the frames, there are no interaction effects to speak of that one could study. However, if accountability should be varied together with incentives in an experimental test of the latter, there is a risk that any improvement in decision making found may be attributed to incentives instead of accountability, as can be seen from the comparison of the UH and AR treatments, where the effect is indeed strong.

The within subject design has the advantage that one can detect preference reversals proper, which provide a stronger test than between subject majority switches in frames. In the literature, however, between subjects tests of framing effects are more common (Kühberger, 1998). Treating the results as between subject data and testing bidirectional framing effects (Kühberger *et al.*, 1999)—i.e.whether choice proportions in each frame differ from indifference between the two choices—another interesting picture emerges. Hypothetical treatments produce a pattern of risk neutrality for gains and risk seeking for losses. Incentives

however have the effect of producing indifference between the sure amount and the prospect in the loss frame, while no effect is found for gains. Finally, there is an interaction effect—accountability is found to reduce risk seeking for losses under real incentives, while no such effect of accountability is found for hypothetical choice. This interaction effect would thus reinforce the effect of incentives if accountability and incentives should be confounded.

These results are generally consistent with previous findings in the literature. Accountability has been found to reduce framing effects for problems of this type, both for within subject designs (Takemura, 1993) and for between subject designs (Miller & Fagley, 1991; Takemura, 1994; see also Sieck & Yates, 1997). Framing effects have been found to persist under monetary incentives (Kühberger, 1998; Kühberger *et al.*, 2002). The effect of incentives on choices in the loss frame that we found is consistent with the general evidence on strong effects of incentives in decisions involving losses (Cummings *et al.*, 1995; Hogarth & Einhorn, 1990; Horowitz & McConnel, 2002; List & Gallet, 2001). Unlike some of to the evidence on effects of incentives on risk attitude in the gain domain (Burke *et al.*, 1996; Harrison, 1994; Kachelmeier & Shehata, 1992; Slovic, 1969), no effects of incentives are found in the gain frame.

The connection between decreases in framing effects and the activation of rational processes is supported by findings on dual processing systems. McElroy & Seta (2003) found that subjects with predominantly analytic/systematic thinking styles are less affected by framing than subjects with a predominantly heuristic/holistic thinking style. McElroy & Seta (2004) found an association of increased preference reversals and activation of areas of the brain where holistic thought processes take place, while absence of preference reversals is associated with the activation of rational parts of the brain. DeMartino *et al.* (2006) found that decision switching for different frames is associated with increased activity of the amygdala, the part of the brain where emotional processes are supposed to be activated. Decreased susceptibility to framing effects is associated with increased activity in the prefrontal and orbital cortex, the part of the brain where rational processing is thought to take place. Evidence on different thinking styles also derives from Sunghan *et al.* (2005), who found that older adults are more affected by framing. Older adults have been known to rely more heavily on heuristic thinking than younger adults (Epstein, 2003; Johnson, 1990).

Beyond the interest of these findings for framing effects *per se*, the general message is

to be found in the importance of keeping manipulations of accountability and incentives separate. Indeed, accountability appears to act as a motivational trigger for analytic thinking styles that increases the strive for coherence between the two frames. This conclusion is also supported by the finding that subjects take on average much longer to complete their decisions under accountability than when they are unaccountable. Incentives seem to rather focus attention on true preferences in the loss frame. If accountability is confounded with incentives, there is a risk that both effects may be attributed to the latter, a conclusion that is not warranted. Next we proceed to examining a decision problem in which such confounding may have even graver consequences.

5.5.3 Choice between simple and compound events

Introduction

People have been known to be affected by biases in the evaluation of probabilities of simple versus compound events (Bar-Hillel, 1973). A simple event such as drawing a red ball from an urn containing 50 red balls and 50 black balls to win a prize is compared to a conjunctive event such as drawing 7 red balls in succession with replacement from an urn containing 90 red balls and 10 black ones to win the same prize. The second, conjunctive, event is thereby generally preferred by a majority of subjects, even though it gives a probability of winning of .48 compared to the .5 of the simple event. When the same simple event is however compared to a disjunctive event, such as drawing at least one red ball in seven trials with replacement from an urn containing 10 red balls and 90 black balls, then the simple event is preferred by a majority of subjects, even though the disjunctive event has a higher probability of .52. It seems plausible that this bias in probability assessment is largely due to low cognitive effort, with the implication that accountability should lead to a more thorough assessment of probabilities and hence to a better final estimate. The potential effect of incentives is less clear.

Method

Task. Six choice pairs of the kind proposed by Bar-Hillel (1973) were used, giving subjects a choice between a simple prospect involving one draw from an urn, and a conjunctive prospect involving repeated draws from an urn with replacement. The choice pairs were selected so

that the overall probability of winning would always be lower in the conjunctive prospect than in the simple prospect. The conjunctive prospects used presented varying levels of calculation difficulties and were more or less close in probability to the simple prospect (see Appendix 5.A).

Incentives. The choices involved can be played out in an incentive-compatible way. The prize for extracting a winning ball (or combination of balls) from the urn was €20.

Encoding. The choice was encoded as a dummy variable, with 0 indicating a choice of the (normatively superior) simple event, and 1 indicating a choice of the conjunctive event. These dummies where then summed for all six choice pairs to obtain a general index ranging from 0 to 6. Figure 5.2 shows the occurrence of this index by treatment.

