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combinations of old ideas*

Jona Linde

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Universiteit van Amsterdam

**Experimenting with new
combinations of old ideas**

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Experimenting with new combinations of old ideas

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1. Introduction

As the title suggests this dissertation has two unifying themes. The first is the use of laboratory experiments which form the core of each of the four main chapters. The second theme is combining old ideas to gain new insights in human behavior. Specialization has greatly increased our understanding of human behavior. As a result behavioral models provide accurate predictions in many situations. However their predictive power is often limited to the type of situations for which they were developed.

For example we have excellent models for decision making under risk and for the influence of the earnings of others on people preferences, but no model to predict behavior in situations where both risk and the earnings of others are relevant. Questions like: “How does the wealth of a neighbor influence a person’s investment decisions?” or “Is career choice influenced by the salaries of your peers?” are therefore left unanswered.

Chapters 2 and 3 both report on experiments that try to fill this gap by examining decision making under risk in situations where social comparison is possible. These two chapters also combine theoretical ideas from decision making under risk and social comparison as a starting point and show that straightforward combinations of models do not describe the observed behavior.

Chapter 2 takes an important concept from theories on decision making under risk, the reference point, and asks whether a social reference point, the earnings of a relevant other, influences behavior in the same way. The reference point forms an important part of (cumulative) prospect theory (Kahneman & Tversky, 1979 and Tversky & Kahneman, 1992), the most successful descriptive theory on decision making under risk. We examine one specific effect of a reference point called the reflection effect: risk attitudes for gains are the opposite of those for losses. Risk aversion is the most common behavior when all possible outcomes are gains relative to the reference point, while risk seeking is the norm when outcomes are losses.

To examine whether a social reference point has the same effect on risk attitudes we experimentally investigate the simplest possible situation with both social comparison and risk: participants choose between two lotteries while a referent faces a fixed payoff. Participants are more risk averse when they can earn at most as much as their referent (loss situation) than when they are ensured they will earn at least as much as their referent (gain situation). Prospect theory with a social reference point would predict the exact opposite behavior. This result shows that social comparison can affect risk attitudes and that straightforward extensions of existing theories to allow for social comparison do not provide accurate predictions.

Chapter 3 examines an other way in which social comparison can influence risk attitudes. Social preference models assume that people care not only about absolute earnings, but also about their earnings relative to those of others. In contrast to most experiments that test these models we develop an experiment where participants take decisions that only influence their own earnings. Nevertheless several social preference models predict a treatment effect. In the social (individual) treatment participants do (not) observe the earnings of others. In the social treatment decisions therefore not only affect absolute but also relative earnings so social preferences can affect decisions in that treatment but not in the individual treatment. This experiment not only examines the importance of taking social preferences into account when analyzing decisions under risk, but also provides a novel test of social preference models. These models were originally constructed to explain why people spend money to affect the earnings of others and the influence of the income of others on reported happiness. However, all outcome-based social preference models also predict a treatment effect in our experiment.

We find that decisions are generally the same in both treatments. This suggests that ignoring social preferences when one is interested in decisions under risk may be harmless. However it also means that outcome based social preference models do not provide an accurate description of behavior in these situations. Rule-based social preference models such as procedural fairness on the other hand did not predict any difference in behavior between treatments. These types of models may therefore allow for a more general model that provides accurate predictions in situations where people can or can not affect the earnings of others.

Besides specialization with regards to the type of decision situations examined researchers also specialize in terms of the methods they use. Chapter 4 combines two different research methods. We use a well-known experimental economics method, the strategy method, to provide input to the evolutionary analysis of the minority game.

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The minority game is a very stylized depiction of many relevant decision situations such as congestion problems, market entry decisions or financial markets. An odd number of players choose each period between two actions. Players who choose the action chosen by a minority of the players earn a point, the others earn zero points. A common analogy is N people choosing between two roads, both of which get congested with more than $N/2$ travelers. So far this game has mainly been studied with evolutionary simulations but the strategies used in these simulations depended on the subjective choices of the researchers involved. For the relevance of these simulations it is important to know how the strategies chosen by these researchers compare to strategies used by real people. Some laboratory experiments have been run, but because of the enormous strategy space and the limited amount of rounds played it is impossible to deduce from these experiments which strategies people actually use. We therefore use an internet based strategy method experiment. Participants explicitly program a strategy which plays on their behalf. A website provides participants with ample opportunity to develop and test their strategies.

We find that the strategies people use are very heterogeneous. Many of the strategies participants use do not fit in the set of strategies used in simulations so far. For example many participants use explicit randomization and condition their actions on more precise outcomes than simply winning or losing. Despite the heterogeneity of the strategies simulations yield aggregate outcomes that closely resemble the symmetric Nash equilibrium and therefore low levels of coordination. However, strategies that survive in evolutionary competition achieve much higher levels of coordination than the complete pool of strategies. The surviving strategies are in general reluctant to change but they will change, with a small probability, when they lose for too long in a row.

Chapter 5 studies libertarian paternalism; the idea that known behavioral biases can be used to overcome other biases and so improve decisions without limiting people's choices. There is ample evidence that libertarian paternalism can indeed help people improve their decisions. So far however researchers have only looked at the decisions they attempted to influence. We study subsequent decisions in situations without libertarian paternalism. We compare people who have experienced libertarian paternalism to people who have not. The policy we study is one of the most popular forms of libertarian paternalism: a helpful default option.

We find that participants who faced a helpful default in the past continue to rely on the default even when it is no longer helpful. As a result they make worse decisions than

participants that did not face a helpful default. Our results provide a note of caution for people implementing libertarian paternalistic policies.

In conclusion chapters 2 and 3 combine two prominent research areas in behavioral economics, social comparison and decision making under risk. Chapter 4 does not combine different theories, but two different research methods, a strategy method experiment and simulations. Chapter 5 applies the idea that previous experiences can affect decisions to libertarian paternalism.

Combining old ideas from different fields goes against a long trend towards specialization in science in general and economics in particular. In behavioral economics in particular specialization has an excellent track record. Human decision making is a complicated business. That is what makes behavioral economics fascinating but also what makes it challenging. Leaving the safety of traditional homo economics models one enters the wilderness of bounded rationality with all its treacherous tricks and turns. Indeed in my own experience, and I believe in that of many others, human behavior is always more complex than anticipated. Behavior is influenced by a myriad of factors, each of which takes clever research methods to uncover and sophisticated models to understand. It is not surprising that as a consequence behavioral economics has branched out in numerous specializations. Specializations both in terms of methods used and the type of situations studied.

In an ideal situation specialization leads to a toolbox of behavioral models (Camerer & Loewenstein, 2004). The applied economist faced with a particular decision situation can dip into this toolbox, take out the appropriate models and make predictions or policy recommendations. In practice however things often turn out to be more difficult. Many real world situations are multifaceted and it is unclear how models can be combined to deal with those types of situations. New research therefore explicitly has to examine multidimensional situations to aid the development of more general behavioral models. The four papers presented in this dissertation aim to contribute to this goal. Of course that does not mean that the “old” specialized research was useless. More specific models can form the basis for both new hypotheses and more general models and research methods can be adapted to include new elements as the papers in this dissertation show.

2. Social Comparison and Risky Choices*

2.1. Introduction

Using comparison to evaluate outcomes or possibilities is a regular feature of human decision making. We compare our own situation to those of others (e.g. Clark, Frijters & Shield, 2008) and what is to what could have been (Loomes & Sugden, 1982) or to what was (Kahneman & Tversky, 1979) and these comparisons often affect our choices. The universal nature of comparison is emphasized by the importance of a reference point in two separate streams of research in behavioral economics: decision making under risk and social preferences. Reference points affect risk attitudes through loss aversion and probability weighting (Kahneman and Tversky, 1979 and Tversky and Kahneman, 1992). People's social preferences, their willingness to pay to raise or lower the payoff of others, are likewise reference dependent as they are influenced by the decision maker's earnings relative her social reference point, the earnings of a peer (Fehr & Schmidt, 1999 and Bolton & Ockenfels, 2000).

Loss aversion features prominently both in the literature on decision making under risk and in social preference theories. In individual decision making “losses loom larger than gains” (Kahneman & Tversky, 1979, p. 279) and, similarly, people care much more about being worse off than others than about being better off (e.g. Fehr & Schmidt, 1999). This similarity begs the question whether a social reference point can also cause well-established behavioral effects of individual reference points, such as the reflection effect. This is not self-evident; some studies have found that individual and social reference points have contrary effects. According to Bault, Coricelli & Rustichini (2008) people may actually be gain seeking relative to a social reference point in some situations. Also, what little information there is about the

*This chapter is based on Linde and Sonnemans (2012)

effect of a social reference point on the shape of the utility function suggest that it is concave in both the gain and loss domain (Vendrik & Woltjer, 2007) while for individual reference points utility is convex in the loss domain (Kahneman & Tversky, 1979).

Although the previous paragraph refers to existing research that allows for some comparison between the effects of individual and social reference points, the extent of their similarity remains largely unexplored. A reason for this gap in understanding is the very different focus of the decision making under risk and social preference literatures. The focus of the first line of research on risk has led to theories that are concerned with the shape of the utility function and the effect of probabilities. Social preference researchers on the other hand are mainly concerned with factors that strengthen or weaken social preferences. Because of the different research agendas there is not nearly enough empirical information to compare the behavioral effects of social and individual reference points.

In this paper we aim to fill some of this gap in empirical information. We explore whether a well-known effect of a reference point, the reflection effect, is exhibited relative to a social reference point. The reflection effect is the behavioral regularity that when all outcomes are losses risk seeking is generally observed, while risk aversion is the norm when all outcomes are gains¹ (Kahneman & Tversky, 1979). If a social reference point has this effect participants will make risk seeking choices when they know they will earn at most as much as a peer and risk averse choices when they know they will earn at least as much as a peer.

In our experiment participants are presented with such situations. Participants choose between lotteries which always yield positive earnings for the decision maker, but we manipulate the earnings of a matched participant, the referent. Particularly, we compare choices between lotteries in a loss setting (the referent earns more), a gain setting (the referent earns less) and a neutral setting (the referent earns the same). Figure 2.1 gives an example of the three kinds of choice situations presented to participants. The decision maker can compare her own earnings to those of the referent but can not affect her referent's earnings nor does she receive any information

¹ For small probabilities the opposite risk preferences are observed.

Social Comparison and Risky Choices

about the decisions of others². However, the decision maker's choice can be influenced by observing the earnings of another participant, her social reference point.

In order to make the referent more relevant matched participants first play a Bertrand game and are shown each other's picture. The nature of the interaction between participants in the Bertrand game may affect the way they perceive the other. Participants who cooperated are likely to have a different relationship than participants who competed. Different relationships may in turn lead to a different effect of social comparison. We therefore measure the social tie between a participant and her referent.

We find that participants chose the safe lottery more often in the loss situation than in the gain situation. This result provides a clear rejection of the hypothesized reflection effect with respect to a social reference point. In fact participants were on average risk averse in all (loss, neutral and gain) situations, but more so in the loss situation. Behavior in the neutral situation is in between that in the loss and gain situations. Social ties nor the type of interaction in the Bertrand game mediate the social comparison effect.

The rest of this chapter is structured as follows. Section 2.2 discusses relevant empirical and theoretical literature on both individual and social decision making and related research where both social influences and risk play a role. Section 2.3 explains the design of our experiment, section 2.4 introduces our research questions and section 2.5 provides the results. Section 2.6 concludes.

² In the Neutral situations the lottery faced by the referent does depend on the choice of the decision maker. Altruism could therefore in principal influence decisions in those situations. Participants could chose a lottery not because they prefer it, but because they think the referent prefers it. For that reason our main comparison will be between choices in the loss and the gain setting. However, we believe that the Neutral setting minimizes the effect of social comparison while remaining as close as possible to the Gain and Loss settings.

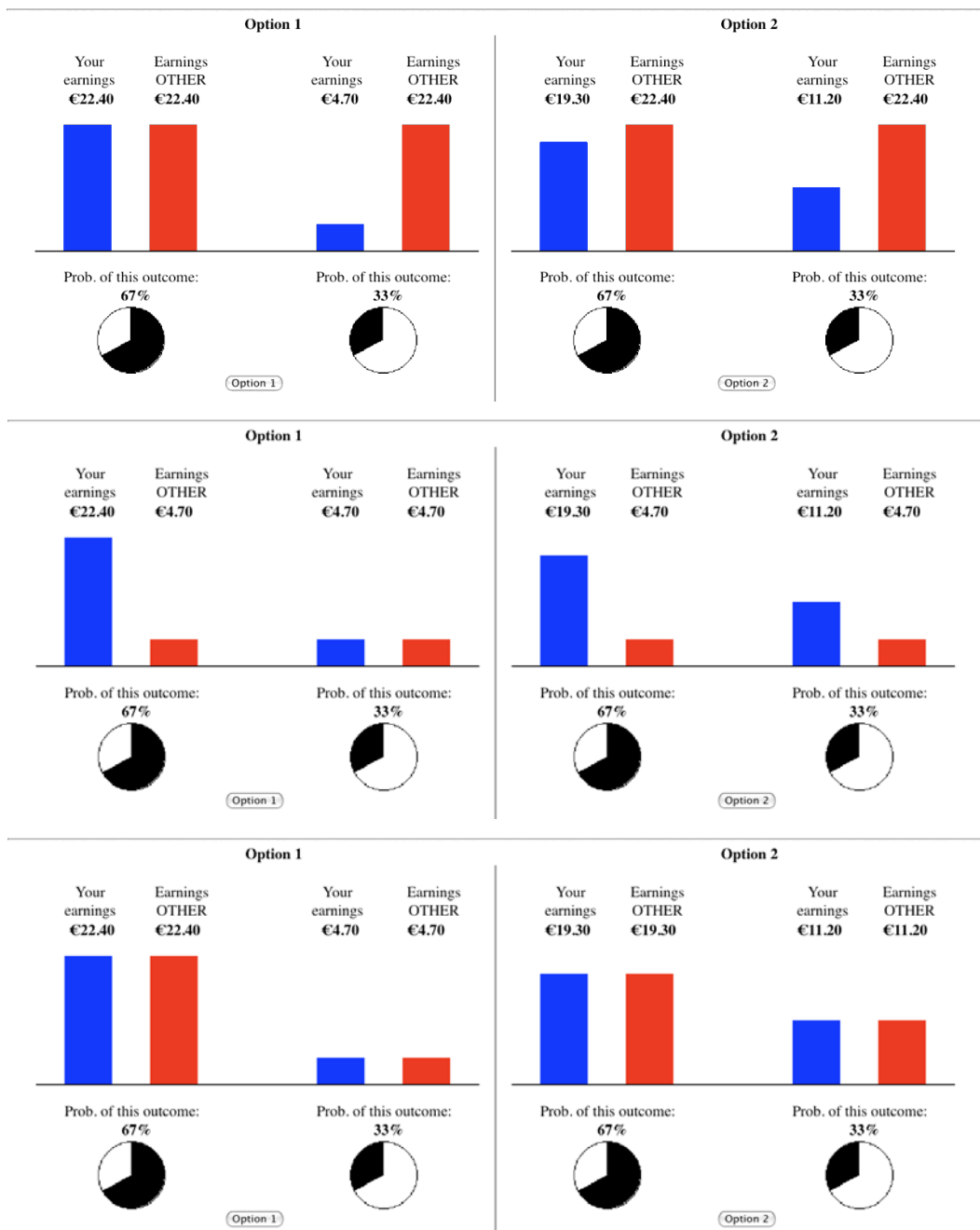


Figure 2.1: Lottery screens.

Each panel shows a decision situation. The blue bar represents the decision maker's earnings, the red bar her referent's earnings. Participants choose between option 1 displayed on the left and option 2 displayed on the right.

Top panel: loss situation, the decision maker earns at most as much as her referent.

Middle panel: gain situation, the decision maker earns at least as much as her referent.

Bottom panel: neutral situation, the decision maker and her referent earn equal amounts.

2.2. Theoretical background and related empirical findings

2.2.1. Reference dependence

Although normatively appealing the descriptive power of expected utility theory is challenged by a great host of observed deviations. Reference dependence is an important characteristic of many theories that try to explain these deviations.

Although expected utility theory holds that only final wealth states matter, experiments show that it is important whether an outcome is coded as a gain or a loss.

Loss aversion is probably the most well-known effect of reference dependence. It explains extreme risk aversion for gambles involving small losses and gains (Rabin 2000). Fishburn and Kochenberger (1979) were first to show that utility functions in terms of changes in wealth are steeper for losses than for gains. Numerous others studies have since confirmed loss aversion (e.g. Tversky and Kahneman (1992), Gneezy and Potters (1997) and Abdellaoui, Bleichrodt and Paraschiv (2007)).

A second behavioral regularity that shows the importance of reference dependence is the reflection effect. Kahneman and Tversky (1979) found that a gamble framed in terms of either gains or losses by changing the initial endowment has a profound effect on risk preferences. For gains they observe risk aversion, but for losses risk seeking is the predominant choice. The most famous illustration of this effect is the “Asian disease” study (Tversky & Kahneman, 1981). In this study participants exhibited a preference for relatively safe policies when outcomes were framed as saving lives (gains) and for relatively risky policies when outcomes were framed as prevented deaths (losses). A meta-analysis (Kühberger, Schulte-Mecklenbeck & Perner, 1999) corroborates the existence of the reflection effect.

Not surprisingly, reference dependence plays a vital role in the most successful theory on decision making under risk: cumulative prospect theory (CPT) (Tversky & Kahneman, 1992)³. Loss aversion is directly incorporated in CPT’s utility function by a kink around the reference point⁴. The reflection effect is explained, mainly, by (cumulative) probability weighting. Probabilities for outcomes far from the reference point are underweighted, if the probability is not too small ($>1/3$) (Prelec, 1998,

³ See Wakker and Tversky (1993) for an axiomatization of this theory and Chateaufneuf and Wakker (1999) for a specific axiomatization under risk. Wakker (2010) provides a great and extensive exposition of CPT.

⁴ Kahneman and Tversky speak of a value function instead of a utility function. We follow Wakker (2010) in using the label utility function.

Wakker 2010)⁵. As a consequence the decision weight of the best gains and the worst losses is often smaller than their probability. These low decision weights in turn lead to risk aversion for gains and risk seeking for losses. The shape of the CPT utility function, concave for gains and convex for losses strengthens this tendency.

As this discussion makes clear the reference point is a driving force for risk preferences. That makes the determination of the reference point very important. According to Kahneman and Tversky: “the reference state usually corresponds to the decision maker's current position, [but] it can also be influenced by aspirations, expectations, norms and social comparisons” (Tversky & Kahneman, 1991, pp. 1046, 1047). Most studies assume that the status quo (e.g. Rabin, 2000, Samuelson & Zeckhauser, 1988) or the lagged status quo (e.g. Thaler & Johnson, 1990) is the relevant reference point. Expectations have however also received attention as a possible reference point (Közegi & Rabin, 2006). The reference point can also be another variable than wealth such as the purchasing price of an asset (Odean, 1998). The use of many different reference points and the suggestion of Kahneman and Tversky beg the question whether the income of a peer may also play this role.

