Economic decisions under uncertainty are often influenced by psychological factors. The studies reported in this thesis empirically identify psychological influences on decisions and suggest theoretical models to incorporate them into formal economic analyses. Effects on individual, strategic and market decisions are considered. The two main areas of examination are the perception of inequality in situations involving risk, and the role of ambiguity where no probabilities of events are known to the decision maker.

Stefan Trautmann (1977) holds a Diplom in Quantitative Economics from the University of Kiel (Germany) and an MPhil in Economics from the Tinbergen Institute. He worked on his PhD thesis at the Erasmus University Rotterdam under the direction of Peter Wakker and Han Bleichrodt. As part of his PhD training, Stefan spend time as a visiting researcher at the Center for Research in Experimental Economics and Political Decision Making in Amsterdam, the Tilburg Institute for Behavioral Economics Research, and the Kennedy School of Government in Cambridge (MA).

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Stefan Tobias Trautmann

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Uncertainty in Individual and Social Decisions

Theory and Experiments

Stefan Tobias Trautmann

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UNCERTAINTY IN INDIVIDUAL AND SOCIAL DECISIONS: THEORY AND EXPERIMENTS

Individuele en sociale beslissingen bij onzekerheid: Theorie en experimenten

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Contents

Chapter 1

Introduction and Outline

In most decisions we have to choose between options that involve some uncertainty about their outcomes and their effect on our well-being. Casual observation and carefully controlled studies suggest that, in making these decisions, we often deviate from the benchmark of expected income maximization. This should not come as a surprise. Our well-being is affected by many factors, and the outside observer does not know the importance of various dimensions of the outcome to the decision maker. Even if goals are well defined, it is far from obvious that we succeed in choosing what is best for us. The psychological literature has shown deviations from optimal behavior in simple decision tasks, and we may expect similar deviations to occur in more complex real life problems. In real life situations, however, experience and market interaction will help to restrain suboptimal behavior.

This thesis examines deviations from expected income maximization in situations involving uncertainty. We focus on deviations generated by social factors. Deviations from income maximization due to social factors need not constitute mistakes. Investing money in the Chinese stock market is one thing. Investing in China and expecting to hear “I told you” from friends in the case of losses is another. Losing money in China when everybody else wins money in Europe is yet another. If we account for such factors, observed behavior can often be explained by maximization and optimal trade offs after all.

Other behavior is more difficult to reconcile with optimization. Preference reversals are situations where someone reveals different preferences between two options under different but theoretically equivalent elicitation procedures. Preference reversals may be due to mistakes and they then pose the difficult question what the decision maker's true preferences are. It can also be the case, however, that the theoretical assumptions underlying the equivalence of the two procedures are not valid. By identifying and modeling psychological factors that lead to preference reversals it is possible to explain and predict decisions under different procedures. As a prescriptive tool, identifying these factors may also help decision makers to determine what their true preferences are.

Individual decision situations are most suited for tests of violations of optimization and for identifying the nature and causes of these violations. Economists are often interested in outcomes of market interactions among many individuals, however. If deviations from optimal behavior can be identified in individual decision settings, it is therefore interesting to study whether they affect market outcomes, and vice versa. Markets provide strong financial and strategic incentives to reduce violations of rationality. Yet, market environments are also rich in factors that potentially bias decisions, and they might therefore create violations of optimality that are not observed in individual decisions. Such situations are examined in this thesis.

In decisions under uncertainty, the outcomes of some actions depend on uncertain events. For instance, you may take a day off on Friday to go to the beach and work on Saturday instead. The payoff from this switch in working days depends on the uncertain weather conditions. In the event of clear skies on Friday and rain on Saturday you get a high payoff from switching. Rain on Friday and sunny weather on Saturday gives you a low payoff. If objective probabilities of all relevant events are known we say the decision maker makes a decision under risk. If objective probabilities are unknown, she makes a decision under ambiguity.

The first three chapters of this thesis concern decisions involving ambiguity. It has often been found that people prefer risky options over ambiguous options, even if the ambiguous options are normatively at least as good as the risky options. This phenomenon is called ambiguity aversion. Psychological research has studied the causes of ambiguity aversion and has found that the possibility of negative evaluation by other people increases ambiguity aversion. Acting on the basis of little or no information about the probabilities of the different outcomes makes the decision maker more vulnerable to criticism if bad outcomes obtain. Chapter 2 introduces a new design to test whether a reduction in social evaluation reduces ambiguity aversion. In a situation where the possibility of blame is eliminated no ambiguity aversion is found.

In economics, ambiguity aversion is commonly modeled as an individual decision phenomenon. Social effects do not enter these models. Social effects on ambiguity may, however, be relevant for economic outcomes. To illustrate this point we discuss evidence on differences in investor behavior in traditional versus anonymous online discount brokerage accounts.

Chapter 3 concerns the strength of ambiguity aversion commonly observed in experiments. Many market phenomena that are anomalies from the point of view of expected utility can be explained by ambiguity aversion. Examples include the home bias in financial investments and the equity premium. Market regulations have been justified on the basis of ambiguity aversion: policies that reduce ambiguity in markets can increase participation from ambiguity averse individuals and thereby increase efficiency. Empirical measurement of the strength and the possible heterogeneity in ambiguity attitudes are therefore relevant to the evaluation of theory and policy. We show that the elicitation of individuals' willingness to pay for risky and ambiguous prospects leads to overestimation of ambiguity aversion compared to choice based evaluations, even generating preference reversals.

Chapter 3 presents a prospect theory based model that explains ambiguity aversion in willingness-to-pay by reference dependence and loss aversion. Reference dependence means that the decision maker evaluates outcomes as gains and losses relative to an arbitrary reference point. Our model assumes that agents take the risky prospect as a reference point when determining their willingness to pay for the ambiguous prospect and therefore frame outcomes under the ambiguous prospect as gains or losses from the point of view of the risky prospect. The model suggests that if loss aversion is widespread and strong, willingness-to-pay measures too much susceptibility to ambiguity aversion.

Chapter 4 studies ambiguity aversion in first-price sealed-bid auction markets where participants have to select between entering either the market for a risky prospect or the market for an ambiguous prospect. Selection into markets is often observed in the real world because of capacity or budget constraints, or legal restrictions. For instance, in simultaneous procurement auctions for oil tract leases a company that has capacity constraints will have to decide to bid for one of the tracts only.

In our experimental markets, the participants' decisions are influenced by their ambiguity attitude and by strategic considerations based on expectations about other participants' ambiguity and risk attitudes. We find that fewer participants select into the markets for the ambiguous prospect than for the risky prospect. Although the markets for ambiguous are consequently much smaller, the prices are equal in the two markets. Two side experiments show that risk and ambiguity aversion are positively correlated and that participants anticipate this correlation. In markets with selection this leads ambiguity averse bidders to bid for risky prospects for which they expect more competition, but competition from less risk prone people. This effect cannot be observed in markets where selection is precluded and each participants can bid for all prospects. Heterogeneity and correlation of risk and ambiguity attitudes makes the effects of ambiguity aversion in markets dependent on the market institution.

Chapters 5 to 7 deal with decisions under risk. In many empirical studies, participants are given experience and learning opportunities to study thoughtful decisions. In Chapter 5 the opposite approach is used. We let the participants make decisions under time pressure to get some insights into the decision making under risk. As expected, we find strong effects in decisions where perceptual factors matter a lot, such as decisions involving losses. More surprisingly, under time pressure we find that aversion to losses and seeking of gains are increased simultaneously. This finding supports recent models of aspiration levels that consider a prospect's overall probability of winning and losing. Passing a certain payoff aspiration level, for instance the initial wealth or some payoff goal, leads to a discrete upward jump in the utility of the payoffs. These models can explain simultaneous loss aversion and its counterpart, gain seeking. Our results, therefore, suggest that aspiration level models may serve as a good description of behavior in more general situations where people make decisions under stress.

Chapters 6 and 7 deal with the interaction of risk and fairness. Much evidence has been collected showing that people have a preference for fairness and equality, and are willing to forgo monetary payoffs to make allocations more fair. In the presence of risk it may not necessarily be the fairness of the final allocation that matters to people most, but the fairness of the allocation process. Chapter 6 introduces a tractable model that formalizes the idea of process fairness and allows its inclusion into economic theories. The model complements existing models of preferences for fair final outcomes. Applications to problems in economics and social choice show how the model can explain and predict individual and group preferences.

Chapter 7 argues that individual fairness preferences can be incorporated in utilitarian social welfare evaluations to remedy the absence of fairness considerations in utilitarianism. Utilitarianism implies the summation of individual utilities to calculate social welfare and therefore precludes preferences of the social planner. The proposed approach bases welfare evaluation on observable individual preferences and avoids imposing arbitrary fairness norms through the social welfare function. We apply process and outcome fairness models to reconsider widely discussed criticisms of utilitarianism.

The final chapter takes a different focus. It studies market behavior under conditions where market participants are uncertain about their true valuation of some good. It has been argued in the theoretical and the empirical literature that in such cases external cues become relevant as reference points for people's valuations. In particular, in auctions the reserve price set by the seller has been suggested as a reference point that influences bidders' valuations. Irrespective of their economic valuation of the good, it hurts buyers if they pay more than the reserve price. Larger bids will therefore decrease the bidder's surplus because of the monetary cost and because of the psychological cost of increasing the difference between the reserve price and the actual bid. This implies that sellers can evoke higher bids by setting a higher reserve price, reducing the difference between the reserve and any bid above the reserve.

We test the hypothesis that reserve prices serve as reference points using a sample of trades from online (English) auctions for football players at *hattrick.org*, an open-ended football manager game played by roughly a million people through the internet. Employing OLS regression we replicate the finding of a strong reference point effect reported in the literature. In We then show that this result is affected by biases caused by censoring and endogeneity, and may have been falsely interpreted as a psychological effect. We split the effect of the reserve price into a psychological factor, the reference point effect, and a mechanical effect of surplus appropriation by the seller that occurs if the reserve price is set between the highest and second highest (potential) bidders' valuations. When controlling for the mechanical effect that is predicted by auction theory, and for censoring and endogeneity, we find no reference point effect.

Chapter 2

Causes of Ambiguity Aversion: Known versus Unknown Preferences

Ambiguity aversion appears to have subtle psychological causes. Curley, Yates, and Abrams found that the fear of negative evaluation by others (FNE) increases ambiguity aversion. This chapter introduces a design in which preferences can be private information of individuals, so that FNE can be avoided entirely. Thus, we can completely control for FNE and other social factors, and can determine exactly to what extent ambiguity aversion is driven by such social factors. In our experiment ambiguity aversion, while appearing as commonly found in the presence of FNE, disappears entirely if FNE is eliminated. Implications are discussed.[1]

2.1. Introduction

In decision under uncertainty people have been found to prefer options involving clear probabilities (*risk*) to options involving vague probabilities (*ambiguity*), even if normative

[1] This chapter is based on Trautmann et al. (2007a).

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 and Weber 1993) and among business owners and managers familiar with decisions under uncertainty (Chesson and Viscusi 2003). People are often willing to spend significant amounts of money to avoid ambiguous processes in favor of normatively equivalent risky processes (Becker and Brownson 1964, Chow and Sarin 2001, Keren and Gerritsen 1999).

Curley, Yates, and 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 Fox and Tversky (1998), Fox and Weber (2002), and Heath and Tversky (1991), showing that ambiguity aversion increases with the perception that others are more competent and more knowledgeable. If people choose an ambiguous option and receive a 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. This explains the enhanced ambiguity aversion. We will call such social effects *fear of negative evaluation* (*FNE*), borrowing a term from psychology (Watson and Friend 1969). A detailed review of the literature on FNE for ambiguity will be presented in Section 2.1.

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 the subjects' private information that cannot be known to the experimenter or to other people unless the subjects explicitly reveal 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 preferences between the DVDs unobservable, 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 is commonly found. Thus, our finding adds to the aforementioned studies showing how important social factors are for ambiguity aversion.

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

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 and Weber 1992, Gilboa 2004, Mukerji and Tallon 2001). A famous example is the home bias in consumption and financial investment (French and Poterba 1991). Implications of our findings regarding FNE for such phenomena will be discussed in Section 2.5.

This chapter proceeds as follows. The next section discusses the FNE hypothesis and its literature. Section 2.2 presents a replication of the Curley, Yates, and 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. Finally, Section 2.6 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 and Baron 1988). This happens in particular if an alternative process with a higher perceived informational content is available (Chow and Sarin 2001, Fox and Tversky 1995, Fox and 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 and Tversky 1991, Taylor 1995) or observes the decision (Chow and Sarin 2002). In Ellsberg's (1961) example the effect leads to preference for the urn with a known probability of winning, about which subjects feel more knowledgeable.

A preference for the more informative process may be explained by 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 (Shafir et al. 1993, Simonson 1989, Lerner and Tetlock 1999). In this way they minimize the risk of being judged negatively by others on their quality as decision makers.

Choosing the unfamiliar process entailed by the ambiguous urn may lead to embarrassment if a losing outcome should obtain (Ellsberg 1963, Fellner 1961, Heath and Tversky 1991, Roberts 1963, Tetlock 1991, Toda and 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 and 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 has yet to materialize (Brun and Teigen 1990, Rothbart and Snyder 1970). It is also consistent with people's unwillingness to act on the basis of ambiguous information (van Dijk and Zeelenberg 2003).

A decision based on more information is generally perceived as better (Tetlock and 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 and Gerritsen 1999). Kocher and Trautmann (2007) find that people correctly anticipate these negative attitudes towards ambiguity. If a bad outcome were to result from a prospect about which an agent had comparatively little knowledge, her failure may be blamed on her incompetence or 'uninformed' choice (Baron and 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 and Tversky 1991, Toda and 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, and 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). First, however, we present an experiment that replicates the findings of Curley, Yates, and 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 chapter because there usually is a clear direction of prediction with a one-sided alternative hypothesis. All results in this chapter 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).

Nineteen 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). Twenty-two 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). Even imagined social encounters have been shown to be sufficient to induce embarrassment and FNE (Dahl et al. 2001, Miller and 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, could FNE 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 Erasmus University Rotterdam in the Netherlands. Most of these 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 greater 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 prospect to win one of the two DVDs. A detailed description of the lottery mechanism is given later. We conducted four treatments that differed with respect to the experimenter's knowledge of the subjects' preference between the two DVDs and to whether there was a price difference between the risky and the ambiguous prospect (the ambiguous card was 50 cents cheaper). Table 2.1 shows the organization of the four treatments. It also indicates the total number of subjects in each treatment and in parentheses the number of students from Erasmus University Rotterdam.

Table 2.1: 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))

KS: Known preference with Same price (i.e., the cell in the second column and the second row); KC, US, and UC refer to the other cells.

Treatment KS replicates the classic Ellsberg (1961) example with known preference and a simple choice between the risky and the ambiguous prospect. At the beginning of the instructions the 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 prospect. 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 were identical for both treatments.

In Treatment KC we endowed subjects with €10 from which they had to buy either the risky prospect for €9.70 or the ambiguous prospect for €9.20, making the ambiguous choice 50 cents cheaper. They were allowed to keep the rest of the money. Preferences were known (same instructions as in Treatment KS). In Treatment UC the ambiguous prospect was again 50 cents 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 prospect (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), and included 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 the subjects had clear preferences between the DVDs.

Lottery Mechanism. The lotteries were conducted as follows. First, the 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 prospect and the other one the ambiguous prospect. 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 prospect the subjects knew that there were exactly three Xs and three Os on the back of each card. In the ambiguous prospect they did not know the number of Xs and Os on cards, and they only knew that there were between zero and six Xs and a complementary number of Os on each card.

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

The mechanism just described 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 prospect. The latter holds the more so as the 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 prospect is possible and did occur for some subjects (details on the measurement of indifference are given in the appendix). This section presents the results including all data. Excluding indifferences from the analysis does not qualitatively change the results (see appendix).

Table 2.2 summarizes the results of the four treatments. It shows the percentage of subjects choosing the unambiguous prospect.

Table 2.2: Percentage of risky choices

	Same price	Ambiguous Card 50c Cheaper
Known Preference	Treatment KS 65% chose risky card (>50%, $p=0.04$)	Treatment KC 43% chose risky card (not significant)
Unknown Preference	Treatment US 33% chose risky card (<50%, $p=0.019$)	Treatment UC 17% chose risky card (<50%, $p=0.0002$)

Tests are binomial. KS: Known preference with same price; KC, US, and UC refer to the other cells.

In Treatment KS significantly more than half of the subjects chose the risky prospect over the ambiguous prospect. We, thus, find ambiguity aversion, in agreement with common findings. Making preference private information in Treatment US eliminates ambiguity aversion. Here, 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 ($t_{78} = 3.04$, $p = 0.0016$).

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.

Table 2.3: Probit regression over all four treatments

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

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

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 cents 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 2.3 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.

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.

Table 2.4: Analysis of ex-post questions

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	no	47	12
guess preference	yes	4	5

The numbers refer to numbers of subjects.

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 2.4.

2.4.3. Discussion of the 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 cents 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 card of the ambiguous prospect 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 preferences the proportion of subjects ready to forego 50 cents 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, then 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 50-50 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 the 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 for both 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 2.3.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. However, owing to the small number of indifferent subjects the effect is not significant. 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 in reducing their probability of winning (Ellsberg 1961, Viscusi and 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, and 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, therefore, that self-evaluation cannot account for our findings.

2.5. 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 and 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 of 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 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 aversion (>50%, $p = 0.0002$, binomial test). The median of the Leary FNE score was 37 on a scale from 12 (low) to 60 (high), and 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 cents per unit of the score ($p = 0.026$).

Table 2.5 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 an 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 ($t_{61} = -0.92$, $p = 0.1807$).

Table 2.5: Median split

	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%

For the low FNE group we observe a moderate but positive WTP difference and a majority of ambiguity averse choices. However, with a score 29 this group is still far from being immune to other-evaluation, 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.

2.6. Implications of Fear of Negative Evaluation

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 and Poterba 1991, Obstfeld and Rogoff 2000): people tend to invest and trade more in their own country than would 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 and Weber 2000, Uppal and 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 and Werner 1998). Additionally, we would expect that highly observable investments of otherwise large and sophisticated investors are more prone to home bias. Obstfeld and Rogoff (2000, p. 359) cite some evidence for this effect.

In our experiments we manipulated other-evaluation in simple laboratory decision tasks. It would 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 and Odean 2001, 2002). Such stocks are often associated with higher ambiguity in the finance literature. Konana and 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.”

2.7. 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 in which preferences between outcomes are private information, so that others cannot judge 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. This shows that FNE is more important than has commonly been thought and that it may even be necessary for ambiguity aversion to arise.

Appendix

2.A1. 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.A2. 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 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 KC 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 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 one of these stickers to each 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 each have **exactly three crosses and three circles** on the back, randomly distributed over the six numbers of the die, e.g. 1: x, 2: 0, 3: 0, 4: x, 5: 0, 6: x.

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 a complementary number of circles, e.g. 1: x, 2: 0, 3: 0, 4: x, 5: 0, 6: 0.

Summary and timeline: you receive the two stickers → you attach the cross and circle sticker to the DVDs as you like → draw a card from the`50-50-stack **or** from the ?-stack → throw the die and observe which symbol wins → take the DVD to which you 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 one of these stickers to each of the two 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 each have **exactly three crosses and three circles** on the back, randomly distributed over the six numbers of the die, e.g. 1: x, 2: 0, 3: 0, 4: x, 5: 0, 6: x. 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 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 a complementary number of circles, e.g. 1: x, 2: 0, 3: 0, 4: x, 5: 0, 6: 0. To draw a card from the ?-stack costs you **€9.20** of your €10 endowment (the rest is yours).

Summary and timeline: you receive two stickers and €10 → you attach the cross and circle sticker to the DVDs as you like → draw a card from the 50-50-stack for €9.70

or from the ?-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 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.A3. Results of Experiment 2 if Indifferences are Excluded

We defined a subject as indifferent if either her valuation difference was zero *or* she explicitly indicated that she was 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 we eliminated any possible bias owing to indifferences. Table 2.A1 summarizes the results of the four treatments. It shows the percentage of subjects choosing the unambiguous prospect.

Table 2.A1: Percentage of risky choices without indifferences

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

Tests are binomial. KS: Known preference with Same price; KC, US, and UC refer to the other cells.

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. Therefore, excluding indifferent subjects does not lead to any relevant changes in the probit results:

Table 2.A2: Probit regression without indifferences

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

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

2.A4. 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 choices on the decision sheet.

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

Second, when everybody has made his or her decisions and indicated them on the decision sheet, you will be invited to announce your decisions 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 decisions 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 it, 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 aforementioned 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)

Chapter 3

Preference Reversals to Explain Ambiguity Aversion

Preference reversals are found in measurements of ambiguity aversion even under constant psychological and informational circumstances. This finding complicates the study of what the “true” ambiguity aversion is. The reversals are not attributable to mistakes and concern reversals within one attribute (ambiguity perception). They are, thus, of a fundamentally different nature than known preference reversals in multiattribute or risky choice. The reversals can be explained by Sugden’s random-reference theory: loss aversion generates an overestimation of ambiguity aversion for willingness to pay. Hence, ambiguity aversion may be less strong than commonly thought.[2]

3.1. Introduction

One of the greatest challenges for the classical paradigm of rational choice was generated by preference reversals, first found by Lichtenstein and Slovic (1971): strategically irrelevant details of framing can lead to a complete reversal of preference. Grether and

[2] This chapter is based on Trautmann et al. (2007b)

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 and 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 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, and 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 and Abrams 1986), relative competence (whether or not there are others knowing more; Tversky and Fox 1995, Heath and Tversky 1991, Fox and Weber 2002), gain-loss framings (Du and Budescu 2005), and order effects (Fox and Weber 2002). Closer to the preference reversals reported in our paper is a discovery by Fox and Tversky (1995), that ambiguity aversion is reduced if choice options are evaluated separately rather than jointly (Du and Budescu 2005, Table 5, Fox and 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 and Götte (2007) and Myagkov and 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 and 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 3.2 presents our basic experiment, and our preference reversals. Section 3.3 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 3.4, showing that this aspect does not affect our findings. Section 3.5 considers a modification of the random lottery incentive system used and shows that this modification does not affect our basic finding either. Section 3.6 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 3.7. Section 3.8 discusses implications, and Section 3.9 concludes.

3.2. Experiment 1; Basic Experiment

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.

[3] This term is used in this chapter. 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.

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.

Incentives. 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 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.

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.

Table 3.1: Willingness to Pay in €

	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, $p < 0.05$	

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.

Table 3.2: Frequencies of WTP-Implied Choices versus Straight Choices

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

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).

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 3.7 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 reversals in Experiment 1 were observed without incentivized WTP and in a classroom setting. WTP with real incentives may differ from hypothetical WTP (Cummins, Harrison, and Rutström 1995, Hogarth and 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 3.7) 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.

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.

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

Table 3.3: CEs in €

	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	$t_{77} = 4.75$, $p < 0.01$	$t_{77} = 4.02$, $p < 0.01$	

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.

Table 3.4: Frequencies of CE-Implied Choice versus Straight Choices

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

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.

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 Willingness to Pay

Subjects. 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).

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

Table 3.5: Willingness to Pay (BDM) in €

	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	

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.

Table 3.6: Frequencies of WTP-Implied Choices (BDM) versus Straight Choices

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

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].

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

[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).

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

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

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.

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.

Table 3.7: Willingness to Pay in € when the Nonzero Prize is €15

	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, $p = 0.01$	

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

Table 3.8: Frequencies of WTP-Implied Choice (Lab) versus Straight Choices

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

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.

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 and van de Kuilen 2006, Cohen and Einav 2007, Donkers et al. 2001, Hartog, Ferrer, and Jonker 2002, Schubert et al. 1999).

Table 3.9 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 and Einav 2007). Booij and 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).

Table 3.9: Age and Gender Effects in the Pooled Data

	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

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 Willingness to Pay

Butler and 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 (Sugden 2003, Schmidt et al. 2005). 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.

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 et al. (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 further 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 and 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.

Straight Choice. We first consider straight choice. In later analyses we will consider subtracting a constant c from all payments, and for convenience we have written c already in Table 3.10. For the current analysis, c can be ignored, i.e., c=0. The following payoffs result under the four events.

Table 3.10. Payoffs for the Risky and the Ambiguous Prospect

	E_1 (B_RB_A)	**E_2 (B_RR_A)**	**E_3 (R_RB_A)**	**E_4 (R_RR_A)**
α	$x-c$	$-c$	$x-c$	$-c$
ρ	$x-c$	$x-c$	$-c$	$-c$

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

$$V(\alpha|0) = W(E_1 \cup E_3)U(x|0) \tag{3.1}$$

and

$$V(\rho|0) = W(E_1 \cup E_2)U(x|0) \tag{3.2}$$

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:

$$\text{Ambiguity seeking in straight choice} \Leftrightarrow W(E_1 \cup E_3) > W(E_1 \cup E_2). \tag{3.3}$$

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

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

generated through the unions with E_1, with different likelihood interactions between E_3 and E_1 than between E_2 and E_1.

Willingness to Pay and Loss Aversion. We next turn to the WTP evaluation task. Consider Table 3.10 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 3.10 is the reference point for the determination of the WTP for α. Consider the prospect in the upper row of Table 3.10, α with the WTP of ρ, c, subtracted. According to the theory of Schmidt et al. (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 is 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 decision weights, and empirical studies have not found big differences so far (Tversky and Kahneman 1992). (Note that loss aversion will be captured through a different parameter, namely λ.) 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, where the utility function depends only on an obtained and a counterfactual outcome for each event considered according to Schmidt et al. (2005).

$$\text{Ambiguity aversion in WTP} \iff W(E_3)U(x-c|-c) + \lambda W(E_2)U(-c|x-c) < 0. \tag{3.4}$$

Here λ is the loss aversion parameter, which usually exceeds 1 indicating an overweighting of losses. We next discuss utility U in some detail, and show that

$$U(x-c|-c) = -U(-c|x-c) \tag{3.5}$$

may be assumed. All cases considered in the literature are special cases of Sugden's

$$U(x|r) = \varphi(U^*(x) - U^*(r)).$$

In general, for moderate amounts as considered here, it is plausible that these functions do not exhibit much curvature, so that

$$U(x-c|-c) \approx x-c-(-c) = x \text{ and } U(-c|x-c) \approx -c-(x-c) = -x.$$

Then Eq. 3.5 follows. In prospect theory, outcomes are changes with respect to the reference point as in

$$U(x|r) = \varphi(x - r), \text{ which implies } U(x-c|-c) = \varphi(x) \text{ and } U(-c|x-c) = \varphi(-x).$$

Tversky and Kahneman (1992) estimated $\varphi(x) = x^{0.88}$ and $\varphi(-x) = -x^{0.88}$. Then Eq. 3.5 holds exactly, also for large outcomes. A similar assumption was central in Fishburn and LaValle (1988). Thus, we assume Eq. 3.5. We divide Eq. 3.4 by $U(-c|x-c)$, and get:

$$\text{Ambiguity aversion in WTP} \iff W(E_3) - \lambda W(E_2) < 0. \tag{3.6}$$

In the above analysis, given symmetry of colors, events E_2 and E_3 will have similar perceived likelihood and ambiguity. In Eqs. 3.4 and 3.5, 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. 3.6 reduces to:

$$\text{Ambiguity aversion in WTP} \iff 1 < \lambda. \tag{3.7}$$

This inequality is exactly what defines loss aversion. Because only single events play a role in Eq. 3.6 and no unions as in Eq. 3.3, ambiguity attitudes did not play a role in establishing Eq. 3.7. 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 et al. (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 due to loss aversion.

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, and 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, and 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 and Weber (1995) found similar ambiguity aversion for WTA as for WTP.

Fox and 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 (Myagkov and Plott 1997, Holt and Laury 2002, Harrison et al. 2002). Its equivalence to a single and payoff relevant decision task has been empirically tested and confirmed (Starmer and Sugden 1991, Hey and Lee 2005). 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 and Tversky 1995, Fox and Weber 2002). Fox and Tversky (1995) similarly found strong ambiguity aversion under joint evaluation, with ambiguity aversion even disappearing under separate evaluation. Du and 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 (Sugden 2003, Schmidt et al. 2005). Our study suggests that the often used willingness to pay measurements overestimate ambiguity aversion.