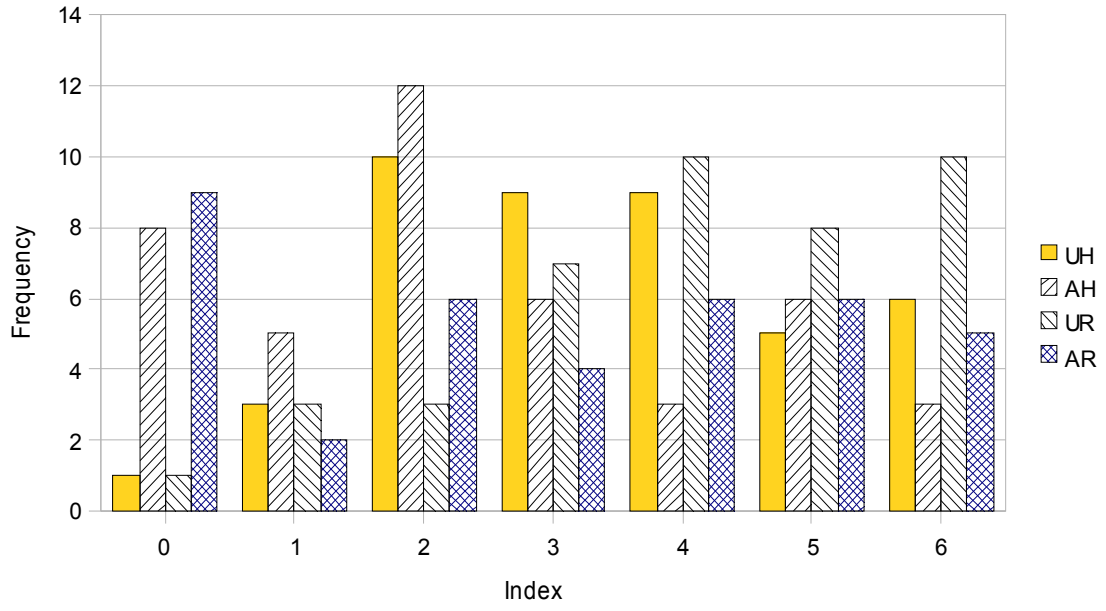


Figure 5.4: Frequency of Choice for the Conjunctive Prospect

The gray area indicates the control treatment UH. Upwards slashes (/) indicate accountability, downward slashes (\ ) real incentives.

Results

Most subjects chose at least some conjunctive events. This was to be expected, as some probabilities were difficult to calculate and close to the ones of the simple prospects (see

appendix 5.A). Overall (aggregating across incentives), accountability significantly improves decisions, leading to more choices of the simple prospect ($Z=3.449$, $p=0.0006$; $r=0.27$). Incentives on the other hand are found to significantly impair decisions, leading to more choices of the conjunctive prospect ($Z=2.018$, $p=0.0436$; $r=0.16$), although the effect size is much smaller than for accountability. This can be seen also from figures 5.3 and 5.4, which show the aggregated data for the accountability manipulation and the incentive manipulation. Remarkably, 17 accountable subjects consistently chose the superior simple event, as opposed to only 2 unaccountable subjects. Table 5.4 shows the average number of choices for the normatively inferior conjunctive prospect by treatment.

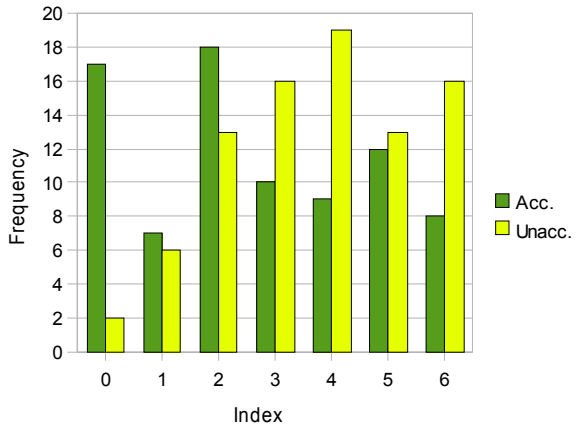


Figure 5.5: Overall Effect of Incentives

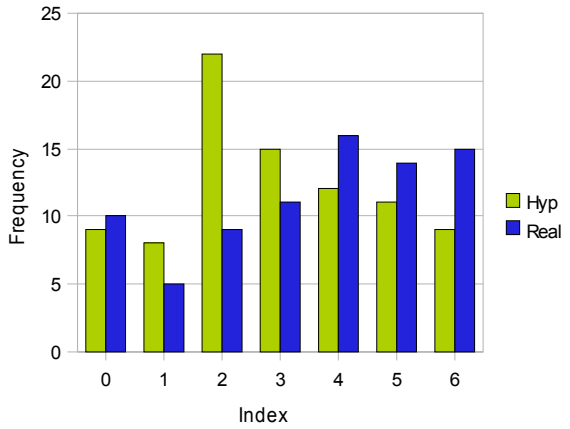


Figure 5.6: Overall Effect of Accountability

	Hypothetical	**R**eal Incentives
Unaccountable	3.42	4.05
Accountable	2.49	2.90

Table 5.20: Incentive and Accountability Influences on Choices for Simple versus Conjunctive Events

Numbers reported refer to the index described above and represent the average number of conjunctive events chosen by subjects in each treatment.

Some additional insights can be gained from the treatment by treatment comparison displayed in table 5.5. In the hypothetical treatments, accountability increases choices for the normatively superior simple prospects. This effect of accountability is replicated under real incentives. For unaccountable subjects, monetary incentives increase choices for the normatively inferior conjunctive prospects. This effect of incentives does however not carry

over to accountable subjects, where the effect of incentives seems to be overwhelmed by the strong accountability effect. Since accountability and incentives produce effects in opposite directions, the strongest difference obtains between accountable subjects under hypothetical conditions and unaccountable subjects under real incentives. Passing from unaccountable hypothetical to real incentives under accountability on the other hand does not result in any significant difference as the two effects cancel out.

Treatments	Statistics	Treatments	Statistics
UH > AH:	p=0.007, Z=2.451 r=0.26	AH < UR:	p=0.0001, Z=−3.747 r=0.41
UH < UR:	p=0.029, Z=−1.898 r=0.21	AH = AR:	p=0.19, Z=−0.869 r=0.1
UH = AR:	p=0.15, Z=1.030 r=0.11	UR > AR:	p=0.008, Z=2.406 r=0.27

Table 5.21: Treatment by Treatment Comparison of Choices for Simple versus Conjunctive Events

The bigger or smaller signs are used to indicate that there are more (>) or less (<) choices for the conjunctive prospects in the first treatment than in the second; the equal sign (=) stands for no statistically significant difference; p-values reported are all one-sided.

Discussion

Accountability exerts a strong influence on decisions, increasing the frequency of choices for the superior simple prospect. Incentives on the other hand increase choices for the conjunctive prospect, although the effect is less strong than for accountability. Indeed, incentives produce an effect size of r=0.16 compared to the effect size of r=0.27 of accountability, and when both manipulations are combined the effect of accountability overwhelms the effect of incentives, as can be seen from the comparison of treatments UH and AR. Confounding accountability and incentives would thus lead to the conclusion that incentives have no effect, a conclusion that is not warranted based on the data presented.