2.2.2. A social reference point

Although social comparison has received little attention as a driver of risk preferences its effect on other types of decisions has received ample attention from economists.⁶ People are willing to raise the earnings of others in a disadvantageous position but lower that of others in an advantageous position (e.g. Fehr & Schmidt, 1999). Kindness or unkindness of the other (e.g. Fehr and Gächter, 2000) and social ties (Sonnemans, van Dijk, and van Winden, 2006), mediate social preferences. Fehr and Schmidt (2006) review much of the evidence in this field as well as models that incorporate the observed behavior.⁷

⁵ Small probabilities are overweighted on the other hand, accounting for playing lotteries and insuring against unlikely losses (Kahneman & Tversky, 1979). Diecidue and Wakker (2001) provide an intuitive explanation for the CPT probability weighting scheme. Wakker (2010) provides references to further empirical evidence on the shape of the probability function in footnote 2 on page 204.

⁶ In psychology social comparison effects are also widely studied starting with Festinger (1954). Most of this research is concerned with evaluating own opinions and abilities. See Buunk and Mussweiler (2001) for a survey. As we are concerned with comparison of income or wealth and not opinions or abilities we will not discuss this.

⁷ Concerns for status or rankings can also affect decisions. Although the mechanism is different from that posited by theories like prospect theory concerns for status can lead to behavior that is similar to reflection effect. Harbaugh & Kornienko (2001) show that concern for local status can lead to risk aversion for gains and risk seeking for losses.

One important characteristic of a reference point, loss aversion, is also present with respect to the earnings of a peer and forms an important part of many influential theories. In the inequity aversion models of Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) the influence of relative earnings on utility is stronger when others earn more than you than when others earn less. Fehr and Schmidt note that their model “essentially means that a subject is loss averse in social comparisons: negative deviations from the reference outcome count more than positive deviations” (Fehr & Schmidt, 1999, p. 824).⁸

Loss aversion observed around the referent's earnings suggests that the role of the social reference point is similar to that played by other reference points. This raises the question whether we can also observe the reflection effect around a social reference point. As discussed above the prevailing explanation for the reflection effect depends on both the shape of the utility function and probability weighting. To date no research examines the effects of a social reference point on probability weighting. There is however some research that attempts to ascertain the shape of the social utility function.

Vendrik and Woltjer (2007) examined the effect of the difference between a household's income and the average income of a likely reference group on reported satisfaction. Their finding is that utility is concave in income, independent of whether the difference between own and reference income is negative or positive. The level of concavity is not significantly different for negative or positive deviations between own and reference income. As these authors observe, this is not in accordance with prospect theory where convexity is expected in the loss domain.⁹ Because convexity in the loss domain is part of the explanation of the reflection effect, the finding that utility is concave in social losses makes a social reflection effect less likely. A value

⁸ For the Fehr and Schmidt model the marginal utility of own earnings is $(1+\alpha_i)/(1-\beta_i)$ times as large when the decision maker earns less than her peer than when she earns more. α_i measures the disutility from disadvantageous inequality and β_i the disutility from advantageous inequality. Fehr and Schmidt's assumptions that $\beta_i \leq \alpha_i$ and $0 \leq \beta_i \leq 1$ ensure that an individual is loss averse unless she does not care about inequality.

Fehr and Schmidt assume $\beta_i \geq 0$, which implies people dislike advantageous inequality. There may however be (gloating) individuals who rejoice in being better off than others. Even these individuals will be loss averse relative to the referent's payoff as long as not being behind is more important to them than being ahead, i.e. if $\alpha_i > -\beta_i$.

⁹ These results are based on reported happiness, not choices. That begs the question whether they have anything to say about decision utility. Abdellaoui, Barrios & Wakker (2007) suggests that it does. They find that when the effect of probability weighting is taken into account utility functions based on choices and introspection agree to a remarkable degree.

function that is concave on both domains predicts risk aversion in both the loss and the gain domain, if we abstract from the possible effect of probability weighting.

The utility function found by Vendrik and Woltjer is steeper for negative deviations than for positive ones, confirming (social) loss aversion. As a result risk seeking choices would actually be less likely in the loss domain than in the gain domain. If agents choose between two lotteries (A and B) by comparing the expected utility (EU) of two lotteries but perceive the expected utility with error she will choose lottery A if: $EU(A) - EU(B) + \varepsilon > 0$ where ε is an error with mean zero. If U is a concave function and $EU(A) > EU(B)$ then $EU(A) - EU(B)$ is bigger if the utility function is steeper. Therefore it is more likely that $EU(A) - EU(B) + \varepsilon > 0$. In general a steeper utility function with the same error makes mistakes less likely. Given that with a concave function risk seeking choices are always mistakes such choices also become less likely.

2.2.3. Related research

Although most theories and empirical investigations concern either social comparison or decision making under risk some recent studies have explored situations where both social concerns and risk are present. Such studies have, among other things, found that uncertainty caused by others, strategic uncertainty, leads to more risk averse behavior than other types of uncertainty (Bohnet & Zeckhauser, 2004). Other studies show that combining ideas developed specifically for either social decisions or decisions under risk do not always predict decisions in situations where both are present. For example, although people are willing to pay to raise the (expected) earnings of others they will not pay to reduce others' risk (Brennan et al., 2008).

Bault, Coricelli and Rustichini (2008) cast doubt on the presence of loss aversion around a social reference point when people make decisions that affect only their own earnings. In their experiment people make choices over lotteries while observing the choices of an other participant who faces the same choice situations.¹⁰ Inequity aversion models, that assume loss aversion around a social reference point, predict that participants try to match the other's choices. Surprisingly, Bault et al. observe the opposite behavior: if participants face an opponent more likely to select

¹⁰ This other was, unbeknownst to the participants, a computer program that is either a value maximizer or extremely risk-averse.

the risky (safe) lottery they are found to be more likely to select the safe (risky) lottery. These findings can only be rationalized by a model where (at least) advantageous inequality is valued positively. Furthermore the positive effect of advantageous inequality has to dominate the negative effect of disadvantageous inequality.

Most closely related to our experiment is an experiment performed by Rohde and Rohde (2011). These authors also study risk taking in a social context where the decision maker has no influence on the payoff of the participants she is coupled with. Three aspects of this study make it difficult to link the observed decisions to a social reference point however. Firstly participants faced not one but ten referents, who may receive different amounts. Therefore it is hard to establish the outcome against which a decision maker could compare her own payoff. Secondly if the referents do get a single fixed amount that amount is, in most periods, somewhere in between the possible lottery payoffs for the decision maker, making it impossible to classify lotteries as concerning gains or losses¹¹. Thirdly, in their study participants did not interact with each other before making risky choices and anonymity was guaranteed, which may result in a less salient social reference point.

2.3. Design

Our experiment is designed to observe choices under risk in situations with one fixed social reference point, the simplest possible situation that includes both risk and social comparison. Table 2.1 shows the experimental tasks and the order in which they were presented. Task 4, the lottery choices, is of primary interest. In this task participants choose between two lotteries, one of which is clearly more risky than the other. Our main interest is in the behavioral difference between the loss and the gain lotteries (see figure 2.1). The other tasks are used to establish and measure social ties which can enhance the likelihood and importance of social comparison.

The experiment starts with a social value orientation test (the circle test) with a randomly determined participant. After this first part participants are coupled with their social referent (labeled "Other"). A Bertrand game is played to make the social referent more salient and to allow participants to develop different social ties, which may affect the effect of the social reference point. After the Bertrand game a second

¹¹ Only 1 pair of questions in Rohde and Rohde's study is comparable with our stimuli, but they find no effect for that pair.

circle-test is administered in which the participant is coupled with the Other. The second circle test is followed by the main part of the experiment where participants choose between lotteries. After this a post-experimental questionnaire is administered. To make social comparison even more focal we present participants with a photograph of their Other. Photos are shown directly after the end of the Bertrand game and on every subsequent screen, including during the lottery part¹².

<i>Part</i>	<i>Coupled with:</i>	<i>Photo displayed</i>	<i>Payment Probability</i>
1. Circle-test	Random participant (<i>not</i> the Other)	No	10%
2. Bertrand Game, 10 rounds Display of photo of OTHER and a short questionnaire	Other Other	No Yes	30%
3. Circle-test	Other	Yes	10%
4. Lottery choices <ul style="list-style-type: none"> • 10 gain • 10 loss • 10 neutral • 12 other 	Other	Yes	50% (1 of the 42 choices of one of the two coupled participants)
5. Questionnaire <ul style="list-style-type: none"> • Personal characteristics • Other's characteristics • Emotions during stage 2 • Decision making during stage 4. 	Other	Yes	

Random determination of the part that will be paid out, and when part 4 is selected, random determination of the relevant participant of the pair and the choice.

Table 2.1: Order of the experimental tasks

¹² Bohnet and Frey (1999) and Andreoni and Petrie (2004) show that providing a picture of matched participants increases contributions in public good games and transfers in dictator games. This suggests that visual identification increases the importance of a matched other.

Only one part of the experiment is paid out to ensure that earnings from an earlier part cannot influence behavior in the lottery part. With a probability of 50% the part where participants make choices over lotteries, with a probability of 30% the Bertrand game and with a probability of 10% each, one of the two circle-tests is paid. If the lottery part is paid, only one of the choices of one of the coupled participants is played out (determined randomly) and that choice determines the total payoff of both participants. This ensures that the decision makers perceive each lottery as independent. Participants answer control questions to confirm their understanding of this and other procedures.

An English translation of the experimental instructions is provided in appendix B, the original Dutch instructions are available upon request. All parts of the experiment are computerized (using PHP/MYSQL). We will now discuss the different parts of the experiment in more detail.

2.3.1. Photograph (Enhancing Social Comparison)

A photograph is taken of each participant before he or she enters the laboratory¹³. Participants are told that they will be matched with the same participant, the Other, during part 2, 3 and 4 of the experiment and that they will see a photo of the Other after part 2 of the experiment. Participants who know each other are requested to sit together in our reception room. We then make sure that they will not be matched¹⁴.

2.3.2. Circle-tests (Part 1 and 3, Measuring Social Value Orientation and Social Ties)

Circle-tests (Sonnemans, van Dijk & van Winden, 2006) are employed to measure the social value orientation of participants and their social tie towards the Other. In the circle-test the participant chooses a point on a circle with a radius of €15. Each point on the circle represents a combination of payoffs for herself and the participant she is matched with, the receiver. The circle-test is presented to the participant without any point selected or payoff combination displayed. When she clicks on a point on the

¹³ One participant chose not to participate in the experiment when we announced photos would be used.

¹⁴ When participants were first shown their referent's photo they were asked whether they knew this person. Only one couple professed a casual acquaintance while ten other participants recollected having seen the Other. All other (114) participants reported having been oblivious to their referents existence prior to the experiment.

circle's perimeter the corresponding payoff combination is shown. The participant can try as many points as she wants before confirming a payoff combination.¹⁵

Selecting a point on the circle involves making a tradeoff between the participant's own payoff and that of the receiver. As all payoff combinations lie on a circle the decision maker's earnings (x) and those of the receiver (y) have to fulfill the condition $x^2+y^2=15^2$. The slope of the circle differs along the circle, which affects the rate of transformation between one's own and the receiver's earnings. At the point of the perfectly selfish (€15, €0) payoff combination the slope is infinitely steep while it becomes ever shallower as one moves away from this point. This allows even weak, positive or negative, feelings about the receiver's payoff to influence the selected point.

A payoff combination can be represented by a vector from the origin to the point on the circle corresponding to that payoff combination. The angle between this vector and the vector representing the purely selfish payoff distribution measures the decision-maker's relative concern for the receiver. When the decision maker chooses a negative amount for the other the angle is recorded as negative.

At the start of the experiment participants perform the first circle-test in which they are randomly matched to an anonymous other participant. They are informed that they will not be matched with this same participant later in the experiment. The second circle-test is administered after the completion of the Bertrand game. At this point participants see their own picture and that of their Other. They only get feedback on either circle-test if this part of the experiment is selected to be paid out at the very end of the experiment. The total payoff to a participant is equal to the amount she allotted to herself plus the amount allotted to her by the matched participant.¹⁶

The outcome of this first circle-test is a measure of the participant's concern for an anonymous other, her social value orientation. The second circle test measures a participant's attitude towards the Other. Finally, the difference in angle between the second and first test measures the social tie to the Other; the importance of the Other's payoff relative to the payoff of an anonymous person (Sonnemans, van Dijk & van Winden, 2006).¹⁷

¹⁵ An English translation of the circle test can be found on: www.feb.uva.nl/creed/people/linde/circletest.html.

¹⁶ In theory this amount could be negative but this never happened in the experiment.

¹⁷ A participant's social tie can be affected by something besides the Bertrand game, for example the attractiveness of the Other (Andreoni & Petrie, 2004). We measure the social ties to examine their

2.3.3. Bertrand Game (Part 2, Creating Social Ties)

In the second part of the experiment participants play a Bertrand game with so called box demand¹⁸. In this game matched participants simultaneously choose an integer from $\{0,1,\dots,99,100\}$ which represents a percentage. The participant who chooses the lowest percentage gets her percentage of €5.-. The participant with the highest percentage gets nothing. If both participants choose the same percentage they share that percentage of €5.-. The game is played ten rounds without re-matching. If the Bertrand game is paid out a subject receives her accumulated earnings over all 10 rounds.

Assuming both participants are selfish, the Nash equilibriums for a one shot version of this game are both participants choosing 0%,1% or 2%. In this (finitely) repeated version of the game playing one of these equilibriums in each round is an equilibrium. Even if a pair plays the Pareto optimal of these equilibriums (2%) in all rounds both participants will earn no more than €0.50. Cooperation can increase earnings substantially. Full cooperation, both choosing 100% in all rounds, results in both participants earning €25.-.¹⁹

The preceding paragraphs show that cooperation is financially attractive in this game; however, defection can also be very lucrative. Choosing 99% instead of 100% when the other player chooses 100% raises earnings in that round from €2.50 to €4.95. The attractiveness of both cooperation and defection make it likely that participants will develop many different types of social ties, depending on how the game unfolds.²⁰ Different social ties allow us to explore the impact of social ties on the effect of social comparison on choices under risk.

influence on social comparison effects. The formation of these ties is not the main topic of the present study.

¹⁸ This type of game was used in other studies of the Bertrand game, e.g. Dufwenberger and Gneezy (2000).

¹⁹ Other equilibriums are possible using punishment strategies. In the last round it is only possible to play one of the one-shot Nash equilibriums. However because there are three different Nash equilibriums with different payoffs there is room for punishment. Punishment in the final round consists of playing a worse equilibrium, e.g. both choosing 0% instead of 2%. This can make both players choosing higher percentages in earlier rounds an equilibrium in the repeated game. Punishment in earlier rounds consists of playing a lower percentage than in the equilibrium. The most effective punishment is reverting to the 0% equilibrium in all subsequent rounds. The equilibrium that yields the highest earnings consists of full cooperation (both players choosing 100%) in the first four rounds, both choosing 64% in round five and half the percentage of the previous round in every subsequent round. In this equilibrium both participants earn €13.15, still substantially less than the €25.- they could earn by complete cooperation. Of course, these kinds of equilibriums are very difficult to coordinate on.

²⁰ The possible identification by their partner after the experiment may well have affected the behavior of participants, especially in the Bertrand game. We do not find this problematic because we are not

Independent of the kind of social tie developed the Bertrand game ensures that all participants have some meaningful interaction with their Other. This is likely to strengthen the effect of social comparison. Of course the bond between a participant and her Other is still less strong than that with peers, friends or foes to which she is likely to compare herself in real life. Also, even though different participants have different types of interaction in the Bertrand game, in some sense all participants share a similar history with their other in the sense that they have played the Bertrand game together.

2.3.4. Lotteries (Part 4, Main Experimental Task)

In the lottery part of the experiment participants face a total of 42 choice situations. In each of these they choose between two different lotteries that simultaneously determine their own payoff and that of the Other. All lotteries are so called simple lotteries²¹ with two possible outcomes. The choice in each situation is between a safe and a risky lottery with the same probabilities but with a larger variance of the outcomes in the risky lottery. In about half of the choice situations the risky lottery is presented on the left. To prevent order effects choice situations are presented to each participant in a different, random, order. The lotteries are displayed in appendix A.

Thirty of the 42 choice situations are created by presenting five original lottery pairs in six different ways. These six presentations are based on modifications in two dimensions. The first dimension is the social reference point, the payoff of the Other. Three kinds of social reference points are used: in *loss* situations the Other's payoff is equal to the highest possible payoff for the decision maker; in *gain* situations the Other's payoff is equal to the lowest possible payoff for the decision maker and in *neutral* situations the Other's payoff is equal to the decision maker's payoff regardless of the choice and outcome of the lottery. Figure 2.1 shows an example of a loss, a gain and a neutral situation.

The second dimension is the expected payoff: either the safe or the risky lottery has a slightly higher expected value. The safe lottery in the original lottery pair is slightly perturbed to create two closely related lottery pairs for each original pair. This manipulation ensures that participants cannot be indifferent in both cases.

primarily interested in the Bertrand game but in the influence of social interaction and social comparison on risky choices.

²¹ As opposed to compound lotteries.

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In the loss and gain situations subjects make choices that affect only their own outcome and cannot observe the choices of others. This eliminates the possibility of social learning, preferences for conformity and concerns about the other's payoff or reciprocity (as the other cannot influence your payoff either) affecting behavior. Twelve other lottery pairs are added to the aforementioned thirty lottery pairs. These are included to obscure the intentions of the experimenters and are not directly related to the research questions at hand.

2.3.5. Questionnaire (Part 5)

Participants are presented with a post-experimental questionnaire in which they are asked to list their field of study, their gender and their age. In addition they are asked to guess the age and field of study of the Other, to characterize the Other's personality (kindness, cheerfulness and helpfulness) and looks and to indicate how similar they think the Other is to them. Personality, looks and similarity are all rated using a seven-point scale. Participants are further asked to report, using a seven-point scale, on the emotions they experienced during the Bertrand game (rage, irritation, envy, joy, surprise and disappointment) and how satisfied they are with the outcome, their own decision and the Other's decisions. Likewise the importance of different aspects of the lotteries is rated.

2.4. Research questions

Our experiment is designed to answer two research questions: first, does a social reference point influence decisions under risk, and if so in what direction?; and second, do social ties or experiences in the Bertrand game influence this effect?. We will now discuss these questions and how the observations in the experiment can answer them in detail.