Appendix

3.A1. 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 hypothetical 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) **<u>or</u>** 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) **<u>or</u>** 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.A2. 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.

Chapter 4

Selection in Markets for Risky and Ambiguous Prospects

We study selection into first-price sealed-bid auctions for a risky prospect (known probabilities) or an ambiguous prospect (unknown probabilities). Most subjects chose to submit a bid for the risky prospect, leading to thinner markets for the ambiguous prospect. Transaction prices for both prospects were equal although subjects expected the markets for the ambiguous prospects to be smaller. Evidence of a positive correlation between risk attitude and ambiguity attitude suggests that the markets for the ambiguous prospect were populated by relatively risk tolerant bidders. A control experiment with selection in a simple choice task shows that subjects correctly anticipate the effects of selection on market size and risk attitudes.[6]

4.1. Introduction

In decision under uncertainty people have been found to prefer prospects involving known probabilities (risky prospects) to prospects with unknown probabilities (ambiguous

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[6] This chapter is based on Kocher and Trautmann (2008).

prospects), even if normative theory (Savage 1954) prescribes indifference. This phenomenon is called ambiguity aversion (Ellsberg 1961). The idea that agents treat decisions under risk differently from decisions under ambiguity dates back to Keynes (1921) and Knight (1921), and numerous studies have demonstrated the empirical relevance of ambiguity aversion. People are often willing to spend significant amounts of money to avoid ambiguous prospects in favor of normatively equivalent risky prospects (Camerer and Weber 1992, Charness and Gneezy 2003, Gilboa 2004, Halevy 2007).

In the real world, decisions under uncertainty often involve only vague probabilities, making ambiguity aversion a possible cause for observed market phenomena which are anomalies from the point of view of expected utility theory. Ambiguity aversion has been incorporated in theoretical models to explain such anomalies. Examples include the home bias in investment and consumption (Uppal and Wang 2003, Huang 2007), the equity premium puzzle (Maenhout 2004), underdiversification of incomes risks (Mukerji and Tallon 2001), and overbidding in first price auctions (Salo and Weber 1995). There is, however, little direct evidence of the effect of individual ambiguity attitudes on market outcomes and, vice versa, of the effect of market interaction on ambiguity attitudes.

This paper studies the effect of selection into markets for risky and ambiguous prospects on market prices and market sizes in two experiments. Recent applications of ambiguity to financial regulation and investment behavior emphasize the effect of ambiguity aversion on self-selection and market size (Easley and O'Hara 2005, Zeckhauser 2006). Ambiguity averse agents shy away from markets for ambiguous investments, leading to reduced competition and lower prices. Often capacity constraints and participation restrictions exist in markets, for instance in simultaneous procurement bidding for multiple projects (Iyer and Pazgal 2007), allowing agents to operate only in one market. In such settings we expect strong selection effects if the agents have to choose between markets of different ambiguity. With selection, strategic considerations become relevant as well. If market participants expect competitors to stay out of ambiguous markets, then there is an incentive for them to enter these markets even if they preferred less ambiguous markets otherwise. Our results support these predictions. We find that selection affects market outcomes through risk and ambiguity attitudes, and through subjects' expectations of other participants' risk and ambiguity attitudes. The results also show that heterogeneity in risk and ambiguity attitudes becomes important because of a positive correlation between these attitudes that makes the effect of ambiguity on prices dependent on the market institution.

Markets for risky and ambiguous prospects have also been studied by Sarin and Weber (1993) and more recently by Bossaerts et al. (2007). Bossaerts et al. let their subjects trade securities with state-dependent payoffs where state probabilities were either risky or ambiguous. They find that equilibrium prices and security holdings are affected by ambiguity and report a positive correlation between ambiguity aversion and risk aversion, consistent with our result. Sarin and Weber report higher prices for risky prospects in first-price sealed-bid auctions and oral double auction markets. In their experiment there was no selection and all subjects bid in all markets. Considering their institutional setup, we will explain their results with our findings.

Camerer and Kunreuther (1989) and Di Mauro and Maffioletti (1996) study ambiguity aversion in insurance markets, i.e. the loss domain, and operationalize ambiguity through compound lotteries (second-order probabilities). Consistent with individual choice evidence on ambiguity attitude for losses (Hogarth and Kunreuther 1985, Smith et al. 2002), these insurance market studies find no effect of ambiguity. The studies discussed in our paper all consider Ellsberg-type uncertainty in the gain domain.

There has been much interest in allocation of projects through license auctions (Binmore and Klemperer 2002, Goeree 2003, Offerman and Potters 2006, Janssen 2006). The firms competing in these auctions bid for a prize with an uncertain outcome. Assuming that firms are risk averse, it has been shown that auctions have an effect on prices in the aftermarket by selecting the most risk seeking firms (Janssen and Karamychev 2007). The selection of relatively risk and ambiguity prone firms into auctions for aftermarkets that have more ambiguous payoff distributions implies that this effect is reinforced for ambiguity.

Ambiguity aversion is commonly modeled as an individual choice phenomenon. It has been shown in the literature, however, that social effects can often explain ambiguity attitude (Curley et al. 1986, Fox and Tversky 1995, Morris 1997, Fox and Weber 2002). Our results support this view. The ambiguity averse majority expect others also to be ambiguity averse and they avoid interaction with subjects who are not ambiguity averse even in the presence of strategic incentives to interact. In markets, the revealed ambiguity attitude is shaped by expectations about other participants' attitudes.

The next two sections present our main experiment in which we offer a large group of subjects the choice to bid from their own money for either a risky or an ambiguous prospect, using first-price sealed-bid auctions. As a real world analog consider the simultaneous auctioning of multiple oil tract leases in different regions. Some tracts might carry more ambiguous risks than others and each company can only bid for one lease because of capacity constraints to exploit tracts.[7] We observe strong ambiguity aversion leading to thinner markets for the ambiguous prospect, but equal transaction prices (maximum bids) for risky and ambiguous prospects. Using a measure of risk attitude from an unrelated decision task, we find that bidders in the market for the ambiguous prospect are less risk averse than those in the market for the risky prospect.

With such sorting in terms of risk attitudes, subjects' expectations about market sizes and about the other bidders' risk attitudes in the two markets affect choices. To isolate the effect of expectations about market size from expectations about sorting, we conduct a control experiment which we discuss in Section 4.4. In a simple choice task one person from the group choosing risky and one person from the group choosing ambiguous is randomly selected for real play. This setting is similar to lotteries for affordable housing. Eligible families can enter a lottery for the right to rent or buy a house at below the market rate, but may be constrained to submit only a limited number of applications. Houses may differ in the ambiguity of the risks they carry (quality of the house, the neighbors, the future development of the community). While the oil company in the above auction situation should clearly consider both the number and the aggressiveness of their competitors, the family in the housing market need only consider their expectations about how many people enter the lotteries for different houses. We find that with such pure size incentives the markets for the ambiguous prospect are no longer thinner that those for the risky prospect, consistent with the strategic incentives for subjects expecting a majority choice of risky.

[7] These risks are assumed to be independent of possible uncertainty about the yield of the tract and signals thereof, like political or environmental risk as modeled in Esö and White (2004).

Section 4.5 discusses our results and relates them to the literature. We also review the evidence about the correlation between risk and ambiguity attitude. The last section offers conclusions for ambiguity-based explanations of market anomalies.

4.2. Experiment 1 (Main Experiment): Design

Subjects. One hundred and seventy-six undergraduate students participated in eight laboratory sessions. In each session there were between 20 and 24 subjects. Students were recruited electronically from a pool of approximately 1200 potential participants and came from different fields of study. Each subject participated only once.

Payoffs. Each subject received a show-up payment of €5. In each session, two subjects could participate in a prospect that paid €30 in case of success and €0 otherwise. These two persons were determined by an auction. Each subject could bid from his or her own money for the right to play one of the two prospects. The two winners in each session had to pay their own bid immediately before playing. Subjects could also earn between zero and €200 from a risky decision task that preceded the auction. At the time of the auction subjects did not know how much they would win from the risky decision task. The auction experiment took approximately 30 minutes.

Procedure. Upon arriving at the laboratory subjects were randomly assigned to computers that are placed in individual cubicles. All parts of the experiment were computerized using the experimental software z-tree (Fischbacher 2007). All randomizations of prospects were conducted by throwing dice or drawing chips from a bag, at the subjects’ desks. Subjects first made decisions in the risky choice task. One decision would be selected for real payment, and the payoffs depended on subjects’ choices and chance. All subjects faced exactly the same decision tasks but the payoff-relevant task was chosen independently for each subject. Before the payoff-relevant decision was selected for each subject and the payoffs determined according to the subjects’ choices, we offered subjects the possibility to participate in a first-price sealed-bid auction for one of two prospects. At this point subjects had no information about their earnings from the risky decision task but each subject had received €5 for showing up on time. We offered to each group of 20 to 24 subjects the following two lottery options that represented the risky and the ambiguous urn in the Ellsberg (1961) two-color choice task:[8]

> "**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 to the experimenter. Then you draw. If the color you announced matches the color you draw, you win €30. If the colors do not match, you get nothing."
>
> "**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 to the experimenter. Then you draw. If the color you announced matches the color you draw, you win €30. If the colors do not match, you get nothing."

Option A offers a risky prospect with a probability of 50% for each color to be drawn. Option B offers an ambiguous prospect because the probabilities for each color to be drawn are unknown. Subjects had to choose the winning color, and it was clear that the experimenter had no possibility to influence the outcome of either option. If subjects are indifferent between betting on either color in option B, the two options are equivalent under expected utility. If subjects believe that there are more than 20 red chips in option B, this option is preferred to option A because it offers a higher expected utility by betting on red. A similar argument holds if subjects believe that there are more than 20 green chips in option B. The order and the letter for the two options in the instructions and on the computer screens were counterbalanced in half of the sessions.

It was explained to the subjects that only one option A and one option B was offered to the whole group and that two persons were determined by an auction to play the prospects, one for each prospect. Each subject could place a bid for *either* the risky option A *or* the ambiguous option B. This was done at the computer by first choosing one option and then placing a bid for that option. Subjects could only submit one bid. After the

[8] The description of the two options is taken from the instructions that were used in the laboratory. Complete instructions can be found in the Appendix.

auction winners were determined they would immediately pay their bid from their own cash and play the prospects at their desks. It was made clear to subjects that they could not use any advances from possible earnings from the experiment to pay their bid. Any bid between €0 and €30 could be submitted and change was available.

The one-shot auction design is applied because of the inherent uniqueness of ambiguous situations and because it avoids possible biases in learning with repeated auctions. Neugebauer and Selten (2006) have shown that feedback on other participants' bids affects bidding in repeated auctions when resale values of the prize are certain[9]. In particular, information about winning bids leads to overbidding. With risky and ambiguous prospects as prizes such effects may be even stronger, and they may potentially be different for the two prospects. Learning through repeated bidding with an immediate resolution of the prospects, on the other hand, is impossible with ambiguous prospects because subjects would learn about the probabilities of the ambiguous option, therefore eliminating the ambiguity.

After submitting their bid, but before learning the maximum bid in the auction in which they participated and whether they were the winner of this auction, subjects answered questions regarding their expectation of how many people would choose the auction for option A, and the expectation about their chances to win the €30 if they were *simply playing* option B. Both questions offered five brackets of 20% as answers: "less than 20%", "between 20% and 40%", "between 40% and 60%", "between 60% and 80%", "more than 80%". These wide brackets were chosen to make the task relatively easy for students and avoid any prominence of the 50% probability. The choices were scored on a five-point scale with 1 referring to the bracket with the smallest expected probability ("less than 20%").

Subjects knew the size of the whole group. Experimental instructions were distributed individually and read aloud to the subjects. Remaining questions were answered privately. After playing the prospects, the experimental earnings from the other decision task were determined. Subjects filled out a demographic questionnaire, were paid for the other task and dismissed from the laboratory.

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[9] Duwfenberg and Gneezy (2002) and Ockenfels and Selten (2005) find similar effects of feedback.

Risk attitudes. In the first part of the experiment subjects made choices between risky prospects. To compare subjects' risk attitudes we constructed a measure of risk aversion from six choices between sure payoffs and risky prospects. Three of the decision problems offer a choice between a risky prospect and its expected value (problems 1 to 3). The other three decision problems are adapted from prospect choices for which a preference of roughly 50% for each option has been found in previous studies (problems 4 to 6), and these choices are therefore likely to distinguish well between subjects (Wakker et al. 2007). Our measure of individual risk aversion is the number of safe choices in the six decisions made by a subject. The six prospects are displayed in Table 4.1, with x denoting a sure payoff and (x, p; y) denoting a prospect that pays x with probability p and pays y with probability $1-p$.

Table 4.1: Decisions Problems for Risk Attitude Measurement

	Risky decision problem					
	1	2	3	4	5	6
Sure option	€10	€13	€12	€14	€13	€11
Risky option	€20, 0.5; €0	€52, 0.25; €0	€15, 0.8; €0	€18, 0.95; €0	€32, 0.5; €0	€200, 0.05; €0

The prospects were played by the subjects for real payoffs using the random incentive lottery system (Starmer and Sugden 1991, Holt and Laury 2002, Harrison et al. 2007). Note that the €30 prize of the prospects sold in the auction lies within the range of payoffs of the prospects we use to measure individual differences in risk attitude.

4.3. Experiment 1: Results and Discussion

All tests in this chapter are two-sided tests unless stated otherwise.

4.3.1. Results

Market Outcomes. We find strong ambiguity aversion in our auction markets. Only 65 of 176 subjects (37%) chose to bid for the ambiguous prospect (p=0.001, binomial test). The details of the eight individual sessions are summarized in Table 4.2a and Figure 4.1.

The number of bidders in the market for the ambiguous prospect is smaller than the number of bidders in the market for the risky prospect in seven out of eight sessions and once it is equal. The percentage of bids for the ambiguous prospect ranges from 14% to 50%, making markets for the ambiguous prospect significantly thinner than markets for the risky prospect (p=0.003, Mann-Whitney test).

Table 4.2: Market Outcomes per Session

	Session number								
	1	2	3	4	5	6	7	8	Mean
a)									
Number of participants	21	21	21	20	24	24	24	24	22.38
Bidders in market for ambiguous prospect	5	3	10	9	7	12	11	8	8.13
Bidders in market for risky prospect	16	18	11	11	17	12	13	16	14.25
Transaction price ambiguous prospect in €	10.02	6.00	8.50	10.00	9.02	10.52	12.06	5.05	8.90
Transaction price risky prospect in €	7.80	10.05	11.00	7.20	14.00	6.35	8	10.01	9.30
b)									
Average risk aversion ambiguous market*	2.2	2.3	2.9	3.2	3.7	2.6	3.7	3.8	3.05
Average risk aversion risky market*	3.5	3.7	3.6	3.7	4	4.5	3.7	4.3	3.88

*Number of safe choices in six risky choice problems shown in Table 4.1.

Although competition is much lower for the ambiguous prospect, transaction prices (maximum bids) are equal for both prospects. The transaction price of the ambiguous prospect is higher than the price for the risky prospect in four out of eight sessions, and average transaction prices for the two options do not differ (p=0.834, Mann-Whitney test). This result is based on the observation of eight markets, but strong support comes from the analysis of the individual bids. The average and median bids are slightly higher for the ambiguous prospect with a mean of €4.30 (median €4.00), compared to a mean of €3.43 (median €3.05) for the risky prospect (p=0.039, Mann-Whitney test). Figure 4.2 shows the distribution of bids for both options. Small bids (≤€2) are much more frequent in the market for the risky prospect and large bids (>€8) are more frequent in the market for the ambiguous prospect. This observation suggests that there is a segmentation of subjects with different risk attitudes into the two markets, with less risk averse subjects choosing to bid for the ambiguous prospect.

Figure 4.1: Market Outcomes per Session

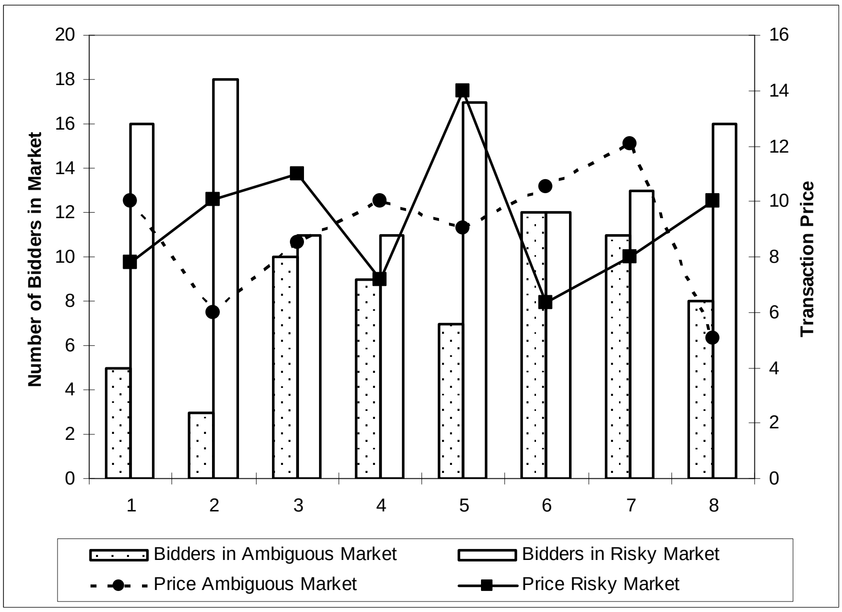
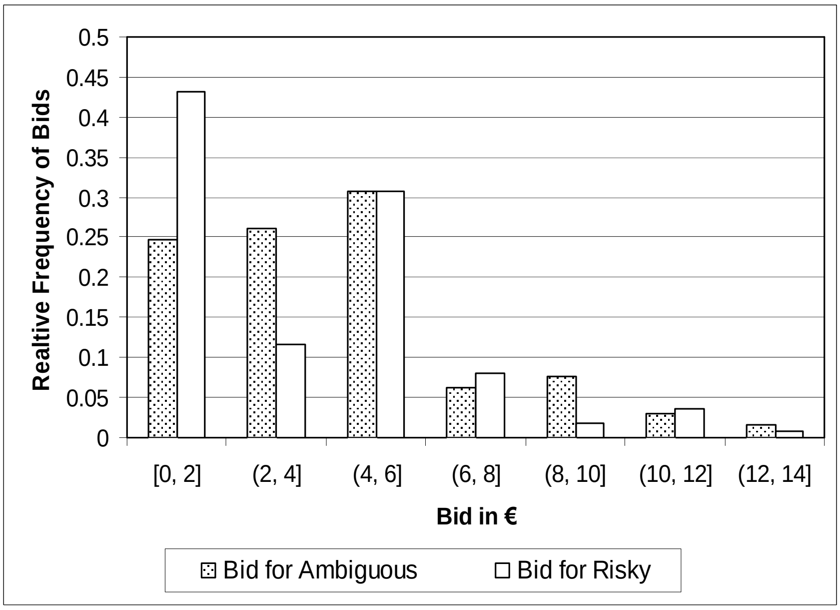


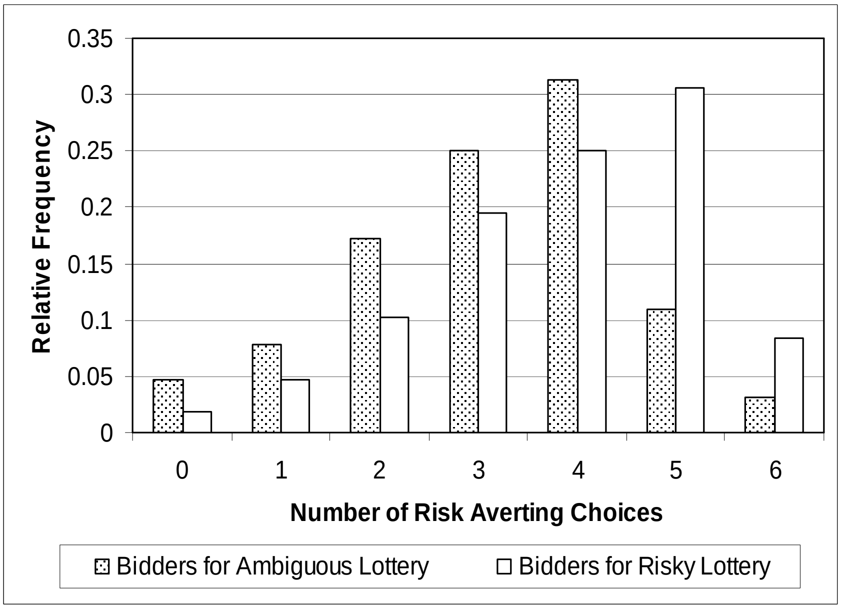
Figure 4.2: Distribution of Bids



The data from the independent risk attitude measurement confirm that subjects in the risky markets were more risk averse on average than subjects in the ambiguous markets (p=0.001, Mann-Whitney test). Table 4.2b shows that the average risk aversion in the ambiguous markets was smaller in seven of the eight sessions. Figure 4.3 gives the distribution of the number of safe choices for both options. Extreme levels of risk aversion with five or six risk averse choices are much more common for subjects in the market for the risky prospect, and risk seeking behavior with zero or one risk averse choice only is more common for subjects bidding for the ambiguous prospect. The probit regression in Model I of Table 4.3 shows that each risk averting choice increases the probability that a subject bids for the risky option by 7 percentage points, controlling for gender. The effect of risk attitudes is robust if we control for expectations about competition and about the winning chances of the ambiguous prospect (Table 4.3, Model II).

If selection of relatively risk tolerant subjects into the ambiguous prospect's market contributes to the equality of prices in the two markets, we would expect lower bids from more risk averse subjects. Linear regression results in Model III of Table 4.3 show that more risk averse subjects indeed submit lower bids in their market. Each risk averting choice decreases a subject's bid by €0.37.

Figure 4.3: Distribution of Risk Attitudes



Further evidence that countervailing effects of risk attitude and expected market size lead to equal transaction prices in the two markets comes from the analysis of the 16 auction winners. The winners of the auction for the risky prospect on average make 3.38 safe choices in the risky decision task, while winners of the ambiguous prospect make only 1.75 safe choices (p=0.062, Mann-Whitney test). However, the winners of the risky prospect expected strong competition in their market (score 4.38 or approximately 75% of all participants), while the winners of the ambiguous prospect expected relatively low competition (score 2.5 or approximately 40% of all participants) in their market (p=0.001, Mann-Whitney test). We conclude that the positive correlation between risk and ambiguity attitude leads to thinner markets with more risk prone subjects in the market for the ambiguous prospect than for the risky prospect. This market segmentation contributes to the equality of transaction prices for the two prospects in our experiment.

Table 4.3: Regression Analyses of Market Choices, Bids, and Transaction Prices

	Model I	Model II	Model III	Model IV
	Probit[b]: Choice of market for the risky prospect	Probit[b]: Choice of market for the risky prospect	OLS: Bid for prospect	OLS: Transaction price
Risk aversion	0.0733** (0.0269)	0.0654* (0.0274)	-0.3748* (0.1498)	
Expected market size risky prospect		0.0608 (0.0428)		
Expected chance to win with ambiguous prospect		-0.1419** (0.053)		
Expected market size own market[a]			0.2447 (0.2418)	
Market size				0.4253* (0.1739)
Choice of market for the risky prospect			-0.821 (0.5737)	-2.2003 (1.4414)
Female	0.1116 (0.0788)	0.0664 (0.0855)	-1.1793* (0.4564)	
# of observations[c]	172	172	172	16

Robust standard errors in parenthesis; *significant at the 5%-level, **significant at the 1%-level.
a: Equal to expected market size of the risky prospect for subjects bidding for the risky prospect and equal to one minus this expectation for subjects bidding for the ambiguous prospect.
b: Marginal effects reported.
c: Four subjects did not make a choice in at least one of the independent risky decision tasks and were excluded from the regression analyses.

Gender differences. Females are more likely to bid for the risky option (p=0.047, Fisher test). Controlling for risk attitudes in the probit regression (Model I in Table 4.3), the effect of gender becomes insignificant, however. We observe significantly lower bids by females after controlling for risk attitude (Model III in Table 4.3). Females do not differ from males in their expectations about the competitiveness of the two markets (p=0.628, Mann-Whitney test). This suggests that they differ from their male counterparts in some dimension that we do not observe and that has an effect on bidding, such as competitive behavior (Gneezy et al. 2003, Gneezy and Rustichini 2004) or loss aversion (Booij and van de Kuilen 2006).

Effects of Expectations. After submitting their bids, but before learning the result of the auction, we asked subjects to indicate their expectations about the competition in the market for the risky prospect and about the chances to win the ambiguous prospect. The latter question is used to approximate in how far subjects perceive the ambiguous prospect as an unattractive option. We would expect that the anticipation of strong competition for the risky prospect makes a bid for the ambiguous prospect more likely. Expecting strong competition in their own market should have a positive effect on subjects' bids in each market. Perceiving the ambiguous prospect as an unattractive option (equivalent to a low probability of winning the price) should make a bid for the risky option more likely.

On average subjects are well-calibrated in their expectations about the competition for the risky prospect. Very few subjects expect more competition for ambiguous, and on the five point scale with brackets of 20%, the average score is 3.8 and the median is 4. This indicates an expectation of about 65% choices for the market with the risky prospect (compared to a true average value of 63%). The probit regression in Table 4.3, Model II shows, however, that the effect of beliefs about the competition for the risky prospect is insignificant and points in the wrong direction. If anything, more expected competition for the risky option seems to increase the probability to bid this option. Figure 4.4 shows that this effect is driven by subjects in the market with the risky prospect holding beliefs of strong competition for this prospect. About 70% of bidders on the risky market expect a majority of participants in their market, and about 30% of bidders on the risky market expect more than 80% of the participants in their market. That is, these latter subjects prefer competing with 16 people in the risky market over competing with 4 people in the ambiguous market.

Figure 4.4: Market Choice and Expectations about Competition

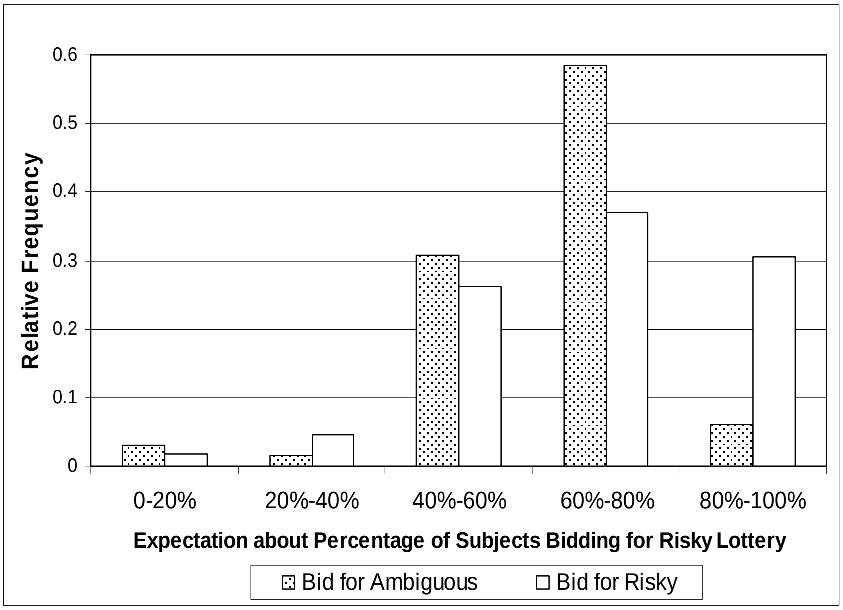
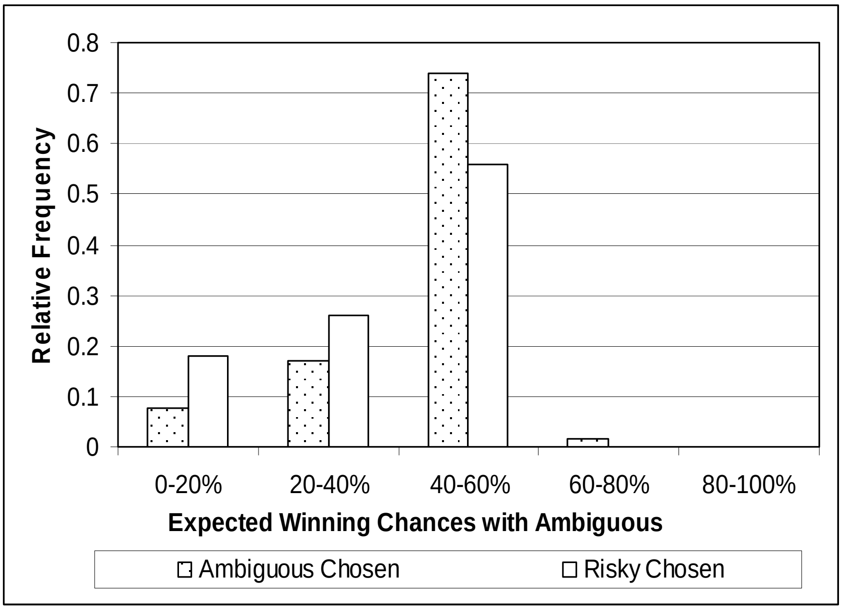


Figure 4.5: Expected Winning Chances with Ambiguous



In contrast to the expectations about competition, the attractiveness of the ambiguous option has the expected effect on market choices. Subjects who perceived the ambiguous prospect as less attractive were more likely to submit a bid for the risky prospect. Figure 4.5 shows, however, that while few people have a distinctly positive attitude towards ambiguity according to the question about winning chances, extremely negative attitudes are also rare for bidders in both markets.