The bias in the evaluation of simple versus compound prospects observed has been attributed in the literature to an anchoring and adjustment process (Holtgraves & Skeel, 1992; Kahneman & Tversky, 1974; Kruglanski & Freund, 1983). Subjects are thought to anchor their probability estimate for the conjunctive prospect at the probability of success in any single stage—thus 9/10 of a red ball for conjunctive event and 1/10 for a red ball for disjunctive event in the example given in the introduction to the present section—and then fail to adjust these initial estimates to a sufficient degree. Anchoring and insufficient adjustment

has been used to explain the conjunction fallacy (Tversky & Kahneman, 2002), the fundamental attribution error (Tetlock, 1985), to model ambiguity aversion (Einhorn & Hogarth, 1985; Hogarth & Einhorn, 1990), and to explain scarce articulation of preferences (Slovic, 1995) and thus preference reversals (Tversky *at al.*, 1988). Anchoring and adjustment has also been used to explain how people predict the preferences of their spouse (Davis *et al.*, 1986), how consumers evaluate product bundles (Yadav, 1994), to criticize contingent valuation studies (Boyle *et al.*, 1997; McCollum, 1997), for property pricing decisions (Northcraft & Neale, 1987), for purchase quality decisions (Wansink *et al.*, 1998), and for a host of other issues. The effects of accountability and incentives found may thus lead to differential predictions according to the measure in which the two elements affect the decisions involved.

Consistently with our findings, accountability has been found in the literature to increase adjustment away from an anchor, and thus to improve decision making (Kruglanski & Freund, 1983). The evidence on the effects of incentives on the other hand is more mixed. The latter fact is partially due to the distinction between internally generated and externally given anchors in the literature (Epley & Gilovich, 2001; Stack & Mussweiler, 1997). This distinction has been based at least in part on the differential effect of incentives found for the two mechanisms (Chapman & Johnson, 2002; Epley & Gilovich, 2005). The distinction between internally generated and externally given anchors seems however to have been exaggerated (Simmons *et al.*, 2006), and there are other reasons for the differential effects of incentives on anchoring found in the literature.

Indeed, most of the judgment tasks previously used in the anchoring and adjustment literature have the limitation that they cannot be incentivized in an incentive-compatible way. Only the best estimates in a group of people are typically rewarded, which may have led to strategic behavior of subjects. Also, the particular incentive structure employed has led to obvious covariation of accountability with incentives. For instance, Epley & Gilovich (2005) vary accountability together with incentives while studying the effect of the latter on adjustment from an anchor. While subjects in the hypothetical condition remain anonymous, subjects in the incentive condition are asked to report their names and addresses on the experimental questionnaire so that they can be contacted—a manipulation that has been found to be sufficient by itself to generate accountability pressures (see e.g. Trautmann *et al.*, 2008a,

study 1). Similar problems also occur in Wright & Anderson (1989) and Simmons *et al.* (2006).

If one compares the findings in this paper on the difference between treatments UH and AR to traditional findings in the literature that suffer from the confounding of accountability and incentives, the results are very similar. It emerges however clearly from the data presented above that this null result is due to the fact that the opposite effects of accountability and incentives cancel out. Taken separately, both accountability and incentives are shown to affect the decision making process.

For the particular choice task employed here, incentives are found to make decisions worse by increasing choices for the normatively inferior conjunctive events. Recent studies contain some indication that at least in some instances high monetary payoffs may trigger emotional reactions (Camerer, 1995; Loewenstein, 2000; Rottenstreich & Hsee, 2001), and can thus lead to the activation of the experiential system. An indication in this direction is obtained by a marginally significant effect of age ($p=0.087$), which is consistent with the finding that for adults the reliance on heuristic processing increases with age (Epstein, 2003; Johnson, 1990). A similar effect has been found by Kirkpatrick & Epstein (1992), where the preference of subjects to bet on urns with larger absolute numbers of winning balls even when they offer inferior probabilities is reinforced by monetary incentives. More general implications of these findings are discussed next.

6. General Discussion

Beyond the importance of the present experimental findings for the literature on the decision biases involved, there is a more general lesson to be drawn. The results generally show the danger to experimentally confound accountability and incentives when trying to test the effect of the latter. Many existing studies on the effects of incentives have that confound.

One should note that the point of these findings is not to dispute the importance of real incentives in experiments. To the contrary, the results constitute a warning for scholars who try to generalize their hypothetical experimental results to the real world. Since monetary incentives are shown to often have effects on the decision making process, the absence of real incentives from experiments threatens to impact the external validity of such experiments. At the same time, there is a strong message for scholars who want to study the effect of

incentives—if accountability is not controlled for in such experimental studies, then any effect that is found (or indeed, not found) cannot be attributed to the incentives themselves, but must rather be ascribed to the interaction between incentives and accountability. There is thus a problem of internal validity.

Accountability variations that have occurred in practice while studying incentives are likely to be weaker than the strong manipulation employed in the experiments of this paper. Indeed, the latter has been used with the purpose of proving a general point. However, the effects found for accountability are extremely strong, and it is known in the social psychology literature that even much weaker variations of accountability can produce sizable effects. Unless such variations are carefully controlled for, one can never be completely sure that monetary incentives—and not accountability variations, however small—are at the root of changes in behavior that have been observed.

The experimental controls implemented in this study to separate accountability from incentives are complicated. However, it does not appear necessary to implement such complicated measures for every investigation of real incentives. The most important lesson to be learned is that accountability should be kept constant between hypothetical and real incentive treatments in order to maintain control over the experimental conditions. The level of accountability at which this is done depends mostly on concerns of external validity, and may well be different according to the exact problem investigated. Keeping accountability constant could thus be achieved by playing out choices under both real incentives and hypothetical conditions, as done by Wiseman & Levin (1996). While in the real incentive condition they actually played out choices and subjects were paid the resulting amount, in the hypothetical condition choices were still played out in front of the experimenter and outcomes were recorded on the instructions. Accountability was thus held constant across conditions. Another type of control was used by Wilson *et al.* (1996). They provided two tasks for both real and hypothetical incentives, one task in which they were truly interested and a filler task. While in one treatment the task of interest was played out, in the other treatment the filler task was played out.