2.4.1. Does a Social Reference Point Influence Decisions Under Risk?

In the loss and gain situations the payoff of the referent is independent of the choice of the decision-maker or the outcome of the lottery. The decision of the participant only influences her own earnings. Assuming the decision maker maximizes expected utility neither selfish preferences nor linear social preferences predict any difference in behavior between gain and loss situations. A social reference point on the other

hand does predict a treatment difference. If the payoff of the Other is the decision maker's reference point all outcomes are gains in the gain situation and losses in the loss situation. According to the reflection effect this induces risk seeking choices in the loss situation and risk averse choices in the gain situations^{22, 23}.

This prediction is a natural extension of (cumulative) prospect theory to a social reference point but, it is doubtful whether such conjectures about behavior in social situations on the basis of theories based on observations of behavior in private settings hold. As Bault et al. (2008) and Brennan et al (2008) show, behavior in settings that include both risk and social comparison is not easy to predict by a straightforward extensions of models developed to account for either social preferences or risky choices. Furthermore the results of Vendrik and Woltjer (2007) suggest that at least one of the forces that drive the reflection effect according to prospect theory, the shape of the value function, may not be present in a social setting. Vendrik and Woltjer's value function is concave for both gains and losses, relative to a social reference point. The level of concavity is equal for gains and losses but the slope is steeper for losses. As discussed above this implies risk aversion in both loss and gain situation and fewer mistakes and therefore fewer risk seeking choices in loss situations²⁴. As in all choice situations in our experiment both lotteries have almost equal expected earnings a concave utility function without error would predict that participants almost exclusively choose the safer lottery. Errors would be the main explanation for participants choosing the risky lottery. Therefore the findings of Vendrik and Woltjer would predict fewer risky choices in the loss situations.

It is not obvious how the behavior in the neutral situations (where the payoff to decision maker and social referent will always be equal) will relate to the behavior in the gain and loss situations. In neutral situations the participant's decision also influences the earnings of the referent. The decision maker may therefore take into account the assumed (risk) preferences of the referent. However, the findings in

²² If the best outcome would have a small probability, probability weighting could reverse this effect, but in our decision situations the probability of the best outcome is always at least 0.33.

²³ If participants are concerned (only) with ranking the same behavior will be observed. In gain situations participants are ensured to earn more than the Other by choosing the safe lottery. In loss situations participants can only have a chance not to earn less than the other by choosing the risky lottery. This makes the risky lottery more attractive in the loss situation than in the gain situations.

²⁴ This result abstracts from the effects of different probability weighting for gains and losses. However, because the utility function found by Vendrik and Woltjer is not in accordance with the prospect theory with a social reference point there is no reason to assume different probability weighting in gain and loss situations in our experiment.

Brennan et al (2008) suggest that the risks faced by others have little impact on decisions. The differences in expected value are small, so it is unlikely that care for the other's expected payoff will influence choices. Consequently, if we accept the typical assumption of social preference theories that equal earnings is a neutral point, social comparison will not influence decisions in neutral situations. It thus seems plausible that in this case all outcomes will be coded as gains. According to (cumulative) prospect theory this means choices should be in line with those for gain lotteries. If the social reference point affects decisions through some other mechanism the effects of a high and a low social reference point are likely to run in opposite directions compared to a neutral situation. In that case the risk aversion in the neutral situation should be in between that observed in the loss and gain situations.

2.4.2. Do Social Ties or Experiences in the Bertrand Game Influence the Social Comparison Effect?

Besides determining whether a social reference point affects decision making under risk our experiment allows us to explore factors that affect the strength of the social comparison effect. In this section we describe these factors and the way in which they can influence social comparison.

A decision maker will only engage in social comparison when she finds her referent relevant. In the case of a positive or negative social tie the Other is apparently not irrelevant. We therefore expect a greater effect of social comparison, resulting in a greater difference in behavior between loss and gain situations, for participants with a positive or negative social tie compared to participants with a neutral social tie. Furthermore a negative social tie may influence social comparison differently than a positive social tie. For example a referent with a positive (negative) social may be relatively more (less) concerned with social comparison in gain situations where the other earns less en relatively more (less) concerned with social comparison in the loss situations where the other earns more. Another possibility is that it is not so much the social tie to the specific Other, but the concern with the referent's payoff as captured by the second circle-test that affects the extent of social comparison.

The tendency to engage in social comparison may also depend on individual characteristics of the decision maker. For practical reasons no personality questionnaires are administered, but participants who behave more pro-socially may have more attention for the payoffs of others compared with egoistic participants.

Therefore we can expect that pro-social participants, as identified by the first circle-test, are more likely to engage in social comparison. It is also possible that a more pro-social person is more concerned with social comparison when the other earns less, but less concerned when the other earns more.

Although the experiences in the Bertrand game are likely to be expressed through the social tie our experiment also allows us to explore the effects of these experiences more directly. In particular, the effect of social comparison may well be different for couples who cooperated (defined as both participants choosing 100 in a round) than for those who did not achieve cooperation. Moreover, when cooperation breaks down due to one participant being “betrayed” by the Other (defined as the participant chooses 100 while the Other chooses a lower percentage after the participants cooperated in the previous round) this yields yet another experience. It is plausible that such different experiences lead to different social comparison effects. The self-reports on emotions experienced during the Bertrand game are also informative about how a participant views the Other. A person who experienced anger is likely to care for the Other’s outcomes in a different way than someone whose partner gave her cause for joy. This motivates the analysis of correlations between the social comparison effects and the self-reported emotions.

Social comparison is known to depend on whether an individual considers the Other to be part of her in-group or her out-group (Mussweiler & Bodenhausen, 2002). Our questionnaire allows for several measures of similarity between the decision maker and the Other. It is more likely that the participant considers the Other as a relevant peer if similarity is higher, therefore we expect a positive relation between similarity and the effect of the social reference point. We further expect that participants will be more likely to engage in social comparison when they perceive of the Other as a better person. We therefore expect that the effect of social comparison will be strengthened if a participant rates his or her Other higher on the positive attributes. It could also be that the earnings of a “better” person are more relevant when the Other earns less than when the Other earns more.

2.5. Results

2.5.1. Social Reference Point Effect

Seven sessions of the experiment were run at the CREED laboratory in Amsterdam in December of 2008. 126 people participated in the experiment. Almost all of them were students from the University of Amsterdam; 46.8% of the participants were students of economics or business and 55.6% were male. A session lasted about 1.5 to 2 hours. All statistical tests in this section report two-sided p-values.

Figure 2.2 shows the average percentage of the time participants choose the safer lottery in the loss, gain and neutral situations. The safe lottery is chosen more often in the loss situation than in the gain situation. This difference is highly significant according to a Wilcoxon matched-pairs signed-rank test ($p=.0001$).

For every loss/gain situation pair we compare choices. Of the 1260 observations (126 participants and 10 loss/gain situation pairs) in 937 cases (74.4%) the choice was the same in loss and gain situations, in 203 cases (16.1%) more safe choices and in 120 cases (9.5%) fewer safe choices were made in the loss situation compare with the gain situation (binomial test $p<.001^{25}$). Studying each loss/gain situation pair separately we find that for 9 out of 10 pairs the safer lottery is chosen more often in the loss situation (binomial test $p=.02$). On the level of participants we find that for 38 participants (30.2%) the social comparison effect is neutral (no switches for 22 participants and the same number of switches in both directions for 16 participants), 61 participants (48.4%) made more safe choices and 27 (21.4%) made fewer safe choices in the loss situations (binomial test $p<.001^{26}$).

Given these tests the effect appears to be robust over situation pairs and participants: choices are more risk averse in situations where the social referent earns more (loss situations) than in situations where the social referent earns less (gain situations). This finding is opposite to the behavior predicted when the referents income is used as a reference point. It is however in line with the prediction made by the utility function of the shape found by Vendrik and Woltjer (2007).

²⁵ Ties are ignored as is the convention. A more conservative test, distributing ties equally yields $p=0.02$.

²⁶ Ties are ignored as is the convention. A more conservative test, distributing ties equally yields $p=0.003$.

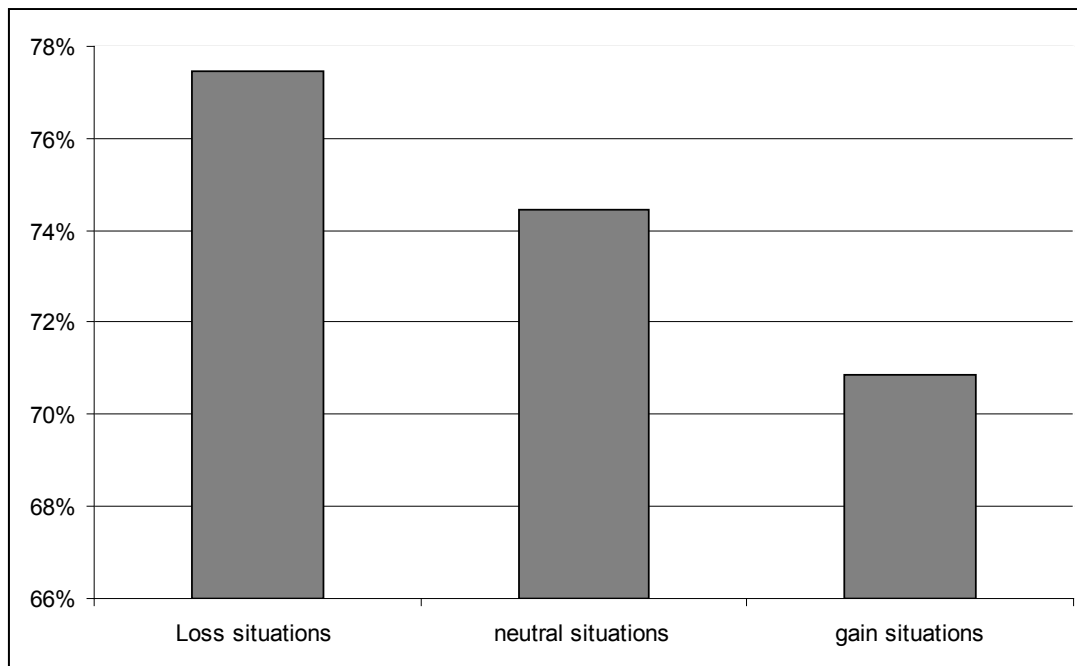


Figure 2.2: Average percentage of safe choices in the loss, neutral and gain situations.

It is difficult to assess the strength of the treatment effect. Most decisions are the same in the loss and gain treatment. That suggests the effect is not extremely large but it is not possible to quantify the effect on an individual level. The fraction of participants for whom we find an effect in the predominant direction is twice as large as in the opposite direction even if it is not a majority of all participants.

In neutral situations the safer lottery was chosen 74.4% of the time. This is in between the percentage of safe choices in the loss and gain situations. Choices in the neutral situations are significantly different from those in the gain situations (Wilcoxon test $p=.04$) and marginally significantly different from those in the loss situations (Wilcoxon test $p=.09$).

Result 1: Social comparison does matter for individual decision making: The risk-averse option is chosen more often in the gain situations than in the loss situations.

Result 2: Behavior in the neutral situation is in between the behaviors in the loss and gain situations.

A linear individual fixed effects regression confirms these findings and allows us to control for other factors. Table 2.2 reports on the regression's results. Most importantly the main social comparison effects remain significant. Several other base

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variables have a significant effect on choices in the expected direction. Firstly, an increase in the difference in variance between the safe and the risky lottery made it more likely that a participant choose the safe lottery. This is what is expected for risk averse individuals. Secondly, a higher probability of the best outcome in the risky lottery makes choosing the safe lottery more likely. The greater underweighting of larger probabilities, as specified by prospect theory, explains this effect. This finding strengthens the view that outcomes are coded as gains. Thirdly, the safe lottery is chosen more often when it has a higher expected value than the risky lottery. Fourthly, participants are also somewhat more risk averse in later periods.

Variable	Coefficient	p-value
Constant	0.449	0.000
Gain situation	-0.036	0.022
Loss situation	0.030	0.052
Difference in variance between safe and risky lottery	0.003	0.000
Probability of the best outcome in the risky lottery	0.237	0.000
Higher expected value for the safe lottery	0.026	0.043
Period	0.002	0.000
Interaction effect ^b gain situation and difference in variance.	-0.005	0.009
Interaction effect ^b gain situation and probability of the best outcome.	0.324	0.011
Interaction effect ^b gain situation and higher expect value for the safe lottery	-0.033	0.289
Interaction effect ^b loss situation and difference in variance.	-0.003	0.178
Interaction effect ^b loss situation and probability of the best outcome.	0.019	0.878
Interaction effect ^b loss situation and higher expect value for the safe lottery	0.028	0.375

Table 2.2: Linear individual fixed effects regression with the probability of choosing the safe lottery as dependent variable. ^a

a. For 11 participants individual fixed effects explain all their decision as these always choose the safe lottery. These individuals are therefore omitted from the regression.

b. Interaction variables are normalized to ensure the coefficients of the original variables are not distorted. Due to the normalization interaction effects are relative to the effects in the neutral situations, while the main effects of the difference in variance, the probability of the best outcome and the higher expected value for the safe lottery are the average effect over loss, gain and neutral situations. There is a significant difference between loss and gain situations for the effect of a probability of the best outcome and for the effect caused by a higher value for the safe lottery, but not for the effect of a difference in variance between the safe and the risky lottery.

We also find two interesting interaction effects of the gain situation with the difference in variance and with the probability of the best outcome. In gain situations the difference in variance no longer has a significant effect, while the effect of the probability of the best outcome is stronger²⁷. This could suggest a somewhat different decision process for gain lotteries. Possible participants made a less careful decision for gain lotteries, paying more attention to striking features like the probability of the best outcome and less to features that require a closer examination like the difference in variance.

2.5.2. The Influence of Social Ties and Bertrand Game

We now turn to the second research question: Do social ties or experiences in the Bertrand game influence the social comparison effect? We start by examining how the observed behavior relates to the social tie as measured by the circle-tests. If the difference between the angle chosen in the first and the second circle-tests is larger than 5 degrees we consider this as a positive or negative social tie (Sonnemans, van Dijk & van Winden, 2006). The participants with no social tie can be divided in two equally large categories: those who choose relatively selfish in both tests, or relatively cooperative in both tests. Table 2.3 displays the average difference between the loss and gain situations for these four categories.

Interestingly, the social comparison effect seems to be smaller for selfish participants who are likely to have less attention for the earnings of others; however, this difference is not statistically significant. Spearman rank correlations between the experimental effect and the social tie, the first angle (the more general social attitude) or the second angle (the social attitude to the specific Other) are also not statistical significant at conventional levels (all $p > .44$).

²⁷ A higher expected value for the safe lottery also has no significant effect in the gain situation, but its effect is not significantly different from the average effect.

	<i>Safe choices in loss situations minus safe choice in gain situations</i>	<i>N</i>
Positive social tie	0.61	33 (26.2%)
Negative social tie	0.60	20 (15.9%)
No tie, angle < 17.5 degrees	0.46	37 (29.4%)
No tie, angle > 17.5 degrees	0.94	36 (28.6%)
Total	0.66	126

Table 2.3: The size of the experimental effect for different social ties

Next we calculate Spearman's rank correlations²⁸ (indicated by *Rho* in the remainder of this section) between the social comparison effect and measures obtained in the Bertrand game, circle-tests and questionnaire. Rough measures of the success of the interaction in the Bertrand game are a participant's own earnings and the difference between her earnings and those of the Other. Neither of these is significantly correlated with the social comparison effect ($p > .4$). The average amount of cooperation in a pair ($Rho = -.04$, $p = .68$) or the occurrence of betrayal (Mann-Whitney test $p = .85$) are not significantly correlated with the social comparison effect either.

Participants report on negative emotions experienced during the Bertrand game. These emotions were rage, irritation, envy and disappointment and are combined into a single scale labeled *anger*²⁹. Three questionnaire items related to the Bertrand game request participants to report their satisfaction with the outcome, their own decisions and the decisions of the Other. These are combined in a scale labeled *satisfaction*. Neither scale nor the reported joy is found to correlate significantly with the social comparison effect ($p > .26$).

Several questions relating to the participant's perception of the Other are combined in a scale labeled *attractiveness*. These questions relate to looks, kindness, cheerfulness, helpfulness, openness and quality of the Other's picture. Two questions, on the intelligence of the Other and whether the Other is thought to be a thinker, are combined in a scale labeled *perceived intellect*³⁰. Neither of these measures correlates significantly with the social comparison effect ($p > .25$).

The perceived general similarity between the participant and her referent and the perceived similarity regarding the age and field of study of the Other, as measured

²⁸ Whenever we mention correlations below we refer to Spearman's rank correlations.

²⁹ Cronbach's Alpha shows that this scale, as well as the attractiveness and satisfaction scales (mentioned below) are internally consistent measures. (Cronbach's Alpha > .69).

³⁰ The answers to the questions on the Other's intelligence and whether the Other is a thinker are significantly correlated: $Rho = .3443$, $p = .0003$.

in the questionnaire, are not significantly related to the social comparison effect. Similarity between the players can also be measured objectively (same sex or different sex, difference in age and same or different field of study). None of these variables is correlated significantly with behavior in the lottery part.

Result 3: No relationship is found between the size of the effect of the social reference point and

- a. Social attitude or social ties as measured by the circle-tests*
- b. Experiences or experienced emotions in the Bertrand game.*
- c. Perceived characteristics of the Other*
- d. Similarity, either perceived or objective*

2.5.3. Additional Analyses

As none of our measures of the experience in the Bertrand game and the beliefs about and attitudes towards the Other are found to correlate with behaviour in the lottery part, it seems legitimate to question whether this is due to the reliability or relevance of these measures. We will therefore take a closer look at the relations between these measures.

As expected the experiences in the Bertrand game are found to influence a participant's social tie. The social tie is positively correlated with the differences in the earnings of matched participants in the Bertrand game ($Rho=.21$, $p=.017$). This effect is mainly caused by participants who earn less than their referent. The correlation between earnings in the Bertrand game and the social tie is found to be marginally significant ($Rho=.17$, $p=.063$). The mean social tie of participants who are betrayed in some period of the Bertrand game is significantly smaller than the social tie of non betrayed participants ($-3.19 < +3.28$, Mann-Whitney test $p=.02$).

The anger and satisfaction scales, based on the reported emotions experienced during the Bertrand game, are found to be significantly correlated with earnings in the Bertrand game, as is the reported joy experienced during the Bertrand game³¹ (Rho is

³¹ Besides these emotions experienced surprise was reported. This, more ambiguous, emotion is not found to be correlated significantly with experiences in the Bertrand game.

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-.61, .67 and .51 respectively, all $p < .001$)³². Anger is negatively related with the social tie ($Rho = -.32$, $p < .001$).