Effects of Market Size. Market segmentation in terms of risk attitudes implies that, while transaction prices should be higher in larger markets for either urn, they should be lower for the risky markets when controlling for market size. Model IV of Table 4.3 reports linear regression results showing that an additional bidder in a market increases the transaction price by approximately €0.40. The transaction price is €2.20 higher in the ambiguous prospect's markets, but this effect is only marginally significant one-sided.

4.3.2. Discussion

We study the effect of selection in first-price auctions and find strong ambiguity aversion leading to smaller markets for ambiguous prospects than for risky prospects, but equal transaction prices in both markets. This result can be explained by a positive correlation between risk and ambiguity attitudes, and we find indeed that the markets for the risky prospect are populated by relatively more risk averse bidders than markets for the ambiguous prospect. Participants correctly anticipate the stronger competition in the risky markets. Therefore the countervailing effects of bidders' risk attitudes and the expected market sizes seem to have led to equal prices for both prospects. An ambiguity-neutral bidder who perceives both prospects as equally good would be indifferent between bidding for the risky or the ambiguous option in our market. There is no ambiguity premium to earn from the ambiguous prospect over and above the risk premium for the equivalent risky prospect.

We also observe many bidders for the risky prospect expecting strong competition in their market. These subjects may be extremely ambiguity averse, making the risky prospect more attractive despite the fierce competition and the strategic incentives to bid for ambiguous. Alternatively, they may correctly anticipate the presence of more risk tolerant bidders in the market for the ambiguous prospect, perceiving their chances to win the auction as being low even with little competition. If a company expects few but very aggressive competitors for an ambiguous project in a procurement bidding, it may well decide to bid for a less ambiguous project with many, but more cautious competitors. A third explanation may be that subjects simply did not understand the strategic incentives provided by possible differences in market size for the two prospects. Huberman and Rubinstein (2002) find evidence for such strategic mistakes in games where there is no rational explanation for subjects' actions given their beliefs.

In order to be able to distinguish between these explanations, we conducted a control experiment (experiment 2) with selection in a simple choice task that isolates the effects of market size.

4.4. Experiment 2: Isolating the Effect of Market Size

4.4.1. Design

Subjects. Another one hundred undergraduate students participated in five laboratory sessions. In each session there were 20 subjects. Students were recruited electronically from the same pool as in experiment 1. Each subject participated only once and had not participated in experiment 1.

Payoffs. Each subject received a show-up payment of €5 and could earn approximately €15 from an unrelated experiment. In each session, two subjects could earn up to €30 from the choice task described below. The whole experiment took approximately 15 minutes.

Procedure. The stimuli in this experiment were identical to those used in experiment 1, and we used the same descriptions of the risky and ambiguous prospects as in experiment 1. In each group of 20 participants subjects had to decide whether they wanted to play the risky option A or the ambiguous option B, under the condition that for each option exactly one subject was randomly selected to play his or her choice for real. That is, subjects' chances to be chosen for play depend only on the group sizes for options A and B. Subjects knew the size of the whole group.

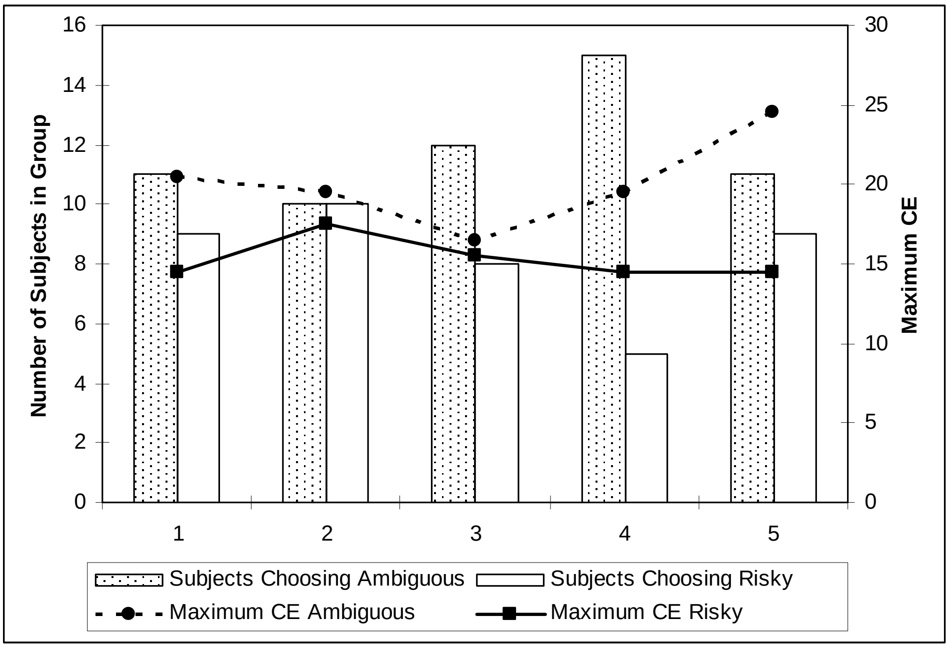
Subjects made their decision between option A and option B by choosing one of two decision sheets. On this decision sheet they then made 30 choices in a choice list between sure payoffs and the prospect they had chosen (see Appendix). With this list we elicit the subjects' *certainty equivalents* (CE) for their chosen option, that is, the sure amount of money that makes subjects indifferent between receiving the sure amount or playing the prospect they have chosen. The certainty equivalent was calculated as the midpoint between the two sure amounts for which a subject switched from preferring the sure money to preferring the prospect.

Subjects handed in their decision sheet in an envelope. One envelope was publicly drawn from the group of subjects choosing option A and one envelope from the group of subjects choosing option B. For these two subjects one of their decisions between a sure amount and their prospect was individually selected by drawing a numbered lot. According to the subject's decision in this choice problem the subject either received the sure amount or played the prospect.

4.4.2 Results

We find that 59 of 100 subjects choose the ambiguous option and we can therefore reject the null hypothesis of thinner markets for ambiguous (p=0.044, binomial test). The details of the five individual sessions are summarized in Figure 4.6.

Figure 4.6: Group Sizes and Maximum Certainty Equivalents in the Choice Task



The groups choosing the ambiguous option are significantly larger than the groups choosing the risky option (p=0.011, Mann-Whitney test). In each session, the maximum CE and the mean CE for the ambiguous option are larger than for the risky option. Pooling all sessions the mean CE equals €12.47 for the ambiguous prospect and €10.82 for the risky prospect (p=0.07, Mann-Whitney test).

4.4.3. Discussion

Experiment 2 isolates the effect of expectations about market size from the effect of expectations about other bidders' risk attitudes on market choice. In our choice task ambiguity averse subjects have an incentive to choose the ambiguous option if they expect that few people will do so. Low competition increases the chance to be randomly selected to play the ambiguous prospect, making this option more attractive than a low chance to be randomly selected to play the otherwise preferred risky option. In contrast to the market situation in experiment 1 the other participants' risk attitudes do not matter for the choice. In the auction in experiment 1 ambiguity averse subjects might choose to bid in the more competitive risky market if they expect the few bidders in the ambiguous market to be more risk tolerant. Our results from the simple choice task with selection in experiment 2 support this explanation. If other subjects' risk attitudes do not matter because of the random selection for real play, the majority of subjects choose the ambiguous prospect in most sessions.

Consistent with this explanation is the fact that maximum certainty equivalents are indeed larger for the ambiguous prospect in all sessions. The subjects with the maximum CE hold the highest valuation of the uncertain prize in the game, and in an auction these subjects would be among the highest bidders. This explains our observation of equal transaction prices for risky and ambiguous prospects, although markets were much thinner for the ambiguous prospect.

The observation of larger groups for the ambiguous option in experiment 2 rejects an explanation of market choices in experiment 1 that is based on strategic mistakes as in Huberman and Rubinstein (2002). Subjects react to strategic incentives, but they seem to apply a rather low level of strategic rationality[10]. As shown in experiment 1, most subjects expect the majority of other bidders to be ambiguity averse. In the simple choice task a majority of subjects chooses ambiguous, invalidating these expectations. The results also confirm that ambiguity aversion is widespread, but not very strong individually (see Figure 4.5). If ambiguity aversion were extreme, the higher probability of being selected for play would not make up for the low expectation of winning the prize from ambiguous for most subjects.

Note that subject's decisions in the simple random selection in experiment 2 are not equivalent to decisions in second-price auctions along the lines of experiment 1. Similar to our incentive compatible CE elicitation of the subjects' valuations of the uncertain prospects the second price auction elicits true valuations in the bidding stage. When deciding whether to enter the second-price auction for the risky or the ambiguous prospect, however, both expected market sizes and expected risk attitudes of the other participants matter. Market sizes and others' risk attitudes influence the subject's chances to win the auction and the price the subject has to pay for the prospect if she wins.

The fact that the influence of market size and risk attitudes on winning chances and price must be considered by the subjects in the decision between the prospects, but not in their bid, requires complex reasoning and makes the second-price auction with selection susceptible to errors. The first-price auction in experiment 1 more naturally involves consideration of market size and risk attitudes at both stages.

4.5. General Discussion

We chose a one-shot first-price auction to study market choices in the presence of ambiguity. This design provides a natural setting for ambiguity with strong strategic incentives. Our experimental data show that ambiguity aversion can persist in competitive markets and can affect market outcomes. This makes ambiguity aversion a potential explanation of market anomalies from the point of view of expected utility theory. We also

[10] Similar levels of rationality have been found for one-shot play or first rounds in other games, e.g. Stahl and Wilson (1994) and Nagel (1995).

find heterogeneity in ambiguity attitude and a positive correlation between attitudes to ambiguity and risk.

The existing evidence on the relation between risk and ambiguity aversion is mixed. Cohen et al. (1985) and Curley et al. (1986) find no relation between the two dimensions, and Potamites and Zhang (2007) find a weak positive correlation. These studies do not use independent tasks to measure risk and ambiguity attitude and they use only a single valuation task to measure each attitude. Presenting risky and ambiguous options jointly in one task will elicit evaluations that are mutually influenced by each other, leading to possibly biased estimates of the correlation. To illustrate the effect, consider a person who has a high ambiguity and risk aversion. Joint evaluation will make the risky option look more attractive because of the presence of the highly unattractive ambiguous option, inducing an artificially high valuation for the risky prospect. The observed degree of risk aversion for this person will be too low, reducing the observed correlation between risk aversion and ambiguity aversion. This effect has been reported by Fox and Tversky (1995, p. 590) and by Halevy (2007, p.532).

Measuring risk and ambiguity attitude by a single valuation task each is likely to yield data that suffer from measurement error, biasing the correlation between risk and ambiguity attitude downwards (Camerer and Weber (1992)). Using independent measures of risk and ambiguity attitude and multiple risky choices to measure risk attitude, Lauriola and Levin (2001) and Lauriola et al. (2007) find a significant positive relation between ambiguity and risk attitude. They show that the correlation is largely due to subjects with extreme risk and ambiguity attitudes. Figure 4.3 shows that this can also be observed in our data.

Further evidence for a positive correlation comes from investment experiments by Charness and Gneezy (2003) and by Bossaerts et al. (2007). In Charness and Gneezy's study the subjects could invest any fraction of a \$10 endowment in either a risky prospect or an ambiguous prospect similar to our options A and B, and keep the rest for sure. If they correctly guessed the color in their chosen prospect, the investment was multiplied by 2.5. Otherwise the investment was lost. Each subject made an individual decision and there was no strategic interaction. Charness and Gneezy find that the subjects who choose the ambiguous prospect invest significantly more than the subjects who choose the risky prospect.

Bossaerts et al. let their subjects trade in markets for risky and ambiguous state dependent assets and find that those subjects who hold more ambiguous assets also tend to hold more volatile portfolios. They argue that market segmentation in terms of risk and ambiguity attitudes can explain the value premium effect: value stocks tend to have higher returns than growth stocks (Fama and French 1998). If growth stocks are associated with higher ambiguity they attract more ambiguity tolerant investors who are also more risk tolerant. Growth stocks may then yield a lower premium than value stocks which are mainly held by ambiguity and risk averse investors. The effect depends on the assumption that ambiguity tolerant investors will not invest in value stocks and drive down returns there. This would be the case if the market were strictly segmented as in our experiment, suggesting that budget constraints and sunk cost of information collection are relevant in real world financial markets.

In our institutional setting, ambiguity leads to market segmentation in terms of risk attitude, and to equal prices for the risky and the ambiguous prospects, although the latter is perceived as less attractive by the majority of subjects. The importance of the market institution for market outcomes in the presence of ambiguity can be illustrated by a comparison of our results with those of Sarin and Weber (1993). Sarin and Weber conducted double-auction markets and first-price sealed bid auctions for risky and ambiguous Ellsberg urn prospects and consistently found lower prices for ambiguous prospects. This price difference can be explained by the fact that a selection of subjects with different ambiguity attitude into the markets for the two prospects was precluded by their market design. All subjects would always participate in each market, holding competition and average risk attitude constant in both markets. The relatively risk tolerant subjects who chose ambiguous in our experiment would bid for the risky prospect in Sarin and Weber’s experiment, driving up prices in this market. The relatively risk averse subjects who chose risky in our experiment would have to bid for the ambiguous prospect in Sarin and Weber’s experiment. These subjects are also ambiguity averse and therefore unlikely to submit high bids, leaving market prices unaffected. Positively correlated ambiguity and risk attitudes can explain both results when taking the different market institutions into account. The comparison of Sarin and Weber's and our results illustrates the importance of the markets size effect of ambiguity aversion that was emphasized by Easley and O'Hara (2005) and Zeckhauser (2006).

Ambiguity aversion is commonly modeled as an individual decision phenomenon. Several papers have argued, however, that ambiguity aversion strongly depends on social factors (Curley et al. 1986, Fox and Tversky 1995, Morris 1997, Fox and Weber 2002, Trautmann et al. 2007a). In markets the agents' revealed ambiguity attitudes depend on their expectations of other participants' attitudes toward risk and ambiguity, and these social effects will generally be more important than in individual decisions.

4.6. Conclusion

Ambiguity aversion has been used in the explanation of market anomalies, regulatory recommendations and investment advice. Our results show that ambiguity attitude is important for market outcomes. Because of the heterogeneity of risk and ambiguity attitudes and the correlation of these attitudes, the effects on market outcomes depend on the institutional setting. Extrapolating ambiguity effects from one market institution to another may give wrong predictions. Apart from their own ambiguity and risk attitudes, the expectations about other market participants' attitudes also influence the agents' decisions. Studying the interaction between agents' expectations and attitudes toward uncertainty in more complex markets seems a promising route to improve ambiguity-based explanations of market behavior and the resulting policy recommendations.

Appendix: Experimental Instructions

Extra Gamble Options!

We offer you the following extra gamble options before the payoffs of the experiment are determined and you receive your payoffs. Please read the descriptions carefully!

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 to the experimenter. Then you draw. If the color you announced matches the color you draw, you win €30. If the colors do not match, you get nothing.

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 to the experimenter. Then you draw. If the color you announced matches the color you draw, you win €30. If the colors do not match, you get nothing.

In experiment 1 the instructions proceeded as follows:

We offer only one option A and one option B to the whole group. We will determine the persons who play the options by an auction for each of the options. If you would like to play either of these options, you have to buy the right to play it from your own cash by making the largest bid for this option. If your bid wins the auction you have to pay your bid immediately and cannot use advance payments from possible earnings from the experiment. If there is more than one highest bid, the computer randomly determines one person as the winner of the auction.

You can place a bid for one option only! Thus you must decide for which option to bid and how much to bid for that option! You bid only once and privately, there are no repeated bids.

Procedure:

On the following screen you will first choose whether you want to bid for Option A or Option B.

Then you will submit a bid in Euro and Cent for that option. You can bid every amount between 0.00€ and 30.00€.

If you do not want to bid from your own money for the option to play one of the gambles, choose one option and make a bid of zero Euros. If there is no positive bid in this auction, you may still win and play with a zero bid.

The auction winners for Options A and Option B are determined. An experimenter will come to each auction winner, collect the bid (we can change money if needed) and play the gamble. If the color matches, the person immediately receives 30€. If the colors do not match, the person does not receive any payoff.

In experiment 2 the instructions proceeded as follows:

We offer only one option A and one option B to the whole group. We will determine the persons who play the options as follows:

Each participant chooses which option he or she prefers to play. We will randomly draw one person from the participants who chose option A and give him or her the opportunity to play option A. We also randomly draw one person from the group of participants who chose option B and give him or her the opportunity to play option B.

Please read the instructions carefully and then make a choice whether you want to play option A or option B.

- If you want to play option A (bet on a color to win €30 from bag with 20 red and 20 green chips), open the envelope with the letter 'A' on it and fill out the sheet in there.
- If you want to play option B (bet on a color to win €30 from bag with unknown proportion of colors), open the envelope with the letter 'B' on it and fill out the sheet in there.

Please leave the envelope of the option you do <u>not choose</u> closed.

Decision Sheet Option A.
You want to play option A (bet on a color to win €30 from bag with 20 red and 20 green chips)

We offer you the following: for each of the choices on the back of this sheet you decide whether you want to <u>play option A</u> **or** <u>receive a sure amount</u> instead. If you are randomly selected for real play from the people who chose option A, the experimenter will come to your desk and you will draw a lottery number to determine which of the choices on the back will be played [indicated by the number in brackets left to the choice option].

Depending on your decision in this choice you will either play option A as described before, or immediately receive the sure amount.

Example 1. You were selected for real play. You draw number '30'. For this number you made the following decision:

[30] Play Option A O **or** X get €30 for sure

You chose the sure amount here. Therefore you immediately receive €30. You will not play the gamble.

Example 2. You were selected for real play. You draw number '1'. For this number you made the following decision:

[1] Play Option A X **or** O get €1 for sure

You chose to play the gamble here. Therefore you play the gamble as described and if you match the colors you get €30, otherwise you get nothing.
Only <u>one number</u> will be drawn, that is, only one of the thirty choices will be played. Make sure that for each choice you make a decision that is in your best interest because this choice might be the payoff relevant choice that determines your payoff!

Please give your desk number here:________________________
Please also indicate your gender: O female O male

Fill out **<u>all 30 choices</u>** on the back now. Then the decision sheets will be collected. One person will be drawn from each group (option A choosers and option B choosers) and we play one randomly selected choice with them.

When we come to collect the sheets, please hand in your decision sheet in the envelope. Leave all other sheets on your desk.

Option A (bet on color from bag with 20 red and 20 green chips to win €30) **<u>or</u>** sure amount of €:

[1]	Play Option A	O	**or**	O	get €1 for sure
[2]	Play Option A	O	**or**	O	get €2 for sure
[3]	Play Option A	O	**or**	O	get €3 for sure
[4]	Play Option A	O	**or**	O	get €4 for sure
[5]	Play Option A	O	**or**	O	get €5 for sure
[6]	Play Option A	O	**or**	O	get €6 for sure
[7]	Play Option A	O	**or**	O	get €7 for sure
[8]	Play Option A	O	**or**	O	get €8 for sure
[9]	Play Option A	O	**or**	O	get €9 for sure
[10]	Play Option A	O	**or**	O	get €10 for sure
[11]	Play Option A	O	**or**	O	get €11 for sure
[12]	Play Option A	O	**or**	O	get €12 for sure
[13]	Play Option A	O	**or**	O	get €13 for sure
[14]	Play Option A	O	**or**	O	get €14 for sure
[15]	Play Option A	O	**or**	O	get €15 for sure
[16]	Play Option A	O	**or**	O	get €16 for sure
[17]	Play Option A	O	**or**	O	get €17 for sure
[18]	Play Option A	O	**or**	O	get €18 for sure
[19]	Play Option A	O	**or**	O	get €19 for sure
[20]	Play Option A	O	**or**	O	get €20 for sure
[21]	Play Option A	O	**or**	O	get €21 for sure
[22]	Play Option A	O	**or**	O	get €22 for sure
[23]	Play Option A	O	**or**	O	get €23 for sure
[24]	Play Option A	O	**or**	O	get €24 for sure
[25]	Play Option A	O	**or**	O	get €25 for sure
[26]	Play Option A	O	**or**	O	get €26 for sure
[27]	Play Option A	O	**or**	O	get €27 for sure
[28]	Play Option A	O	**or**	O	get €28 for sure
[29]	Play Option A	O	**or**	O	get €29 for sure
[30]	Play Option A	O	**or**	O	get €30 for sure

[Analogously for option B]

Chapter 5

Tempus Fugit: Time Pressure in Risky Decision

We study the effect of time pressure on risky decision separately in the domain of pure gains, of pure losses and of mixed prospects involving both gains and losses. We find that risk aversion for gains is robust under time pressure whereas risk seeking for losses turns into risk aversion under time pressure. For mixed prospects subjects become more loss averse and more gain seeking under time pressure, depending on the framing of the prospects. The results support aspiration level theories that consider the overall probabilities of success and failure. We also test the consistency of risk attitudes across elicitation methods and introduce a new method to endow subjects with money for decisions involving real losses.

5.1. Introduction

Time pressure is common to many economic decisions. Traders make orders in financial markets within seconds after new information becomes available (Busse and Green 2002). Last-minute bidders in auctions learn about common value components and adjust their valuation in an instant (Roth and Ockenfels 2002). Negotiators must often reach agreements before a deadline (Roth et al. 1988, Sutter et al. 2003). This paper studies the effect of time pressure on decision under risk. Risk attitudes are important for economic policy decisions (Barsky et al. 1997, Bazerman 2006, Harrison et al. 2006), and the effects of time pressure on risk attitudes should be considered by regulators of fast-paced markets.

Behavioral predictions based on data without time pressure may not be valid in such environments.

Self-selection of individuals into occupations and a lack of comparable decision environments with different degree of time pressure complicate the study of time pressure in the real world. We therefore use a laboratory experiment to identify the effect of time pressure on risk attitude. We study decisions in the gain domain, decisions in the loss domain, and decisions with both gains and losses involved. For gains we observe strong risk aversion that is robust under time pressure. For losses we find that subjects become more risk averse and that mild risk seeking turns into risk aversion under time pressure. For mixed prospects, the tendency to weigh gains and losses differently can become important. We call such weighting differences *gain-loss attitude*, with loss aversion denoting an overweighting of losses and gain seeking an overweighting of gains. Gain-loss attitude is not robust under time pressure. Both loss aversion and gain seeking can be increased under time pressure depending on simple framing manipulations.

We also provide half of our subjects with information about the expected value of prospects. We observe that risk attitude is not affected by expected value information in pure gain or pure loss decisions, but it is strongly affected for mixed prospects where choices are closer to risk-neutral expected value maximization if the information is available. This holds for decisions with and without time pressure. The result suggests that subjects use information that helps them to eliminate the influence of economically irrelevant gain-loss framing on their decisions (Slovic 1972, Hilton 2003).

Time pressure in risky decision has received little attention in the economics literature. Bollard et al. (2007) study the effect of time pressure in an experiment where subjects can buy prospects with different variance and expected payoff in the gain domain. They find more variance aversion for time pressure. Given that subjects had to pay for the prospects, however, all their decisions involve gains and losses and the increased variance aversion can be explained by our finding for loss aversion under time pressure. There is a larger psychological literature on time pressure in risky decision (Ben-Zur and Breznitz 1981, Payne et al. 1993, Payne et al. 1996, Maule et al. 2000). These studies focus on information processing and identify two strategies to cope with time pressure. First, behavior becomes more heuristic. Second, subjects exert more cognitive effort. These findings are consistent with evidence on decision-making cost in economics (Wilcox 1993, Camerer and Hogarth 1999, Moffatt 2005). Ben-Zur and Breznitz (1981) consider risk attitudes for mixed prospects involving both gains and losses. They find more risk aversion under time pressure, and our results suggest this is due to increased loss aversion.

Our study is the first to consider risk attitude separately for gains, for losses and for mixed gambles, and it can explain previous results under time pressure in terms of the different components of risk attitude. It also suggests that risk attitude for losses and gain-loss attitude may be less robust than risk attitude for gains. The generality of risk seeking for losses has been questioned by studies using repetition and financial incentives (Myagkov and Plott 1997, Laury and Holt 2007), and our results show that time pressure provides another environment in which risk aversion for losses may prevail. Loss aversion often exerts strong influence on choices (Abdellaoui et al. 2007, Fehr and Götte 2007, Gächter et al. 2007), but it may not be as robust and ubiquitous as sometimes assumed (Schmidt and Traub 2002, Ert and Erev 2007). Under time pressure gain-loss attitude is strongly affected by economically irrelevant framing effects and loss aversion is not necessarily a valid assumption if gains become more attractive because they allow agents to break even.

Different theories of decision under risk predict different effects of time pressure. By comparing the predictions of the models with our results we can test these theories in a new way. Our results support expected utility with an aspiration level (Payne 2005, Diecidue and van de Ven 2007), but they also suggest that probability weighting as assumed under prospect theory is important and that it differs for gains and losses (Abdellaoui 2000).

The experimental design that we use allows us to consider two methodological issues in experimental research on risk attitude, namely the consistency of different elicitation methods and the effect of a new method for endowments of money for losses if real payoffs are used. We elicit risk attitude for gains by (i) aggregating separate binary choices into an index and (ii) through a choice list (Holt and Laury 2002). We find that risk attitudes from these two methods are closely associated. The choice list, however, seems more demanding. More subjects fail to complete the list within the time limit, although this limit was less stringent per choice on the list than the one for separate choices of the index.

To install real payoffs for loss and mixed prospects we let subjects earn their endowments through risky lotteries *after* the decisions involving losses. This is necessary in our experiment because the knowledge of a fixed endowment before subjects make the decisions involving losses is more likely to lead to an integration of the endowment when there is sufficient time for calculations. Differences in behavior between time pressure conditions could then be due to differences in the integration of the endowment. By letting subjects play the endowment lotteries after the loss decisions the endowment information is withhold in a natural way because it depends on choices and chance events and cannot be given beforehand. We also avoid undesirable spillovers from the behavior in the endowment lotteries toward the loss decisions that could occur if endowment lotteries were played first. Our replication of the typical finding of mild risk seeking for losses and risk aversion for gains supports the use of our method for losses in experiments where real incentives are desirable and the tendency to integrate endowments may be correlated with the treatment variable.

The chapter is organized as follows. The next section discusses how time pressure can affect risk attitude under different theories of risky choice. Section 5.2 introduces the experimental design and the time pressure conditions. In Section 5.3 we discuss our measures of risk attitude. Section 5.4 presents the experimental results for time pressure and Section 5.5 presents results concerning the consistency of risk attitudes and the endowment for losses. Section 5.6 discusses the results and concludes the paper.