The particular tasks employed may have led to especially strong effects of our manipulations. This is especially true for the choice tasks between simple and compound events, which have a clearly correct answer that can be calculated. Once again it is important

to stress how effects of both accountability and incentives can occur for any kind of decision, and how their interaction can fundamentally undermine the finding of a study that co-varies both elements. Indeed, effects of accountability have been found also for problems where no "correct" answer exists (Huber & Seiser, 2001; Ratner & Kahn, 2002; Sedikides *et al.*, 2002), and so have effects of incentives (Harrison, 2007; Slovic, 1969). In which cases one manipulation may have effects and the other one not, or the effect of one manipulation may be overwhelmed by the effect of the other is an empirical question.

7. Conclusion

Traditional studies of monetary incentives are likely to have varied accountability together with monetary incentives, thus making clear causal attributions of any effects found (or not found) problematic. Conducting experiments in which accountability and monetary incentives are carefully kept apart we demonstrated the existence of such confounding effects. Mis-attributions of the effects of accountability to incentives are thus likely to have occurred in the literature. The message is thus that accountability needs to be carefully controlled for in studies of monetary incentives. Based on this evidence, the effects of real incentives may have to be reassessed using careful accountability controls. If accountability and monetary incentives are co-varied, we can only attribute any effect that may be found to the two phenomena jointly, but not to one or the other.

Appendices

2.A Instructions Experiment 1

(Please report your NAME and EMAIL here:

_____

_____

A researcher from the Economics Department may contact you to ask for some explanations concerning your choice.**)**

Consider the following two hypothetical lottery options:

Option A gives you a draw from a bag that contains exactly 40 poker chips. They are either red or green, in an unknown proportion. Before you draw, you choose one color. Then you draw. If the color you have chosen matches the color you draw you win €16. If the colors do not match, you get nothing.

Option B gives you a draw from a bag that contains exactly 20 red and 20 green poker chips. Before you draw, you choose one color. Then you draw. If the color you have chosen matches the color you draw you win €15. If the colors do not match, you get nothing.

Imagine you had a choice between these two lottery options. Which one would you choose?

- **O** Option A (bet on a color to win €16 from bag with unknown proportion of colors)
- **O** Option B (bet on a color to win €15 from bag with 20 red and 20 green chips)

2.B Instructions Experiment 2

In Treatments KS and US the instructions started with the following part:

In front of you there are two DVDs: *About a boy* and *Catch me if you can*. Take your time now to have a look at the boxes and then decide which one you would like to receive.
Write down the name of your preferred movie here:

_____

Please also write down your name and movie preference in the list the experimenter will give to you!

In Treatments US and UC this part was replaced by the following text:
In front of you there are two DVDs: *About a boy* and *Catch me if you can*. Take your time now to have a look at the boxes and then decide which one you would like to receive, but **do not tell your preference to the experimenter**.

In Treatments KS and US the first part was followed by the following text:
Next, the experimenter will give you two stickers, one with a **cross** on it, and one with a **circle** on it. Please attach each sticker to one of the DVDs **as you like**. The symbol (cross or circle) has nothing to do with your preference between the movies.

Then the experimenter will offer you a **choice** to draw a card from either of two stacks of cards: this card is used to determine which DVD you will win. This is done as follows: On each card there are numbers 1 to 6 and either a cross or a circle next to each number. (See example card.) After drawing a card you will throw a six-sided die to determine the winning number and thereby the winning symbol, cross or circle. You obtain the DVD to which you attached the winning symbol before the game.

The two stacks of cards.
One stack of cards, called "50/50", contains cards that have **exactly three crosses and three circles** on the back, randomly distributed over the six numbers of the die.

The other stack of cards, called "?", contains cards that have an **unknown number of crosses and circles** on the back, but the sum of the number of the two symbols is equal to six again: that is, there are between zero and six crosses on the back, distributed randomly over the six numbers of the die, and
(6-(number crosses)) circles.

Summary and timeline (see also illustration on next page): you receive the two stickers → you attach the cross and circle sticker to the DVDs as you like → draw a card from 50/50-stack **or** from ?-stack → throw the die and observe which symbol wins → take the DVD to which you before attached this symbol. End of the experiment.

Please carefully consider all the information given to you about the **chances** of the two

stacks of cards and your **personal preferences** between the DVDs, before making your choice between a draw from the 50/50-stack or the ?-stack of cards!

In Treatments KC and UC the first part was followed by the following text:

Next the experimenter will give you two stickers, one with a **cross** on it, and one with a **circle** on it. He will also give you €10 to be used during the game. Please attach each sticker to one of the DVDs **as you like**. The symbol (cross or circle) has nothing to do with your preference between the movies.

Then the experimenter will offer you a **costly choice** to draw a card from either of two stacks of cards (you have to choose one and can use the €10 to pay for it): this card is used to determine which DVD you will win. This is done as follows:
On each card there are numbers 1 to 6 and either a cross or a circle next to each number. (See example card.) After drawing a card you will throw a six-sided die to determine the winning number and thereby the winning symbol, cross or circle. You obtain the DVD to which you attached the winning symbol before the game.

The two stacks of cards.
One stack of cards, called "50/50", contains cards that have **exactly three crosses and three circles** on the back, randomly distributed over the six numbers of the die. To draw a card from the 50/50-stack costs you **€9,70** of your €10 endowment (the rest is yours).
The other stack of cards, called "?", contains cards that have and an **unknown number of crosses and circles** on the back, but the sum of the number of the two symbols is equal to six again: that is, there are between zero and six crosses on the back, distributed randomly over the six numbers of the die, and
(6-(number crosses)) circles. To draw a card from the ?-stack costs you **€9,20** of your €10 endowment (the rest is yours).

Summary and timeline (see also illustration on next page): you receive two stickers and €10 → you attach the cross and circle sticker to the DVDs as you like → draw a card from 50/50-stack for €9,70 **or** from ?-stack for €9,20 and use the €10 to pay for it → throw the die and observe which symbol wins → take the DVD to which you before attached this symbol. End of the experiment.