Greater perceived attractiveness and perceived similarity are positively and significantly correlated to the social tie. (Rho is .27 ($p = .008$) and .24 ($p = .01$) respectively). Perceived intellect is not significantly correlated with the social tie. The Other is reported as both less attractive and less similar if the respondent experienced betrayal. (Mann-Whitney test $p = .011$ and $p = .062$ respectively.)

We conclude that the social tie is related to the experiences in the Bertrand game and the perception of the referent in expected ways; the failure to find a relation between the social tie and the social comparison effect cannot result from an ineffective measurement of the social tie.³³

Finally, in the questionnaire we also asked about the goals of participants in the lottery part. A competitive goal ("I found it important to earn more than the Other") is negatively correlated with the attractiveness of the Other ($Rho = -.31$, $p = .002$) and correlates weakly with the social comparison effect ($Rho = .16$, $p = .08$). We find a somewhat stronger social comparison effect for participants who reported paying more attention to the amount of the Other ($Rho = .169$, $p = .060$).

2.6. Conclusion

Real life risky decisions are hardly ever made in social isolation: professional traders observe their colleagues, investors their neighbors and athletes their competitors. The effect of social comparison on decisions has received ample attention in social preferences theories and experiments, but the social context is remarkably ignored in the field of decision making under risk. Our lottery choice task considers the simplest possible situation where both risky choices and social comparison are present; choosing between two lotteries while comparing ones own payoffs to the fixed payoff of one social referent, the Other³⁴. We find that participants are more risk averse when

³² A higher score on all three scales signifies a stronger experience of the emotion.

³³ The possibility remains that we fail to find an effect because the effect of the social tie on the social comparison effect is present but noisy and/or not very strong or that either the induced social ties or the social comparison effect itself is not strong enough to find the presence of this effect.

³⁴ The manipulations used to make the Other relevant, the Bertrand game and the picture make the situation somewhat more particular. However given that we find that different pairs have very different social ties and that social ties do not correlate with the social comparison effect we believe that the observed effect is quite general and not related to these specific manipulations.

they can earn at most as much as the Other (loss situation) than when they are ensured they will earn at least as much as the social referent (gain situation).

It is well established that a non-social reference point (like present wealth) leads typically to risk seeking in the loss situation and risk aversion in the gain situation (the reflection effect), as predicted by prospect theory. Our results with a social reference point are the opposite of this prediction. We find that social comparison influences decision making under risk but that this effect cannot be explained by straightforward extensions of theories on decision making under risk to social situations.

The finding that this social reference point influences the behavior in another direction than the standard reference point is intriguing. It is however less surprising given the results of other recent studies which also find unexpected results in situations that include both social comparison and risk. Bault et al. (2008) show that while losses loom larger in individual decision making tasks, gains loom larger than losses in the social situation they study. Brennan et al (2008) show that people aren't willing to pay to reduce other's risk despite their social preferences over expected outcomes. More directly related is the finding by Vendrik and Woltjer (2007) on the effect of social comparison on the utility function. They establish that both below and above the social reference point value functions are concave, while other reference points lead to a convex value function for losses. As the value function is also steeper for losses this implies a fewer risk seeking choices in the loss domain which provides a possible explanation for our findings.

Although this provides a possible explanation for our result it is certainly early days to be definite about behavior in situations with risk and social comparison as the number of studies in this emerging field is very limited. However, our findings, together with research mentioned above, show the importance of studying such situations. It also shows that models will have to make great strides to incorporate the observed behavior.

2.7. Appendix A: The Lottery Pairs

		Prob. A (%)	Option 1				Option 2			
			Outcome A		Outcome B		Outcome A		Outcome B	
			SELF	OTHER	SELF	OTHER	SELF	OTHER	SELF	OTHER
1	l	67	22.40	22.40	4.70	22.40	19.30	22.40	11.20	22.40
2	g	67	22.40	4.70	4.70	4.70	19.30	4.70	11.20	4.70
3	n	67	22.40	22.40	4.70	4.70	19.30	19.30	11.20	11.20
4	l	67	22.40	22.40	4.70	22.40	19.10	22.40	11.20	22.40
5	g	67	22.40	4.70	4.70	4.70	19.10	4.70	11.20	4.70
6	n	67	22.40	22.40	4.70	4.70	19.10	19.10	11.20	11.20
7	l	56	19.40	19.40	6.30	19.40	16.30	19.40	10.40	19.40
8	g	56	19.40	6.30	6.30	6.30	16.30	6.30	10.40	6.30
9	n	56	19.40	19.40	6.30	6.30	16.30	16.30	10.40	10.40
10	l	56	19.40	19.40	6.30	19.40	16.20	19.40	10.20	19.40
11	g	56	19.40	6.30	6.30	6.30	16.20	6.30	10.20	6.30
12	n	56	19.40	19.40	6.30	6.30	16.20	16.20	10.20	10.20
13	l	67	11.40	20.80	14.70	20.80	8.30	20.80	20.80	20.80
14	g	67	11.40	8.30	14.70	8.30	8.30	8.30	20.80	8.30
15	n	67	11.40	11.40	14.70	14.70	8.30	8.30	20.80	20.80
16	l	67	11.30	20.80	14.50	20.80	8.30	20.80	20.80	20.80
17	g	67	11.30	8.30	14.50	8.30	8.30	8.30	20.80	8.30
18	n	67	11.30	11.30	14.50	14.50	8.30	8.30	20.80	20.80
19	l	67	21.10	21.10	8.90	21.10	16.20	21.10	19.10	21.10
20	g	67	21.10	8.90	8.90	8.90	16.20	8.90	19.10	8.90
21	n	67	21.10	21.10	8.90	8.90	16.20	16.20	19.10	19.10
22	l	67	21.10	21.10	8.90	21.10	16.10	21.10	18.80	21.10
23	g	67	21.10	8.90	8.90	8.90	16.10	8.90	18.80	8.90
24	n	67	21.10	21.10	8.90	8.90	16.10	16.10	18.80	18.80
25	l	56	6.90	18.40	14.40	18.40	3.60	18.40	18.40	18.40
26	g	56	6.90	3.60	14.40	3.60	3.60	3.60	18.40	3.60
27	n	56	6.90	6.90	14.40	14.40	3.60	3.60	18.40	18.40
28	l	56	6.70	18.40	14.30	18.40	3.60	18.40	18.40	18.40
29	g	56	6.70	3.60	14.30	3.60	3.60	3.60	18.40	3.60
30	n	56	6.70	6.70	14.30	14.30	3.60	3.60	18.40	18.40
31		67	21.70	10.90	21.70	15.80	21.70	7.90	21.70	21.70
32		67	7.90	10.90	7.90	15.80	7.90	7.90	7.90	21.70
33		67	21.70	10.70	21.70	15.80	21.70	7.90	21.70	21.70
34		67	7.90	10.70	7.90	15.80	7.90	7.90	7.90	21.70
35		56	23.80	22.40	23.80	9.70	23.80	19.60	23.80	13.40
36		56	9.70	22.40	9.70	9.70	9.70	19.60	9.70	13.40
37		56	23.80	22.40	23.80	9.70	23.80	19.40	23.80	13.40
38		56	9.70	22.40	9.70	9.70	9.70	19.40	9.70	13.40
39		50	6.90	11.40	22.50	18.10	11.40	6.90	18.10	22.50
40		50	6.90	11.20	22.50	18.10	11.20	6.90	18.10	22.50
41		67	14.20	18.20	14.20	18.20	12.20	18.20	18.20	18.20
42		67	14.20	12.20	14.20	12.20	12.20	12.20	18.20	12.20

The second column indicates the category of the lottery pair: l(oss), g(ain) or n(eutral).
The choice situations were presented in a random order.

2.8. Appendix B: Experimental Instructions

(Translated from Dutch. Original Dutch instruction available upon request)

General Instructions

This experiment consists of 4 parts. You will receive instructions on each part prior to the start of the part concerned.

If you have any questions during the experiment, raise your hand.

The OTHER

During each part you will be coupled with another person in this room who we will call the OTHER. In parts 2, 3 and 4 this is always the **same** person. The person to whom you are coupled in part 1 is an **other** person than the person you are coupled with in parts 2, 3 and 4.

Photo

Before the start of the experiment we made a photograph of all participants. After part 2 (and not before) you get to see a photo of the person you are coupled with in parts 2, 3 and 4.

When you get to see a photo of the OTHER he or she will also get to see a photo of you. When you are not yet seeing a picture of the OTHER, the OTHER will not see a photo of you either.

Payout

During this experiment you can make money. **The earnings of only 1 of the 4 parts will be paid out.** Which part this will be is determined after the end of the last part. With 10% chance this will be part 1, with 30% chance this will be part 2, with 10% chance this will be part 3 and with 50% chance this will be part 4. How much you earn in a specific part depends on the choices made by you and/or the OTHER. Besides their earnings in the experiment everyone will receive €10,-.

[Control questions: the participant had to answer questions concerning the matching process, the payout probabilities and the point in the experiment where photos would be displayed.]

Instructions for part 1

Choice

In this part you have to choose between combinations of earnings for yourself and the OTHER. All possible combinations are represented on a circle like the one shown above. Later you can click on any point on the circle. Which point you choose determines how much money you and the OTHER earn. **You can not click on the circle yet.**

Earnings

The axes in the circle represent how much money you and the OTHER earn when you choose a certain point on the circle. The horizontal axis shows how much you earn: the more to the right, the more you will earn. The vertical axis shows how much the OTHER will earn: the more to the top, the more the OTHER earns. The distribution can also mean negative earnings for you and/or the OTHER. Points on the circle left of the middle mean negative earnings for you, points below the middle mean negative earnings for the OTHER. When you click on a point on the circle the corresponding

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combination of earnings, in cents, will be displayed in the table to the right of the circle. You can try different points by clicking on the circle using your mouse. Your choice will only become definite when you click on the “send” button.

The OTHER is presented with the same choice situation. Your total earnings in this part consist of the amount allotted by you to yourself and the amount allotted to you by the OTHER by his or her choice.

Pay out

The OTHER’s chosen combination is only made public if this part is paid out (this happens with a chance of 10%, see the general instructions on the paper on the table).

After this part you will be coupled to a different participant for parts 2, 3 and 4. (see the general instructions on paper.)

[Control questions: the participant had to choose some specified distributions on the circle.]

Instructions for part 2

The OTHER

You are now coupled to a different person than in part 1. From now on you will be coupled to this person.

Decisions

This part consists of 10 rounds. Every round both you and the OTHER make a decision. This decision consists of choosing a percentage, at least 0 and at most 100. This percentage should be a whole number. The percentages chosen by you and the OTHER determine what you and the OTHER earn in a round.

Earnings

The earnings in each round are determined in the following way:

- If you and the OTHER choose the same percentage you both get half of €5,- multiplied by the percentage chosen by you.
- If the chosen percentages are different the one who choose the lowest percentage will get €5,- multiplied by that percentage. The person who chose the highest percentage will get nothing in that case.

Total earnings in this part are equal to the earnings over all 10 rounds added together.

Pay out

This part is paid out with 30% chance; see the general instructions on the paper on the table.

[Control questions: participants had to calculate earnings of themselves and the OTHER resulting from specified percentages chosen by themselves and the OTHER]

Instructions for part 3

This part is the same as part 1 except that you are coupled to a different person, the person you were matched with in the previous part. So you again have to choose between combinations of earnings for yourself and an OTHER. The other is now the person you were coupled with in part 2.

Choice

In this part you have to choose between combinations of earnings for yourself and the OTHER. All possible combinations are represented on a circle like the one shown above. Later you can click on any point on the circle. Which point you choose determines how much money you and the OTHER earn. **You can not click on the circle yet.**

Earnings

The axes in the circle represent how much money you and the OTHER earn when you choose a certain point on the circle. The horizontal axis shows how much you earn: the more to the right, the more you will earn. The vertical axis shows how much the OTHER will earn: the more to the top, the more the OTHER earns. The distribution can also mean negative earnings for you and/or the OTHER. Points on the circle left of the middle mean negative earnings for you, points below the middle mean negative earnings for the OTHER. When you click on a point on the circle the corresponding combination of earnings, in cents, will be displayed in the table to the right of the circle. You can try different points by clicking on the circle using your mouse. Your choice will only become definite when you click on the “send” button.

The OTHER is presented with the same choice situation. Your total earnings in this part consist of the amount allotted by you to yourself and the amount allotted to you by the OTHER by his or her choice.

Pay out

The OTHER’s chosen combination is only made public if this part is paid out (this happens with a chance of 10%, see the general instructions on the paper on the table).

[Control questions: participants had to select a specified payoff combination and answer questions concerning payout probabilities and the matching process.]

Instructions for part 4**Choices**

In this part you have to choose between 2 different lotteries on every screen. In total you will be presented with such a choice situation 42 times.

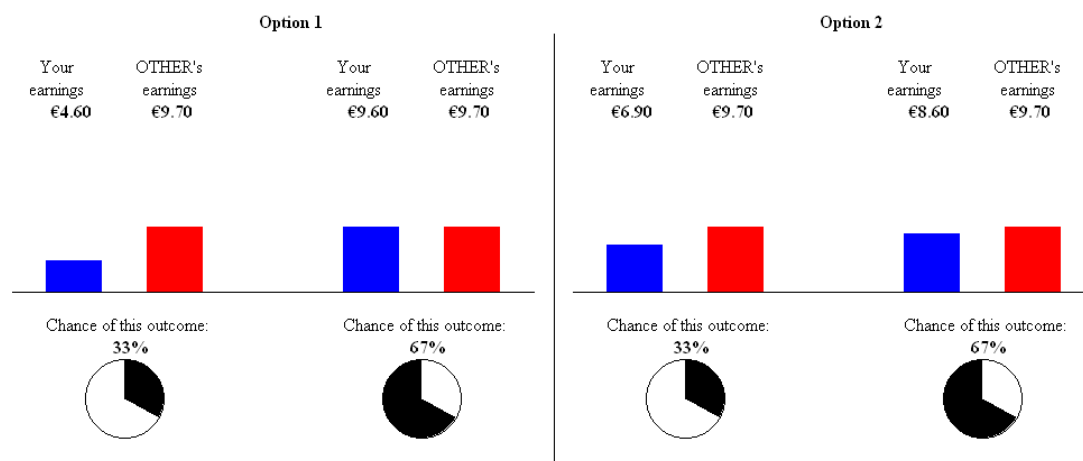
The lotteries in this part determine both your earnings and those of the OTHER. Below you can see an example of a screen like the screens you will get to see later. On the screen you can see two lotteries between which you can choose. One to the left of the line in the middle of the screen, the other to the right. The blue bar represents how much you will earn in the outcome concerned. The red bar how much the OTHER will earn. The amounts are also written below the bars. The chance of a certain outcome is represented by the circle below the bars. The dark colored part of the circle represents the chance of the outcome concerned. Below the circle the chance in percentages is written.

Choice situations

The choice situation below is only an **example**. You will not be asked to choose between the lotteries you see here.

In this example the earnings of the OTHER are equal, independent of your choice or the outcome. This may be the case in the choice situations you will be presented with later, but it will not be the case in all choice situations.

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Earnings

If this part is selected to be paid out it is first determined whether one of yours or one of the OTHER's choice situations will be detrimental. Thereby there is just as much chance that it will be one of your choice situations as that it will be one of the choice situations of the OTHER. Then it will be determined which of the 42 choice situations of the selected person will be looked at. Each choice situation has an equal chance of being selected. The selected choice situation will then be looked at to determine which of the two lotteries was chosen by the selected person (you or the OTHER). This lottery is then played out and determines the total earnings of both you and the OTHER.

Pay out

The chance that this part is paid out is 50% (see the general instructions on paper which are on your table).

If one of the choice situations presented to you is selected the payoff to you and the OTHER is determined **only** by the lottery chosen by you in that choice situation. **That means that when you make a choice you can assume that only that choice determines the total earnings of you and the OTHER.**

[Control questions: participants had to answer questions regarding their understanding of the payout probabilities and presentation of the lotteries.]

3. Social Preferences in Private Decisions*

3.1. Introduction

Other-regarding preferences have supplanted pure egoism in many economic models, from labor economics (e.g. Demougin, Fluet and Helm, 2006) to optimal taxation (e.g. Choi, 2009). Two sets of empirical observations have precipitated this development. Firstly, behavior in games where decisions makers influence the earnings of others cannot be explained by egoism (see Fehr & Schmidt, 2006). Secondly, reported happiness appears to depend on relative as well as absolute income, the so-called Easterlin paradox (1974) (see Clark, Frijters and Shields, 2008).

Despite these origins other-regarding preference models also make behavioral predictions in other domains, like decision-making under risk. Such novel predictions provide an excellent test of these models. We consider situations in which the decision maker cannot influence the earnings of others but where the prospects determine not only the decision maker's absolute earnings but also her earnings relative to her peers. The possibility to compare one's own earnings with the earnings of peers should influence decisions, according to outcome-based social preference models. For example, inequity aversion (Fehr & Schmidt, 1999 and Bolton & Ockenfels, 2000) implies that people dislike gambles that lead to a large dispersion in earnings, i.e. where they either end up with a lot more or a lot less than their peer(s).

Earlier experiments show that social concerns can indeed influence decisions under risk. For example, Bohnet en Zeckhauser (2004) show that risk caused by others is more aversive than other forms of risk. In an earlier paper (Linde & Sonnemans, 2012) we show that people become more risk averse in a socially

*This chapter is based on Linde and Sonnemans (2011)

disadvantageous position. However, some anticipated effects of other-regarding preferences on decision making under risk are typically not observed. For example, although people are willing to pay to raise the (expected) earnings of others they will not pay to reduce others' risk (Brennan et al, 2008 and Güth, Vittoria Levati & Ploner, 2008). Trautmann and Vieider (forthcoming) provide an extensive overview of research on other-regarding preferences and risk.

In this paper we study situations where people take risky decisions without affecting the earnings of others. Participants make pair-wise choices between sets of three cards (figure 3.1). At the end of the experiment one choice situation is randomly selected. Participants blindly draw a card from their preferred set. The number on the card they draw determines their earnings.

Participants are randomly assigned to either the individual or the social treatment. In the individual treatment all participants draw from a separate set and are not informed about the earnings of others. They therefore face an entirely private lottery. In the social treatment three participants draw from the same set, without replacement. As a consequence a set of cards not only implies a gamble but also a distribution of earnings between three participants. Therefore other-regarding preferences can influence behavior in the social treatment, but not in the individual treatment. However, because all participants draw a card from the set of their own choice, participants in neither treatment can influence the earnings of others by choosing a specific set.