5.2. Time Pressure and Risk Attitude under Different Theories of Decision under Risk

Theories of decision under risk differ with respect to the possible components of risk attitude that they model and therefore make different predictions about behavior under time pressure. We consider the effect of time pressure on risk attitude under expected utility (von Neumann and Morgenstern 1944), under prospect theory (Kahneman and

Tversky 1979, Tversky and Kahneman 1992), and under expected utility with an aspiration level (Diecidue and van de Ven 2007).

Under expected utility probabilities enter linearly into the evaluation function and the curvature of an agent's utility function for outcomes therefore determines her risk attitude (Friedman and Savage 1948). Because probabilities are given and utility of outcomes can be assumed to be quite stable, we expect time pressure to affect risky decisions only through an increase in errors and would therefore expect behavior to become more random (Schmidt and Neugebauer 2007). If utility of outcomes did change, however, then similar behavior for gains and for losses is predicted by expected utility. If utility becomes more concave for example, more risk aversion both for gains and for losses would obtain.

Prospect theory distinguishes between three components of risk attitude: utility curvature, probability weighting, and gain-loss attitude (Köbberling and Wakker 2005, Schmidt and Zank 2007). Outcomes are framed as gains or losses from a reference point. Utility curvature can derive from two factors, namely the utility of outcomes and diminishing sensitivity toward changes in numbers (Kahneman and Tversky 1979, Myagkov and Plott 1997). For small outcomes like the ones that we consider in our experiment this implies concave utility for gains and convex utility for losses. Under prospect theory probabilities enter the evaluation function through possibly nonlinear rank-dependent decision weights and the weighting functions may differ for gains and losses. A gain-loss asymmetry is assumed where losses receive more weight than equal sized gains, leading to loss aversion.

Diminishing sensitivity, probability weighting and gain-loss framing are perceptional factors that do not necessarily represent the agent's valuation of economic outcomes (Kahneman 2003). All three factors are conceivably affected by time pressure and their effects on risk attitudes may point in different directions. Some structure can be put on the predictions. If diminishing sensitivity changes, risk attitude should change into opposite direction for gains and for losses. Diminishing sensitivity promotes risk aversion for gains and risk seeking for losses, and if diminishing sensitivity becomes 'stronger', we would observe stronger risk aversion for gains and stronger risk seeking for losses. Loss aversion is due to framing effects and should be more pronounced under time pressure where a re-framing of outcomes in terms of the final wealth level seems more difficult. Finally, no clear predictions for probability weighting are obvious. Probability weighting may differ for gains and for losses and can affect risk attitude in various ways through rank-dependent decision weights.

Aspiration level theory assumes that subjects care about the overall probability of success and failure (Payne 2005). Diecidue and van de Ven (2007) show that an expected utility model that includes the overall probabilities of success and failure compared to some aspiration level is equivalent to expected utility with a discontinuous utility function at the aspiration level. That is, a discrete jump occurs at the aspiration level, with small losses being evaluated distinctly worse than zero and small gains distinctly better than zero. A prospect $X=(p_1, x_1;\ldots ; p_n, x_n)$ is evaluated by the following evaluation functional V:

$$V(X) = \sum_{j=1}^{n} p_j u(x_j) + \mu P^+ - \lambda P^- , \quad \mu, \lambda \in \mathbb{R}^+, \tag{5.1}$$

where *u* is a real valued utility function and P^+ and P^- denote the overall probabilities of success and failure. Assuming that the aspiration level is zero in simple prospect choices the model predicts risk aversion for pure gains and risk seeking for pure losses for prospects that include the zero outcome. For losses risk seeking occurs because the zero outcome of the risky prospect receives a particularly high value compared to other (negative) outcomes, making the prospect more attractive than its negative expected value. For gains the zero outcome of the prospect receives a particularly low value compared to other (positive) outcomes, making the gamble less attractive than its positive expected value.

For mixed prospects the model predicts simultaneous loss aversion and gain seeking. Assume linear utility u and strictly positive μ and λ in Eq.1. Consider a pure gain prospect. A mean preserving spread of this prospect will be less preferred than the prospect if the increase in variance reduces P^+ and increases P^-, although utility is linear. The threat to fall short of the aspiration level creates loss aversion. Similarly, consider a pure loss prospect. A mean preserving spread of this prospect will be more preferred than the prospect if the increase in variance increases P^+ and reduces P^-, although utility is linear. The opportunity to attain the aspiration level creates gain seeking.

Diecidue and van de Ven argue that aspiration levels are the consequence of a simplifying decision heuristic. We therefore expect the phenomena predicted by the model to become more pronounced under time pressure where simplification allows the decision maker to reduce decision times and meet the deadline.

5.3. Experimental Design

5.3.1. Subjects and Payoffs

One hundred and seventy-six undergraduate students from the University of Amsterdam in the Netherlands participated in eight laboratory sessions and were randomly assigned to treatments. Students were recruited electronically from a pool of approximately 1200 potential participants and came from different disciplines. Each subject participated only once.

Subjects received a show-up payment of €5 and could earn between zero and €200 based on their choices in the experiment. The average earnings were €17.15 and the experiment took between 30 and 50 minutes depending on the treatment.

5.3.2. General Procedures

Our experiment employs a 2×2 between-subject factorial design. The two factors we vary are the degree of *time pressure* and the availability of information about the *expected values* of the risky prospects. The four treatments are summarized in Table 5.1.

In all four treatments subjects made choices between risky prospects in three separate experimental parts. Part I consisted of separate choices between pure gain prospects that were individually time constrained. Part II consisted of a choice list of seven choices adapted from

Table 5.1: Overview of the Treatments

	No time pressure	Time pressure
No EV information	NTP-NEV N = 42	TP-NEV N= 41
EV information	NTP-EV N = 45	TP-EV N = 48

EV = expected value; N = number of observations.

Holt and Laury (2002) that had to be completed together before one common time constraint elapsed. This time constraint was higher than the time constraint for single choices. The third part consisted of two subparts: in Part IIIA separate choices involving both gains and losses were made and were individually time constrained. In Part IIIB subjects made separate choice between pure gain prospects with a smallest payoff of €20 to cover possible losses from Part IIIA. When making their choices in Part IIIA subjects did not know how much money they would win in Part IIIB. The order of the three parts was fixed.

Figure 5.1: Structure of the Experiment

Part I: pure gain prospects, separate choices

Part II: pure gain prospects, Holt and Laury choice list, seven choices simultaneously

Part IIIA: pure loss, pure gain and mixed prospects, separate choices, no information about the endowment at this point available

Part IIIB: pure gain prospects paying at least €20, separate choices

Determination of payoffs: one part randomly selected, one decision randomly selected within this part and played for real according to the subject's choice (if part III was selected, one decision from part A and one decision from part B was played according to the subject's choice).

At the end of the experiment one part was selected with equal probability. If part I or II were selected, then within this part one decision was selected with equal probability for real play[11]. If Part III was selected for payment, then one randomly selected decision from

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

Part IIIA with possible losses and one randomly selected decision from Part IIIB were played for real. The decision in Part IIIB was selected independently of the Part-IIIA decision and the endowment earnings from Part IIIB would always be paid, irrespective of actual gain or loss from Part IIIA. The random selection of the payoff-relevant task was done independently for each subject. The structure of the experiment is summarized in Figure 5.1.

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

Figure 5.2 shows how the prospects were presented to the subjects. Subjects always chose between two prospects A and B represented by the second and third row in Figure 5.2. The first row of the figure shows the faces of a twenty-sided die. The payoffs of the prospects depended on the outcome of a throw of the die. Each face of the die corresponds to a 0.05 probability. In the example prospect A therefore pays €20 with probability 0.5 and zero with probability 0.5. Prospect B pays €10 for sure. The procedure was explained in detail to all subjects (instructions in the appendix).

Figure 5.2: Presentation of Prospect Choices

<table>
<tr><th></th><th>1</th><th>2</th><th>3</th><th>4</th><th>5</th><th>6</th><th>7</th><th>8</th><th>9</th><th>10</th><th>11</th><th>12</th><th>13</th><th>14</th><th>15</th><th>16</th><th>17</th><th>18</th><th>19</th><th>20</th></tr>
<tr><td>A</td><td colspan="10">€20</td><td colspan="10">€0</td></tr>
<tr><td>B</td><td colspan="20">€10</td></tr>
</table>

5.3.3. Time Pressure and Expected Value Manipulation

We manipulated the available decision time and the availability of expected value information. In the treatments without time pressure we constrained the decisions by introducing a maximum decision time that was very large and then measured actual decision times. Decisions in these treatments were practically unconstrained but we could use identical wording in all instructions by providing some threshold in the treatments without time pressure. Decisions in Part I and Part III were presented and constrained individually. Decisions in Part II had to be made on one screen and were constrained simultaneously, that is, all seven decisions of the choice list had to be made within the time limit. In the time pressure treatments we restricted the decision times such that there was significant time pressure but subjects would still have sufficient time to make decisions at the computers[12]. All subjects within a treatment faced identical time constraints because we use a between-subject design. Table 5.2 summarizes the maximum and the actual decision times for each part of the experiment.

Table 5.2: Maximum and Actual Median Decision Times per Decision in Seconds

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

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

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

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

For each decision problem subjects had to click a button to make their choice between options A and B, and then click an ‘OK’-button to confirm their choice within the time limit. The clock was clearly visible at the top of the screen. An example screenshot is given in the appendix. If the subject failed to submit and confirm a choice before time runs out, this decision would pay the minimum payoff possible in either of the two prospects. In decisions involving losses this would be the maximum loss. In the Part IIIB endowment decisions this would be €20. If the subject failed to submit all seven decision in Part II within the time limit, she would earn zero for each possibly selected decisions in this part of the experiment. Between the individual decisions a waiting screen occurred for 2 seconds in all treatments. This ensured that subjects could properly prepare for the next decision problem, especially under time pressure. Before each part of the experiment

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

specific instructions were distributed and read aloud. This gave subjects time to rest between the parts.[13]

Expected value information was manipulated by providing the expected value of each prospect next to the button that had to be clicked for the decision. This allowed subjects to access this information efficiently and possibly without consideration of the actual prospects (see screenshot in the appendix). The expected value was explained to the subjects before the experiment and on a sheet of paper on their desk.

5.4. Prospects and Dependent Variables

We analyze the effects of time pressure and expected value information on dependent variables that measure attitudes towards risk under gains and losses, and gain-loss attitude. The variables and the prospects used to construct them are summarized in Table 5.3.

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

RAG=EV (from Part I) uses only the three choices between prospects and their expected value from RAG. This variable is used to calibrate the average risk attitude in a group.

RAGHL (from Part II) measures **r**isk **a**version for **g**ains using a **H**olt and **L**aury (2002) choice list with pure gain prospects. We scaled up their low payoff treatment (2002, p. 1645) by a factor of six and used only choices with probabilities between .2 and .8 including. The variable indicates the percentage of safer choices a subject makes if there was a unique point where the subject switched from the safer to the riskier option as the probability of the larger payoff increased. Subjects who switched twice or switched from

[13] The working of the mouse was essential for subjects to enter decisions rapidly into the computer. We checked the mice with each subject before the experiment for proper working.

risky to safe as the probability of the higher payoff increased were excluded from the analysis.

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

RAL=EV (from Part IIIA) uses only the three choices between prospects and their expected value from RAL to calibrate the average risk attitude for losses in a group.

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

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

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

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

For each variable we have slightly different sample sizes because subjects could violate the time constraint. Subjects who violated the time constraint in at least one of the decision problems used to construct a variable were excluded from the analysis of this variable.

Table 5.3: Dependent Variables and Prospects

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

5.5. Experimental Results

All tests in the chapter are two-sided tests and the abbreviation *ns* designates nonsignificance.

5.5.1. Time Pressure Manipulation

Table 5.2 in Section 5.1.3 shows that median decision times under time pressure were approximately half the size of median decision times under no time pressure. We tested for each decision problem and under both expected value information conditions the null hypothesis that decision times are equal under both time pressure conditions using Mann-Whitney tests. Equality of decision times was rejected for all choice problems. The smallest z-value was z=3.171 (p=0.0015), indicating that decision times have been much lower under time pressure. We lose between four and eight observations per variable in Part I and Part III because of violations of the time limit in the separate choices. The time constraint in these decisions has been substantial but not prohibitive therefore. We lose twenty observations in the choice list in part II which appeared to be quite heavily constrained with 30 seconds[14].

In a post-experimental questionnaire subjects indicated their stress level and the difficulty of the experiment on a five point Likert scale. Subjects in the time pressure treatments felt more stressed during the experiment (Mann-Whitney test, z=5.520, p=0.0000) and considered it a more difficult experiment to participate in (Mann-Whitney test, z=2.230, p=0.0257) than subjects in the unconstrained treatments.

The correlation between the dependent variables and decision times was practically zero for all variables in the treatments without time pressure. That is, there were not certain types of subjects in terms of risk attitude that were more constrained than others; for instance, more risk averse subjects did not deliberate longer before making a decision.

In the unrestricted treatments where subject could take their time if they wanted the actual decisions times for gains in Part I and for losses and mixed prospects in Part IIIA do

[14] Another ten subjects were eliminated because they switched more than once in the choice list, nine of them under time pressure.

not differ (Mann Whitney tests, z<1.093, ns). This suggests that pure gain, pure loss and mixed decisions were of similar difficulty for subjects.

5.5.2. Time Pressure and Risk Attitude

A summary of the means and standard deviations of all variables in the four treatments is given in the appendix. Here we first consider results for pure gain and pure loss decisions and then we consider results for decisions involving mixed prospects. For each variable in Table 5.3 we run linear regressions of the form

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

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

5.5.2.1. Pure Gain and Pure Loss Decisions

The linear regressions in Table 5.4 show that risk attitude for pure gains is not affected by time pressure. The variables RAG, RAGHL and ENDOW involve different payoff ranges and time constraints, and they are measured in different parts of the experiment. There is no direct effect of time pressure on either of these variables. Expected value information does not affect these variables, nor does its interaction with time pressure. Risk attitude for gains is robust under time pressure.

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

Table 5.4: Linear Regression Results for Pure Gains

Dependent variable	RAG	RAG	RAGHL	RAGHL	ENDOW	ENDOW
Time pressure	–0.011 (0.035)	–0.001 (0.051)	0.01 (0.038)	–0.054 (0.055)	–0.026 (0.039)	–0.032 (0.057)
Expected value information	0.069 (0.035)	0.079 (0.05)	0.025 (0.037)	–0.024 (0.048)	0.011 (0.039)	0.006 (0.055)
Time pressure × expected value		–0.019 (0.071)		0.121 (0.075)		0.011 (0.079)
Female	0.121** (0.037)	0.12** (0.038)	0.059 (0.039)	0.062 (0.039)	0.122** (0.042)	0.122** (0.042)
# observations	172	172	146	146	170	170

Standard errors in parenthesis; ×: interaction; * significant at 5% level, ** significant at 1% level

For losses, however, subjects become more risk averse under time pressure (Table 5.5). For both variables RAL and RALMPS the percentage of safe choices under time pressure increases. No effect is found for expected value information or for its interaction with time pressure.

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

Table 5.5: Linear Regression Results for Pure Losses

OLS	RAL	RAL	RALMPS	RALMPS
Time pressure	0.078* (0.039)	0.11* (0.056)	0.143* (0.057)	0.203* (0.082)
Expected value information	0.0003 (0.039)	0.031 (0.054)	0.046 (0.057)	0.103 (0.08)
Time pressure × expected value		–0.061 (0.077)		–0.114 (0.113)
Female	0.084* (0.041)	0.083* (0.041)	0.109 (0.06)	0.106 (0.06)
# observations	171	171	173	173

Standard errors in parenthesis; ×: interaction; * significant at 5% level, ** significant at 1% level

To show the effect of time pressure on average risk attitudes for gains and losses under both time pressure conditions we consider the variables RAG=EV and RAL=EV, pooling the data from both expected value information treatments. These variables involve only choices between prospects and their expected values and the average risk attitude can be determined by testing whether subjects chose on average more than half of the safe options (Table 5.6).

Table 5.6: Average Percentage of Safe Choices

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

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

In the baseline condition with no time pressure, our data show the common pattern of 'partial reflection' (see Wakker et al. (2007a) for an extensive review of empirical findings). There is strong risk aversion for gains, but mild and insignificant risk seeking for losses. Under time pressure we obtain risk aversion for both gains and losses. Risk seeking for losses turns into risk aversion for losses under time pressure. We observe that under time pressure subjects have strong preferences for safer options for both gains and losses, clearly rejecting the conjecture that choices were more random under time pressure.

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

5.5.2.2. Decisions Involving Gains and Losses

The variables PLA and PGS study gain-loss attitude. PLA measures the percentage of choices of a pure gain prospect over a mixed prospect with higher expected value. These decisions always involve one prominent loss and apart from risk aversion through utility curvature and probability weighting, which were relevant to RAG and RAL, also loss aversion can lead subjects to choose the pure gain prospect. PGS measures the percentage of choices of a mixed prospect over a pure loss prospect with higher expected value. These decisions always involve one prominent gain. Loss aversion would lead to fewer choices of the mixed prospect while gain seeking, which is, overweighting of gains relative to losses, may lead subjects to choose the mixed prospect. The linear regressions in Table 5.7 show that subjects avoid more mixed gambles in PLA and take more mixed gambles in PGS under time pressure. They are more likely to avoid the prominent loss and seek the prominent gain under time pressure.

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

Table 5.7: Linear Regression Results for Mixed Prospects

OLS	PLA	PLA	PGS	PGS
Time pressure	0.17** (0.055)	0.115 (0.08)	0.253** (0.043)	0.186** (0.062)
Expected value information	– 0.12* (0.056)	–0.173* (0.078)	– 0.125** (0.043)	–0.188** (0.06)
Time pressure × expected value		0.106 (0.11)		0.129 (0.086)
Female	0.13* (0.059)	0.133* (0.059)	–0.006 (0.046)	–0.003 (0.046)
# observations	172	172	168	168

Standard errors in parenthesis; ×: interaction; * significant at 5% level, ** significant at 1% level

A simultaneous increase in loss aversion and gain seeking under time pressure cannot be explained by a change in gain-loss attitude under prospect theory. An increase in loss aversion implies that losses receive more weight relative to gains under time pressure than without time pressure. An increase in gain seeking implies the opposite effect.

Aspiration level theory predicts such an effect, however, because time pressure is likely to enhance the underlying simplification heuristic. In the PLA decisions the overall probability of a gain is lower for the mixed gamble, leading to loss aversion, and in the PGS decisions the overall probability of a gain is higher for the mixed gamble, leading to gain seeking. The correlations between the variables PLA, PGS, RAG and RAL that are shown in Table 5.8 corroborate the interpretation in terms of aspiration levels. PLA is positively correlated with risk aversion under gains and losses and PGS is negatively correlated with risk aversion, as would be expected[16]. Interestingly, a strong and positive correlation is observed for PLA and PGS. That is, subjects who avoid the prominent-loss mixed prospect in PLA are also more likely to seek the prominent-gain mixed prospect in PGS. This observation suggests that subjects do not have a stable gain-loss attitude such as loss aversion, but that gain-loss attitude is mainly driven by the interaction of framing and a subject's susceptibility to aspiration levels[17].

Table 5.8: Correlations between Variables

	PLA	PGS	RAG
PGS	0.31**		
RAG	0.27**	–0.11	
RAL	0.05	–0.21**	0.17*

* significant at 5% level, ** significant at 1% level; all subjects

[16] For PLA a significant positive correlation is observed with RAG. This makes sense because the largest gain, which can be obtained through the mixed prospect, will most strongly be affected by RAG. Large RAG then discourages a choice of the mixed prospect and thus *increases* PLA. For PGS a significant negative correlation is observed with RAL. Here the largest loss, which can be obtained through the mixed prospect, will most strongly be affected by RAL. Large RAL then discourages a choice of the mixed prospect and thus *decreases* PGS.

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

To illustrate how susceptibility to framing and aspiration level affects subjects' expected earnings we calculate the average expected payoffs over the six decisions in PLA and PGS for each treatment. Because decisions in PLA involve expected losses and decisions in PGS involve expected gains, an expected value maximizing subject would on average face a small expected loss of – €0.28. The actual average expected payoffs in the four treatments are shown in Table 5.9.

Table 5.9: Effect of Framing on Expected Payoffs

	No expected value information	Expected value information
No time pressure	– €0.93 [3.3×EV]	– €0.63 [2.3×EV]
Time pressure	– €1.21 [4.3×EV]	– €1.09 [3.9×EV]

If subjects are unconstrained and have expected value information available they make choices that imply about twice the expected loss of an expected value maximizer. On the other extreme, with time pressure and no information, this becomes roughly four times the expected loss. Susceptibility to framing becomes very expensive for subjects under time pressure. Expected value information makes the framing effect more transparent by showing that a choice of a superficially attractive prospect may be quite costly and it helps to reduce the effect of gain-loss attitude through the aspiration level.

5.5.2.3. Gender and Risk Attitude

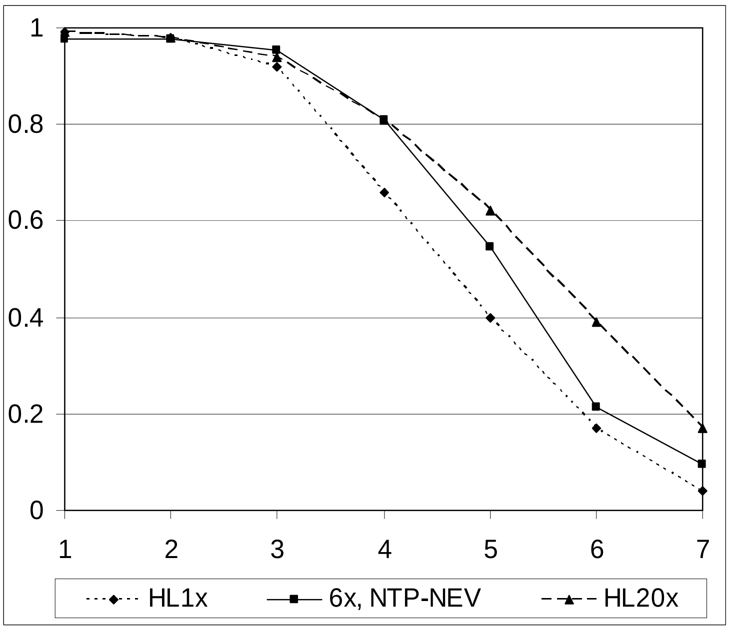
There has been much interest in gender differences in risk attitude (Barsky et al. 1997, Schubert et al. 1999, Donkers et al. 2001, Dohmen et al. 2005, Booij and van de Kuilen 2006, Fehr-Duda et al. 2006, Croson and Gneezy 2007). We control for gender in our regressions and find that females are more risk averse both under gains and under losses. There were no significant interactions between gender and time pressure or gender and expected value information.

5.6. Consistency of Risk Attitudes and Endowment for Losses

5.6.1. Consistency of Risk Attitudes

Our design allows us to consider the consistency of the risk attitudes of our subjects with other findings in the literature and across elicitation methods. In part II of the experiment subjects made choices identical to those in Holt and Laury (2002) with payoffs as in their low payoff treatment scaled up by a factor six (and in Euros). We excluded the choices with .1, .9, and 1.0 probability of the larger payoff. These choices did not differentiate much between subjects in Holt and Laury (2002) and removing them facilitated the presentation of the task under time pressure. Holt and Laury compare the distribution of safe choices in their choice lists under the low payoff condition and under payoffs scaled up by a factor 20. They observe increased relative risk aversion for the high payoff condition. In Figure 5.3 we plot the proportions of safe choices for each decision observed by Holt and Laury for the low and for the factor-20 choices, and the factor-6 choices from our experiment (baseline treatment).

Figure 5.3: Proportion of Safe Choices in Seven Decisions in Part II



We observe that the distribution of choices with factor-6 payoff scale lies between the distributions of the low and factor-20 payoffs in Holt and Laury. This confirms their finding that relative risk aversion in the choice list increases with payoff scale and shows that our data compare well with the existing evidence. The figure also shows that for decision number four the percentage of safe choices seems relatively large compared to Holt and Laury, being closer to their factor twenty treatment, and then drops more sharply, particularly for decision number six. This effect may be due to the elimination of the two decisions with highest probability to win and the decision with the lowest probability to win. Through this asymmetric elimination decision number four becomes the unique center of the choice list while decisions five and six look more extreme than in the longer list. The pattern supports the finding of Harrison et al. (2007) that the setup of the choice list influences the revealed risk attitudes.

We also compare risk attitude for gains aggregated from separate choices in RAG and the choice list RAGHL. In the baseline treatment without time pressure and expected value information the correlation equals $\rho=0.73$ ($t_{40}=6.72$, $p<0.001$). Including all data the correlation becomes $\rho=0.42$ ($t_{40}=5.57$, $p<0.001$). There is a considerable positive correlation between the two measures of risk attitude in the gain domain.

Interestingly, under time pressure we lose 29 subjects in the RAGHL choice list task but only four in the RAG task. This happens although the decision time per decision was larger for the choice list (30 seconds for the seven choices from the list versus 4 seconds for single choices), fewer buttons hat to be clicked for the choice list, and the choices were arranged in a systematic fashion. Lammers et al. (2006) find many subjects switching repeatedly between the safe and the risky option in the Holt and Laury choice list. They argue that many of the subjects might have been indifferent between the two options in the choices around their true switching point, thereby not being inconsistent by switching back and forth. Weak preferences between options around the switching point in the choice list can explain our finding for time pressure because they make decisions more difficult and increase required decision time (Moffatt 2005).

5.6.2 Endowment for Losses

To implement proper time pressure we need real incentives. To study risk attitude under losses we therefore have to endow subjects with money from which they can suffer losses. Losses from an endowment are problematic because subjects may integrate the endowment into the loss prospects and treat them as gain prospects. In this case the experiment would not study risky behavior under losses.

The problem of integration of endowments is common to all experiments studying losses with real incentives. In our experiment the ability of the subjects to integrate endowments differs between time pressure treatments, making integration more likely in the unconstrained treatments where subjects have enough time for calculations if they wanted. We therefore do not provide subjects with a fixed endowment before facing the loss prospects, but let them earn the endowment through risky choices after the part involving losses. The information about the endowment is withhold in a natural way because it depends on subjects' choices and chance events in the later task and cannot be known beforehand. We also avoid the problem that the behavior in the endowment lotteries may influence behavior for losses if endowment decisions were made first but resolved later.

Our results for losses are very similar to previous findings in the literature, suggesting that risk attitudes for losses were not biased because of the anticipation of an endowment. To test explicitly if subjects reframed losses from endowments as gains we included the decision problem (– €20, .75) vs. – €15 in the loss part, and included the decision problem (€20, .25) vs. €5 in the gain part. Because of the lottery choices the endowment has been between €20 and €60. We reframed the loss gamble using a €20 endowment because this amount might have been suggested to the subjects by the size of the maximum losses in the loss prospects that subjects faced before they made this decision (the control decision came last). Table 5.10 shows the percentage of safe choices for losses and reframed gains, separately for the two time pressure conditions. For each time pressure condition this analysis is within subject.

Table 5.10: Percentage of Safe Choices under Gain and under Loss Frame

	No time pressure	Time pressure
Gain frame (loss reframed as gain)	74.7 ($p<0.001$)[a]	61.6 ($p=0.02$)[a]
Loss frame (loss from endowment)	48.3 ($p=0.666$)[a]	65.1 ($p=0.003$)[a]
Wilcoxon Signed-Rank Test	$z=3.888$, $p<0.001$	$z=0.557$, ns

a: binomial test for the proportion being equal to 50%

Clearly, with no time pressure the typical result of mild but insignificant risk seeking for losses and risk aversion for gains prevails. There is a strong difference between the risk attitude under the loss frame and under the gain frame, showing that no reframing of losses took part. In this treatment subjects had enough time to reframe if they wanted to. Under time pressure we find risk aversion for both gains and losses, which is consistent with the change of risk attitudes under time pressure as shown in section 5.5.