Please carefully consider all the information given to you about the **chances** and the **prices** of the two stacks of cards, and your **personal preferences** between the DVDs, before making your choice between a draw from the 50/50-stack or the ?-stack of cards!

2.C Results of Experiment 2 if Indifferences are Excluded

We defined a subject as indifferent if either her valuation difference was zero *or* she explicitly announced to be indifferent in the unknown preference condition. In Treatments KS and KC a subject could therefore be indifferent only if her valuation difference equals zero, while in Treatment US and UC either condition could apply. This leads to relatively more indifferences in the unknown preference treatments. We chose this measure of indifference to restrict the data to subjects with a clear preference and make sure to eliminate any possible bias owing to indifferences. The following Table summarizes the results of the four treatments. It shows the percentage of subjects choosing the risky prospect.

	Same price	Ambiguous Card 50c Cheaper
Known Preference	Treatment KS (N=36) 69% risky card (>50%, p=0.014)	Treatment KC (N=28) 43% risky card (not significant)
Unknown Preference	Treatment US (N=29) 31% risky card (<50%, p=0.031)	Treatment UC (N=25) 20% risky card (<50%, p=0.002)

Table A.22: Percentage of Risky Choices without Indifferences

Tests are binomial. KS: Known preference with Same price; KC, US, and UC are defined similarly.

Excluding indifferent subjects, the average valuation difference between the two DVDs was slightly higher at €2.66, and there was no significant effect of known versus unknown preference on valuation differences. Excluding indifferent subjects does not lead to any relevant changes in the probit results:

Probit	Dependent variable: **choice of risky prospect**		
	I	II	III
Unknown	–0.3232 (0.0868)**	–0.339 (0.0873)**	–0.3578 (0.1003)**
price	–0.2094 (0.0917)*	–0.2149 (0.0931)*	–0.1512 (0.1196)
valuation difference (ex-post)			0.0202 (0.0239)
controls (gender, age)		yes	yes
# observations	118	117	90

Table A.23: Probit Regression without Indifferences

The table reports marginal effects; standard errors in brackets; * significant at 5% level, ** significant at 1% level, two-sided;

2. D Instructions Experiment 3

On the table in front of you there are two bags. Each of them contains 40 poker chips which can be red or green. Bag one (white) contains exactly 20 red and 20 green poker chips. Bag two (beige) contains an unknown proportion of red and green chips.

First you will be called upon to make two choices. You will be asked to choose the bag from which you want to draw. You will also indicate the color on which you want to bet. You will indicate the choice on the decision sheet.

The other people participating in the experiment will make a choice analogous to yours.

Second, when everybody has made his or her decision and indicated it on the decision sheet, you will be invited to announce your decision in front of the experimenter and the other people present, and to draw a chip from the bag you have chosen. If the chip you draw from the bag is of the color you have indicated, you will immediately be paid €15; if it is of the other color you receive nothing.

The order in which everybody announces his or her decision and draws from his or her preferred bag will be randomly determined. Chips that are drawn will immediately be replaced in the bag such that the proportions do not change for the next person.

After everybody has drawn from a bag, you will obtain the €10 from the first experiment, the €15 from the second experiment if you won them, and sign a receipt; then you can leave the room.

Please no conversations during the experiment!

Decision sheet

Choice Task:

Please indicate the bag you want to draw from:

O bag 1 (20 red and 20 green chips) or **O** bag 2 (unknown proportion)

Please indicate the color that you bet you will draw from <u>your chosen bag</u>:

O red chip or **O** green chip

Additional <u>hypothetical</u> question:

Imagine you had to pay for the right to participate in a draw from the above described bags with the possibility to win €15. How much would you pay for the right to participate in the prospects? Please indicate your valuations:

I would pay _____ € to participate in a draw from bag 1 (20 red and 20 green chips).

I would pay _____ € to participate in a draw from bag 2 (unknown proportion).

3. A Instructions Experiment 1 and 2

Both experiments' instructions started with the following description of prospects:

Consider the following two lottery options:

Option A gives you a draw from a bag that contains exactly 20 red and 20 green poker chips. Before you draw, you choose a color and announce it. Then you draw. If the color you announced matches the color you draw you win €50. If the colors do not match, you get nothing. (white bag)

Option B gives you a draw from a bag that contains exactly 40 poker chips. They are either red or green, in an unknown proportion. Before you draw, you choose a color and announce it. Then you draw. If the color you announced matches the color you draw you win €50. If the colors do not match, you get nothing. (beige bag)

In experiment 1 the subjects were then asked to make a straight choice and give their WTP for both options:

You have to choose between the two prospect options. Which one do you choose?

O Option A (bet on a color to win €50 from bag with 20 red and 20 green chips)

O Option B (bet on a color to win €50 from bag with unknown proportion of colors)

Additional <u>hypothetical</u> question:

Imagine you had to pay for the right to participate in the above described options with the possibility to win €50. How much would you maximally pay for the right to participate in the prospects? Please indicate your valuations:

I would pay €_____ to participate in Option A (bet on a color to win €50 from bag with 20 red and 20 green chips).

I would pay €_____ to participate in Option B (bet on a color to win €50 from bag with unknown proportion of colors).

In experiment 2 the subjects were asked to make a straight choice and 18 choices between sure amounts and the prospects:

Below you are asked to choose between the above two options and also to compare both options with sure amounts of money. Two people will be selected for real play in class. For each person one decision will be randomly selected for real payment as explained by the teacher.

[1, 2] You have to choose between the two prospect options. Which one do you choose?

O Option A (bet on a color to win €50 from bag with 20 red and 20 green chips)

O Option B (bet on a color to win €50 from bag with unknown proportion of colors)

Valuation of prospects.
Now determine your monetary valuation of the two prospect options. Please compare the prospect options to the sure amounts of money. Indicate for both options and each different sure amount of money whether you would rather choose the sure cash or try a bet on a color from the bag to win €50!