Comparing behavior in the individual and social treatments reveals the impact of other-regarding preferences. If participants care about outcomes in terms of relative earnings, sets that lead to more (less) desirable earnings distributions are relatively more (less) attractive in the social treatment than in the individual treatment. We consider four different models of this kind that make different predictions. The first of these is inequity aversion (Fehr & Schmidt, 1999 and Bolton & Ockenfels, 2000). This model predicts that sets that result in a greater dispersion of earnings are less attractive in the social treatment than in the individual treatment. The second model, inequity seeking, (Bault, Coricelli & Rustichini, 2008), predicts the exact opposite. Thirdly, maximin preferences (Rawls 1971) predict that sets where the lowest possible earnings are highest are more popular in the social treatment. Fourthly, according to models where utility depends on one's rank in the group (e.g. Robson, 1992) the dispersion of earnings does not matter but the resulting ranking does.

In addition to the outcome-based models described above there exist rule-based models of social preferences. According to these models people do not care about outcomes in themselves, but about how these outcomes are reached. Procedural fairness (e.g. Trautmann, 2009), where people care about equality in terms of expected, but not realized earnings, is an example of such a model. These and other rule-based social preference models predict no differences between the treatments.

Section 3.2 describes the experimental design and section 3.3 presents the theory and the hypotheses. Section 3.4 reports the results of our experiment. In short we find that behavior in the social treatment is indistinguishable from that in the individual treatment. Our findings are therefore in line with procedural fairness. Section 3.5 concludes.

3.2. Experimental design

Although we introduce social concerns in one of the treatments, the experimental setup stays as close as possible to common individual decision-making experiments. The individual (control) treatment consists of a series of choices between two lotteries. The social treatment retains the same general structure but introduces social comparison without changing the incentives for a person who does not care about relative income or her position in the income distribution. Importantly our social treatment does not introduce the possibility to influence the payoff of others.

3.2.1. Individual treatment

Participants face 20 pair-wise choice situations in an individually randomized order. In each of these situations participants choose between two sets of three cards³⁵. Each card has an integer number between 1 and 29 on it. The numbers on the three cards in a set always add up to 31. Figure 3.1 shows a screen shot of a choice situation and table 3.1 displays all 20 choice situations.

³⁵ Which of these sets appears left or right is randomly determined for each participant individually.

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Choose one of the sets of cards below. The numbers on the three cards in a set always add up to 31.

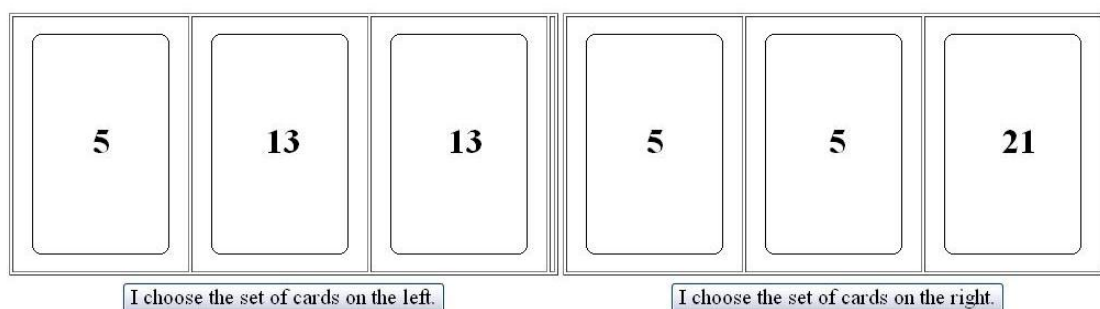


Figure 3.1: A translated example of the choice situation as presented to participants in both treatments.

When all participants have made their decisions one choice situation is randomly selected. Participants are informed about the selected choice situation and reminded of the set they chose in that situation. They then blindly draw one card from the set they preferred. The participant's earnings in Euros are the number on the card they draw divided by two. Choosing a set of cards implies the choice of a lottery.³⁶

Because the sum of the numbers on three cards in a set is always 31 the lotteries represented by the sets of cards all have the same expected value. There are three different types of sets: LLH sets with two low numbers (L) and one high number (H), LHH sets with one low number (L) and two high numbers (H) and LMH sets with three different numbers, a low (L), middle (M) and high number (H).

3.2.2. Social treatment

In the social treatments participants face the same choice situations as in the individual treatment. One of these choice situations is again randomly selected. In contrast to the individual treatments however participants are then matched with two others who chose the same set in that choice situation. These three participants successively, blindly, draw a card from this set without replacement. As a result a set of cards not only represents a lottery over the decision maker's own earnings, but also over her relative earnings.

³⁶ The experiment was computerized using php/mysql and no actual cards were used. Appendix A gives the English translation of the instructions.

<i>Choice situation</i>	Set A^a			Set B			Hypotheses^b				Results (% A)		
	#	<i>Cards</i>	<i>Type</i>	#	<i>Cards</i>	<i>Type</i>	<i>1 Inequity aversion</i>	<i>2 Inequity seeking</i>	<i>3 Maximin prefs</i>	<i>4 Ranking prefs</i>	<i>Indiv.</i>	<i>Social</i>	<i>p^c</i>
1	1	1,1,29	LLH	4	1,15,15	LHH	<	>	0	>	5.41%	11.90%	0.195
2	1	1,1,29	LLH	7	8,8,15	LLH	<	>	<	0	8.11%	7.14%	0.321
3	1	1,1,29	LLH	2	1,8,22	LMH	<	>	0	>	8.11%	19.05%	0.101
4	1	1,1,29	LLH	5	5,11,15	LMH	<	>	<	>	13.51%	9.52%	0.237
5	4	1,15,15	LHH	7	8,8,15	LLH	<	>	<	<	24.32%	33.33%	0.135
6	4	1,15,15	LHH	2	1,8,22	LMH	>	<	0	<	83.78%	78.57%	0.192
7	4	1,15,15	LHH	5	5,11,15	LMH	<	>	<	<	48.65%	45.24%	0.171
8	7	8,8,15	LLH	2	1,8,22	LMH	>	<	<	>	16.22%	21.43%	0.192
9	7	8,8,15	LLH	5	5,11,15	LMH	>	<	>	>	32.43%	38.10%	0.163
10	2	1,8,22	LMH	5	5,11,15	LMH	<	>	<	0	18.92%	19.05%	0.225
11	3	5,5,21	LLH	6	5,13,13	LHH	<	>	0	>	10.81%	11.90%	0.273
12	3	5,5,21	LLH	8	9,9,13	LLH	<	>	<	0	18.92%	11.90%	0.171
13	3	5,5,21	LLH	9	9,11,11	LHH	<	>	<	>	16.22%	19.05%	0.221
14	3	5,5,21	LLH	10	10,10,11	LLH	<	>	<	0	13.51%	14.29%	0.253
15	6	5,13,13	LHH	8	9,9,13	LLH	<	>	<	<	48.65%	50.00%	0.177
16	6	5,13,13	LHH	9	9,11,11	LHH	<	>	<	0	40.54%	54.76%	0.082
17	6	5,13,13	LHH	10	10,10,11	LLH	<	>	<	<	24.32%	26.19%	0.201
18	8	9,9,13	LLH	9	9,11,11	LHH	<	>	0	>	35.14%	23.81%	0.108
19	8	9,9,13	LLH	10	10,10,11	LLH	<	>	<	0	18.92%	14.29%	0.205
20	9	9,11,11	LHH	10	10,10,11	LLH	<	>	<	<	43.24%	40.48%	0.175

Table 3.1: The choice situations used in the experiment.

^a In the experiment set A and B were randomly displayed on the left or right side, without labels.

^b Shows per hypothesis whether choosing set A over set B is more (>), less (<), or equally (0) likely in the social treatment than in the individual treatment. Hypothesis 5 predicts no difference between treatments. These hypotheses are explained in section 3.

^c P-values for a two-sided Fisher-exact test.

It is possible that the number of participants choosing a set is not a multiple of three. In that case the number of participants choosing one set is always a multiple of three plus one and the number choosing the other set a multiple of three minus one. We then randomly select one of the participants who chose the set chosen by a multiple of three plus one and reallocate him or her to the other set. Participants are aware of this. Given this procedure there is at most one participant per session who does not get to choose from his or her preferred set. Therefore participants in both treatments have an incentive to choose the set they prefer.

Compared to the individual treatment the social treatment only changes one thing: sets of cards now also imply a distribution of earnings between three peers. All other aspects of the decision situation such as the implied risk or the presentation of the decision situation remain the same. Importantly, although we introduce social comparison, participants cannot affect the set from which another participant draws a card or influence earnings of other participants in any way. Altruism or similar concerns therefore cannot affect participants' decisions.

3.2.3. Related experiments

This experimental design is similar to so-called “veil of ignorance” experiments, inspired by Rawls' (1971) classic thought-experiment. In such experiments, participants choose an income distribution for a group without knowing their place in the distribution (e.g. Beckman et al., 2002 and Carlsson et al., 2005). Schildberg-Hörisch's (2010) experiment comes closest to our design because she compares behavior in treatments with and without the possibility of social comparison.

The fundamental difference between our design and veil of ignorance experiments is that in the latter decisions makers affect the earnings of others while we exclude this possibility. As discussed above this allows us to exclude several other-regarding concerns such as altruism. As far as we know only three other experiments on decision making under risk and other-regarding preferences share this feature: Bault Coricelli and Rustichini (2008), Rohde and Rohde (2011) and Linde and Sonnemans (2012). All three of these experiments test a different and/or narrower set of hypotheses than we do here and do not directly compare behavior in contexts with and without the presence of social comparison.

3.3. Theory and hypotheses

As social preferences have gained credence they are ever more often incorporated into applied economic models (e.g. Demougin, Fluet & Helm, 2006 and Choi, 2009). The kind of other-regarding preferences that are assumed can have a profound impact on the predictions and policy recommendations of these applied economic models. Although the existence of other-regarding preferences is hardly ever questioned anymore the exact form these preferences take is still up for discussion.

The decision situations in our experiment have been designed to distinguish between some of these models. The primary distinction is between outcome-based social preferences, which predict a treatment effect, and rule-based social preferences, which predict the same behavior in both treatments. Furthermore, different types of outcome-based models make different predictions.

3.3.1. Outcome-based fairness

Most models of other-regarding preferences assume that people care about outcomes. Here we discuss four models that are all successful in explaining much of the existing evidence: inequity aversion, inequity seeking, maximin preferences and ranking preferences. Each of these makes a different prediction in our experiment.

Inequity aversion

Inequity aversion models such as those of Bolton & Ockenfels (2000) and Fehr and Schmidt (1999) provide an accurate description of behavior in many games where the division of money is at stake such as the dictator game and the ultimatum game. These models explain this behavior by an aversion to unequal earnings. Both earning more and earning less than peers lead to a loss in utility. In other words: an aversion to inequity implies distaste for more dispersed income distributions, for a given level of (expected) own earnings.

To see the implications of these models in our experiment we compute the difference in the utility, according to the model of Fehr and Schmidt (1999), of drawing from a certain set in the individual and the social treatment. Independent of the type of set (LLH, LHH or LMH) this difference is given by³⁷:

$$-\frac{1}{3}(\alpha_i + \beta_i)(H - L) \quad (3.1)$$

α_i and β_i are the disutility caused by disadvantageous and advantageous inequality respectively.

Formula 1 shows that the difference in utility between treatments is directly related to the difference between the highest (H) and the lowest amount (L). Both α_i and β_i are assumed to be positive by Fehr and Schmidt (1999). Therefore sets with a larger difference between H and L should be relatively less attractive in the social

³⁷ Appendix B provides the proof of this expression. It further shows that also according to Bolton and Ockenfels' (2000) ERC model the difference in utility between treatments is directly related to the difference between H and L for LLH and LHH sets. HML sets yield a slightly different expression but this expressions leads to the same expected treatment effects.

than in the individual treatment. Importantly, the difference in value between the social and individual treatment is independent of the type of set (LLH, LHH or LMH).

Given this analysis inequity aversion models lead to the following hypothesis:

Hypothesis 1: (inequity aversion): Sets where H-L is larger are chosen less often in the social than in the individual treatment.

In the literature on reported happiness similar types of social preference model are prevalent as an explanation for the Easterlin (1974) paradox, the finding that happiness scores are strongly increasing in income within countries but much less so between countries (Clark, Frijters and Shields, 2008).³⁸ The usual explanation for this observation is that happiness is at least partly determined by relative income (e.g. Layard, 1980 and Clark and Oswald, 1996). In contrast to inequity aversion models these models assume that utility is increasing in advantageous inequity. People with such preferences are not inequity averse, but envious. The disutility caused by disadvantageous inequality is commonly held to be greater than the utility of advantageous inequality; in terms of the Fehr and Schmidt model $\beta_i < 0$ and $\alpha_i > -\beta_i$ and thus $\alpha_i + \beta_i > 0$. Although the assumption about the utility of advantageous inequity in such models is different than in the Fehr-Schmidt model, the predicted behavior is in line with hypothesis 1.

Inequity aversion creates a kink in the utility function around the earnings of a peer in the same way loss aversion in Prospect Theory (Kahneman & Tversky, 1979 and Tversky & Kahneman, 1992) causes a kink around the reference point. So if the earnings of a peer are considered a reference point in the sense of Prospect Theory, this theory also predicts behavior in line with hypothesis 1.³⁹

Inequity seeking

Although inequity aversion provides an accurate description of behavior in some situations, research by Bault, Coricelli and Rustichini (2008) implies that it may not be an accurate description of behavior when people make risky choices that only

³⁸ This claim only holds within the set of developed countries. If one looks at developing countries there is a positive effect of average income on happiness scores.

³⁹ Probability weighting as modelled in cumulative prospect theory predicts a force in the opposite direction by placing a lower decision weight on the worst outcome. However, the strong risk aversion observed for gambles that allow for both gains and losses shows that the effect of loss aversion trumps the effect of probability weighting. (Wakker 2010, chapters 8 and 9)

affect their own outcome. In their experiment participants chose between two lotteries and observed the choices and outcomes of one other participant facing the same choices. The other participant was, unknown to the participants, actually a computer who made either very risk averse or risk neutral choices. Inequity aversion models, as well as a preference for conformity, predict that participants would try to match their “peer’s” choices. Bault et al. observed the opposite behavior. Participants matched to a risk averse (neutral) computer became more risk tolerant (averse).

People are apparently willing to risk earning less in order to have the chance to earn more than their peer. Translated in terms of the Fehr and Schmidt model this means that people have a β_i that is negative and in absolute terms larger than α_i . From formula 1 it then follows that a larger difference between the best and the worst outcome (H-L) actually increases the utility of an option in the social treatment. This leads to a hypothesis that is the exact opposite of the inequity aversion hypothesis:

Hypothesis 2 (inequity seeking): Sets where H-L is larger are chosen more often in the social than in the individual treatment.

A possible explanation of the difference between the Fehr and Schmidt model and Bault et al.'s findings is that the Fehr and Schmidt model is based on situations where distributing money was at stake, like ultimatum games. In that case altruism may lead to an observed dislike of advantageous inequality and reciprocity to a stronger dislike of disadvantageous inequality. Both altruism and reciprocity are not present in the situation studied by Bault et al. (2008) so their finding may be a better description of people’s preferences over outcomes per se. If so, it provides a better prediction of behavior in situations, such as that studied here, where people's decisions only influence their own earnings.⁴⁰

⁴⁰ Our experiment can be seen as a reexamination of the effect observed by Bault et al. using methods more acceptable to economists, i.e. without deceiving subjects. Moreover Bault et al.'s findings rely on the assumption that participants form correct beliefs about their “peer's” behavior. Participants in Bault et al.'s experiment may for instance have believed that participants who took more risk in the past were actually less likely to take risk in the future. In that case the observed behavior would actually be an attempt to match the other's choices and thereby avoid unequal outcomes. Lastly, behavior observed by Bault et al. may also be an attempt to express individuality by consciously choosing something different than the other. Neither beliefs nor a preference to express individuality affect decisions in our experiment as participants have full information about the resulting distribution and no information about the choices of others.

Maximin preferences

Rawls' (1971) maximin principle is one of the most well-known philosophical ideas about distributive justice. According to this principle resources in a society should be divided in the way that most benefits the least well off. Rawls justified the maximin principle with a veil of ignorance thought-experiment. He thought that if we do not yet know our place in a society we would prefer a society in which the least well off are best off.

The empirical relevance of maximin preferences has been demonstrated experimentally. Veil of ignorance experiments (e.g. Beckman et al. , 2002, Carlsson et al., 2005 and Schildberg-Hörisch, 2010) show that people indeed care about the least well off, but also about average earnings or efficiency. Even without a veil of ignorance maximin preferences may have a bite. Engelmann and Strobel (2004) and Charness and Rabin (2002) show that a combination of efficiency concerns and maximin preferences give the best description of behavior in their experiments. In our experiment efficiency does not play a role because total earnings are equal in all sets. Maximin preferences would therefore predict that sets where the amount earned by the person who earns the least (L), are relatively more attractive in the social treatment:

Hypothesis 3 (maximin preferences): Sets where L is larger are chosen more often in the social than in the individual treatment.

In many cases this hypothesis provides the same prediction as inequity aversion. There are however choice situations where that is not the case. Take for example choice situation 1 (table 3.1): 1-1-29 versus 1-15-15. In this case the lowest amount a participant can earn is the same in both sets. Maximin preferences would therefore predict no treatment difference in this situation. On the other hand the difference between H and L is smaller in 1-15-15 and inequity aversion therefore predicts that this will be chosen more often in the social treatment than in the individual treatment.

Ranking preferences

Reported happiness studies do not only posit models where utility is based on income share or income relative to the average income level, but also models where utility is based on the agent's income *rank* within the population (e.g. Layard,1980 and Robson,

1992). Both types of models fulfill their goal of explaining the Easterlin paradox because in both models a higher average income for others lowers a person's utility, either through a lower relative income or a lower rank. It is difficult to distinguish between these types of models with field data, however, in our experiment we created situations where these two theories make different predictions.

As discussed in the design section the sets of cards used in our experiments can be divided into three kinds. In the social treatment a LLH set means that one person will hold top rank while the two others will share bottom rank: one winner and two losers. In contrast, in a LHH set there will be two winners and only one loser. The third kind of set (LMH) results in a complete ranking without ties. Intuitively, in a game of chance is it much nicer to be the sole winner than one of the two winners and in case of losing the pain will be less if there is a fellow sufferer, which suggests that LLH would be more attractive than LHH in the social treatment (compared with the individual treatment). We will now formulate this intuition more formally.

We label ranks, from top to bottom, 1, 2 and 3 and ties as 1.5 for two winners and 2.5 for two losers. The expected utility of a set in the social treatment if people care about rank can then be represented by the following formulas⁴¹:

$$\text{LLH: } \frac{1}{3}H + \frac{2}{3}L + \frac{1}{3}R(1) + \frac{2}{3}R(2.5) \quad (3.2)$$

$$\text{LHH: } \frac{2}{3}H + \frac{1}{3}L + \frac{2}{3}R(1.5) + \frac{1}{3}R(3) \quad (3.3)$$

$$\text{LMH: } \frac{1}{3}H + \frac{1}{3}M + \frac{1}{3}L + \frac{1}{3}R(1) + \frac{1}{3}R(2) + \frac{1}{3}R(3) \quad (3.4)$$

where $R(r)$ is the function that represents the effect of rank on an agents utility.