5.7. Discussion and Conclusion

Utility curvature, probability weighting, and loss aversion have been modeled as components of risk attitude, with the valuation of monetary payoffs and diminishing sensitivity as the factors affecting utility curvature. Risky behavior is clearly not becoming more random under time pressure in our experiment. We find stable risk aversion for gains under time pressure while mild risk seeking for losses turns into risk aversion under time pressure. If utility of outcomes were affected by time pressure, risk attitude for gains and losses should change in the same direction. Diminishing sensitivity predicts that increased risk aversion for losses concurs with decreased risk aversion for gains. In both cases risk attitude for gains and risk attitude for losses should be affected. Abdellaoui (2000) and Wu et al. (2004) find evidence for differences in probability weighting for gains and for losses under prospect theory. An effect of time pressure on probability weighting for losses but a more stable probability weighting for gains can therefore explain our result, and it supports different weighting for gains and for losses. The result also suggests that risk seeking for losses in small stake experiments is mainly driven by probability weighting and not by the convexity of the utility function.

We use mixed prospects with prominent losses or prominent gains to study gain-loss attitude. Under time pressure subjects avoid the prominent loss, consistent with loss aversion, but they also seek the prominent gain, which implies the opposite gain-loss attitude. Under both time pressure conditions expected value information reduces both loss avoidance and gain seeking. These results show that gain-loss attitude is sensitive to framing effects and becomes more pronounced in situations that promote simple heuristics. Ert and Erev (2007) argue that loss aversion is a situation dependent heuristic and may be less robust than sometimes assumed. Our data support this view. Under time pressure decisions become more heuristic (Payne et al. 1993), and gain-loss attitude becomes more influential but also more frame dependent. Expected utility with an aspiration level (Payne 2005, Diecidue and van de Ven 2007) successfully predicts our results for mixed gambles, suggesting that gain-loss attitude results from a discontinuity of the utility function and not from a kinked utility function as assumed under prospect theory.

Fehr and Götte (2007) show that individual loss aversion can predict negative wage elasticities of daily effort for a sample of bicycle messengers, supporting a model of reference dependent preferences. We observe a significant positive correlation between the avoidance of losses and the seeking of gains. Our results therefore suggest that it may not be loss aversion per se, but a general propensity of being influenced by aspiration levels that drives their result.

The distinction between pure gains, pure losses and mixed prospects in our study helps to explain previous results under time pressure. Ben-Zur and Breznitz (1981) find more risk aversion under time pressure in mixed prospects and Bollard et al. (2007) find more risk aversion under time pressure in a game where subjects had to buy pure gain prospects with a zero outcome and therefore may lose their payment. Losses were prominent in these studies similar to our decisions in PLA, leading to stronger loss aversion under time pressure.

Females are more risk averse for gains and for losses and there are no interactions of gender with time pressure or expected value information. The effect of gender appears to be driven by preference and not by perceptional factors, which are more likely affected by time pressure. Schubert et al. (1999) show, however, that gender differences in risk attitude are context dependent and may be less clear in financial settings than in abstract prospect choices.

Finally, our observation of a strong positive correlation between risk attitudes measured through different elicitation methods under gains supports the view of stable underlying preferences. If framing becomes more relevant such as in mixed prospects, then choices may less reflect this stable component but may be more influenced by heuristics. Endowments for losses were earned through lotteries after the decisions involving losses were made. Standard risk attitude patterns for losses show that this method is valid and allows us to study the interesting domains of pure loss and mixed prospects with real incentives in situations where the tendency to integrate endowments may be correlated with the treatment variable.

Experimental studies in decision under risk often try to observe experienced and thoughtful decisions through repetition and strong incentives (Myagkov and Plott 1997, Holt and Laury 2002). Our experiment shows that valuable insights into risk attitudes can be gained also from the opposite approach, by introducing time pressure in risky decision.

The importance of heuristics and aspiration levels may not be confined to situations where agents make decisions within a few seconds as in financial markets and auctions. Many real world economic decisions are more complex and time limits of a few hours or even a few days may lead to serious time pressure and therefore to possible changes in the evaluation of risky options.

Appendix

5.A1. Instructions

Instructions Part I

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

Example 1:

<table>
<tr><th></th><th>1</th><th>2</th><th>3</th><th>4</th><th>5</th><th>6</th><th>7</th><th>8</th><th>9</th><th>10</th><th>11</th><th>12</th><th>13</th><th>14</th><th>15</th><th>16</th><th>17</th><th>18</th><th>19</th><th>20</th></tr>
<tr><td>A</td><td colspan="10">11€</td><td colspan="10">9€</td></tr>
<tr><td>B</td><td colspan="5">20€</td><td colspan="15">21€</td></tr>
</table>

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

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

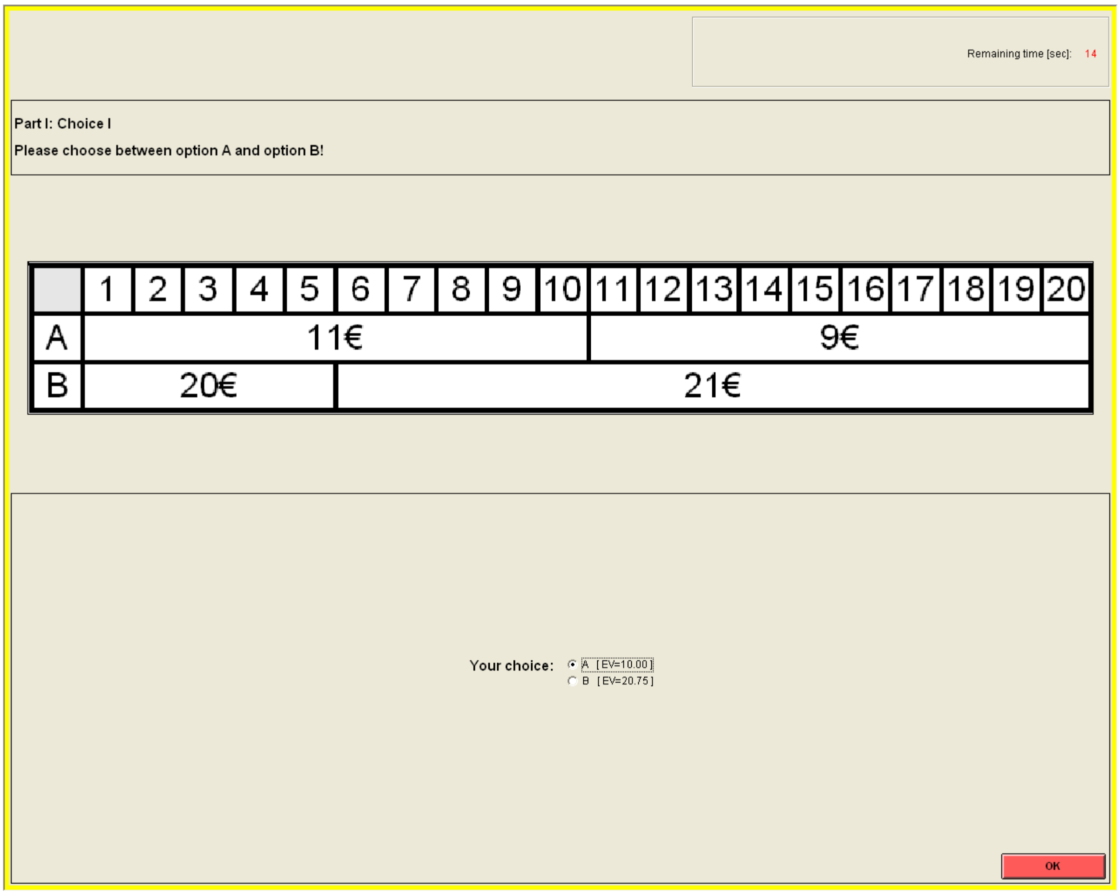
Another example:

Example 2:

<table>
<tr><th></th><th>1</th><th>2</th><th>3</th><th>4</th><th>5</th><th>6</th><th>7</th><th>8</th><th>9</th><th>10</th><th>11</th><th>12</th><th>13</th><th>14</th><th>15</th><th>16</th><th>17</th><th>18</th><th>19</th><th>20</th></tr>
<tr><td>A</td><td colspan="10">11€</td><td colspan="10">9€</td></tr>
<tr><td>B</td><td colspan="20">10€</td></tr>
</table>

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

5.A2. Example Screen Shot



5.A3. Means and Standard Deviations of Variables by Treatment

Treatment / Variable	NTP-NEV	TP-NEV	NTP-EV	TP-EV	Mann-Whitney-Tests
RAG	0.56 (0.24)	0.56 (0.27)	0.65 (0.21)	0.63 (0.23)	1<3, p=0.03
RAGHL	0.65 (0.20)	0.60 (0.29)	0.64 (0.16)	0.70 (0.26)	-
RAL	0.56 (0.27)	0.68 (0.25)	0.60 (0.24)	0.65 (0.25)	1<2, p=0.04
RALMPS	0.44 (0.39)	0.65 (0.36)	0.56 (0.37)	0.64 (0.38)	1<2, p=0.01
PLA	0.47 (0.38)	0.59 (0.33)	0.31 (0.35)	0.53 (0.39)	3<4, p=0.01 1<3, p=0.05
PGS	0.28 (0.28)	0.47 (0.30)	0.10 (0.18)	0.41 (0.32)	1<2, p<0.01 3<4, p<0.01 1<3, p<0.01
ENDOW	0.20 (0.27)	0.18 (0.22)	0.22 (0.29)	0.20 (0.26)	-

Standard deviations in parenthesis

Chapter 6

Fehr-Schmidt Process Fairness and Dynamic Consistency

This chapter proposes a model of individual preferences for process fairness that complements the Fehr-Schmidt model for outcome fairness. The process model successfully predicts data from bargaining games that neither outcome-based nor reciprocity models can explain. Introducing process fairness raises issues of dynamic consistency of fairness preferences. The paper discusses theoretical and policy implications of inconsistency in dynamic decision contexts. Applications to welfare improvements through coercion-free paternalism and to Machina's parental example for non-expected utility illustrate the integration of the process model in economic theory.[18]

6.1. Introduction

Consider a mother who has one son and one daughter, but only one candy. She is indifferent between giving the candy to either child, but, in apparent violation of expected

[18] This chapter is based on Trautmann (2007b).

utility, she strictly prefers to flip a coin to decide which child will receive the candy (Machina 1989). She accordingly flips the coin and gives the treat to the winning child, say the daughter. Her son complains that his sister receives a candy while he does not. The mother does not grant his complaint pointing out the procedure was fair.

The parental example illustrates two notions of fairness: the mother cares about *process* fairness and her son considers *outcome* fairness. Both types of fairness have been found to matter empirically, also in the presence of real incentives (Kagel and Roth 1995, Cox 2004, Bolton et al. 2005, Frey and Stutzer 2005, Brockner 2006, Gächter and Riedl 2006). Incorporating fairness into economic theories requires operational models of individual preferences. Fehr and Schmidt (1999) proposed a parsimonious model of inequality aversion that has been widely used in economic applications. It efficiently combines empirical realism and analytical tractability. It, however, considers only outcome fairness.

This paper proposes a model of process fairness for allocation problems that is based on the Fehr-Schmidt functional form. It provides an operational and tractable way to incorporate process fairness into economic analyses and complements the outcome based Fehr-Schmidt model. In an ultimatum game framework with random proposals the model explains experimental data that neither outcome fairness nor intention-based reciprocity can explain (Rabin 1993, Fehr et al. 1993, Offerman 2002, Dufwenberg and Kirchsteiger 2004, Falk and Fischbacher 2006, Cox et al. 2006).

With both process and outcome evaluations of fairness possibly being applied by agents, consistency problems in a dynamic decision context under risk can occur (Volij 1994, Caillaud and Jullien 2000, Cubitt et al. 2004). This is the case if prior to the resolution of risk the agent cares about the fairness of the process and after the resolution of risk she cares about the fairness of the outcomes. Dynamic inconsistency is theoretically undesirable because with such preferences agents can be turned into money pumps (Schick 1986, Cubitt and Sugden 2001). However, using arguments of Strotz (1955-1956) and Machina (1989), we find that agents may consistently adhere to the procedural view.

Empirically, dynamic inconsistency matters for policy questions. Inconsistent preferences must be considered in the derivation of optimal institutions and policies. This has been a prominent issue in risky choice and intertemporal choice (Caplin and Leahy 2003, Bernheim and Rangel 2005). Within the framework of the Fehr-Schmidt models for outcome and process fairness, the implications of dynamic consistency problems for policy can be formally analyzed also for fairness preferences.

The model of process fairness successfully predicts experimental data, but it is not limited in its application to the analysis of laboratory games. The tractable preference function can be applied more generally to economic problems where the allocation of resources is uncertain, or can be made uncertain by introducing random procedures. We consider applications of the process model to two problems: first, preferences for process fairness can give rise to situations in which a coercion-free form of paternalism increases organizational efficiency and individual welfare. Second, Machina's (1989) parental example for non-expected utility is discussed under the assumption of fairness preferences. The process model supports Machina's (1989) notion of “resolute choice” in a situation where incomplete modeling of the decision problem leads to non-expected utility preferences. These applications show how the model can be fruitfully integrated in economic theories.

Allocation mechanisms like auctions or divide-and-choose have sometimes been called fair processes because they lead to fair outcomes (Young 1994). The empirical fairness literature has shown, however, that people care about the fairness of a process independently of the actual outcomes (Tyler and Lind 2000, Brockner et al. 2005). In a simple divide and choose procedure the second mover may derive satisfaction from getting at least half of the pie (outcome fairness), but also directly from the fact that both players were put on equal footing and could expect equal shares a priori (process fairness). Such preference for process fairness becomes particularly relevant in allocation problems involving indivisible goods, as in the parental example or in a military draft lottery, where no fair allocation of outcomes is possible.

The chapter is organized as follows. The next section introduces the process extension of the Fehr-Schmidt model. Section 6.3 derives predictions in ultimatum games with random allocations and discusses empirical data. Section 6.4 deals with consistency problems in a dynamic decision framework under risk. Section 6.5 presents applications to economic theories and the last section concludes.

6.2. The Process Fehr-Schmidt Model

This section introduces a model of fair processes that is based on the functional form of the Fehr-Schmidt (1999) model of inequality aversion. The Fehr-Schmidt model has been widely used in economics to model outcome fairness because of its efficient combination of empirical realism and analytical tractability. Using its basic structure allows us to model process fairness in a similarly operational and tractable way.

Let there be two agents A and B who face uncertainty about their payoffs. Let X denote the random variable from which agent A's payoff is drawn, with E[X] its expectation. Y denotes the random variable from which agent B's payoff is drawn and E[Y] its expectation. Let $x \in \mathbb{R}$ and $y \in \mathbb{R}$ denote the actual payoffs of agents A and B. Fehr and Schmidt (1999, p. 822) propose the following utility function, the *Outcome Fehr-Schmidt Model*, to account for fairness preferences:

$$U_A(x, y) = x - \alpha_A \max\{y - x, 0\} - \beta_A \max\{x - y, 0\} \tag{6.1}$$

with $0 \leq \beta_A < 1$ and $\beta_A \leq \alpha_A$. This utility function takes the agent's social preferences into account by reducing the utility of the final monetary payoff in the case of unequal payoffs, i.e. $x \neq y$. The utility reduction is larger when the inequality is disadvantageous than when it is advantageous, because $\beta_A \leq \alpha_A$. From $\beta_A < 1$ it follows that the agent's utility is always increasing in own payoff, and non negativity of the parameters implies that there is no inequality seeking.

The Fehr-Schmidt model explains preference for fair outcomes. In allocation problems, however, agents often care about the fairness of the process that generates and implements the outcomes (Brockner et al. 1995, Frey and Stutzer 2005). Determinants of process fairness that have been identified in the empirical literature include the availability of equal chances, kindness and decent treatment, and the clarity of the allocation procedure. These factors are potentially important for process fairness, but most of them cannot easily be formalized and measured to incorporate them in economic theory. In economic settings involving uncertainty, the agents' *expected* payoffs may be used as a measure of the fairness of the allocation process. Expected payoffs will directly influence process fairness perceptions in many situations, and they can also serve as a proxy for other, more intangible determinants of fairness. An ambiguous allocation process may lead agents to hold lower payoff expectations than a clear allocation process.

To obtain a tractable formal model we therefore assume that agents evaluate the fairness of a situation involving a random allocation procedure by comparing expected payoffs. The following utility function, the *Process Fehr-Schmidt Model*, accounts for preferences for fair income distributions by considering expected payoff differences between the agents:

$$U_A(x, X, Y) = x - \alpha_A \max\{E[Y] - E[X], 0\} - \beta_A \max\{E[X] - E[Y], 0\} \tag{6.2}$$

with $0 \leq \beta_A < 1$ and $\beta_A \leq \alpha_A$. This utility function takes the agent's social preferences into account by reducing the utility of the final monetary payoff in the case of unequal expected payoffs, i.e. $E[X] \neq E[Y]$. As in the final payoff model the utility reduction is larger when the inequality is disadvantageous than when it is advantageous. Now $\beta_A < 1$ implies that the agent's expected utility is increasing in own expected payoff. Non-negativity of the parameters implies that there is no inequality seeking in expected payoffs.

The different fairness concepts of the process model and the outcome model lead to different evaluations of identical outcomes, depending on the generating process. The value of an allocation (x,y) with $x \neq y$, drawn from random variables X and Y with equal expected value, is reduced because of inequality in the outcome model but not in the process model. On the other hand, the value of an allocation (x,y) with x = y, drawn from random variables X and Y with unequal expected values, is reduced in the process model but not in the outcome model.

Although various factors influence fairness perceptions, modeling process fairness through expected payoff differences maintains the tractability of the model while serving as a good approximation in many settings. Expected payoffs are comparable across situations and people, and the utility function can easily be assessed empirically (Camerer and Fehr 2004).

The outcome Fehr-Schmidt model has been used in theoretical work on finance (Gebhardt 2005), contract theory (Englmaier and Wambach 2005, Fehr et al. 2007), and social choice (Trautmann 2007). These applications involve decisions under uncertainty and process fairness becomes potentially important. Complementing the outcome Fehr-

Schmidt model by an extension to expected payoffs provides a unified framework for the analysis of economic problems if both outcome fairness and process fairness can play a role. A similar approach was taken by Bolton et al. (2005). To explain empirical data they considered a procedural extension of the outcome fairness model of Bolton and Ockenfels (2000).

The process fairness model has been introduced in this section for the two-person case. An extension to the N-person case is given in the appendix. A crucial assumption in the N-person case is that an agent compares her own expected payoff to each other agent's expected payoff. That is, in a three person situation an agent will discount her outcome utility if she faces an expected payoff ξ and the other two persons have expected payoffs $\frac{1}{2}\xi$ and $\frac{3}{2}\xi$. Being better off in expectation than one person and being worse off than the other person both reduces her utility, although her expected payoff equals the group's average expected payoff.

The multiple comparisons in the Fehr-Schmidt framework imply very different predictions in N-person games compared to models based on Bolton and Ockenfels (2000), where agents compare themselves to the average, or Charness and Rabin (2002), where agents consider only the worst-off person and the group's total payoff. Engelmann and Strobel (2004) discuss the different predictions of these models for outcomes fairness in three person games.

6.3. Random Ultimatum Game Predictions

This section derives the prediction of the process Fehr-Schmidt model in a bargaining game with risk and discusses experimental evidence. The process model successfully predicts empirical data that can neither be explained by outcome fairness nor by reciprocity.

6.3.1. Definitions and Notation

Consider the following *Random Ultimatum Game*: A random device proposes a partition of a pie of size 1 into a share x offered to a responder, and a share 1-x for a passive player.

Before the proposal is randomly selected, the responder has to announce which offers she will accept and which she will reject. In case of acceptance of an actually selected offer, both players receive their proposed share. If the offer is rejected, both players receive nothing.

Let X denote the random variable from which the *offer* x is drawn. Let F(x) denote its distribution function and E[X] its expectation. Accordingly, E[1-X] is the passive player's expected offer. A random proposal is *fair* if $E[X]=\frac{1}{2}$, and *unfair* if $E[X]\neq\frac{1}{2}$. The random process used to determine the offers is common knowledge.

The utility of a responder who applies the outcome Fehr-Schmidt model is

$$U(x)=x-\alpha\max\{(1-x)-x,0\}-\beta\max\{x-(1-x),0\}. \tag{6.3}$$

The utility in the case of a rejection is assumed to be zero: in this case there is no payoff and no inequality.

Let $\tilde{X}$ and $\tilde{Y}$ denote the random variables from which the actual *payoffs* of the responder and the passive player are drawn conditional upon the announcement of the responder. These random variables are generated by the original offer distribution and possible rejections of the responder. The utility of a responder who applies the process Fehr-Schmidt model becomes

$$U(x,\tilde{X},\tilde{Y})=x-\alpha\max\{E[\tilde{Y}]-E[\tilde{X}],0\}-\beta\max\{E[\tilde{X}]-E[\tilde{Y}],0\}. \tag{6.4}$$

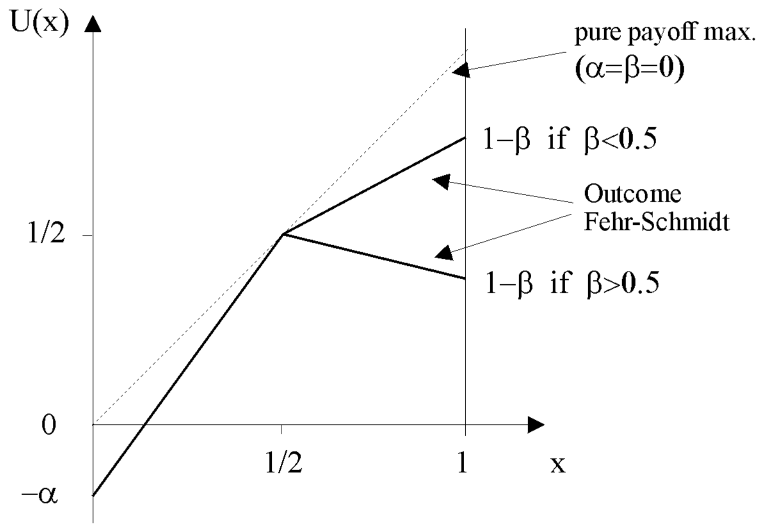
If the responder does not reject any offer, the expected actual payoffs are equal to the expected offers, i.e. $E[\tilde{X}]=E[X]$ and $E[\tilde{Y}]=E[Y]$.

It will be shown that in both models the responder's rejection behavior can be described by a threshold value, her *minimum acceptable offer* (hereafter MAO): all offers larger than the MAO are accepted, and all lower offers are rejected.

6.3.2. Predictions of the Outcome Model

In the outcome Fehr-Schmidt model the responder's utility can become negative for small offers. It is increasing in her own offer and always positive for shares larger than half of the pie. Figure 6.1 plots the utility for shares between zero and one.

Figure 6.1: Responder's Utility as Function of own Share



In this model the responder anticipates her ex-post utility of being offered a possibly unfair payoff share before the uncertainty is actually resolved. To maximize her expected utility in the random ultimatum game she will announce to reject all offers that give her negative utility and to accept those that give her positive utility ex-post, regardless of the underlying offer distribution F(x). If a rejected offer is drawn, her actual utility will be zero. Since utility is always positive for advantageous inequality, we calculate the MAO (denoted as x_m) by considering only disadvantageous inequality and maximizing $\int_{x_m}^{0.5} [x-\alpha(1-x-x)]dF(x)$ with respect to x_m . We obtain the expected utility maximizing MAO

$$x_m = \frac{\alpha}{1+2\alpha}. \tag{6.5}$$

The MAO lies between zero and half of the pie, and is strictly increasing in α. For all subjects with $\alpha > 0$, i.e. for those who are not purely selfish, the MAO is strictly positive for all possible underlying offer distributions.

This threshold is identical to the one that Fehr and Schmidt (1999, p. 826) derive for decisions under certainty. A subject who evaluates fairness from the final outcome perspective announces the same positive MAO for both fair and unfair offer processes.

6.3.3. Predictions of the Process Model

In the process model the responder's utility depends on both players' expected payoffs. If the expected payoffs $E[\tilde{X}]$ and $E[\tilde{Y}]$ are not equal, the responder will experience a reduction in utility for both accepted and rejected offers ex-post. By announcing to reject some unfair offers before the proposal is drawn, she can affect the actual payoff distribution and make the expected payoffs more equal ex-ante. There is, however, a trade-off between fairness and monetary payoffs, because by rejecting offers she always reduces her own expected monetary payoff.

First, consider a fair random offer process with $E[X]=0.5$. Then the inequality aversion terms drop from the process Fehr-Schmidt utility function (6.4) and the responder's utility becomes $U(x,\tilde{X},\tilde{Y})=x$. She will not reject any offer because otherwise her expected payoff will be reduced and expected advantageous inequality will be created. Thus for all levels of inequality aversion to expected payoff differences, the responder will announce a zero MAO.

Second, consider an advantageously unfair random process with $E[X]>0.5$. Rejecting some of her advantageous offers, the responder can reduce the expected payoff difference $E[\tilde{X}]-E[\tilde{Y}]$. By rejecting such offers expected payoffs can become equal at some point. As we have seen, at this point she will not reject any more offers, and we therefore consider only the advantageous inequality aversion term in utility function (6.4) when calculating her expected utility. As long as there is some difference in expected payoffs, however, the responder will experience a utility reduction for both accepted and rejected offers ex-post. Letting D denote the set of accepted offers, the responder's expected utility becomes

$$\int_{x\in D}[x-\beta(E[\tilde{X}]-E[\tilde{Y}])]dF(x)+\int_{x\notin D}[0-\beta(E[\tilde{X}]-E[\tilde{Y}])]dF(x). \tag{6.6}$$

This equals (see appendix)

$$\int_{x\in D}[x-\beta(2x-1)]dF(x)$$

and we observe that because of $\beta<1$ expected utility is maximized by accepting all offers. The loss in expected utility owing to a rejection of any advantageous offer is larger than the gain from reducing expectational inequality. Facing an advantageously unfair offer process the responder will announce a zero MAO.

Third, consider a disadvantageously unfair random process with $E[X] < 0.5$. If the responder announces a zero MAO, she will experience a utility reduction because of the expected payoff difference. Increasing her MAO somewhat, she can increase her expected utility if the marginal reduction in her own expected payoff is smaller than the marginal gain from reducing expected payoff differences. She will continue to increase the MAO until either of two cases obtains. In the first case, by increasing her MAO at some point the marginal gain from reducing unfairness equals the marginal loss from own expected payoff reduction, but her expected payoff is still lower than the passive player's expected payoff. The optimal MAO therefore involves disadvantageous inequality in expected payoffs, and for all actual offers, whether they are accepted or rejected, she will experience a utility reduction. This optimal MAO can be calculated considering only the disadvantageous inequality term in (6.4) by maximizing the expected utility

$$\int_0^{x_m}[0-\alpha(E[\tilde{Y}]-E[\tilde{X}])]dF(x)+\int_{x_m}^1[x-\alpha(E[\tilde{Y}]-E[\tilde{X}])]dF(x) \tag{6.7}$$

with respect to x_m (see appendix). Maximization gives $x_m = \alpha/(1+2\alpha)$: in this case where the responder does not eliminate expected disadvantageous inequality completely, the optimal MAO is identical to the one in the outcome Fehr-Schmidt model.

In the second case, by increasing her MAO at some point the players' expected payoffs become equal, but the current MAO is still lower than the value $x_m = \alpha/(1+2\alpha)$. Because there are no more gains possible from increasing the MAO, this value is her expected utility maximizing MAO. The second case obtains if there is a solution x_m to the equation

$$E[\tilde{X}] = E[\tilde{Y}] \Leftrightarrow \int_{x_m}^1 x dF(x) = \int_{x_m}^1 (1-x)dF(x) \tag{6.8}$$

that is smaller than $x_m = \alpha/(1+2\alpha)$.