Option A (bet on color from bag with 20 red and 20 green chips to win €50) **or** sure amount of €:

[3]	Play Option A	**O**	**or**	**O**	get €25 for sure
[4]	Play Option A	**O**	**or**	**O**	get €20 for sure
[5]	Play Option A	**O**	**or**	**O**	get €15 for sure
[6]	Play Option A	**O**	**or**	**O**	get €10 for sure
[7]	Play Option A	**O**	**or**	**O**	get €5 for sure
[8]	Play Option A	**O**	**or**	**O**	get €4 for sure
[9]	Play Option A	**O**	**or**	**O**	get €3 for sure
[10]	Play Option A	**O**	**or**	**O**	get €2 for sure
[11]	Play Option A	**O**	**or**	**O**	get €1 for sure

Option B (bet on color from bag with unknown proportion of colors to win €50) **or** sure amount of €:

[12]	Play Option B	**O**	**or**	**O**	get €25 for sure

[13]	Play Option B	**O**	**or**	**O**	get €20 for sure
[14]	Play Option B	**O**	**or**	**O**	get €15 for sure
[15]	Play Option B	**O**	**or**	**O**	get €10 for sure
[16]	Play Option B	**O**	**or**	**O**	get €5 for sure
[17]	Play Option B	**O**	**or**	**O**	get €4 for sure
[18]	Play Option B	**O**	**or**	**O**	get €3 for sure
[19]	Play Option B	**O**	**or**	**O**	get €2 for sure
[20]	Play Option B	**O**	**or**	**O**	get €1 for sure

Make sure that you filled out all 18 choices on this page!

In both experiments we asked the following question at the end:

Please give your age and gender here:

Age:_____ Gender: male **O** female **O**

3.B Instructions Experiment 3

In experiment 3 the hypothetical WTP questions have been replaced by the following real payoff WTP decision using the BDM mechanism:

You have to buy the right to make a draw from the above described bags with the possibility to win 50€. The procedure we use guarantees that a truthful indication of your valuation is optimal for you, see details below at (*). How much do you maximally want to pay for the right to participate in the prospect options? Please indicate your offers:

I will pay €_____ to participate in Option A (bet on a color to win €50 from bag with 20 red and 20 green chips).

I will pay €_____ to participate in Option B (bet on a color to win €50 from bag with unknown proportion of colors).

*

The procedure is as follows: The experimenter throws a die to determine which option he wants to sell. If a 1,2, or 3 shows up, Option A will be offered; if a 4,5, or 6 shows up, Option B will be offered. After the option for sale has been selected, the experimenter draws a lot from a bag that contains 50 lots, numbered 1, 2, 3, …, 48, 49, 50. The number indicates the experimenter's reservation price (in Euro) for the selected option: if your offer is larger than the reservation price, you pay the reservation price only and play the option. If your offer is smaller than the reservation price, the experimenter will not sell the option. You keep your money and the game ends.

4.A: Risky choice pairs

I	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€72																			€0
B	€55																			

II	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€100		€0																	
B	€14																			

III	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€100																			€0
B	€78																			

IV	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€50									€0										
B	€21																			

V	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€200																		€0	
B	€131																			

VI	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€300										€0									
B	€20																			

VII	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€200										€0									
B	€76																			

VIII	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€50																		€0	
B	€37																			

IX	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€200										€0									
B	€100																			

X	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€200		€0																	
B	€20																			

XI	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€100														€0					
B	€52																			

XII	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€100										€0									
B	€36																			

XIII	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€50				€0															
B	€9																			

XIV	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€50				€0															
B	€25																			

XV	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	€96										€0									
B	€39																			

4.B Instructions Study 2 (one example)

In this experiment you are called upon to choose repeatedly between a lottery and a certain amount of money. For example, in the first choice-list below you are first asked to indicate whether you prefer obtaining €97 for sure or playing a lottery that gives you a 95% probability to win €100 or nothing otherwise (€0 with a 5% probability); the you should indicate whether you prefer €95 for sure or the same lottery, etc. You should indicate your preference for each line by crossing either the square to the left of the certain amount or the square to the left of the lottery.

Imagine you were to play these gambles for real money. Imagine also that for each choice-list one of the lines would be randomly selected, and then according to your choice you would either receive the sure amount or play the lottery. Please consider carefully the monetary amounts and probabilities, and then make your choices between the lottery and the certain amount for each line.

Choice-list 1

☐ €97 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise

☐ €95 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise

☐ €93 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise

☐ €91 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise

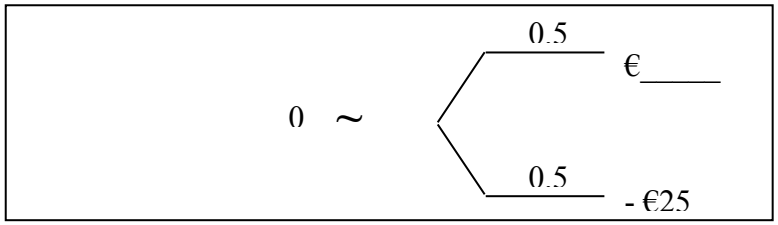
☐ €89 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €87 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €85 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €83 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €81 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €79 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €77 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €75 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €73 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €71 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise
☐ €69 for sure or ☐ a lottery with a 95% chance to win €100 and €0 otherwise

4.C Instructions Loss Aversion

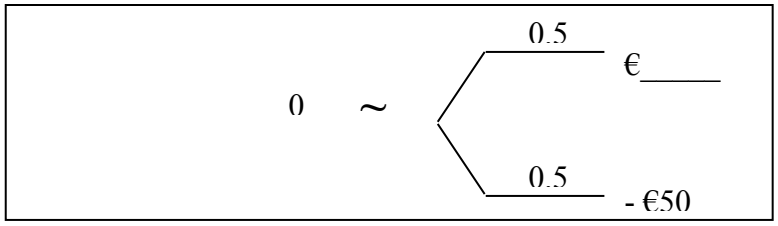
Below some pairs of gambles are presented to you. The pairs involve a tradeoff between a certain amount and a gamble, and in some cases between two gambles. Please fill in the amounts that are missing from the right-hand gamble that make the two gambles equally good for you. Imagine one of the two gambles in each pair would be randomly chosen by the experimenter for real play: what amount would make you indifferent between the left hand gamble and the right-hand gamble?

Gambles are described both verbally and graphically. In the graphical representation, ~ represents indifference (the two gambles are equally good for you). The gambles are represented by means of a ramification, where probabilities are indicated above each branch and amounts to be won or lost are indicated at the end. Please pay close attention to the amounts to be won and to the signs of the amounts, as both gains and losses are involved. Probabilities always stay at 0.5 in the gambles.