Predicted treatment effects are caused by the part of the utility function that is different between the social and individual treatments, the terms that contain the function $R(r)$. The difference in difference between the value of LLH and LHH between the social and individual treatment is given by:

$$\begin{aligned} & \left(\frac{1}{3}R(1) + \frac{2}{3}R(2.5) - \frac{2}{3}R(1.5) - \frac{1}{3}R(3) \right) = \\ & \left(\frac{1}{3}(R(1) - R(1.5)) + \frac{1}{3}(R(2.5) - R(1.5)) + \frac{1}{3}(R(2.5) - R(3)) \right) \end{aligned} \quad (3.5)$$

⁴¹ Like the Fehr & Schmidt model we assume linear utility in own income, but as this component is the same in both treatments this does not affect the hypotheses.

So in the social treatment LLH becomes relatively more attractive than LHH when

$$(R(1) - R(1.5)) + (R(2.5) - R(3)) > (R(1.5) - R(2.5)) \quad (3.6)$$

In words: the extra utility of winning alone above winning together plus the extra utility of losing together above losing alone should be larger than the difference between winning together and losing together. This inequality holds for a function that is relatively flat in the middle, compared to the average slope at the top and the bottom.⁴² Or put differently, it holds if coming first and/or not coming last is more important to the agent than moving up a place in the ranking in the middle⁴³. In our view this type of preference is intuitively plausible.

If we compare the LMH sets to LLH and LHH sets we find a similar set of inequalities. The LMH set is relatively attractive compared to the LHH set if

$$\frac{1}{3}R(1) + \frac{1}{3}R(2) > \frac{2}{3}R(1.5) \quad (3.7)$$

The LMH set is relatively unattractive compared to the LLH set if:

$$\frac{2}{3}R(2.5) > \frac{1}{3}R(2) + \frac{1}{3}R(3) \quad (3.8)$$

By the same reasoning as above we believe it plausible that both inequalities will hold. Compared to the LMH sets the LLH sets exclude the chance to be the only loser while compared to LHH sets, LMH sets introduce the chance to be the only winner. That results in the following hypotheses:

Hypothesis 4 (ranking preferences):

- a. LLH sets are chosen over LHH sets more often in the social treatment than in the individual treatment.*
- b. LLH sets are chosen over LMH sets more often in the social treatment than in the individual treatment.*
- c. LMH sets are chosen over LHH sets more often in the social treatment than in the individual treatment*

⁴² Straightforward functions such as $(C-r)^\alpha$ with $0 < \alpha < 1$ or $\ln(C-r)$ fulfill this requirement. (C can be any arbitrary number larger than 3. This ensures that the part between brackets is positive, because 3 is the maximum rank number possible.)

⁴³ Evidence of such preferences can be found in athletic competitions. In such competitions the prizes are typically Gold, Silver, Bronze or no medal (which can be interpreted as losing). Medvec, Madey and Gilovich (1995) find that Bronze winners are typically happier than Silver winners. This suggests that the difference in utility between losing and Bronze is high and that an improvement from Bronze to Silver adds little utility (in their study even negative). The authors explain this by a change in reference point; Silver winners focus on the Gold that they missed and Bronze winners on the losers who get no medal.

3.3.2. Procedural fairness

The foundation of many other-regarding preference models rest on the observation that people are often willing to pay in order to raise, or lower, the earnings of others. An obvious interpretation is that they do so because they prefer the situation created by their actions. All other-regarding preference models discussed so far indeed assume that people's action are caused by a preference over *outcomes* in terms of relative earnings. There are however competing explanations. People can have rule-based preferences, that is, preferences over the procedures or actions that determine the outcomes. This distinction between outcome-based and rule-based preferences follows the philosophical distinction between consequentialist and deontological ethics (Alexander & Moore, 2007).

An example of rule-based preferences is procedural fairness (e.g. Trautmann, 2009). According to this model agents have preferences over the rule that determines earnings. As long as people perceive everyone as having (had) a fair chance they do not care about the dispersion in final earnings. Such model can explain results in the traditional Ultimatum and Dictator Games just as well as outcome-based models, but makes different predictions in situations with uncertainty. For example, Krawzyck and Le Lec (2010) study a version of the dictator game in which the dictator divides the (100%) probability of winning a prize between herself and the recipient. The two possible final outcomes are that either the dictator or the recipient wins the prize. If the dictator dislikes disadvantageous inequity more than advantageous inequity, the dictator should keep 100% to herself, according to outcome-based preference models⁴⁴. Procedural fairness in contrast motivates a division of the probabilities which is what Krawzyck and Le Lec find.

Several other studies also show that, as predicted by procedural fairness, people care mainly about equality in expected rather than final earnings. Bartling and Von Siemens (2011) show this in an experiment on team production. In their experiment wage schemes with the same level of ex-ante inequality but different levels of ex-post inequality are valued about the same. Brennan et al (2008) and Gth, Vittoria Levati and Ploner (2008) show that people are not willing to reduce the risk others face and thereby the expected inequality. This is in line with procedural fairness because changing others risk does not influence ex-ante inequality.

⁴⁴ If the dictator dislikes advantageous inequity more than disadvantageous inequity (β smaller than α in Fehr and Schmidt's model) she should donate all chances to the recipient.

Happiness studies at first sight appear to provide strong evidence for outcome-based preferences. Research in this field shows that happiness is strongly correlated with relative income (Clark, Frijters & Shields, 2008). Participants in these studies rate the situation they are in, so apparently their feelings have to be based on their preference over different possible situations. However, besides the inherent problem with self-reported, non-incentivized data, there are alternative explanations for this pattern. People may not feel bad about their relative earnings or wealth per se, but because they feel they did not receive a fair chance to become rich (Alesina, Di Tella & MacCulloch, 2004).

In our experiment participants cannot affect the earnings of others and all three matched participants in the social treatments have the same expected earnings so there is no ex-ante inequality. Participants who base their decisions on procedural fairness only should therefore behave the same way in the social and the individual treatments. This leads to the following hypothesis⁴⁵:

Hypothesis 5: (procedural fairness): choices are the same in the social and the individual treatment.

3.4. Results

The experiment was run at the CREED lab in June 2010. A total of 79 participants participated in 4 sessions, 42 in the social treatment and 37 in the individual treatment. 58% was male and 40% were economics majors. All participants had first participated in another, unrelated experiment. That experiment was a pure individual experiment where social comparison was impossible (Sonnemans & van Dijk, forthcoming). The experiment took about 20 minutes and the average earnings were around 5.2 euro (in addition to the show up fee and the earnings in the other experiment).

To test our hypotheses we calculate, for each hypothesis, per individual how often she chose the lottery predicted to be more attractive in the social treatment. This we take as an independent observation. We then compare the distribution of percentages in both treatments to test the hypotheses. All tests in this section are two-sided.

⁴⁵ A person who is not concerned with fairness would also behave the same way in both treatments. However, there is overwhelming evidence that many individuals have social preferences.

3.4.1. Hypothesis 1 (inequity aversion) and 2 (inequity seeking),

Inequity aversion predicts an aversion to a greater dispersion of earnings: sets with a larger difference between the highest amount (H) and the lowest amount (L) should be relatively less attractive in the social than in the individual treatment (hypothesis 1). On the other hand findings by Bault et al. (2008) suggests that the exact opposite behavior, leading to hypothesis 2.

Table 3.2 shows that neither of these hypotheses holds up. People choose the set with the smaller difference between H and L about as often in the individual treatment as in the social treatment. A Wilcoxon-Mann-Whitney test shows that the difference between treatments is far from significant ($p=0.81$).

Statistic (percentage of choices)	Individual treatment	Social treatment	Difference	p-value Wilcoxon-Mann-Whitney test ^a
Hypothesis 1 (Inequity Aversion) and 2 (Inequity seeking) ^b (20 choice situations)	76.90%	75.35%	1.55%	0.81
Hypothesis 3 (Maximin preferences) (15 choice situations)	69.73%	69.20%	0.53%	0.90
Hypothesis 4 (Ranking preferences)				
a) LLH versus LHH (8 choice situations)	41.22%	38.99%	2.23%	0.72
b) LLH versus LMH (4 choice situations)	43.24%	42.26%	0.98%	0.74
c) LMH versus LHH (2 choice situations)	33.78%	38.10%	4.31%	0.44
Total (14 choice situations)	40.57%	40.14%	0.43%	0.94

Table 3.2: Treatment differences: Average percentage of choices for the set predicted to be more attractive in the social treatment.

^a One individual is one independent observation.

^b In this case the percentages shown are the percentage of choices for the set predicted to be more attractive in the social treatment by hypothesis 1.

3.4.2. Hypothesis 4 (ranking preferences)

Ranking preferences hypothesize that behavior is not influenced by the size of the difference between earnings, but only by the implied ranking. In our experiment that means only the type of set, LLH, LHH or LMH, matters. Specifically hypothesis 4 states that in the social treatment LMH sets should be relatively more attractive than LHH sets and LLH sets should be relatively more attractive than both LMH and LHH sets. However the treatment difference predicted by this hypothesis is not observed. Testing the three parts of hypothesis 4 on the choice between LLH and LHH sets (a), LLH and LMH sets (b) and LHH and LMH sets (c) separately shows that for none of these types of decision situations behavior is different in the social and individual treatments. (Wilcoxon-Mann-Whitney all p-values >0.44) and also taking the three parts of the hypothesis together does not show a difference ($p=0.94$).

Rejecting hypothesis 4 provides evidence against the importance of rank as a driver of behavior. However caution is warranted because hypothesis 4 is based on some specific assumptions about the type of ranking preferences used. For example, linear ranking preferences, where each change in position is equally important, predicts no difference in behavior between treatments.

3.4.3. Hypothesis 5 (procedural fairness)

The rejection of all other hypotheses is in line with hypothesis 5 that states that behavior should be the same in both treatments. It is however possible that behavior is influenced by some other type of social preferences over outcomes. Comparing behavior in each of the 20 choice situations shows that this is not the case. According to a Fisher exact test there is no difference in choices between the social and individual treatments for any choice situation (all 20 p-values are larger than 0.08), see table 3.1. This persistent rejection of outcome-based fairness models is an implicit support for procedural fairness models that predict no treatment effects.

3.5. Conclusion

Other-regarding preference models were developed to explain consistent violations of selfishness, like the spending of money to affect the earnings of others in ultimatum and other games. As always, it is easier to explain old facts than predict new ones. A

real test of these models can be found in novel situations that were not yet available when the models were created. Our experiment provides such a situation.

In our experiment the decisions of the participants influence only their own outcomes and in that sense they face purely individual and non-strategic decisions. We compare an individual treatment without peers with a social treatment where social comparison is possible (there are winners and losers). Models that assume preferences over outcomes, like inequity aversion or seeking, maximin preferences and ranking preferences, all predict that the introduction of social comparison would affect behavior in our experiment. Procedural fairness on the other hand predicts no effect. Our results are in line with this second type of model.

Historically the development of models in experimental and behavioral economics about social preferences on the one hand and the models of individual decision-making on the other hand occurred parallel without much interaction. However, economists who try to predict real world behavior or give policy advice face the problem that many real situations combine elements of both fields and they have to fit two kind of models together in some way. Camerer and Loewenstein, (2004) suggest viewing behavioral economics as a toolbox. After looking at a situation the economist can turn to this “toolbox”, select the appropriate behavioral models and combine them as required. In practice things can be more difficult than the analogy suggests because many different models are available and it is not always clear what will be the best choice in these specific circumstances. Of course, these are in essence empirical questions. Our research gives an answer for one particular situation, to wit a situation where both risk and social comparison are relevant: it suggests that outcome-based models such as inequity aversion or seeking, maximin preferences or ranking preferences are not relevant here.

An obvious conclusion of our study is that the outcome-based fairness models we studied are less general than supposed and are only valid in situations where decision makers can influence the earnings of others. However, this would mean that we need two separate models. One for situations where people can influence the earnings of others and an other for situations where they cannot. However, procedural fairness, which so far provides accurate predictions in both situations, allows for one more general model.

We do not claim that this is the end-all answer. Other research has found social comparison effects on decisions under risk, behavior predicted by none of the

existing models. For example Bohnet en Zeckhauser (2004) show that the source of the risk, “nature” or other people needs to be considered. As we show in an earlier paper (Linde & Sonnemans, 2012) the relative position of a person prior to making the decision also influences risk attitudes. A general model that aims to describe behavior in situations with both social comparison and risk will need to incorporate these findings.

3.6. Appendix A: Experimental instructions

(Translated from Dutch. Original Dutch instruction available upon request)

Instructions individual treatment

Your earnings in this experiment are determined by drawing 1 card from a set of 3 cards. Each card has a number on it: the number of points you get if you draw that card. Your earnings in euros are the number on the card divided by 2 (each point is worth 50 cents). **The numbers on the 3 cards in a set always add up to 31.**

When drawing a card you do not get to see the number on the card and each card has the same chance to be in a certain position. **Therefore you cannot know which card you will draw.**

The set of cards you will draw from depends on your choices. In total you will be asked 20 times to choose between two sets of cards. One of these choice situations is randomly selected. The set from which you will draw a card is the set you choose in that choice situation. **Therefore you should always choose the set you prefer.**

Instructions social treatment

In this experiment you are matched with 2 other participants. Your earnings are determined by consecutively, without replacement, drawing a card from a set of 3 cards. You cannot draw a card that has already been drawn by another participant. Each card has a number on it: the number of points you get if you draw that card. Your earnings in euros are the number on the card divided by 2 (each point is worth 50 cents). **The numbers on the 3 cards in a set always add up to 31.**

When drawing a card you do not get to see the number on the card and each card has the same chance to be in a certain position. **Therefore you cannot know which card you will draw.** You cannot see which numbers are on the cards already drawn by the other participants.

The set of cards you will draw from depends on your choices. In total you will be asked 20 times to choose between two sets of cards. One of these choice situations is randomly selected. You are then matched to 2 other participants who choose the same set in the selected choice situation. Then all three of you will draw, in a randomly determined order, a card from the set you choose. Sometimes it is impossible to match everyone to two others who choose the same set. In that case in participant is randomly selected to draw from the set he or she did not choose. The chance you do

not get to draw from the set you choose is therefore very small. **Therefore you should always choose the set you prefer.**

3.7. Appendix B: Inequity aversion hypothesis

The hypothesized effect of inequity aversion can be found using the Fehr & Schmidt model. For two peers becomes the utility function of this model is:

$$x_i - \frac{1}{2}\alpha_i \left(\max(x_1 - x_i, 0) + \max(x_2 - x_i, 0) \right) - \frac{1}{2}\beta_i \left(\max(x_i - x_1, 0) + \max(x_i - x_2, 0) \right) \quad (3.9)$$

with $\alpha_i \geq \beta_i$ and $0 \leq \beta_i \leq 1$

In this formula x_i are the earnings of the decision maker and x_1 and x_2 the earnings of her two peers. α_i and β_i are the disutility caused by respectively disadvantageous and advantageous inequality. Using this utility function the expected utility, according to the Fehr and Schmidt model, of choosing a certain set in the social treatment can be computed. The expected utility of a LLH set is:⁴⁶

$$\frac{1}{3} \left(H - \beta_i (H - L) \right) + \frac{2}{3} \left(L - \frac{1}{2} \alpha_i (H - L) \right) = \frac{1}{3} H + \frac{2}{3} L - \frac{1}{3} (\alpha_i + \beta_i) (H - L) \quad (3.10)$$

By the same reasoning the expected utility of a LHH set is:

$$\frac{2}{3} \left(H - \frac{1}{2} \beta_i (H - L) \right) + \frac{1}{3} \left(L - \alpha_i (H - L) \right) = \frac{2}{3} H + \frac{1}{3} L - \frac{1}{3} (\alpha_i + \beta_i) (H - L) \quad (3.11)$$

And for a LMH set:

$$\begin{aligned} & \frac{1}{3} \left(H - \frac{1}{2} \beta_i (H - L) - \frac{1}{2} \beta_i (H - M) \right) + \frac{1}{3} \left(M - \frac{1}{2} \alpha_i (H - M) - \frac{1}{2} \beta_i (M - L) \right) + \\ & \frac{1}{3} \left(L - \frac{1}{2} \alpha_i (H - L) - \frac{1}{2} \alpha_i (M - L) \right) = \\ & \frac{1}{3} H + \frac{1}{3} M + \frac{1}{3} L - \frac{1}{6} (\alpha_i + \beta_i) (H - L) - \frac{1}{6} (\alpha_i + \beta_i) (H - M) - \frac{1}{6} (\alpha_i + \beta_i) (M - L) = \\ & \frac{1}{3} H + \frac{1}{3} M + \frac{1}{3} L - \frac{1}{3} (\alpha_i + \beta_i) (H - L) \end{aligned} \quad (3.12)$$

The last term of all these three formulas: $-\frac{1}{3}(\alpha_i + \beta_i)(H - L)$ is relevant to determine

the hypothesized treatment difference as this term is only relevant in the social

⁴⁶ The Fehr & Schmidt model assumes linear utility as a simplification. This does not allow for anything but risk neutrality when social concerns are irrelevant. This is obviously an inaccurate description of observed behavior. Risk attitudes can however easily be incorporated by a non-linear utility function of own earnings. This would not change the difference between the social and individual treatments illustrated here as there wouldn't be any difference in this regard between treatments.

Social Preferences in Private Decisions

treatment, as in the individual treatment there is no social comparison. It shows that, according to the Fehr and Schmidt model sets where H-L is large become relatively unattractive in the social treatment.

To make the Bolton and Ockenfels ERC model most comparable to the Fehr and Schmidt model we assume a utility function that is separable in terms of the individual and social component and linear in the social component with a kink at the social reference point. Advantageous inequality yields a disutility of β_i , disadvantageous inequality a disutility of α_i . Such a utility function fulfills Bolton and Ockenfels' assumptions.