A subject who evaluates fairness from the process perspective announces a zero MAO if she faces a fair offer process or an advantageously unfair offer process. For disadvantageously unfair offer processes she announces a MAO smaller or equal to

$x_m = \alpha/(1+2\alpha)$, the optimal MAO for all processes under the outcome fairness perspective.

6.3.4. Experimental Evidence

The following table summarizes the predictions of the process and the outcome Fehr-Schmidt model for the empirically relevant cases of fair and of disadvantageously unfair random offers. It also includes intention-based reciprocity models that predict zero MAO for all random offer processes: low offers cannot be interpreted as unkindness and the other player is therefore not punished by a rejection. The table assumes α>0.

Table 6.1: Predictions for Random Offers

	Fair random offers $E[X]=0.5$	Unfair random offers $E[X]<0.5$
Outcome Fehr-Schmidt	MAO = $\alpha/(1+2\alpha)$	MAO = $\alpha/(1+2\alpha)$
Process Fehr-Schmidt	MAO = 0	0< MAO $\leq \alpha/(1+2\alpha)$
Intention-based Reciprocity	MAO = 0	MAO = 0

Experimental results in Blount (1995), Bolton et al.(2005) and Cox and Deck (2005) indicate the empirical relevance of the pattern of rejections in fair and unfair random treatments predicted by the procedural fairness model. Blount showed that responders who are willing to sacrifice significant amounts of money to avoid unfair outcomes in human proposer treatments, i.e. those for whom fairness considerations matter, often accept very unfair offers in an a priori fair random proposal treatment. Bolton et al. also reported low rejection rates for disadvantageously unequal actual offers from fair random processes. This stands in contrast to the predictions of the outcome Fehr-Schmidt model, but can be explained by the process model or reciprocity.

On the other hand, Bolton et al. and Cox and Deck found significant rejection rates for unequal offers drawn from unfair random processes. Cox and Deck for instance report rejection rates of 23% for the unfair alternative in a mini-ultimatum game with randomly determined offer (see treatment “Punishment Control 2” on p. 627). The passive proposer’s expected offer was \$6.50 and the responder's expected offer \$3.50, establishing an unfair procedure. Reciprocity cannot explain rejections in this game, but both inequality aversion models can account for this rejection behavior. Only the predictions of the procedural model are in line with the overall pattern of rejections observed in these random ultimatum game experiments.

Numerous empirical studies have established the importance of outcome fairness and reciprocity for economic decision making. Fewer studies have looked into the role of process fairness. The above discussed bargaining studies have shown that apart from outcome fairness and intentionality, process fairness is another relevant factor in social preferences. Broome (1984) discusses real life examples in which process fairness considerations explain decisions in legal cases, in medical problems (allocation of scarce organ donations), and in the recruitment for military service (Vietnam draft lottery). Operational models of process fairness can complement models of outcome fairness and intention-based reciprocity and are, therefore, a step towards more complete models to integrate the different fairness concepts while maintaining tractability.

A unified theoretical framework for outcome and process fairness can stimulate new empirical tests of the possible interaction between the two fairness notions and reciprocity. Falk, Fehr and Fischbacher (2007) study second-mover behavior in a game where the first-mover may either help or hurt the second-mover. They compare second movers' reactions to first-mover proposals made by other subjects with second-movers' reactions to random proposals drawn from an advantageous distribution with helpful moves more likely. In line with both reciprocity and process fairness, little punishment or rewarding was observed for the advantageous random proposals, although the data suggest at least some influence of outcome fairness. These results could be compared with second-movers' reactions to random proposals drawn from a disadvantageous distribution. Given the new prediction of the process model for this case, this would identify the effects of reciprocity, outcome fairness, and process fairness within one experimental setting.

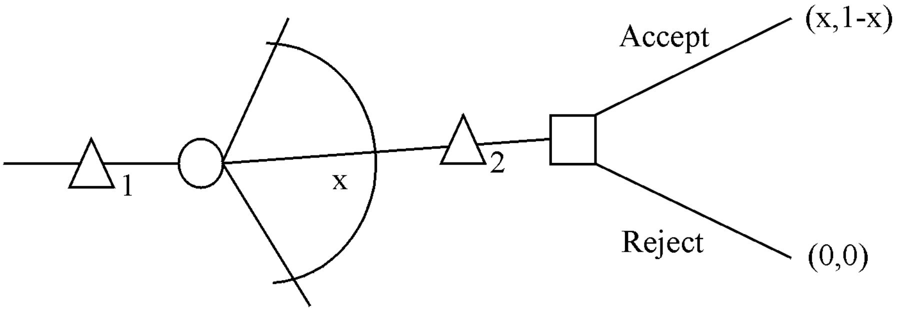
6.4. Process vs. Outcome Fairness in a Dynamic Decision Context

The consistency of preferences in a dynamic decision context has been widely discussed in the literature for risky choice and intertemporal choice, but not for fairness (Strotz 1955-

1956, Machina 1989, Loewenstein and Prelec 1992, Volij 1994, Caillaud and Jullien 2000, Caplin and Leahy 2001, Cubitt et al. 2004). With both process and outcome fairness evaluations possibly made by agents, dynamically inconsistent fairness preferences can occur. This happens if before the resolution of uncertainty agents care about the fairness of the process and after the resolution of uncertainty they care about the fairness of the outcomes. We show that dynamic inconsistency is theoretically undesirable for the process model, and that arguments by Strotz (1955–1956) and Machina (1989) can be used to defend the consistent application of the process fairness view.

Consider the random ultimatum game defined above and describe it as a dynamic choice problem for the responder as depicted in Figure 6.2.

Figure 6.2: Decision Tree Illustrating the Responder's Dynamic Choice Problem



In this decision tree a round chance node (the random proposal of the players' shares) is followed by a square choice node (accept/reject the proposal), defining a dynamic choice setting. The outcomes in this decision problem are the final allocation of the pie for the two players (x, 1-x), $x \in [0,1]$, in case of acceptance, and the allocation (0,0) if the responder rejects. Further, let Δ denote the point in time where the responder has to commit to a decision in the decision node for each possible final allocation (x, 1-x). Within the Fehr-Schmidt framework, this is equivalent to committing to an acceptance threshold, or MAO, as discussed above. In particular, Δ_1 refers to a commitment prior to the resolution of the uncertainty, while Δ_2 refers to a posterior commitment, i.e. after the uncertainty has been resolved. Whereas the former implies a decision with future risk, the latter involves no future risk.

At both the prior and posterior point of commitment (1 or 2), both the process (P) and the outcome (O) perspective on fairness are conceivable. The following table organizes the four possible combinations.

Table 6.2: Dynamic Consistency under Process and Outcome Fairness

	Prior commitment Δ_1	Posterior commitment Δ_2
Process Fehr-Schmidt	P_1	P_2
Outcome Fehr-Schmidt	O_1	O_2

An agent who consistently applies the outcome model before and after the uncertainty is resolved has the choice pattern (O_1, O_2). Which final allocations she accepts and rejects depends on her inequality aversion parameters and on the fairness of the final outcomes. If she accepts (rejects) an allocation at the prior point of commitment, then she will also accept (reject) that allocation at the posterior point of commitment. The outcome model requires the agent to evaluate fairness in each branch of the decision tree separately in a forward looking manner.

An agent who consistently applies the process model, on the other hand, has the choice pattern (P_1, P_2). Her acceptance or the rejection of final allocations depends on her inequality aversion parameters and the fairness of the expected outcomes in the random ultimatum game. Again, if she accepts (rejects) an allocation at the prior point of commitment, then she will also accept (reject) that allocation at the posterior point of commitment. Note that the process model always requires the agent to consider the whole decision tree when deciding on which allocations to accept or to reject (Machina 1989). The fairness of the *expected outcomes* in the random allocation is not affected by the fact that the randomization resulted in a final allocation that gives very unequal *final outcomes* to the agents. Only the fairness of the expected outcomes matters in the process fairness model.

Consistent application of either fairness view is desirable for reasons discussed below. It is, however, conceivable that at the prior point of commitment Δ_1 the responder considers the fairness of the process in her decision, and at the posterior point Δ_2 she considers the fairness of the outcomes. Before the uncertainty is resolved, only process information is available and she may not take a strictly consequentialist view and may not anticipate her valuation for every possible outcome before the uncertainty is actually resolved. At point Δ_2 the responder is actually confronted with a final allocation outcome and there is no more uncertainty about the payoffs. The final outcome is more salient than the risks borne in the past by the players and she may not consider the prior chances of allocations that did not materialize, therefore applying the outcome view. The agent is led to a dynamically inconsistent choice pattern (P_1, O_2) and may want to reject different allocations before the resolution than after the resolution of the uncertainty.

As an example consider a random allocation that gives the whole pie either to the responder or to the passive player, with equal chance. Finding herself in position Δ_1 facing the fair random process, the responder may decide not to reject any offers because of a procedural fairness evaluation. Once she reaches position Δ_2 and faces the situation where the other person receives the pie and she receives nothing, the unfairness of the actually determined allocation becomes more relevant and she may therefore apply the outcome perspective on fairness. She prefers to change her plan made at position Δ_1 and reject the previously accepted offer.

Dynamically inconsistent preferences are theoretically undesirable because agents with such preferences can be turned into money pumps (Schick 1986, Cubitt and Sugden 2001). Consider an agent holding the process view before, and the outcome view after the resolution of uncertainty. Assume that according to the outcome model she would reject an outcome of x=0.2 if the other person receives 1-x=0.8. Endow her with a fair random ultimatum game in which she is *required to reject* the outcome x=0.2. All other outcomes can be rejected or accepted as she likes. Because the process is fair, prior to the random allocation she wants to accept all offers and is therefore willing to pay some small amount ε to turn the required rejection of x=0.2 into a *required acceptance*. Then the random allocation is determined. For all x≠0.2 she will make her acceptance decision as described by the outcome Fehr-Schmidt utility function, but her payoff is lower by ε compared to her initial position because of the payment before. For x=0.2 she is now required to accept, but, because of her outcome fairness preference, she would rather like to reject this allocation such that both agents receive zero. She will therefore be happy to pay some small amount ε to switch back to a rejection of the outcome x=0.2. The agent accepted a sequence of trades that led her from her initial endowment to a situation that is worse by at least ε in every state of the world. She has been turned into a money pump.

Strotz (1955--1956) discussed dynamic inconsistency in risk free consumption choices. He distinguished three possible ways in which the individual may deal with the inconsistency. We can apply these to our setting of fairness under risk. First, the subject may be ignorant about her inconsistent preferences. In a situation as described above she will therefore change her previous decision at point Δ_2 if given the opportunity to do so, leading to choices (P_1, O_2) and possible exploitation by arbitrageurs. Alternatively, the subject may recognize at the prior point that her preferences will change once she will reach the posterior point Δ_2. She can then follow two strategies at point Δ_1. Either she can try to irrevocably commit to a decision, thereby imposing her current preferences also on her future self at position Δ_2. This leads to consistent choices (P_1, P_2). Or she can follow the strategy of consistent planning without commitment, anticipating her own future preferences and making a choice at Δ_1 that will be optimal given her preferences at Δ_2. This latter strategy implies that she has to accept the final outcome model at the prior point and make choices (O_1, O_2).

Apart from possibly costly pre-commitment, agents may also avoid dynamically inconsistent behavior by a “resolute choice’ (McClennen 1988, Machina 1989). Machina argued that non-expected utility maximizers violate separability across mutually exclusive events prior to the resolution of uncertainty in a dynamic risky choice situation: the outcome in one state of the world is not evaluated independently of outcomes in other states, although only one of these outcomes will eventually be obtained and consumed. Dynamic inconsistency can occur if after the resolution of uncertainty the agent evaluates the obtained outcome in isolation from the forgone outcomes in other states. This would imply that in the posterior situation the agent applies the separability and uses the outcome evaluation that she has rejected at the prior point. Machina argues that non expected utility maximizers will not perceive risks borne in the past as irrelevant and will want to incorporate forgone alternatives into the posterior evaluation. They therefore adhere to their prior decision and do not make dynamically inconsistent choices.

The same point holds for agents having the procedural view on inequality: prior to the resolution of the uncertainty the agents do not regard the mutually exclusive final allocation outcomes as separate. They reject consequentialism. After the risk has been resolved they will not perceive the forgone alternatives as irrelevant for the evaluation of the fairness of the situation and will resolutely adhere to their prior choice, leading to dynamically consistent choices (P_1, P_2). They do not perceive unfairness knowing that the process was fair. Psychological evidence supports this argument. A primacy effect has been found in fairness evaluations: if process information is made available before outcome information, procedural fairness evaluations are more prevalent. If process information succeeds outcome information, outcome evaluations are more common (Tyler and Lind, 2000). In the above framework, early outcome information leads agents to focus on separate outcomes and to apply the consequentialist perspective. Early process information, on the other hand, emphasizes the relation between outcomes in different states of the world, leading to a global perception with outcomes non-separable, which is maintained also after the actual outcome has been revealed in agreement with resoluteness.

The arguments of Strotz and Machina show that the procedural view can indeed be consistently implemented by either sophisticated pre-commitment or a resolute choice. In the former case the agent understands her consistency problem and is willing to incur some costs to impose her prior preferences on her posterior self. In the latter case the agent rejects separability of the outcomes in mutually exclusive events and does not have a consistency problem at all. By immunizing an agent with process fairness preferences against exploitation by money pumps, these arguments provide a sound theoretical basis for process fairness.

If dynamically inconsistent fairness preferences matter empirically in allocations involving uncertainty, however, this must be considered in the design of optimal policies and institutions. There can be justification for welfare improving government intervention once the welfare criterion has been defined. The definition of the welfare criterion is not always obvious under inconsistent preferences (Bernheim and Rangel 2005). In the following section an application is discussed where a planner can unambiguously improve the welfare of dynamically inconsistent agents.

6.5. Applications

This section considers implications of process fairness for two problems discussed in the economics literature: welfare improvements through coercion-free paternalism, and Machina's (1989) parental example for non-expected utility ("Machina's Mom"). For simplicity the examples assume that agents care about fairness, i.e. inequality aversion parameters α and β are both positive, and that the parameters are identical for different agents.

Coercion-free Paternalism. Procedural fairness evaluation can give rise to situations in which a coercion-free form of paternalism can be applied to increase societal efficiency and individual welfare (Camerer et al. 2003, Thaler and Sunstein 2003). Consider a society consisting of two myopic and dynamically inconsistent agents A and B, each having utility function (6.2) at the prior and (6.1) at the posterior point in a random allocation. We repeatedly allocate an indivisible dollar to one of the agents using a fair random device. If either agent rejects an allocation, none of them receives the dollar. The myopic subjects perceive each allocation period as separate and no side payments are possible. The total utility of an agent is the sum over all period utilities, and there is no discounting.

Prior to the resolution of uncertainty in every period the agents want to accept both possible final outcomes, because the indivisible dollar is allocated by a fair random process and the procedural view on fairness is applied. After the risk has been resolved, however, the agents apply the outcome view on fairness and the respective utilities of the proposed allocation are $1–\beta$ and $–\alpha$. The agent who does not receive the dollar rejects the allocation so that both agents receive nothing. This happens every period and the total utility from the allocation of the indivisible dollars will be zero for both agents. Applying the outcome view ex-post but sticking to the ex-ante planned acceptance of all offers, their average total utility would equal $0.5(1–\alpha–\beta)$ in the limit. On average each agent gets half of the dollars, but experiences negative utility from inequality of the allocations.

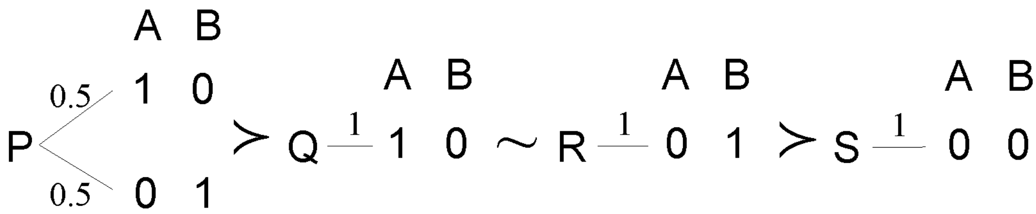
Now consider a social planner who in every period can oblige an agent to costlessly and irrevocably commit to her decision at any point in time in that period if the agent is not strictly against the commitment. The planner can increase total utility for both agents if $1-\alpha-\beta>0$ by making them commit to their prior decision in every period. If the inequality holds, the negative utility experienced from unequal allocations ex-post by each agent is lower than the gain from getting the dollar in half of the periods on average. Because prior to the random allocation the agents want to accept any outcome and do not anticipate the change in their preferences, they are indifferent between committing or not committing to maintain their prior decision at the posterior point. The planner helps the agents to overcome their problems of myopic fairness evaluations and total utility maximization. No agent is forced, however, to make a choice that is strictly less preferred than some alternative and is stipulated by the social planner: the planner makes a choice for the agent only in a case where the agent is indifferent, and the less preferred choice of accepting a bad outcome at the posterior point is stipulated by the agent herself.

Furthermore, the planner need not decide whether the ex-ante process view or the ex-post outcome view represents the preferences of the agents that should be used in welfare analysis. For both preferences total utility is higher for the agent under the commitment policy.

In practical applications such coercion-free paternalism can take the form of letting the agents of an organization publicly announce their agreement with the procedure before the risk is resolved. If social norms exist that support such public agreements, the commitment increases organizational efficiency when procedures are fair but outcomes are usually unfair.

Machina's Mom. Machina (1989) gave the example of a mother who has a single indivisible item that she can either give to her daughter Abigail or to her son Benjamin. She is indifferent between giving it to either child and strictly prefers both situations to that where neither child receives the item. However, in seeming violation of expected utility theory, the mother strictly prefers a fair coin flip over either sure allocation of the item (and over any unfair random allocation). Her preferences are summarized in Figure 6.3.

Figure 6.3: Mother's Preferences in Machina's Parental Example



Assume that there is no positive utility of gambling involved and that the children have to accept any decision made by their mother. Given that the mother is indifferent between giving the item to either child for sure, it seems difficult to explain her strict preference for randomization. We can, however, give a justification of the *mother's* preferences in terms of other-regarding preferences *of her children*. Let V denote the mother's utility and let U_A and U_B denote her children's utilities. The mother's utility is a function of her children's utilities, $V(U_A, U_B)$, and her utility is increasing in both arguments. Consider three cases for the children's preferences: her children are selfish (normalize $U_i(0)=0$ and $U_i(1)=1$), care about outcome fairness, or care about process fairness. For each case and each allocation procedure P, Q, R , and S, the following table shows the utilities of Abigail and Benjamin (U_A, U_B):

Table 6.3: Children's Utilities under Different Fairness Preferences

	P	Q	R	S
Selfish	$(1, 0)$: $\frac{1}{2}$; $(0, 1)$: $\frac{1}{2}$	(1, 0)	(0, 1)	(0, 0)
Outcome F-S	$(1-\beta, -\alpha)$: $\frac{1}{2}$; $(-\alpha, 1-\beta)$: $\frac{1}{2}$	$(1-\beta, -\alpha)$	$(-\alpha, 1-\beta)$	(0, 0)
Process F-S	$(1, 0)$: $\frac{1}{2}$; $(0, 1)$: $\frac{1}{2}$	$(1-\beta, -\alpha)$	$(-\alpha, 1-\beta)$	(0, 0)

We observe that if the mother is indifferent between Q and R, a preference for the gamble P over the sure allocations Q or R cannot be explained if her children are selfish or inequality averse in final outcomes. In the selfish case we have V(1,0)=V(0,1) because the mother is indifferent between Q and R, and the gamble P is therefore not preferred. The children only care about their own payoff and, thus, randomization does not make anyone better off. In the outcome inequality aversion case, we have $V(1-\beta, -\alpha) = V(-\alpha, 1-\beta)$ and again the gamble P is not preferred. The child that does not receive the item will inevitably start weeping both with and without randomization.

If her children apply the procedural view on fairness, however, a choice of P over both Q and R is rational under expected utility. The mother is indifferent between giving the item either to Abigail or to Benjamin directly, and her utility is increasing in both children's utility. Therefore

$$V(-\alpha, 1-\beta) = V(1-\beta, -\alpha) < V(1, 0) = V(0, 1). \tag{6.9}$$

Under expected utility the mother will prefer the gamble P over either of the sure allocations Q and R. While the mother appears to violate expected utility if we describe her outcomes as (A,B) = (Abigail receives the item, Benjamin receives the item), she does not violate expected utility if we incorporate procedural fairness preferences of her children and describe her outcomes as (U_A, U_B). She will not end up having a headache because neither child has negative utility under fair randomization. Her preferences are rational.

In the context of temporal risk Machina (1984) argued that a complete modeling of the decision problem is often impossible or makes the analysis intractable. Incomplete modeling can lead to violation of expected utility, however, and non-expected utility models must be used to describe agents' preferences. This happens in the parental example if we cannot model the children's social preferences explicitly.

In the previous section we used Machina's (1989) concept of resolute choice to argue for a consistent implementation of the process Fehr-Schmidt model. Conversely, the process model can also support the resoluteness concept. In the parental example Benjamin tries to convince his mother after an unfavorable coin flip to repeatedly randomize. Abigail won the lottery and the mother wants to give her the item, but Benjamin argues that she announced to prefer a coin flip over giving it to Abigail and that she should randomize again. He tries to impose consequentialism on her after the first flip and make her behave dynamically inconsistently. The mother rejects to flip the coin again, however, and resolutely adheres to her prior decision. The process model can explain her resoluteness as we have seen above: giving in to Benjamin's request for a second chance would change the children's expected payoffs. It would lead to *unequal* expected payoffs and a lower expected utility for the mother than resolutely giving the item to Abigail after the (first) coin flip came out in her favor.

6.6. Concluding Remarks

The extension of the Fehr-Schmidt model to expected payoffs provides an operational and parsimonious way to formalize preferences for fair processes. It complements the outcome-based Fehr-Schmidt model, allowing for discussions of dynamic choice problems in fairness. The model's empirical predictions are supported by experimental evidence from ultimatum games involving risk, indicating the relevance of process fairness next to outcome fairness and intention-based reciprocity.

Consistent implementation of the procedural view in dynamic choice settings can be supported by Strotz's argument for sophisticated commitment and by Machina's argument for “resoluteness” of choice. Coercion-free paternalism in an organizational efficiency problem, and Machina's parental example have been discussed to illustrate how the model can be applied in the analysis of economic problems. An application to utilitarian welfare analysis is considered in Trautmann (2007).

In modern societies random allocation procedures are ubiquitous and outcomes are commonly accepted by the parties. Most modern societies follow the principle of *equality of opportunities* and not the principle of *equality* per se. This indicates the importance of process fairness evaluations outside the experimental laboratory and the need for operational models of process fairness preferences.

Appendix

6.A1. N-Person Process Fairness Model

Let there be N>1 agents. Let X_j denote the random variable from which agent j's payoff is drawn, with $E[X_j]$ its expectation. $X=(X_1,... X_N)$ is the vector of the random variables from which the agents' payoffs are drawn, and $x_j \in \mathbb{R}$ denotes the actual payoff of agent j. The N-person extension of the process Fehr-Schmidt model makes two assumptions concerning the payoff comparisons made by the agents. First, an agent will compare her own expected payoff with the expected payoff of each other agent, discounting her outcome utility if any of these expectations differ from her own. Second, the agent does not consider differences in third parties' expected payoffs directly in her utility apart from the effect through her own expected payoff. In these assumptions we follow the N-person model of Fehr and Schmidt (1999).

In the N-person case, the utility of an agent i who considers process fairness is given by the following function:

$$U_i(x_i, X) = x_i - \frac{\alpha_i}{N-1}\sum_{j\neq i}\max\{E[X_j] - E[X_i],0\}$$

$$-\frac{\beta_i}{N-1}\sum_{j\neq i}\max\{E[X_i] - E[X_j],0\} \qquad (6.A1)$$

with $0 \leq \beta_i < 1$ and $\beta_i \leq \alpha_i$. The normalization of the process fairness terms by N-1 ensures that the impact of fairness on the utility remains constant if the number of agents increases.

6.A2. Optimal MAOs in the Process Fehr-Schmidt Model

For an advantageously unfair random process (page 119) the responder's expected utility in the process model is

$$\int_{x\in D}[x - \beta(E[\tilde{X}] - E[\tilde{Y}])]dF(x) + \int_{x\notin D}[0 - \beta(E[\tilde{X}] - E[\tilde{Y}])]dF(x), \qquad (6.A2)$$

where D is the set of all offers she announces to accept. This can be rewritten as

$$\int_{x\in D} xdF(x) - \beta\int_0^1 (E[\tilde{X}] - E[\tilde{Y}])dF(x)$$

$$= \int_{x\in D} xdF(x) - \beta\int_0^1 \int_{z\in D}(z - (1-z))dF(z)dF(x)$$

$$= \int_{x \in D} [x - \beta(2x-1)] dF(x), \qquad (6.A3)$$

and is maximized by accepting all offers, thus announcing zero minimum acceptable offer.

For a disadvantageously unfair random process (page 120) the responder's expected utility in the process model is

$$\int_0^{x_m} [0 - \alpha(E[\tilde{Y}] - E[\tilde{X}])] dF(x) + \int_{x_m}^1 [x - \alpha(E[\tilde{Y}] - E[\tilde{X}])] dF(x). \qquad (6.A4)$$

This can be rewritten as

$$\int_{x_m}^1 x dF(x) - \alpha \int_0^1 (E[\tilde{Y}] - E[\tilde{X}]) dF(x)$$

$$= \int_{x_m}^1 x dF(x) - \alpha \int_0^1 \int_{x_m}^1 (1 - z - z) dF(z) dF(x)$$

$$= \int_{x_m}^1 [x - \alpha(1 - 2x)] dF(x).$$

Optimizing with respect to x_m we obtain $x_m - \alpha(1-2x_m)=0$ implying the expected utility maximizing MAO $x_m = \alpha/(1+2\alpha)$.

Chapter 7

Individual Fairness in Harsanyi’s Utilitarianism: Operationalizing All-inclusive Utility

Fairness can be incorporated into Harsanyi's utilitarianism through all-inclusive utility. This retains the normative assumptions of expected utility and Pareto-efficiency, and relates fairness to individual preferences. It makes utilitarianism unfalsifiable, however, if agents' all-inclusive utilities are not explicitly specified. This note proposes a two-stage model to make utilitarian welfare analysis falsifiable by specifying all-inclusive utilities explicitly through models of individual fairness preferences. The approach is applied to include fairness in widely discussed allocation examples.[19]

7.1. Introduction

Harsanyi (1955) derived the utilitarian social welfare function assuming expected utility for individuals and the social planner, and assuming the Pareto-principle. The result has been criticized because the linear form of the social welfare function, allegedly, precludes

[19] This chapter is based on Trautmann (2007a).

considerations of fairness by the social planner. Fairness can be incorporated into Harsanyi's framework through a description of social allocations that includes, apart from the individuals' personal situations, all interpersonal comparisons (Harsanyi 1955, Broome 1991, Binmore 1994, Karni 1996). Individual utilities over these complete descriptions of allocations are all-inclusive.

The all-inclusive utility approach allows the social welfare function to be based on the normatively convincing assumptions of expected utility and Pareto-efficiency, while considering agents' individual attitudes toward fairness. Fairness has a clear interpretation in terms of individual preferences and need not be considered separately by the social planner (Fischer and Torgler 2006, van Winden 2007). Without explicit specification of the individual fairness preferences, however, the approach deprives Harsanyi's theory from predictive power and makes it unfalsifiable. Utility becomes context-dependent and every preference of the social planner between allocations can be accommodated by including all fairness issues in the agents' utilities. This has usually been the stalemate position in which debates about utilitarianism end, with either accepting the unsatisfactory lack of fairness considerations or accepting a loss of operationality.