Obtaining 0 for sure and a gamble giving a loss of €25 and a gain of €_____, each with probability 0.5, are equally good for me.

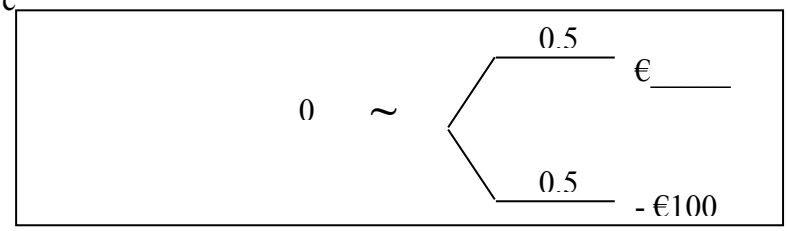


Obtaining 0 for sure and a gamble giving a loss of €50 and a gain of €_____, each with probability 0.5, are equally good for me.

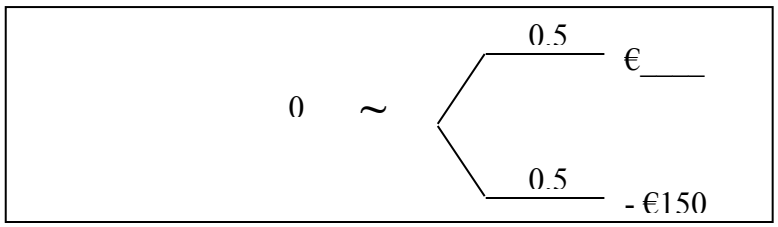


Obtaining 0 for sure and a gamble giving a loss of €100 and a gain of €_____, each with probability 0.5, are equally good for me.

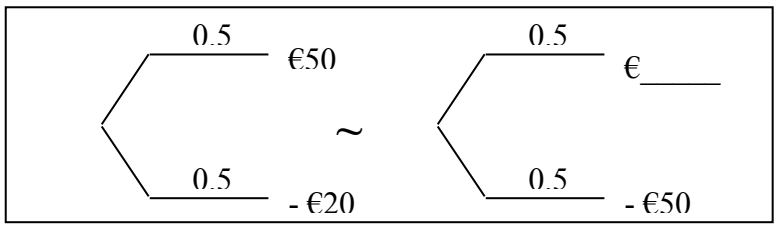
c



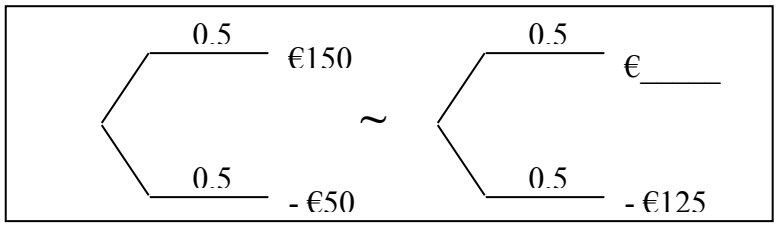
Obtaining 0 for sure and a gamble giving a loss of €150 and a gain of €_____, each with probability 0.5, are equally good for me.



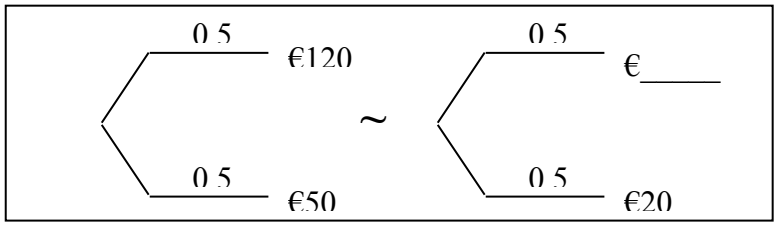
A gamble giving a loss of €20 and a gain of €50 each with probability 0.5 and a gamble giving a loss of €50 and a gain of €_____, each with probability 0.5, are equally good for me.



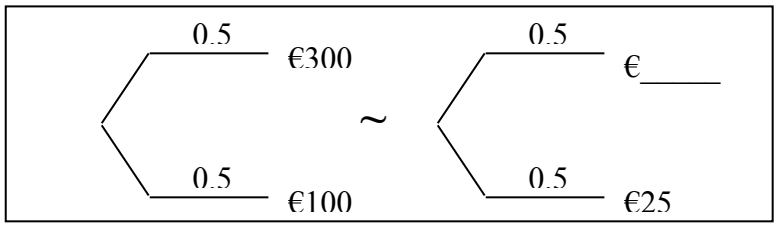
A gamble giving a loss of €50 and a gain of €150 each with probability 0.5 and a gamble giving a loss of €125 and a gain of €_____, each with probability 0.5 are equally good for me.



A gamble giving a gain of €50 and a gain of €120 each with probability 0.5 and a gamble giving a gain of €20 and a gain of €_____, each with probability 0.5 are equally good for me.



A gamble giving a gain of €100 and a gain of €300 each with probability 0.5 and a gamble giving a gain of €25 and a gain of €_____, each with probability 0.5 are equally good for me.



5.**A: Choices between Simple and Compound Prospects**

Below 6 hypothetical problems are presented to you. Each one of them involves choosing between an option that involves one single extraction from a bag and one that involves multiple extractions from a different bag. In the multiple extraction option, the poker chip you have extracted will be placed back in the bag and the chips in the bag will be mixed before you extract again, so as to keep the composition of the bag constant. This holds true for all the problems below. Please pay attention however to both the composition of the bags and the number of extractions, which vary across problems. Your answers will be completely anonymous.

Problem 1
Imagine you were given a choice between two options to win €20. Option A involves extracting one chip from a bag containing 10 red and 10 green chips. If you extract a red chip, you win €20; if you extract a green chip, you win nothing. Option B involves extracting 7 chips in sequence with replacement from a bag containing 18 red chips and 2 green chips. If all 7 chips extracted are red you win €20; if one or more of the chips extracted are green, you win nothing. What would you choose?