The most important difference with the Fehr and Schmidt model is that agents compare their own outcome to a fair share of the pie instead of with the earnings of each referent. In our experiment a fair share is always $10 \frac{1}{3}$ because the numbers on the cards always add up to 31. This yields the following utility function:

$$x_i - \alpha_i \left(\max \left(10 \frac{1}{3} - x_i, 0 \right) \right) - \beta_i \left(\max \left(x_i - 10 \frac{1}{3}, 0 \right) \right) \quad (3.13)$$

The expected utility of a LHH set is therefore given by:

$$\frac{1}{3} \left(H - \beta_i \left(H - 10 \frac{1}{3} \right) \right) + \frac{2}{3} \left(L - \alpha_i \left(10 \frac{1}{3} - L \right) \right) \quad (3.14)$$

As the numbers in a set add up to 31 we know that $H - L = 31 - 3L = 3 \left(10 \frac{1}{3} - L \right)$ and

$$H - L = H - \frac{31 - H}{2} = \frac{1}{2} 3 \left(H - 10 \frac{1}{3} \right). \text{ Combining these equalities with function A6}$$

yields:

$$\frac{1}{3} H + \frac{2}{3} L - \frac{1}{3} \beta_i \left(\frac{2}{3} (H - L) \right) - \frac{2}{3} \alpha_i \left(\frac{1}{3} (H - L) \right) = \frac{1}{3} H + \frac{2}{3} L - \frac{2}{9} (\alpha_i + \beta_i) (H - L) \quad (3.15)$$

For the LLH set we have the following expected utility:

$$\frac{2}{3} \left(H - \beta_i \left(H - 10 \frac{1}{3} \right) \right) + \frac{1}{3} \left(L - \alpha_i \left(10 \frac{1}{3} - L \right) \right) \quad (3.16)$$

In this case $H - L = H - (31 - 2H) = 3 \left(H - 10 \frac{1}{3} \right)$ and

$$H - L = \frac{31 - L}{2} - L = \frac{1}{2} 3 \left(10 \frac{1}{3} - L \right). \text{ Combining with 3.16 yields:}$$

$$\frac{1}{3}L + \frac{2}{3}H - \frac{2}{9}(\alpha_i + \beta_i)(H - L) \quad (3.17)$$

As with the Fehr and Schmidt model the last term shows the hypothesized treatment effect. Dropping the assumption of linear social effects causes the effect of $H-L$ to be non-linear, but utility is still decreasing in $H-L$ in the social treatment.

For LMH set the utility is given by:

$$\frac{1}{3}\left(H - \beta_i\left(H - 10\frac{1}{3}\right)\right) + \frac{1}{3}\left(M - \beta_i\left(M - 10\frac{1}{3}\right)\right) + \frac{1}{3}\left(L - \alpha_i\left(10\frac{1}{3} - L\right)\right) \quad (3.18)$$

if M is bigger than $10 \frac{1}{3}$. If M is smaller it is given by:

$$\frac{1}{3}\left(H - \beta_i\left(H - 10\frac{1}{3}\right)\right) + \frac{1}{3}\left(M - \alpha_i\left(10\frac{1}{3} - M\right)\right) + \frac{1}{3}\left(L - \alpha_i\left(10\frac{1}{3} - L\right)\right) \quad (3.19)$$

From A10 and A11 the utility in the social treatment decreases if an amount is transferred from L to H . Utility is no longer linearly decreasing in $H-L$ though. For example if M is bigger than $10 \frac{1}{3}$ transferring an amount from H to M decrease $H-L$ but does not affect the disutility from inequity. However for all choice situations in our experiment it still holds that the set where $H-L$ is bigger should be relatively less attractive in the social treatment.

4. Evolution and Strategies in the Minority Game: A Multi-Round Strategy Experiment *

Should I stay or should I go now?

If I go there will be trouble

An' if I stay it will be double

So come on and let me know!

Lyrics of *Should I Stay or Should I go* The Clash, 1981

4.1. Introduction

Many problems in economics, as well as in other social sciences, center around the competition for (the use of) scarce resources. Often a market institution, or some type of other central agency, serves as a coordination device to allocate these resources, for example through competitive prices. The absence of such an institution, however, may lead to severe coordination problems that are not easy to resolve in a decentralized manner. Such problems may arise when firms need to choose whether or not to enter a new market, or decide which (geographical) market to cater to, or when companies try to be the first to invest in a new technology. Other examples are traders deciding when to buy a rising, or sell a falling stock, commuters choosing a route between their home and the workplace, workers deciding on union membership or high school graduates selecting a college program to enroll into.

*This chapter is based on Linde, Sonnemans and Tuinstra (2011)

In all of these examples, the payoffs (e.g. (future) profits, utility or travel speed) for individual agents crucially depend upon the decisions of the other agents facing exactly the same problem. Certainly in situations where the agents are (nearly) symmetric, the payoffs for successful agents are large and there is not an intuitive focal solution, one can imagine that coordination failure and instability may emerge easily.

The *minority game* provides a very stylized, but intuitively appealing way to model these problems. The minority game has an odd number of players that each simultaneously have to decide between two options, with the players making the minority choice being rewarded, and the others not. The minority game leads to very asymmetric payoffs, making the pure strategy Nash equilibria (any distribution of players across options leading to the largest possible minority) a poor prediction for behavior in the game. The symmetric mixed strategy Nash equilibrium on the other hand (with each player choosing each option with equal probability) may lead to small minorities, and hence suboptimal outcomes. These two observations suggest that the repeated minority game may lead to inherently unstable behavior.

The minority game has been extensively studied by means of a large range of simulation models where different agents use different strategies to play the game repeatedly. These simulation models typically give rise to complex adaptive multi-agent systems, with perpetual fluctuations in the aggregate choices made by agents, but a higher level of coordination on large minorities than in the symmetric mixed strategy Nash equilibrium.

A major drawback of this approach is that the strategies used in these simulation models are selected more or less subjectively by the researchers. Whether decision makers would actually use those strategies is unclear. Since the choice of strategies is a crucial determinant of the dynamic behavior of the game this is highly unsatisfactory. There have been some laboratory experiments on the minority game, but these experiments typically focus on aggregate outcomes. Moreover, due to the relative small number of periods in these experiments and the large strategy space, it is quite difficult to distill the strategies that were used by the participants.

In this paper we introduce an experiment on the repeated (five-player) minority game that employs the strategy method. After gaining some experience with the minority game in the laboratory, participants have to program a strategy. We only impose a few reasonable limitations on the set of strategies they can choose from. A

computer tournament between all submitted strategies then determines a ranking (and the participants that submitted the five highest ranked strategies receive a monetary reward). After providing students with feedback about the performance of their strategy in the computer tournament, they can revise their strategy for the next round (there are five rounds in total – each separated by a week). In this way we elicit explicit strategies from participants, which can subsequently be used for simulation studies with the minority game.

The aim of the paper is twofold. First, we will analyze and classify the strategies that have been submitted by the participants. Second, we will use all strategies submitted in the five rounds (107 unique strategies in total) to run an evolutionary competition, and see which strategies eventually remain.

We find that the strategies submitted lead to aggregate outcomes that are comparable to those under the symmetric mixed strategy Nash equilibrium. Moreover, there is no significant increase in coordination over the five rounds, and learning by participants seems to be limited. The evolutionary competition between the strategies gives four, relatively simple, strategies that remain. Surprisingly, these strategies did not perform very well in the actual experiment. Coordination is enhanced substantially through evolution.

The remainder of the paper is organized as follows. In the next section we will discuss the minority game in more detail and review the computational and experimental literature on this game. We will also discuss a number of related models. Section 4.3 discusses the design of the experiment. In Section 4.4 we analyze the strategies submitted by the participants and in Section 4.5 we use these strategies to establish which of them survives in an evolutionary competition. Section 4.6 summarizes the results.

4.2. The minority game

4.2.1. Definition and relevance

The minority game was introduced by Challet and Zhang (1997) as a stylized version of Arthur's famous *El Farol* bar game (1994). Arthur considers a population of 100 people deciding every Thursday night whether or not to visit the El Farol bar in Santa Fe. This will only be a pleasant experience if at most 60 people are there, otherwise it is too crowded and staying at home would be preferable. Arthur (1994) uses computer

simulations to analyze the interaction of 100 agents. Each agent chooses, from its own set of predictors of the number of attendants, the most accurate one up to that point in time. Aggregate bar attendance turns out to exhibit persistent fluctuations, with average attendance converging to the capacity of the bar, i.e. 60.

The minority game is a symmetric version of the El Farol bar game. There is an odd number of players N , who simultaneously have to choose one of two sides (say *Red* and *Blue*). All players that make the minority choice are rewarded with one ‘point’, the others earn nothing. More specifically, let $s_i = 1$ when player i chooses *Red* and $s_i = 0$ when player i chooses *Blue*. Payoffs for player i are then given by

$$\pi_i(s) = \begin{cases} s_i & \text{when } \sum_{j=1}^N s_j \leq \frac{N-1}{2} \\ 1-s_i & \text{when } \sum_{j=1}^N s_j \geq \frac{N+1}{2} \end{cases} . \quad (4.1)$$

Note that the minority game is one of the simplest games one can think of: there are only two actions to choose from, and only two possible payoffs. Furthermore, the game is symmetric.

The one-shot minority game has many Nash equilibria. First note that any action profile where exactly $\frac{N-1}{2}$ players choose one side constitutes a pure strategy Nash equilibrium. There are $\frac{N!}{\left(\frac{N+1}{2}\right)! \left(\frac{N-1}{2}\right)!}$ of such pure strategy Nash equilibria, which is a substantial number even for moderate values of N . These pure strategy Nash equilibria lead to a very asymmetric distribution of payoffs, with otherwise identical players receiving different payoffs. Moreover, these equilibria are not *strict*: every player in the majority is indifferent between staying in the majority and unilaterally deviating to the minority, which then would become the majority choice. There also exists a symmetric mixed strategy Nash equilibrium, where every player chooses *Red* with probability $\frac{1}{2}$. Expected payoffs in this Nash equilibrium are the same for each player, but aggregate payoffs can easily be smaller than in a pure strategy Nash equilibrium, since there is a positive probability that the minority will be strictly smaller than $(N-1)/2$. Finally, there are infinitely many asymmetric

mixed strategy Nash equilibria. Take for example the profile where $(N-1)/2$ players choose *Red* with certainty, $(N-1)/2$ players choose *Blue* with certainty and the remaining player randomizes with any probability. Note that this particular type of mixed strategy Nash equilibrium always leads to an efficient outcome.

In this paper we will be primarily interested in the (finitely) repeated minority game, played with a fixed group of players. The multiplicity of (asymmetric) pure strategy Nash equilibria in this symmetric game means that it may be difficult to coordinate on one of those. Moreover, the asymmetry in payoffs and non-strictness of the equilibria make it doubtful that a pure strategy Nash equilibrium, once obtained, will persist very long. It is costless for players in the majority to switch to the other option (although they would prefer other members of the majority to switch), and players in the minority, foreseeing this, may preemptively switch.⁴⁷ The symmetric mixed strategy Nash equilibrium has the obvious disadvantage of giving lower average payoffs than any pure strategy Nash equilibrium. It is therefore unclear what type of behavior to expect in the repeated minority game.

Since its inception the minority game has received quite a lot of attention from physicists, but initially not so much from economists.⁴⁸ There are several reasons for its popularity in physics. First, it is a simple game that allows for studying the interaction of heterogeneous agents as a complex adaptive system. Simulation methods and analytical tools from statistical physics (see e.g. Cavagna et al., 1999, Challet et al., 2000b) have been extensively applied to identify emergent macroscopic properties of these multi-agent systems. Secondly, it has been advanced as a stylized model of a financial market (see e.g. Challet et al. 2000a, 2001), and has become one of the canonical models in the field of ‘econophysics’. In that interpretation the two sides of the minority game correspond to ‘buying’ and ‘selling’ a stock, respectively. If there are more (less) buyers than sellers, the price will be high (low) and sellers (buyers) make a profit.

⁴⁷ Note that in the repeated minority game there exist pure strategy Nash equilibria where players rotate over the two options in such a way that every player spends the same number of periods in the minority. Total payoffs would then be the same for each player. However, in the absence of the possibility of communication, it seems very hard to coordinate on such an equilibrium, even if the number of players is relatively small. For a folk theorem on the infinitely repeated minority game see Renault et al. (2005).

⁴⁸ For example, a search on Web of Knowledge (<http://www.webofknowledge.com>) gives more than 200 published articles with the phrase “minority game” in the title between 1998 and 2011. About 85% of these articles have appeared in physics journals with the rest evenly spread between the fields of computer science, complex systems research and economics.

Although the interpretation of the minority game as a model of a financial market may be criticized for being too simple, the minority game is closely related to, and a stylized representation of, many important economic problems. *Congestion games* for example (see Rosenthal, 1973, for a definition and Huberman and Lukose, 1997, for an application to internet congestion) are games where players make use of limited resources and payoffs are determined by how many other players use that resource. In fact, the minority game is a very simple example of a congestion problem with two routes and N users, where each route has a capacity of exactly $(N - 1)/2$ users, and becomes fully congested when more than $(N - 1)/2$ users choose it. An early laboratory experiment on route choice can be found in Iida et al. (1992), more recent experimental evidence is presented in Selten et al. (2007). In these experiments participants have to minimize travel time by repeatedly choosing between two routes, where travel time depends positively (and more gradually than in the minority game) upon the number of users of the route. In equilibrium travel times are the same between the two routes. Both experiments show that aggregate route choices are volatile, fluctuating around the equilibrium, and that there is substantial heterogeneity in participants' behavioral rules. Selten et al. (2007), for example, show that many participants can be classified as either using a 'direct response mode', where a road change follows a bad payoff, or as using a 'contrary response mode', changing routes after a good payoff. Moreover, the number of road changes is negatively correlated with individual payoffs.

Another problem closely related to the minority game is modeled by the *market entry game*. In such a game each of a number of n firms has to decide independently and simultaneously whether to enter a (new) market or not. The payoff for entering depends upon the total number of firms entering and is typically linearly decreasing in that number, e.g. $\pi_E = k + r(c - m)$, where $c < n$ is the capacity of the market, m is the number of entering firms and k and r are positive payoff parameters. Not entering gives payoffs of $\pi_N = k$. In a (pure or symmetric mixed strategy) Nash equilibrium (in expectation) between $c - 1$ and c firms will enter and in such an equilibrium the expected payoff difference between entering and not entering will be small or zero. Coordination in these market entry games has been extensively studied by means of laboratory experiments, see e.g. Sundali et al. (1995), Erev and Rapoport (1998), Rapoport et al. (1998) and Duffy and Hopkins (2005). A robust finding from

this literature is that aggregate behavior is roughly consistent with Nash equilibrium, but that a large variation in strategies can be observed at the individual level, with some subjects always entering, others never entering and yet other subjects conditioning their behavior on the outcome in previous rounds.

Note that the El Farol bar game is in fact a special case of a market entry game, with a payoff function that does not linearly decrease in m but is a step function with a discontinuity exactly when $m = c$. The payoff function of the El Farol bar game is flat everywhere else. Related to this, Zwick and Rapoport (2000) study a market entry experiment where a fixed prize is equally shared between the entrants, provided there is no over-entry. Entrants have to pay an entry fee, and in case of over-entry their payoffs will be negative. Not entering gives a payoff of zero. Just as in the El Farol bar game this market entry game has a discontinuity at $m = c$ and is flat for $m > c$ (payoffs do depend upon m when $m \leq c$, however). In this case the Nash equilibrium does not give a good description of aggregate behavior and, as was the case in the earlier experiments, there is substantial heterogeneity on the individual level.

An important difference between the market entry / El Farol bar games on the one hand and minority / congestion games on the other is that in the former there is always the safe option of not entering, whereas in the latter all alternatives are subject to strategic uncertainty with payoffs of every choice depending on the decisions made by the other agents.⁴⁹ In two earlier papers on coordination games (Ochs, 1990, and Meyer et al., 1992) a safe option was also absent. In these games players have to choose between different markets at which limited resources are available. For laboratory experiments with these coordination games adaptive strategies seem to give a better description of individual behavior than mixed strategy Nash equilibria. Moreover, whenever participants succeed in coordinating on a pure strategy Nash equilibrium, this coordination turns out to be fragile and is typically not maintained in the subsequent periods.

On a more general level the minority game is an abstract version of games where actions are strategic substitutes. Well-known examples of such games are

⁴⁹ Another difference is that the (pure strategy) Nash equilibrium is Pareto efficient in minority and most congestion games, but not in the market entry game. Total payoffs in a market entry game increase when the number of entrants decreases below capacity. The non-entrants then achieve the same payoffs as they do in equilibrium whereas the payoffs of the entrants are higher. However, this type of tacit collusion is typically not observed in market entry experiments, possibly because these experiments often involve rather large groups of about 12 to 20 participants.

cobweb markets (Ezekiel, 1938) and Cournot oligopolies. For example, if most producers in a cobweb market predict next period's price to be higher than the rational expectations (RE) equilibrium price and therefore produce more than the RE equilibrium quantity, the actual market clearing price will be lower than the RE equilibrium price. In such a cobweb market it is therefore better to disagree with the majority prediction. A similar argument holds for Cournot oligopolies. If the other firms on average have high production levels, it is optimal to supply a limited amount, and the other way around.

The minority game is therefore a relevant, although stylized, model for a number of important economic problems. What sets it apart from most other coordination problems is that the pure strategy Nash equilibria lead to Pareto efficient but very asymmetric payoff distributions whereas the symmetric mixed strategy Nash equilibrium equalizes expected payoffs between all players, but those are lower than average payoffs in a pure strategy Nash equilibrium. One might therefore expect less stable outcomes in minority games than in market entry or route choice problems. In the next subsection we review some of the computational and experimental literature that has been done in the past decade on the minority game and that corroborates this conjecture.

4.2.2. Strategies in the minority game: computational and experimental research

In the physics literature the minority game is studied by using computer simulations. In these simulations (see e.g. Challet and Zhang, 1997, 1998) the number of agents is large (typically between $N = 101$ and $N = 1001$) and every player has a fixed set of S strategies (typically $S = 2$, but sometimes higher values of S are used), randomly drawn from the set of all strategies with memory M (typically smaller than 10). Such a strategy maps the history of the past M winning sides into a prediction of the next winning side. The number of different histories is therefore equal to 2^M and since any history can be mapped into one of two sides, the total number of different strategies is 2^{2^M} , a number that increases fast with M (e.g. for $M = 5$ the total number of strategies is already about 4.3×10^9). Note that these strategies do not use information

about the *size* of the minority and that they do not allow for randomization.⁵⁰ Agents collect how well the strategies in their set predict the winning side (but do not consider the effect that a strategy they did not use might have had on the outcome) and in every period choose side according to that strategy, from their set, that is the best predictor up to that period.⁵¹ Numerical simulations show that the number of agents choosing one side fluctuates around 50%. The higher the volatility of fluctuations (implying that small minorities occur more often) the less efficient is the outcome. One of the most celebrated results on the minority game is that of the dependence of ‘cooperation’ on the parameter $\rho = 2^M / N$, for the first time identified by Savit et al. (1999). For small values of ρ , where the number of agents is relatively large compared to the number of possible histories, aggregate behavior in the minority game is dominated by a cycle of period 2 and volatility is higher than under the symmetric mixed strategy Nash equilibrium. However, for moderate values of ρ , volatility drops below that of the symmetric mixed strategy Nash equilibrium, reaching a minimum value at some critical level $\rho = \rho^c$, and increasing towards volatility under the symmetric mixed strategy Nash equilibrium again, when ρ increases beyond that critical value.⁵²

In the typical analysis of the minority game, as discussed above, each agent has a set of strategies from which it chooses one every period. The set of strategies of an individual agent is fixed and randomly drawn from the total set of strategies.⁵³ It therefore contains arbitrary strategies, that may lack any rationale, but nevertheless the agent will hang on to these strategies forever. A number of models have been

⁵⁰ An exception is Johnson et al. (1999) (see discussion below). Cavagna et al. (1999) develop a continuous version of the minority game where instead of making a binary choice each agent submits a ‘bid’ that may lie somewhere in between the two extremes.