This paper offers a way out of the stalemate based on models of individual fairness preferences. The predictive power and falsifiability of utilitarianism with all-inclusive utility can be maintained by explicitly specifying testable and context-independent fairness preferences for individuals. A model is introduced that applies Sugden's (2000) two-stage procedure for game theory to operationalize fairness in all- inclusive utility and give empirical meaning to fairness in Harsanyi's utilitarianism. At the first stage agents evaluate risky *outcomes* (health states, wealth levels) by *self-interested* von Neumann-Morgenstern (vNM) utilities that evaluate risky options without social comparison. No fairness considerations enter at this stage. At the second stage the self-interested vNM utilities are then taken as inputs in models of individual fairness (Fehr and Schmidt 1999, Trautmann 2007b) to obtain *all-inclusive* vNM utilities. These models provide operational functional forms for fairness preferences and successfully explain empirical data. They can numerically be assessed for individual agents from observable choices.

The two-stage approach is applied to well known examples by Diamond (1967) and Broome (1991) which criticize utilitarianism on fairness grounds. Distinguishing between self-interested and all-inclusive vNM utilities allows us to derive convincing empirical predictions in these examples under utilitarianism.

The paper is organized as follows. The next section discusses fairness-based criticisms of utilitarianism and all-inclusive utility. Section 7.3 introduces the two-stage model to operationalize fairness and obtain meaningful all-inclusive utilities. Section 7.4 applies the approach to analyze Diamond's and Broome's examples with individual fairness preferences. Section 7.5 discusses the results and the last section concludes.

7.2. Utilitarianism, Fairness, and All-Inclusive Utility

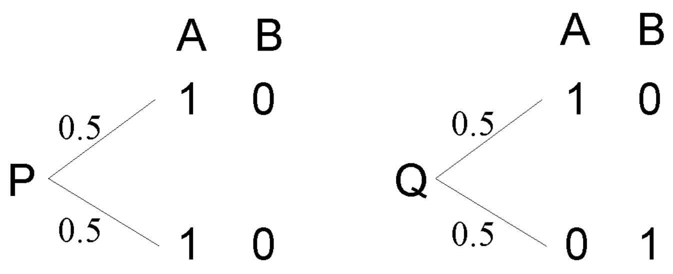
Harsanyi (1955) used cardinal utility obtained from choices between risky allocations to derive a social welfare function. He assumed that the social planner and the individual agents use expected utility to evaluate risky prospects over outcomes, and that the Pareto-principle holds. The latter requires that indifference between two prospects for each individual implies indifference from the social standpoint. Harsanyi showed that these three assumptions imply a social welfare function W of the utilitarian form, $W = \sum_i U_i$, where the U_i's are the individual agents' vNM utilities over outcomes.

7.2.1. Fairness-Based Criticisms of Utilitarianism

Harsanyi's result has been criticized because of the absence of fairness considerations in the evaluation of the utilitarian social planner. Consider the following two examples in which the utilitarian prediction of which random allocation should be preferred by society has been argued not to be convincing.

EXAMPLE 1. (Diamond 1967) Consider the two random allocations P and Q of utilities for agents A and B in Figure 7.1.

Figure 7.1. Random Allocation of Utilities in Diamond's Example

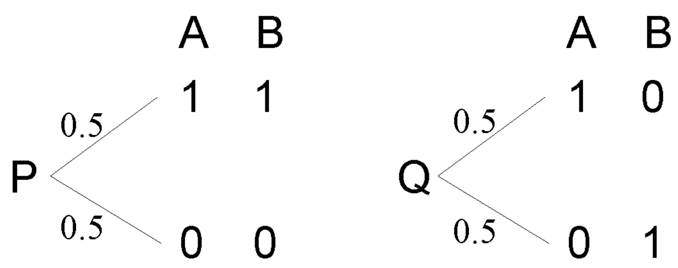


Under utilitarianism the planner is indifferent between procedures P and Q because both imply an expected social welfare of 1. Indifference can be unconvincing because with procedure P agent B will receive nothing while A receives positive utility for sure. With procedure Q both agents have a fair chance of the same utility.

□

EXAMPLE 2. (Broome 1991, p. 185) Consider the allocations in Figure 7.2.

Figure 7.2. Random Allocation of Utilities in Broome's Example



Under utilitarianism the planner is indifferent between procedures P and Q because both imply an expected welfare of 1. Indifference can be unconvincing because allocation procedure P always leads to equality in terms of utilities, while Q always leads to inequality.

□

In Diamond's example utilitarianism violates *process fairness*, and in Broome's example it violates *outcome fairness*.

7.2.2. Non-Utilitarian Social Welfare vs. All-Inclusive Utility

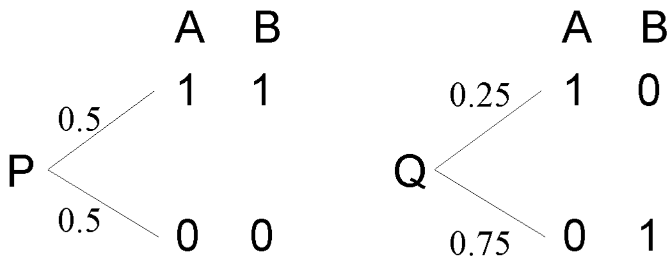
To incorporate fairness in Harsanyi's framework, two approaches can be followed. First, the assumptions of expected utility and Pareto-efficiency can be weakened and a non-utilitarian social welfare function can be derived that accounts for fairness preferences of the social planner (Epstein and Segal 1992, Kelsey 1994, Wakker and Zank 1999, Grant et al. 2006, Zank 2007). However, it is not obvious that a social welfare function should be based on the assumption of non-expected utility evaluation of risky prospects by the social planner. Further, it is not clear whose fairness preferences are represented by this social evaluation if no agent in the society individually cares about fairness (van Winden 2007).

Second, it can be assumed that the description of the social allocations does not only include the individual agents' personal outcomes, but also all possible interpersonal comparisons (Harsanyi 1955, Luce and Raiffa 1957, Broome 1991, Binmore 1994, Karni 1996). The agents' vNM utilities of a social allocation, which are used in the social evaluation of the allocation, are all-inclusive.

The second approach seems normatively more convincing because it retains expected utility and Pareto-efficiency, and derives fairness from individual preferences (Karni 1996, Karni and Safra 2002). In particular, the individual fairness preferences may differ across agents. All-inclusive utility, however, also suffers from problems if taken in full generality. When observing an empirical violation of utilitarianism, it can always be argued that a refined description of outcomes that explicitly incorporates fairness can accommodate the empirically observed social preference. Thus, utilitarianism is deprived from predictive power and becomes unfalsifiable, as illustrated by the following example.

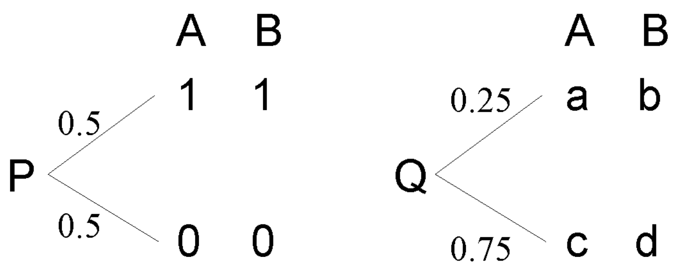
EXAMPLE 3. Consider again Example 2 and interpret the entries as all-inclusive vNM utilities. The social planner is indifferent between both procedures under utilitarianism. Now consider a new decision for the planner that is identical to the one in Example 2, except that probabilities under procedure Q are 0.25 for the upper branch and 0.75 for the lower branch. Assume that the all-inclusive utilities of the two agents in Figure 7.2 are due to a purely selfish utility evaluation of outcomes, that is, agents do not care about fairness. Then the social choice with new procedure Q is given by the entries in Figure 7.3. The planner is indifferent because the expected social welfare is equal to 1 for both procedures.

Figure 7.3. Broome's Example with Different Procedure Q: Selfish All-Inclusive Utilities



Alternatively, the all-inclusive utilities in Example 2 when taken completely general may include individual preferences for fairness. Assume for instance that the all-inclusive utilities depend on monetary payoffs and social comparisons of these payoffs. Assume that agents prefer more money to less and that they also prefer smaller interpersonal differences in expected payoffs because they value process fairness. The U_i's in Figure 7.2 then are functions of agent i's own payoff and of the difference between her own expected payoff and the other agent's expected payoff. In this case, changing the probability distribution under procedure Q affects the agents' all-inclusive utilities, because the difference between the agents' expected payoffs changes. The social choice is defined by the entries in Figure 7.4 for unknown all-inclusive utilities a, b, c, and d, and indifference of the planner need not hold because 0.25 (a+b) + 0.75 (c+d) need not be equal to 1.

Figure 7.4. Broome's Example with Different Procedure Q: All-Inclusive Utilities when Agents Care About Differences in Expected Payoffs



□

Assuming all-inclusive utilities, no prediction can be made about the society's decision in Example 3 based on the knowledge that the society is indifferent in Example 2.

Indifference in Example 2 can be due to either selfishness or preference for fair expected outcomes, with very different implications for the social decision in Example 3. Without explicit and falsifiable assumptions concerning individual fairness preferences, the model with all-inclusive utility has no predictive power (Machina 1989, Broome 1991). It cannot be applied to evaluate social allocations.

7.3. Operationalizing All-Inclusive Utility

7.3.1. The Two-Stage Model

A two-stage model that is based on Sugden's (2000) methodology to incorporate social interaction in game-theoretic analyses is proposed to operationalize fairness in all-inclusive utility. Let there be N agents with all-inclusive vNM utilities U_i and a social planner with social welfare function W. u_i denotes agent i's self-interested vNM utility and z_i denotes her actual outcome. Welfare is evaluated by the following model:

$$W = \sum_{i}^{N} U_i \qquad \text{(utilitarianism)} \qquad (7.1)$$

$$U_i = f_i(u_1, ..., u_N) \qquad \text{(second stage)} \qquad (7.2)$$

$$u_i = g_i(z_i) \qquad \text{(first stage)} \qquad (7.3)$$

At the first stage the agents' individual outcomes (health states, wealth levels) are evaluated through the self-interested vNM utilities that evaluate risky options without social comparison. Agents consider only their personal situation, and no fairness considerations enter at this stage. At the second stage, these self-interested vNM utilities are then taken as inputs in models of individual fairness preferences $f_i(\cdot)$ to obtain all-inclusive vNM utilities for utilitarian welfare evaluation. An agent's all-inclusive utility may depend on both her own and all other agents' self-interested utilities.

The first-stage self-interested utilities are functions of possibly non-numerical outcomes. The second-stage all-inclusive utilities are functions of numerical self-interested utilities. This allows for fairness comparisons also if outcomes are not numerical as with health states. Differences or expected values of self-interested utilities of health states can be calculated at the second stage to obtain the all-inclusive utilities.

A tractable parametric model of individual fairness that can be used at the second stage has been proposed by Fehr and Schmidt (1999). They consider individual preference for outcome fairness, that is, agents are averse to unequal outcomes. Trautmann (2007b) extends the model to individual preference for process fairness by assuming aversion to unequal expected outcomes. Notice that outcome fairness and process fairness refer to the allocation of self-interested utility in the application of these models in the two-stage model. We will see that specifying individual fairness preferences explicitly through parametric models at the second stage makes all-inclusive utility operational and quantitatively formalizes the discussion of fairness under utilitarianism.

7.3.2. Models of Individual Fairness

Let the N agents face uncertainty about the allocation of self-interested utilities. Let X_i denote the random variable from which agent i's self-interested utilities will be drawn and let $E[X_i]$ denote its expectation. Assuming that the agents dislike an unequal ex-post allocation of self-interested utilities we apply the following all-inclusive utility function, the *Outcome Fehr-Schmidt Model* (Fehr and Schmidt 1999), to account for fairness preferences:

$$U_i(u_1,...,u_N) = u_i - \frac{\alpha_i}{N-1}\sum_{j\neq i}^{N}\max\{u_j - u_i, 0\} - \frac{\beta_i}{N-1}\sum_{j\neq i}^{N}\max\{u_i - u_j, 0\} \qquad (7.4)$$

with $0 \leq \beta_i < 1$ and $\beta_i \leq \alpha_i$. This utility function takes the agent's fairness preferences into account by reducing the all-inclusive utility in the case of unequal actual self-interested utilities, i.e. $u_i \neq u_j$. The utility reduction is larger when the inequality is disadvantageous ($u_i < u_j$) than when it is advantageous ($u_i > u_j$), because $\beta_i \leq \alpha_i$. From $\beta_i < 1$ it follows that the agent's all-inclusive utility is always increasing in her own self-interested utility, and non-negativity of the parameters implies that there is no inequality seeking. The normalization of the fairness terms by N–1 ensures that the impact of fairness remains constant if the number of agents increases.

The outcome Fehr-Schmidt model provides an operational and tractable form to model preferences for outcome fairness and has been very successful in predicting empirical data. Evidence from experiments using random allocations has shown, however, that agents also care about whether procedures are fair from the ex-ante perspective (Cox and Deck 2005, Bolton et al. 2005). Using the basic form of the Fehr-Schmidt model, but assuming that agents care about fair processes, Trautmann (2007b) proposed the following all-inclusive utility function, the *Process Fehr-Schmidt Model*, which accounts for preferences for fairness by considering differences in the expected self-interested utilities between the agents:

$$U_i(u_i, X_1,..., X_N) = u_i - \frac{\alpha_i}{N-1}\sum_{j\neq i}^{N} \max\{E[X_j] - E[X_i],0\}$$
$$- \frac{\beta_i}{N-1}\sum_{j\neq i}^{N} \max\{E[X_i] - E[X_j],0\} \quad (7.5)$$

with $0 \le \beta_i < 1$ and $\beta_i \le \alpha_i$. This utility function takes the agent's fairness preferences into account by reducing the all-inclusive utility in the case of unequal *expected* self-interested utilities, i.e. $E[X_i] \neq E[X_j]$. As in the outcome Fehr-Schmidt model, the utility reduction is larger when the inequality is disadvantageous than when it is advantageous. Now $\beta_i < 1$ implies that the agent's expected all-inclusive utility is increasing in expected own self-interested utility. Non-negativity of the parameters implies that there is no ex-ante inequality seeking. The process model is able to accommodate experimental data that the outcome model cannot explain.

Both fairness concepts have been found to matter empirically and the Fehr-Schmidt type models successfully predict experimental data. Individual agents' utility functions can be assessed by observing their choices between actual allocations $(u_1,...,u_N)$ for the outcome model and risky allocations $(u_i, X_1,..., X_N)$ for the process model (Camerer and Fehr 2004, Rohde 2007). The self-interested vNM utilities can be assessed through standard utility measurement procedures (Keeney and Raiffa 1976, Wakker and Deneffe 1996, Abdellaoui 2000). Applying these tractable models therefore allows for a quantitative welfare evaluation under utilitarianism that is based on empirically relevant individual attitudes towards fairness.

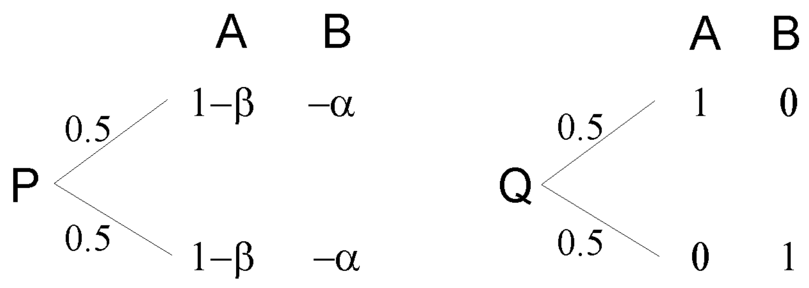
Process and outcome fairness are closely related to a priori and a posteriori fairness. An agent with process fairness preferences takes the a priori view on fairness both before and after the resolution of uncertainty. An agent with outcome fairness preferences takes the a posteriori view at both points (Machina 1989, Trautmann 2007b).

7.4. Incorporating Individual Fairness in Diamond's and Broome's Examples

We discuss Diamond's (1967) and Broome's (1991) examples of social choices between random allocations when agents have Fehr-Schmidt preferences for either outcome or process fairness and N=2. We apply the two-stage procedure and assume that the utilities in Figure 7.1 and Figure 7.2 are self-interested vNM utilities that have been derived from self-interested choices between risky prospects over outcomes. These self-interested utilities enter the second-stage fairness models as inputs to obtain all-inclusive utilities. For agent A and B's inequality aversion parameters in the outcome and process Fehr-Schmidt model we assume for simplicity that $\alpha_A = \alpha_B > 0$ and $\beta_A = \beta_B > 0$.

EXAMPLE 1'. It has been shown above that without consideration of fairness the social planner's indifference between P and Q in Diamond's example can be unconvincing. Including fairness considerations at the individual level by applying the process Fehr-Schmidt model (7.5) to the allocation of self-interested utility specified in Figure 7.1, we obtain the all-inclusive utilities given in Figure 7.5.

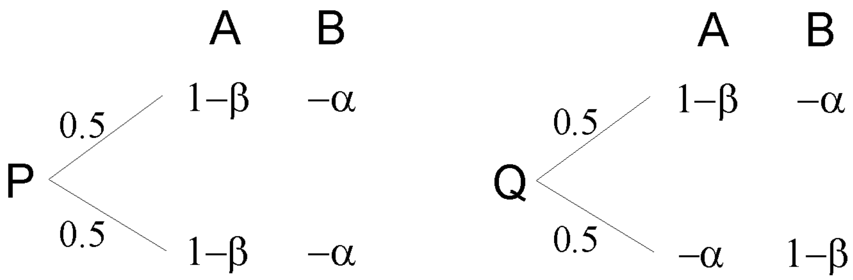
Figure 7.5. Process Fehr-Schmidt Utilities in Diamond's Example



Allocation procedure Q with expected welfare 1 is preferred over procedure P with expected welfare $1-\alpha-\beta$ by the social planner if subjects hold the process fairness view. The process model can immunize utilitarianism against the criticism raised by Diamond's example.

Applying the outcome Fehr-Schmidt model (7.4) to the example, we obtain the utilities given in Figure 7.6. The utilitarian planner is indifferent between P and Q if subjects care about outcome fairness: under both allocation procedures they will always obtain the same degree of outcome inequality. The example's criticism cannot be accommodated because it is always B who is worse off under process P, making the society's indifference unconvincing.

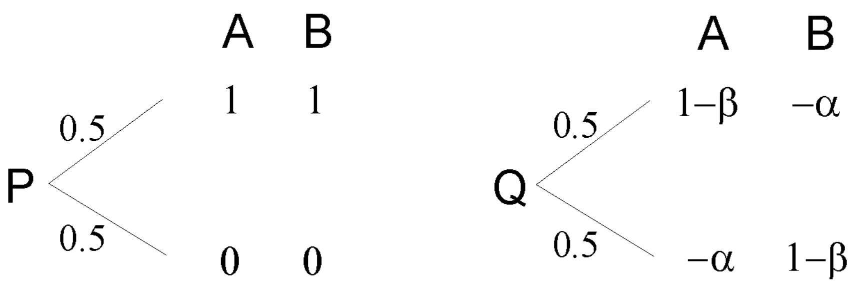
Figure 7.6. Outcome Fehr-Schmidt Utilities in Diamond's Example



□

EXAMPLE 2'. In Broome's example indifference between procedures P and Q may not be convincing because Q always involves unequal self-interested vNM utilities while P gives both agents equal self-interested VNM utilities in each state. Applying the outcome Fehr-Schmidt model we obtain the all-inclusive utilities in Figure 7.7.

Figure 7.7. Outcome Fehr-Schmidt Utilities in Broome's Example



Procedure P with expected welfare 1 is preferred over procedure Q that gives expected welfare $1-\alpha-\beta$. Including preferences for outcome fairness in the utilitarian welfare assessment accommodates the criticism raised by the example. If we apply the process Fehr-Schmidt model (7.5) to the allocations in Figure 7.2 to obtain all-inclusive utilities, however, we obtain the same numbers as in Figure 7.2: both subjects have equal expected self-interested utility under both procedures and inequality aversion terms drop out of the all-inclusive utility function. Both subjects, and therefore the social planner, are indifferent between allocation procedure P and Q. Utilitarianism with process fairness preferences does not account for the criticism raised by Broome's example.

□

The following table summarizes the appraisal of Harsanyi's utilitarianism in the two examples for the different fairness concepts.

Table 7.1: Appraisal of Utilitarianism under Different Individual Fairness Preferences

	Broome's example	Diamond's example
Self-interested	–	–
Outcome Fehr-Schmidt	+	–
Process Fehr-Schmidt	–	+

+ criticism accommodated; – criticism not accommodated

Applying self-interested individual utility in welfare assessments, the utilitarian prediction of the planner's preferences is not convincing for either example. Assuming that individuals' utilities include outcome fairness norms, utilitarianism can accommodate the criticism raised by Broome's example. Assuming process fairness, Diamond's criticism can be accommodated.

7.5. Discussion

By observing individual preference for process fairness or outcome fairness, the social planner can use the two-stage approach with the respective individual fairness model to determine the optimal allocation under utilitarianism. The social welfare evaluation considers fairness through all-inclusive utilities.

At the second stage we used Fehr-Schmidt inequality aversion models to include individual fairness preferences. However, other fairness models can be incorporated in the two-stage approach if they appear more appropriate descriptions of individual preferences. Examples include the outcome and process fairness models of Bolton and Ockenfels (2000) and Bolton et al. (2005) that make different assumptions than Fehr-Schmidt in the N-person case, or the model of Charness and Rabin (2002) in which agents focus on efficiency and the worst-off person. Krawczyk (2007) provides a model of both outcome and process fairness. Gächter and Riedl (2006) and Traub et al. (2006) discuss more complex fairness norms at the individual level for which second-stage models could be developed and implemented through the two-stage approach. Very different and possibly complex individual fairness notions can be accommodated by utilitarianism with all-inclusive utility.

Diecidue (2006) derives utilitarianism from a book-making argument under certainty. He assumes that the planner aggregates over many economic policies and that monetary equivalents exist for all consequences that the subjects face. Fairness can be included in this model by applying the second stage fairness model to individual monetary outcomes aggregated over all policies. These all-inclusive utilities can be used in the calculation of the aggregate welfare.

For some individual fairness second-stage models the utilitarian prediction may turn out to be identical to the prediction of some non-utilitarian social welfare model. If the two-stage model and the non-utilitarian model are empirically indistinguishable, the non-utilitarian model might be seen as a reduced form of the true two-stage utilitarian model (Machina 1984).

7.6. Conclusion

This paper argues that fairness can be incorporated in Harsanyi's (1955) utilitarian welfare function at the individual level through all-inclusive utilities. A two-step procedure is proposed that uses tractable parametric models of individual fairness to operationalize all-inclusive utility. The approach retains Harsanyi's normative assumptions and relates fairness in welfare evaluation to observable individual preferences. Falsifiability and predictive power of utilitarianism are preserved.

Fehr-Schmidt models of outcome and process fairness have been used to accommodate fairness-based criticisms of utilitarianism. The two-stage model is, however, a flexible tool to incorporate fairness in utilitarian social welfare. If tractable models of individual fairness are available which are empirically more successful or allow for more complex notions of fairness, then these can be used as well to incorporate fairness in utilitarianism.

Chapter 8

Reserve Prices as Reference Points—Evidence from Auctions for Football Players at *hattrick.org*

We study the effect of reserve prices on transfer prices for football (soccer) players in online auctions at *hattrick.org*. We distinguish between mechanical effects through surplus appropriation by the seller as predicted by auction theory, and psychological effects through reserve prices serving as reference points as predicted by behavioral models. Controlling for censoring if players are not sold at their reserve price and for endogeneity of reserve prices, we observe the mechanical effect but no effect through reference dependence.[20]

8.1. Introduction

Reference dependence is an important factor in decision making. Its role is empirically well established for individual choice situations and it has successfully been modeled through prospect theory (Kahneman 2003). Less is known about the effects of reference dependence in markets, however. A central problem in all applications of reference dependence concerns the choice of the reference point (Schmidt 2003, Bleichrodt 2007),

[20] This chapter is based on Trautmann and Traxler (2008).

and market situations usually offer a number of possible candidate points. List prices, other persons' valuations, or prices of alternative products may serve as reference points for the agent's valuation of a product.

Markets where reference points would be expected to exert a strong influence on decisions are the rapidly growing online auction platforms like eBay or uBid. On these auction websites consumers and businesses sell to consumers directly, substituting for traditional intermediaries. Buyers face a complex environment with competition, changing prices, minimum bids, buy-it-now options and time pressure. To submit optimal bids they need to have a clear idea of their own valuation even if current prices are still below the price range where the auction is going to end up. In this environment it is conceivable that the mother-to-be looking for a stroller on eBay and hoping for a bargain is influenced in her valuation by cues provided within the auction process.

Theoretical work by Rosenkranz and Schmitz (2007) suggested the seller's reserve price (minimum bid) as a possible reference point that influences buyers' willingness-to-pay in auctions. Buyers experience a disutility that is proportional to the difference between the price they pay and the reserve price. The larger the actual bid is compared to the reserve price, the larger the experienced disutility. For a fixed true value of the good, an incremental increase in the bid reduces the consumer's surplus by an additional increment in the disutility and therefore reduces optimal bids.

The empirical evidence for this reference point effect is inconclusive. Ariely and Simonson (2003) and Kamins et al. (2004) show positive effects of reserve prices on selling prices in field data and in field experiments. Bajari and Hortacsu (2003) find no evidence for reference dependence in their field study. Hoppe and Sadrieh (2007) conduct a field experiment and find no evidence either. We review the methods and findings of these papers and discuss related literature in Section 8.2.

When studying consumer behavior with real products on eBay and similar platforms there are two confounding factors that cannot easily be controlled. First, buyers cannot verify the quality of the product ex-ante, introducing uncertainty and asymmetric information between buyers and sellers. Apart from private values for the product based on some fixed quality, a common value component and a winner's curse problem become relevant. The auction winner will likely be too optimistic about the true quality. In such a situation reserve prices can serve as quality signals and therefore influence bids even in the absence of reference dependence. The second and related problem is the importance of trust and reputation in online auctions. Reserve prices and quality information of high reputation sellers will be more trustworthy and we therefore expect that the interaction of reputation and reserve prices affects bids.

We overcome all problems associated with quality uncertainty by studying reserve and selling prices for virtual football players traded in English auctions on *hattrick.org*. Hattrick is the world's largest online football manager game with almost one million participants. Every day about forty thousand players are traded on the Hattrick transfer market. Sellers can choose a non-negative reserve price (public minimum bid). When Hattrick players are on the market, all relevant information concerning their quality becomes publicly available. That is, there is no information asymmetry between buyers and sellers. Reserve prices cannot contain any quality signal and there is no scope for winner's curse or trust. In Section 8.3 we argue that the auction market in Hattrick provides clear and strong incentives for the participants and that successful trading is crucial for success in this open-ended manager game.

We use data on 364 players that were posted on the transfer market. For 313 of these players the sellers fixed a positive reserve price. 237 of all players were actually sold, showing that reserve prices were set competitively. Of the players for which a transfer took place, 41 sold exactly at the reserve price. Because some of the players with a positive reserve price were not sold, we have a censored sample. Because the reserve prices are set by the sellers there can be an endogeneity problem, that is, some unobserved variable affecting both reserve and selling prices and biasing the results for the reserve prices.

Running an OLS regression on the censored sample, we find the obvious result that a larger reserve price significantly increases the transfer price. The OLS excludes those players that are not sold, which means that buyers held valuations that were lower than the reserve price. This information is not considered in the OLS and the effect of the reserve price suffers from an upward bias. If we control for censoring of unsold players by estimating a censored normal regression, the effect is reduced but still significant.

We show, however, that this effect is mainly caused by the players that sell at exactly the reserve price. For these players the seller appropriates some of the highest bidder's surplus in the second price auction by setting a competitive minimum bid (Riley-Samuelson 1981). In the English auction the highest bidder's surplus is the difference between her valuation and her bid, and the bid is one bidding increment larger than the second highest bidder's valuation. If the reserve price falls between the highest bidder's and the second highest bidder's valuation, the surplus of the highest bidder is only the difference between her valuation and the reserve price (instead of the lower second highest bidder's valuation). The selling price is mechanically higher than without a reserve price.

This effect is predicted by auction theory and does not relate to reference dependence in valuations. If we control for censoring and surplus appropriation there is still some evidence for an effect of reserve prices on transfer prices through reference dependence. Controlling for endogeneity of the reserve price, however, only the mechanical effect of surplus appropriation prevails. We find no evidence for reference dependence effects of reserve prices in our Hattrick auctions.