- Option A (extract 1 time from a bag with 10 red and 10 green chips, win if red)
- Option B (extract 7 times from a bag with 18 red and 2 green chips, win if 7 times red)

Problem 2
Imagine you were given a choice between two options to win €20. Option A involves extracting one chip from a bag containing 5 red and 15 green chips. If you extract a red chip, you win €20; if you extract a green chip, you win nothing. Option B involves extracting 5 chips in sequence with replacement from a bag containing 15 red chips and 5 green chips. If all 5 chips extracted are red you win €20; if one or more of the chips extracted are green, you win nothing. What would you choose?

- Option A (extract 1 time from a bag with 5 red and 15 green chips, win if red)
- Option B (extract 5 times from a bag with 15 red and 5 green chips, win if 5 times red)

Problem 3
Imagine you were given a choice between two options to win €20. Option A involves extracting one chip from a bag containing 5 red and 15 green chips. If you extract a red chip, you win €20; if you extract a green chip, you win nothing. Option B involves extracting 7 chips in sequence with replacement from a bag containing 16 red chips and 4 green chips. If all 7 chips extracted are red you win €20; if one or more of the chips extracted are green, you win nothing. What would you choose?

- Option A (extract 1 time from a bag with 5 red and 15 green chips, win if red)
- Option B (extract 7 times from a bag with 16 red and 4 green chips, win if 7 times red)

Problem 4
Imagine you were given a choice between two options to win €20. Option A involves extracting one chip from a bag containing 2 red and 18 green chips. If you extract a red chip, you win €20; if you extract a green chip, you win nothing. Option B involves extracting 4 chips in sequence with replacement from a bag containing 10 red chips and 10 green chips. If all 4 chips extracted are red you win €20; if one or more of the chips extracted are green, you win nothing. What would you choose?

- Option A (extract 1 time from a bag with 2 red and 18 green chips, win if red)
- Option B (extract 4 times from a bag with 10 red and 10 green chips, win if 4 times red)

Problem 5
Imagine you were given a choice between two options to win €20. Option A involves extracting one chip from a bag containing 4 red and 16 green chips. If you extract a red chip, you win €20; if you extract a green chip, you win nothing. Option B involves extracting 6 chips in sequence with replacement from a bag containing 15 red chips and 5 green chips. If all 6 chips extracted are red you win €20; if one or more of the chips extracted are green, you win nothing. What would you choose?

- Option A (extract 1 time from a bag with 4 red and 16 green chips, win if red)
- Option B (extract 6 times from a bag with 15 red and 5 green chips, win if 6 times red)

Problem 6
Imagine you were given a choice between two options to win €20. Option A involves extracting one chip from a bag containing 6 red and 14 green chips. If you extract a red chip, you win €20; if you extract a green chip, you win nothing. Option B involves extracting 2 chips in sequence with replacement from a bag containing 10 red chips and 10 green chips. If all 2 chips extracted are red you win €20; if one or more of the chips extracted are green, you win nothing. What would you choose?

- Option A (extract 1 time from a bag with 6 red and 14 green chips, win if red)
- Option B (extract 2 times from a bag with 10 red and 10 green chips, win if 2 times red)

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Samenvatting in het Nederlands

Dit preofschrift onderzoekt de relevantie van sociale invloeden als *accountability*, de verwachting van de besluitvormer dat zij haar beslissingen misschien moet verdedigen, op individuele besluitvorming. Deze sociale invloeden worden daarbij afgezet tegen marktinvloeden, en de relevantie van deze twee wordt vergeleken. Hoofdstuk 1 geeft een gedetailleerde behandeling van sociale invloeden, en geeft een algemene overzicht van dit proefschrift.

Hoofdstuk 2 presenteert een tal experimenten, die onderzoeken wat de oorzaak van *ambiguity aversion*, de voorkeur voor bekende over normatief equivalente onbekende kansen, is. We vinden als belangrijke oorzaak *fear of negative evaluation*, dat wil zeggen de angst van mensen om negatief beoordeeld te worden als men verlies zou lijden in een proces met onbekende kansen. Als niemand anders kan weten of men wint of verliest, dan hebben mensen geen speciale afkeer meer tegen onbekende kansen. Ook vinden we dat een keuze van het proces met de bekende kansen samenhangt met de persoonlijkheid-factor van *fear of negative evaluation*.

Hoofdstuk 3 gaat verder met het onderwerp van *ambiguity aversion*, en presenteert een nieuwe form van preferentie-omkering. Als mensen gevraagd worden om tussen twee processen te kiezen om een prijs te gewinnen, kiezen de meesten liever het proces met de bekend kansen dan het proces met de onbekende kansen. Wij vinden dat zelfs mensen die de onbekende kans gekozen hebben vaak een *hogere* prijs voor de loterij met de bekende kansen bieden als ze gevraagd worden de loterijen te kopen. Dit impliceert een preferentie-omkering. Wij vinden dit resultaat in meerdere experimenten en concluderen dus dat het stabiel is. Een theorie wordt gepresenteerd welke het verschijnsel verklaart door verlies-afkeer.

Hoofdstuk 4 onderzoekt de invloed van *accountability* op gedrag in risico-situaties. Risico attitude wordt in drie onderdelen ontleed: een nuts-functie voor de waarde van aanwinsten, kans-attitude, en verlies-afkeer (*loss aversion*). We vinden dat *accountability* geen invloed heeft op nuts-functies en op kans-attitude, maar wel op verlies-afkeer. Dit gebeurt omdat deelnemers wel aanvoelen dat verlies afkeer eigenlijk niet normatief is, en dus een lagere verlies- afkeer aangeven als ze hun antwoorden voor iemand moeten verdedigen.

Het vijfde en laatste hoofdstuk gaat over een methodologische punt. In de onderzoek

naar het effect van real incentives (mensen krijgen echt de consequenties van hun beslissingen uitbetaald) in experimenten werd *accountability* vaak samen met aansporingen veranderd. Probleem is daarbij dat eventuele resultaten niet duidelijk aan een of de andere invloed kunnen worden toegewezen, maar alleen maar aan de twee invloeden samen. Met een experimentele opzet die de twee factoren (real incentives en accountability) duidelijk onderscheidt, wordt getoond hoe de twee elementen vaak verschillende effecten kunnen hebben. Dat leidt tot de conclusie dat vele traditionele resultaten over de invloed van real incentives op besluiten opnieuw onderzocht moeten worden.

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