We discuss the data and the empirical strategy to identify reserve price effects in Section 8.4 and present the results in Section 8.5. The last section discusses the results and offers conclusions for further research.

8.2. Reserve Prices as Reference Points: Review of the Literature

The effect of reserve prices in auctions has been studied in field experiments where the researcher sells items at different reserve prices on eBay, and in field studies using data from real auctions. One of the first studies was conducted by Ariely and Simonson (2003) who use both formats. In their field study Ariely and Simonson regress the final price for college football tickets on reserve prices. They find that each dollar increase in the reserve price increases the sales price by 80 cents, and they suggest that this indicates reference dependence in bids. There are no unsold tickets, hence there cannot be a censoring bias. The explanatory power of their regression was extremely high with over 90% of the variation in sales price explained, however. Together with the strong effect of the reserve price this suggests that many tickets were actually sold exactly at the reserve price. That is, the effect that Ariely and Simonson find might partly be due to sellers' competitive minimum bids that appropriate some of the highest bidders' surplus.

In their field experiment Ariely and Simonson sell various items at either low or high reserve prices on eBay. In some auctions the same item was offered at both the high and the low reserve price simultaneously. They find a positive effect of the high reserve price on average bids, but only if there was no low reserve price item offered simultaneously. Although there were no unsold items, indicating modest reserve prices, some items might have been sold exactly at the reserve price. It is therefore not entirely clear that the effect is caused by reference dependence rather than surplus appropriation.

Kamins et al. (2004) sell sets of unsearched coins[21] at either low or high reserve prices on eBay and find a positive effect on selling prices. There were bids exactly at the reserve price, however, so that surplus extraction can explain part of the effect. Additionally, the items sold here carry some uncertainty, and the reserve price might convey a signal about whether the sets are indeed unsearched. If the seller prefers keeping the sets instead of selling at a low price she signals that she expects some valuable coins to be still in the set.

Negative evidence on the effect of reserve prices comes from Hoppe and Sadrieh (2007). They study the effect of reserve prices on selling prices for DVDs and coins on eBay. They find no effect of reserve prices on selling prices. Their result is somewhat surprising because there were many observations for items that sold at exactly the reserve price where an effect is predicted by auction theory. There was also one item that remained unsold and which they exclude from their analysis. This introduces a small upward bias through censoring, making the result of no effect of reserve prices even stronger.

Bajari and Hartacsu (2003) study the uncertainty about product value and winner's curse in a sample of auctions for coins on eBay. They also test for an effect of reserve price on selling price and find a positive effect in an OLS regression. Controlling for censoring, however, they find no effect. As in Hoppe and Sadrieh, the complete absence of

[21] "Unsearched" means that the seller has not searched and removed valuable coins from the set. The true value of the set should therefore be unknown to both sellers and buyers.

reserve price effects is somewhat unexpected from the theoretic point of view, even in the absence of reference dependence.

The results of these four studies are inconclusive. Reference dependence might have subtle effects that can not be identified in all samples. The effect of surplus appropriation depends on the presence of competitive reserve prices and significant valuation differences between bidders. If the valuation difference between the highest and second highest bidder is small there is only little to gain from a competitive reserve price and the effect on selling prices will be negligible. We will address both effects separately in our data analysis.

8.3. Auctions for Football Players at Hattrick

Hattrick is an open-ended football manager game played on the internet by roughly a million participants[22]. Each participant owns a team that plays in one of many divisions in his country of residence. The goal is to promote to higher divisions and win the national championship and cup. Successful managers can become manager of the national team and play in the world cup against other national teams. A Hattrick season (and year) lasts 16 weeks in the real world.

Managers have to make decisions about training, match orders and line up, tactics and team psychology, the development of the club and the stadium, recruiting of youth players, and transfers. The transfer market is a crucial aspect of the Hattrick universe, mainly because of the specific training institution. Players have different skills which define their strength on a certain position in the team. Improving players' skills by one increment takes many weeks and each week only one skill can be trained. That is, most managers will specialize on training of one skill, say scoring, and thereby produce a constant stream of good forwards. These forwards can be sold on the transfer market and the proceeds can then be used to buy players for other positions, for instance a playmaker, or to improve the

[22] In this section only the features of Hattrick that are most relevant to our auction data are discussed. The complete game rules and more information about the Hattrick universe can be found at www.hattrick.org.

club’s stadium or hire more assistant coaches. Proceeds from player sales form a main source of income, together with income from sponsors and attendance at home games.

A player’s quality is determined by his skills. The owner of the player knows all his skills and there are various computer programs that can be purchased to calculate the player’s strength on different positions based on his skills. If a player is posted on the transfer market his skills become public information. There is no asymmetric information between buyers and sellers about players on the market. The value of the player for a manager depends on his fit with the whole team, the training and game strategy and the team’s position in the league.

The transfer market is organized as a standard English auction with increasing bids. Players are posted on the market and stay there for three days. A non-negative public reserve price can be declared for the player. Potential buyers can submit bids that are at least as high as the reserve price and higher than the current highest bid. The reserve price and the current highest bid with information about the bidder are shown next to the player information (see screenshot in the appendix). If bids are submitted in the last 3 minutes before the deadline, the deadline is extended by 3 minutes. The highest bidder wins the auction and pays his bid. All transfers of money and players are automatized. If no bidder submits a bid at the given reserve price the player is removed from the transfer list after three days and his skill information becomes private information again, available only to the owner.

Participation in transfer auctions is a regular task for Hattrick team managers and they get a lot of experience in the market. The sellers in our sample have participated on average in 149 trades and the buyers in 131 trades.

8.4. Empirical Strategy

We use a sample of 364 players that were posted on the Hattrick transfer market to study the effect of reserve prices on selling prices. We restricted the sample to playmakers with strength “excellent” (score 8 on a scale from 0 to 20) but allow variation in all other dimensions. These players are good enough to always provide positive market value, but are also interesting and affordable for a large group of buyers. We have all background variables for the players, variables for the transaction (e.g. the day of the transaction), variables for the interaction between seller and player (e.g. tenure in the team), the reserve prices, and the selling prices if the player was traded (see appendix for a list of all variables and explanations). 137 players were not sold in the market and 41 were sold exactly at the reserve price (Table 8.1).

Table 8.1: Structure of the sample

All players	364		
Positive reserve price		313	
Sold for more than reserve price			135
Selling price = reserve price			41
Not sold			137
Zero reserve price (all sold at positive price)		51	

Because of the unsold players we have to correct for censoring bias. If the player was not sold we know that the highest valuation for this player in the market was below the reserve price. Note that each player had an individual reserve price and therefore a different censoring point. We use a censored normal regression, an extension of the Tobit that allows for variable censoring points, to control for censoring (Wooldridge 2002). Our model assumes that for each observation there is a true market price that depends on player attributes, possibly on the reserve price through reference dependence, and on a normally distributed noise component as shown in Eq. 1.

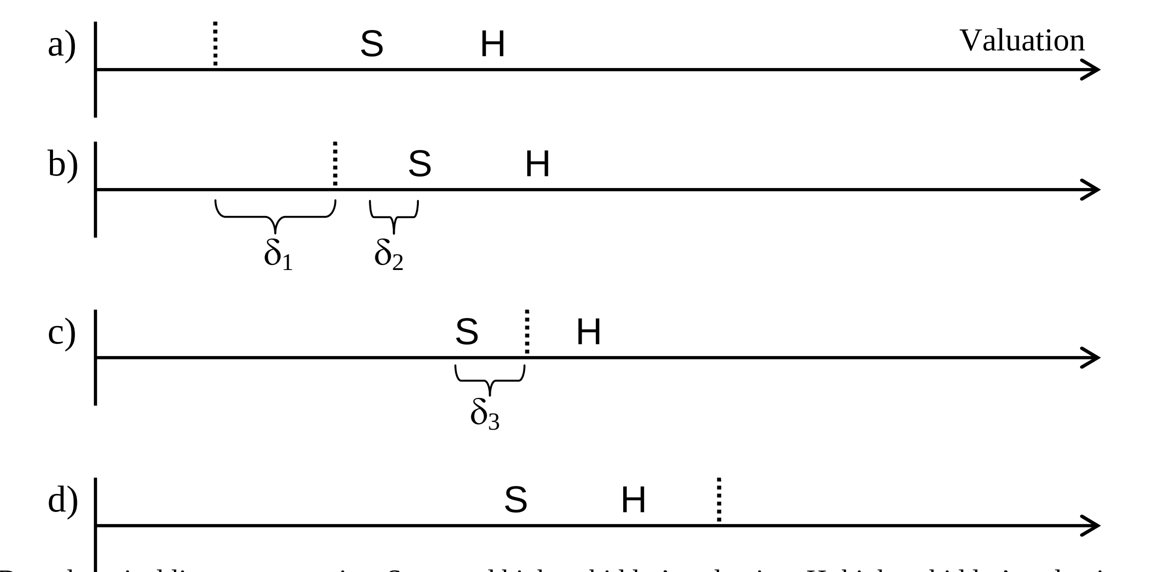
$$\textit{transfer price} = \alpha \times \textit{reserve price} + \beta \times \textit{player attributes} + \varepsilon, \quad \varepsilon \sim N(0,\sigma^2) \qquad (8.1)$$

The true transfer price is only observed if it is larger than the reserve price, however. That is,

$$\textit{transfer price}_{\mathrm{obs}} = \max(\textit{reserve price, transfer price}). \qquad (8.2)$$

It does not matter how we code the missing observation for the observed transfer price in the cases where the player is not sold. The relevant information is that the true transfer price was lower than the reservation price. Figure 8.1 illustrates the effect of reference dependence graphically.

Figure 8.1: Reserve Price as Reference Point



Dotted vertical line: reserve price; S: second highest bidder's valuation; H: highest bidder's valuation

On the horizontal lines we measure the valuation for the player. H denotes the valuation of the highest bidder and S denotes the valuation of the second highest bidder. In the English auction the price that the highest bidder pays will be slightly above the second bidder's valuation S. The dotted vertical line represents the reserve price. Consider Figure 8.1a. The reserve price for this player is relatively low and there is competition, the selling price is S. Figure 8.1b shows the same player, but at a reserve price that is δ_1 larger than the reserve price in Figure 8.1a. If the reserve price serves as a reference point and has a positive effect on valuations, we expect S and H to increase if the reserve price increases. For simplicity we assume in Figure 8.1 that S and H increase by the same amount. In figures 8.1a and 8.1b an increase in the reserve price by δ_1 has an effect of δ_2 on the selling price.

Now consider the situation in Figure 8.1c. The reserve price for the same player is even higher, and it falls between the highest and the second highest bidder's valuation. Again, we observe the reference dependence effect with a shift in S and H. The selling price will be even higher, however, because the highest bidder now pays the reference price instead of S. There is an additional mechanical effect δ_3 of the reserve price that simple follows from the auction design and not from reference dependence.

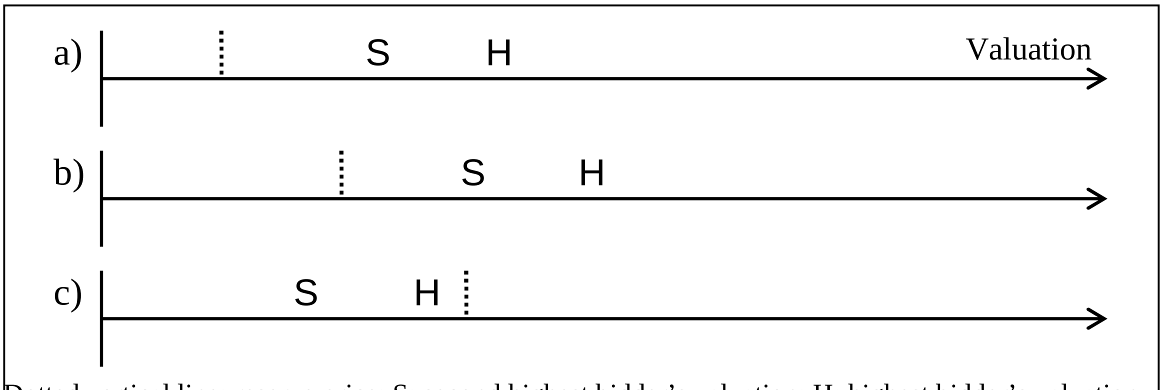
To control for this effect in our estimation we include an additional slope parameter for those players for which there is exactly one bidder. Let the indicator I(*one bid*) equal one if there was exactly one bidder and zero otherwise. We then estimate the population equation

$$\textit{transfer price} = \alpha \times \textit{reserve price} + \gamma \times \mathrm{I(one\ bid)} \times \textit{reserve price} + \beta \times \textit{player attributes} + \varepsilon, \quad \varepsilon \sim N(0,\sigma^2) \qquad (8.1^*)$$

The parameter α estimates the effect of reference dependence and the parameter γ estimates the mechanical effect of surplus appropriation. The total effect of the reserve price for those who sold exactly at the reserve price is ($\alpha+\gamma$).

Now consider Figure 8.1d. The reserve price is higher than the highest bidder's valuation, the player is not sold. The observation does not preclude an effect of reference dependence however, as the example shows. Valuations are higher in Figure 8.1d compared to Figure 8.1c, but not high enough to lead to a sale. There are situations, however, where censoring provides negative information for reference dependence. Such a situation is shown in Figure 8.2.

Figure 8.2: Informational Content of Censoring



Dotted vertical line: reserve price; S: second highest bidder's valuation, H: highest bidder's valuation

Assume that figures 8.2a, 8.2b, and 8.2c show valuations of three players that are identical in all attributes but differ with respect to their reserve price in the auction and the noise component. Excluding the unsold player 8.2c leaves us with strong evidence for reference dependence. The higher reserve price for player 8.2b seems to have a positive effect on the selling price. If we take the censored observation 8.2c into account, however, the evidence becomes much less convincing. The reserve price is higher for player 8.2c than for 8.2b, but the true unobserved market value must be smaller than the selling price of the player in 8.2b. This observation is evidence against reference dependence. The exclusion of censored observations from the sample will lead to an upward bias of the effect of the reserve price on market valuations.

The censoring problem and the mechanical effect of surplus appropriation potentially affect results in all studies where reserve prices are set competitively. Another problem occurs in field studies only, where reserve prices are not exogenously set. If the reserve prices that are set by the sellers reflect some quality of the player that is observed by buyers and sellers but not by the econometrician, the reserve prices suffer from endogeneity and will bias the estimates. For instance, the players in Hattrick all have names. We could observe the names but could not in any way code them to include them in the analysis. If a player has a name that is similar to that of a popular real world football player, the seller might ask a higher price and buyers might be willing to pay more. Similar unobserved effects are likely in auctions for football tickets or collectors' coins that have been studied in the literature

To correct for endogeneity of the reserve price in our censored regression model we use the Smith and Blundell (1986) two-step procedure. The first step consists of a linear regression of the reserve price on all player attributes and instruments. As instruments we use three variables that measure the interaction of the player with the seller and that are likely to affect the reserve prices. These variables are (1) whether the player comes from the seller's own youth team[23], (2) how many years the player has been in the team, and (3) by how much the player improved during his tenure on the team. In the second step the residuals of the fist step OLS are included in the censored regression model. This procedure gives consistent estimates of all parameters of interest even in the presence of endogeneity of the reserve price. The t-statistic on the residuals in the second step regression provides a test of the endogeneity.

[23] This includes original players that were already on the team when the manager received the team.

8.5. Results

All regressions in this section include our full set of player and transaction attributes as controls. We report the effects of the reserve price and four key player attributes to illustrate effect of misspecification in our study. The player attributes that we report, and their expected effect on selling prices based on the economics of the Hattrick game rules are shown in Table 8.2.

Table 8.2: Expected Effects of Player Attributes on Selling Prices

Attribute	Sign of the effect on transfer price
Total skill index	+
Age	–
Form	+
Wage	–

Ordinary least squares. Model I in Table 8.3 shows the linear regression of the transfer price on the reserve price and player characteristics for those players for which we observe a transfer. This regression does not control for censoring, endogeneity of the reserve price, or the mechanical effect of surplus appropriation by the seller through a competitive reserve price. We find that a €1 increase in the reserve price increases the transfer price by 68 cents. The effect is highly significant and the regression explains 85% of the variation in transfer price. Counterintuitively, we observe that higher skills have a negative but insignificant effect on transfer price and higher wage cost increase the transfer price.

Censored normal regression. Model II in Table 8.3 shows regression results for a censored normal regression of the transfer price on the reserve price, controlling for the effect of censoring for the players that were not sold at their reserve price. Here we do not consider endogeneity or surplus appropriation. The parameter estimates can be interpreted as population parameters, i.e. as effects for both censored and uncensored observations. We observe that there is a positive effect on the transfer price of 49 cents per €1 increase in the reserve price. The effects of skills, age, form and wage point into the expected direction.

Table 8.3: Regression Analyses – Determinants of the Transfer Price

	Model I	Model II	Model III	Model IV
	OLS	Censored normal regression	Censored normal regression	Censored normal regression, consistent under endogeneity
Reserve price	0.6825** (0.1116)	0.4858** (0.0401)	0.256** (0.0554)	–0.0905 (0.1029)
Reserve price × one bidder only			0.3868** (0.0576)	0.3565** (0.05516)
Residuals from first stage regression				0.3936** (0.0974)
Total skill index	–7.4069 (4.5442)	6.8092** (2.1061)	1.9172 (2.1472)	11.3465** (3.1463)
Age	–9448.874** (3494.497)	–12084.11** (2661.161)	–12919.73** (2589.894)	–15144.63** (2555.188)
Form	4788.523 (3650.861)	9921.662** (3659.352)	10292.64** (3572.873)	11489.55** (3448.153)
Wage	45.7121** (13.3955)	–12.4506** (3.8387)	–1.7254 (3.9906)	–17.2281** (5.7177)
# of observations	227	364	364	364

Standard errors in parenthesis (robust standard errors for OLS); *significant at the 5% level, **significant at the 1% level. All player and transaction characteristics included as controls.

In Model III we include the interaction between the reserve price and the observation of a single bidder for the player as shown in Eq. 8.1*. Here the interaction term measures the mechanical effect of optimal reserve price setting in the second price auction, while the direct reserve price effect can be interpreted as a reference point effect. We find that for players who sell above the reserve price the effect on the transfer price is 26 cents per €1 increase in the reserve price. For players who sell at exactly the reserve price both effects matter. The mechanical effect leads to an additional 39 cents increase in the transfer price for these players. We cannot reject the hypothesis that the two effects are equally sized (p=0.21). The player attributes point in the expected direction but wage and total skill are not significant.

Endogeneity of the reserve price. In Model III we control for censoring and for surplus appropriation, but there might still be an endogeneity problem for the reserve price. To obtain consistent estimates of the reserve price effect under endogeneity we apply the Smith-Blundell two-step procedure. In the first step we estimate a linear regression of the reserve price on all player characteristics and the three player-seller interaction attributes: the tenure of the player in the team, his improvement during tenure, and whether he comes from the own youth of the seller. An F-test reveals a significant effect of these variables on the reserve price ($F(3,328)=4.69$, $p=0.0032$). In the second step we calculate the residuals of the first step reduced-form OLS and include them in the censored regression Model III.

Table 8.3 shows a significant effect of the first-stage residuals in Model IV, suggesting endogeneity of the reserve price in Model III. The consistent estimate of the reserve price in Model IV is insignificant. The effect of surplus appropriation is virtually the same as in Model III. The consistent estimates of the four key player characteristics in Model IV have the expected sign and are highly significant.

The censored normal regression model assumes normality of the dependent variable and a logarithmic transformation can be used to control for possible non-normality (Wooldridge 2002). Estimating the logarithmic specification for model II, III and IV confirms the results of the linear specification in Table 8.3.

8.6. Discussion and Conclusion

A strong positive effect of reserve prices on transfer prices has been found in some empirical studies in the literature. Running a simple linear regression of transfer prices on reserve prices we replicate this result. We cannot conclude, however, that the mechanism driving the effect is reserve prices serving as references points in bidders' valuations. If we control for censoring of the observed transfer prices and for the fact that the reserve price can mechanically lead to an increase of the transfer price if it falls between the highest and the second highest bidders' valuations, we find that both the mechanical effect and the psychological effect add to the total effect of the reserve price. If we also control for possible endogeneity of the reserve price because of unobserved variables that affect both sellers' reserve prices and buyers' bids, only the mechanical effect prevails. The parameter estimates for the key player attributes with clear economic predictions for the sign of their effect show that such variables may serve as a warning signal for misspecification of the transfer price regression.

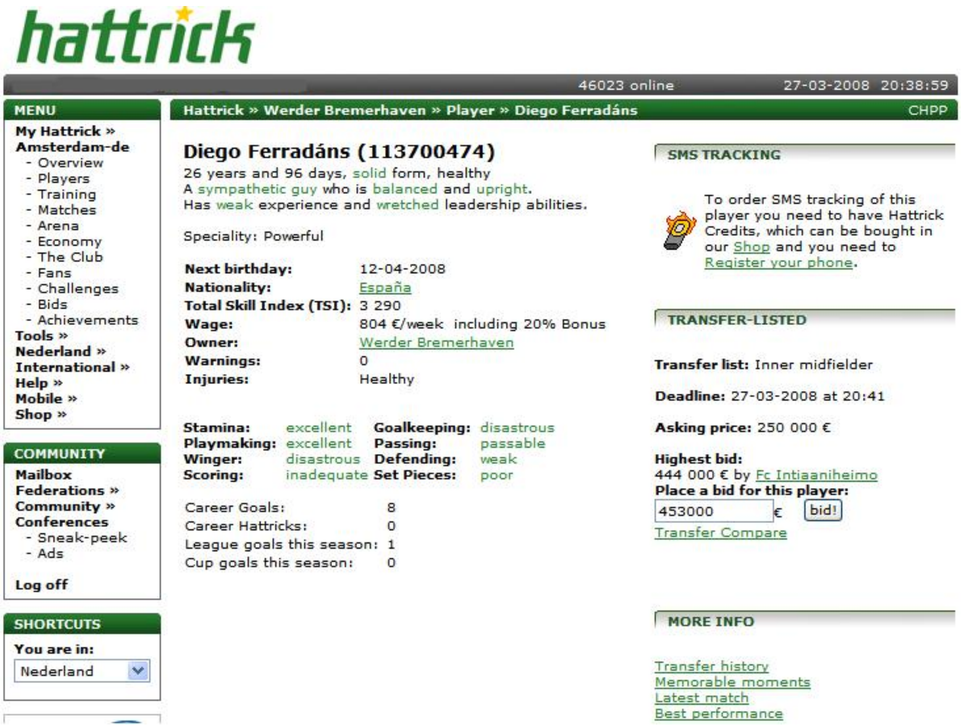
Our results suggest that some of the positive reserve price effects in the literature may better be interpreted in terms of the auction theoretic prediction that sellers will appropriate some of the highest bidders' surplus by setting a relatively high reserve price, and not as a psychological reference point effect. The theoretical work in Rosenkranz and Schmitz (2007) predicts variation in the reference point effect, however, depending on the availability of a clear independent economic valuation. This prediction is supported by the findings in Ariely and Simonson (2003) where the reference effect is reduced if multiple items are simultaneously sold at different reserve prices. In our Hattrick auctions the market participants are very experienced with more than hundred trades on average, as both sellers and buyers. They have clear economic incentives and can apply various tools to evaluate the value of players for their team. This setting might reduce the influence of the reserve price as a reference point compared to casual buyers of consumer goods on eBay.

Another problem with both field studies and field experiments is the effect of reserve prices on the number of bidders (Bajari and Hortacsu 2003). If low reserve prices attract many initial bidders and lead to strong competition and irrational "auction fever", this may increase prices compared to auctions with high initial reserve and few bidders. An existing reference point effect may not be observed after all.

The study of reserve price effects in field settings with high external validity is complicated by various identification problems. Laboratory experiments that allow controlling for sample size, endogeneity of reserve prices, and possibly for true valuations may be a promising route to complement to the existing field evidence.

Appendix

8.A1. Screenshot of a Player on the Transfer Market



8.A2. List of Variables

Player characteristics

Total skill index, age, form, wage, winger, scoring, goalkeeping, passing, defending, set pieces, injured, experience, leadership, popularity, popularity×leadership (clown), stamina, career goals, aggressiveness, honesty, dummies for special skills (e.g. head)

Transaction characteristics

Data were collected on six days between July 11, 2007 and July 18, 2007. We include five day dummies. The previous market price for the player and the seller experience (trades) are included to control for unobserved effects.

Player-seller interaction characteristics

Tenure of the player on the team, total skill improvement during tenure, descendant from own youth team or original player

Chapter 9

Conclusion

In this thesis it has been shown that psychological effects matter in individual and social decision making under uncertainty. Empirical studies identified new effects and theoretical models included these effects into economic analyses. Compared to individual decision settings, market interaction may reduce some biases, but it can also introduce new ones. Our results suggest that an extrapolation of results from individual decision to market does not necessarily lead to good predictions. Small differences in the market institution can affect the way in which psychological factors influence market outcomes.

In individual decisions there are often competing rational and psychological theories to explain behavior. In market settings additional effects can occur that do not depend on the individuals' behavior but on the mechanics of the market. It then becomes more difficult to identify the individual and the market components that influence the outcomes. The complementary use of field studies with high external validity and laboratory studies that allow more control seems warranted in these cases.

The results in this thesis support the view that psychological factors influence decisions. They also suggest a difficult trade off between psychological realism and simplification in theorizing. Policy recommendations based on a simplification of some psychological effect may not be a good approximation of the optimal policy. Psychological effects that are difficult to formalize may well be relevant to regulation and policy.

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Nederlandse Samenvatting

Summary in Dutch

De meeste beslissingen die we in ons dagelijks leven nemen, gaan gepaard met een onzekerheid over de mogelijke uitkomsten. Deze dissertatie bestudeert de invloed van psychologische effecten op individuele en sociale beslissingen bij onzekerheid. Door middel van experimenten worden nieuwe psychologische factoren geïdentificeerd en deze door theoretische modellen voor economische analyses geformaliseerd.

De resultaten van de empirische studies in deze dissertatie tonen aan dat extrapolatie van gedrag in markten op basis van gedrag in individuele situaties niet altijd tot nauwkeurige voorspellingen leidt. De manier waarop psychologische factoren de markuitkomsten beïnvloeden kan zelfs met kleine verschillen in de marktregels duidelijk veranderen. Vergeleken met situaties waar individuele beslissingen genomen worden, kan interactie tussen deelnemers in markten niet alleen de invloed van psychologische factoren verminderen, maar ook tot nieuwe afwijkingen van het economische model leiden.

Vaak zijn zowel rationele als psychologische theorieën in staat om een bepaald gedrag in individuele beslissingssituaties te verklaren. In marktsituaties hangen de uitkomsten naast het individuele gedrag tevens af van de effecten van het marktmechanisme. Hierdoor is het vaak moeilijk om de individuele en de marktcomponenten via de uitkomsten te identificeren. Het complementaire gebruik van experimenten met hoge interne validiteit en veldstudies met hoge externe validiteit is dan nodig om de verschillende economische en psychologische theorieën tegen elkaar te testen.

Uit de resultaten van deze dissertatie blijkt dat psychologische factoren van belang zijn voor economische beslissingen. Tevens blijkt een goede afweging nodig tussen

psychologisch realisme en theoretische vereenvoudiging: een beleidsaanbeveling die afgeleid wordt uit een theoretisch model dat gebaseerd is op de simplificatie van een psychologisch effect hoeft niet per se een goede benadering van het optimale beleid voor te stellen. Psychologische effecten die niet makkelijk te formaliseren zijn, kunnen wel degelijk van belang zijn voor beleid en reglementering.

The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus Universiteit Rotterdam, Universiteit van Amsterdam and Vrije Universiteit Amsterdam. The Institute is named after the late Professor Jan Tinbergen, Dutch Nobel Prize laureate in economics in 1969. The Tinbergen Institute is located in Amsterdam and Rotterdam. The following books recently appeared in the Tinbergen Institute Research Series:

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