⁵¹ Alternatively, in some papers the choice of strategy is assumed to be probabilistic with the probability that a strategy is chosen positively related to its success, for example through a logit specification (see e.g. Cavagna et al., 1999, and Challet et al., 2000b).

⁵² Cavagna (1999) shows numerically that this result is maintained if the actual history is replaced with a ‘fake’ history: what is important for cooperation is that agents react to the *same* information, whether accurate or not. Ho et al. (2005), on the other hand, provide numerical evidence that for large scale minority games with about 3000 agents knowledge of the true history is relevant.

⁵³ In some contributions an agent’s strategies are not drawn independently from the set of all strategies. Challet et al. (2000b), for example, assume that an agent’s second strategy is always chosen such that it is exactly opposite to its first strategy. Yip et al. (2003) consider strategies that are slightly biased to one alternative and show that this improves efficiency. Finally, Wang et al. (2009) consider a minority game with ‘heterogeneous preferences’, meaning that there are agents of different types K , with $K=0,1,\dots,2^M$, where an agent of type K takes the first side for exactly $K/2^M$ (randomly determined) histories.

advanced in which the set of strategies used evolves over time under evolutionary pressure. Li et al. (2000a), for example, consider an evolutionary model where after each generation (consisting of 10.000 periods) all agents are ranked according to their performance in that generation. Of the lowest ranked 20% (or 10% or 40%) of the agents, 50% is randomly selected and replaced with new agents, each endowed with a new and random set of strategies (with the same memory length M). It turns out that this improves efficiency considerably – volatility is now always lower than under the symmetric mixed strategy Nash equilibrium, even for low values of M – although volatility is still a non-monotonic function of ρ . Li et al. (2000b) extend this model by assuming that the strategies of the new agents may have a memory length M that differs from that of the agents they replace. They find that agents with memories below a certain threshold \bar{M} perform very well, whereas agents with longer memories are eventually driven out through evolutionary competition. This result is in sharp contrast to some early evolutionary simulations in Challet and Zhang (1997, 1998), who also establish that evolution substantially increases efficiency, but who show that the average memory length increases through evolution.⁵⁴ Sysi-Aho et al. (2005) use a genetic algorithm to update the strategy set for the poorest performing agents of each generation through crossover between two of the original strategies. They find that fluctuations decrease substantially in such a setting and almost maximal efficiency may be obtained.

Johnson et al. (1999) take a different approach to study the evolution of agent strategies in the minority game. Each agent collects, for each sequence of winning sides, the outcome that prevailed the last time that sequence obtained. An agent then chooses with probability p (which differs between agents) the same outcome as the last time the sequence appeared and with probability $1 - p$ the other outcome. If the agents aggregate score falls below a certain threshold he changes his strategy by drawing a new probability p from an interval around his old value. Simulations with this model show that segregation emerges: the distribution of the values of p within the population becomes bimodal with peaks at 0 and 1, implying that many agents

⁵⁴ The evolutionary model of Challet and Zhang (1997, 1998) is slightly different, however. After each generation the worst ranked agent is replaced by a clone of the best ranked player, with an additional mutation process where, with some small probability, one of the strategies of the clone is replaced by a random other strategy (in absence of this mutation process efficiency will decrease dramatically through evolution).

always replicate the outcome stored in their memory and many agents always do exactly the opposite.

Clearly, aggregate behavior in minority games crucially depends upon the strategies agents are assumed to use and how these strategies evolve over time. Results on a number of recent laboratory experiments on the minority game may help in understanding which strategies would actually be played by humans. Bottazzi and Devetag (2003, 2007), for example, consider groups of five players playing the minority game for 100 periods, varying the memory length and information provided to the participants, and find that, although aggregate choices are volatile, (allocative) efficiency is higher than in the symmetric mixed strategy Nash equilibrium.⁵⁵ Increasing the memory length has a slightly positive effect on efficiency, but providing additional information does not.⁵⁶ Participants seem to repeat their choices, particularly when they have more information and after a win. Moreover, participants seem to revert to pure strategies towards the end of the experiment.

The failure of the symmetric mixed strategy Nash equilibrium to describe human behavior in the minority game is confirmed in Devetag et al. (2011). They consider a three-player minority game experiment where each player is represented by a team of three participants. Teams are video recorded and their discussion is analyzed to learn about strategies used for playing the minority game. Again, allocative efficiency is higher than in the symmetric mixed strategy Nash equilibrium (but not higher than with individuals instead of teams). Analysis of the video recordings reveals that teams rarely use a randomization strategy and that they tend to focus more on their own past behavior than on other teams over time, in particular when they have been successful.

For most of the experiments on the minority game discussed above it is impossible to determine whether participants randomize or not. Chmura et al. (2010) study a three-player minority game experiment where participants can explicitly use mixed strategies. Moreover, there is random re-matching of groups after each period, which makes the mixed strategy Nash equilibrium a more obvious candidate for

⁵⁵ This is consistent with the findings of Platkowski and Ramsza (2003) who run an experiment in which a group of fifteen students plays the minority game for 200 consecutive periods.

⁵⁶ On the other hand, Chmura and Pitz (2006) show, in a minority game experiment with groups of nine players, a positive effect of adding information about the distribution of individual choices upon efficiency. They also establish a negative correlation between the number of changes of a subject with its cumulative payoff. This negative correlation was also established in simulations with the minority game by Challet and Zhang (1997).

individual behavior. They find that there is considerable heterogeneity in decision rules, and the behavior of only about a quarter of the participants is best described by the symmetric mixed strategy Nash equilibrium.

Other laboratory experiments on the minority game are Wang et al. (2009), who show that a model of fixed rule learning with heterogeneous preferences gives a better description of their experimental minority game data than the standard fixed rule learning model that is typically used in the minority game literature, and Liu et al. (2010) who show that the behavior of fish in a minority game experiment is remarkably similar to that of university staff members: as experience of the subjects increases the variance of decisions first decreases and after a certain point increases again. Finally, Laureti et al. (2004) discuss an interactive web-based experiment where one human agent plays the 95-player minority game against computer agents (with different degrees of sophistication). Humans perform better than computer agents in environments where the latter have a memory up to 4 periods, but more sophisticated computer agents typically outperform the human players.

Although laboratory experiments can shed some light on the type of strategies that humans employ for playing the minority game, it is still difficult to infer exactly the strategies being used, and therefore to draw conclusions on what type of behavior is relevant for minority games. A strategy experiment, where participants have to submit strategies to play the repeated minority game therefore seems appropriate. The strategy method has been applied before to related games, such as cobweb markets (Sonnemans et al., 2004), predictions in asset markets (Hommes et al., 2005), market entry games (Seale and Rapoport, 2000) and the El Farol bar game (Leady, 2000). Also the famous strategy tournament on the repeated prisoner's dilemma in Axelrod (1984) is related to our work. In a recent paper Brandts and Charness (2011) provide an overview of experiments that directly compare the strategy method with the 'direct response' method. They find that in most studies these methods yield qualitatively similar results.

In this paper we use the strategy method to elicit explicitly the strategies used by human players of the minority game. We will analyze those strategies and use them to study evolutionary competition, in order to understand the type of strategies and behavior that is relevant for minority games.

4.3. Design

We designed an experiment in which participants have to submit a strategy to play the five-player minority game for 100 periods. The experiment consists of five rounds, each separated by a week, and took place in April 2009. Participants are students of the so-called “beta-gamma” bachelor program, which is one of the most challenging programs of the University of Amsterdam. These students follow courses in the natural sciences as well as the social sciences and they are typically well above average in motivation and capabilities. In particular, their programming experience is substantially higher than that of the average undergraduate student at the University of Amsterdam.

The first stage of the experiment takes place in the CREED laboratory of experimental economics at the University of Amsterdam. The minority game is explained to the participants and they play the minority game two times for 10 periods in two different groups of players. After getting acquainted with the minority game in that way, participants are explained – on a handout and via the computer screen – how to formulate a strategy. Their understanding of formulating strategies is checked by letting them program two verbal strategies using the interface, after which they formulate, test and submit their first strategy.⁵⁷ After a few days all participants receive, by email, the results of the first round. From then on they can login on the website and try out new strategies against the population of strategies of the previous round. Within a week after the laboratory experiment they have to submit their new strategy (which could be identical to their old one) and fill in a short questionnaire. Two days after the deadline they receive the results of the second round. The whole procedure is repeated for another three times. A week after the fifth and final round we explain the goals of the experiment in class, announce the results of the final round and pay out the earnings.

4.3.1. Formulating strategies

Figure 4.1 shows the computer screen where the participants can formulate their strategy.⁵⁸ A strategy has the form of a list of IF-statements that (if the condition is

⁵⁷ The participants can ask the experimenters for further instructions during the initial laboratory experiment. For the later rounds the experimenters were available for assistance via e-mail, although participants made no use of this possibility.

⁵⁸ The experiment is programmed in php/mysql and runs on a (Apache) web server. An English translation of the experiment can be found on www.creedexperiment.nl/minor/english and the

met) returns a number in the interval $[0, 1]$, which is the *probability of changing color*. If a condition in an IF-statement is fulfilled, the subsequent IF-statements are ignored (the second and following IF-statements are treated like ELSE IF statements). If none of the conditions are met, the strategy returns 0 (i.e. no change of color). The number of IF-statements is unlimited and strategies can use logical expressions such as AND, OR, (in)equality and negation. In the instructions ample examples were given; see Appendix 1. The strategies can use the history of the last 5 periods, which consists of the outcome in each of these previous periods (i.e. the size of the group) and whether the strategy changed colors in that period or not.

Strategy

IF statement AND OR () negation ! == > < + - clear

	Changed	Number of players with your color				
		Win		Lose		
Previous period:	SC[1]	SWin[1]		\$Lose[1]		
		SW1[1]	SW2[1]	\$L3[1]	\$L4[1]	\$L5[1]
2 periods ago:	SC[2]	SWin[2]		\$Lose[2]		
		SW1[2]	SW2[2]	\$L3[2]	\$L4[2]	\$L5[2]
3 periods ago:	SC[3]	SWin[3]		\$Lose[3]		
		SW1[3]	SW2[3]	\$L3[3]	\$L4[3]	\$L5[3]
4 periods ago:	SC[4]	SWin[4]		\$Lose[4]		
		SW1[4]	SW2[4]	\$L3[4]	\$L4[4]	\$L5[4]
5 periods ago:	SC[5]	SWin[5]		\$Lose[5]		
		SW1[5]	SW2[5]	\$L3[5]	\$L4[5]	\$L5[5]

Lost last period with 5 players with my color

```

IF (condition) {
  RETURN number ;
}
ELSE {RETURN 0;}(This is always added: when none of the conditions is met you will not change color)

```

to simulation

Figure 4.1: Computer screen as seen by the participants when they formulate a strategy.

interested reader is invited to formulate a strategy and run simulations with that strategy against actual strategies of our participants.

We have restricted the strategy space in two ways. First, note that the minority game is a symmetric game where the labels of the two sides (red and blue) have no intrinsic meaning. We therefore impose symmetry in our design by letting strategies decide on changing color instead of choosing a color. Although individuals may have a preference for one of the colors, for example preferring winning when choosing blue over winning when choosing red, this limitation seems reasonable: using colors directly would double the number of variables per period.

Another restriction is the length of the history. We took this to be equal to five periods, which we believe gives a sufficient amount of flexibility for participants to develop strategies.⁵⁹ The information about the last five periods is complete and contains whether the strategy made the winning decision, what the distribution of choices is and whether the strategy changed colors in that period (the total number of possible histories is therefore $5^5 \times 2^5 = 100,000$). Note that in the simulation studies of the minority game the information used by strategies only contains the winning sides of the last M periods, but not the distribution of choices. On the other hand, in laboratory experiments on the minority game (e.g. Bottazzi and Devetag, 2007) the full distribution of choices is given in some treatments. Also note that in the bulk of the simulation studies randomization is not allowed, and in almost no laboratory experiments it is observed explicitly (an exception is Chmura et al., 2011). In our design participants can explicitly submit mixed strategies, and in fact about 75% of all submitted strategies make use of randomization.

4.3.2. Simulations by participants

A novel feature of our design, compared to other strategy method experiments, is that participants can run simulations with a strategy of their own making. Simulations are ran with four randomly drawn strategies (without replacement) from other participants from the previous round.⁶⁰ Since strategies can use a history of up to 5 periods, first 5 random outcomes are drawn. After that, 100 periods are played according to the five

⁵⁹ No more than 17% of all strategies submitted in our experiment uses information from 4 periods ago, and only 13% of the strategies uses information from 5 periods ago, whereas more than 90% uses information of the previous period, about two thirds of the strategies use information from two periods ago and about half of the strategies uses information from three periods ago. Limiting the history to five periods therefore seems to be relatively innocuous.

⁶⁰ In the first round no strategies from participants are available. The participants are informed that the strategies they compete against in the simulations they run in the first round are pre-programmed and are not necessarily similar to the strategies the other participants will submit. There are eleven pre-programmed strategies that do not condition on the history of outcomes and change with probability q , where $q = 0, 0.1, 0.2, \dots, 0.9, 1$, respectively.

strategies. After each simulation the results of the 100 periods, as well as those of the first five random periods are presented (see Appendix 1). In the presentation the choices of the other four strategies are sorted in each period (first the red and then the blue choices) making it close to impossible to infer what the other strategies in the simulation are. In addition summary statistics are displayed: the total number of points and the number of times the outcome was in category W1, W2, L3 L4 and L5, respectively, where W1 (W2) represents winning in a group of 1 (2) and L3 (L4, L5) represents losing in a group of 3 (4, 5).

Participants can run as many simulations and try as many strategies as they want. They can use these simulations to see how successful their strategy is, but also to check whether their strategy behaves as they intended it to. Our approach therefore gives ample opportunities for participants to gain experience with the game and to learn how to play it.

4.3.3. Computer tournament, feedback and earnings

After the deadline a computer tournament with all submitted strategies is run as follows. For every possible combination of five strategies a simulation of 100 periods is done (after five initial randomly selected outcomes), implying that the total number of simulations with about 40 strategies is around 10^8 . Subsequently we determine for each strategy the average number of points it earned in all the simulations it was involved in and use this to rank the strategies, where the first five random outcomes in each simulation are not used in determining the average number of points. After each round, all participants receive an email with a ranking of the strategies and the average number of points earned by each strategy in the simulations.⁶¹ In addition they learn their earnings for that round. In the first four rounds the top five strategies receive 75, 60, 45, 30 and 15 euro, respectively. In the fifth and final round these amounts are doubled (and therefore are 150, 120, 90, 60 and 30 euro). In addition in each round every participant who submits a strategy and fills out a short questionnaire receives 5 euro. One of the questions in the questionnaire is about how confident the participant is about the success of his/her strategy. To have an incentivized check on this question the opportunity is presented to wager the 5 euro. If this option is chosen for a certain round, an extra reward is given when the strategy ends up in the top 5 of

⁶¹ Strategies are identified by the nicknames of participants. It was not possible to observe the strategies used by other participants.

75, 60, 45, 30 and 15 euro, respectively, in that round. Note that with 45 participants the expected value of this option is 5 euro and (risk neutral) participants that expect to have a strategy of above average quality should use this option. Average earnings for the whole experiment were 58.70 euro per participant, ranging from a minimum of 0 euro to a maximum of 260 euro.

4.3.4. Questionnaire and web-server data.

In the laboratory session we administered a short questionnaire about the background of the participants (age, gender, programming experience, etc). After submitting the strategy in each round a few questions about the (formulation of the) strategy are asked: how difficult it was to formulate the strategy, whether they had any problems with the formulation and how confident they are that the strategy will be successful. Finally, the incentivized question about confidence described above was asked. Besides the submitted strategy and the answers to the questionnaire, the web server also saves all actions of the participants: when and how often they log in; which strategies they try; the results of the simulations they run.

4.4. Results from the multi-round strategy experiment

In this section we describe the most important results from the multi-round strategy experiment. In the first subsection we present some results on the aggregate outcomes of the simulations and performance of the individual participants. In the second subsection we will take a closer look at individual strategies, and categorize them by means of cluster analysis.

4.4.1. Aggregate outcomes and performance of participants

For the repeated five-player minority game that we are considering the best outcome is one where the minority consists of two players in every period. This happens in any pure strategy Nash equilibrium (PSNE). However, in the symmetric mixed strategy Nash equilibrium (MSNE), inefficiencies do occur since randomization implies that, with a positive probability, the minority will be smaller than two. In fact, it can be easily checked that in the symmetric MSNE the probability that a minority of two results is 62.50%, whereas the probability of obtaining a minority of 1 (0) is 31.25% (6.25%).

Round	1	2	3	4	5	Symmetric MSNE	PSNE
Number of participants	42	36	34	36	32		
Outcome							
3-2	63.90%	61.66%	64.57%	66.07%	64.32%	62.50%	100%
4-1	30.65%	32.25%	30.26%	29.33%	30.59%	31.25%	0%
5-0	5.45%	6.08%	5.17%	4.60%	5.09%	6.25%	0%
Points							
Average	31.69	31.12	31.88	32.29	31.85	31.25	40
Standard Dev.	1.49	5.17	3.10	3.20	6.36		
Minimum	29.31	21.65	24.98	28.00	18.93		
Maximum	34.68	41.65	36.96	39.87	43.06		
Average change propensity	47.61	38.15	45.99	38.51	40.29		
Pearson correlation change and points	-0.531	0.028	-0.690	-0.493	-0.878		

Table 4.1: Distribution of outcomes and performance of participants over the rounds

As explained above, in each round of the strategy experiment we run a simulation of 100 periods for each possible combination of five submitted strategies. The first round started with 42 participants submitting a strategy; in the subsequent rounds the number of submitted strategies was between 32 and 36.⁶² Rows 3 – 5 of table 4.1 show the distribution of minorities resulting from the simulations with these submitted strategies. These distributions are very similar to those one under the symmetric mixed strategy Nash equilibrium (seventh column of table 4.1). Coordination of the strategies on larger minorities is slightly better than under the symmetric MSNE in most rounds – with the highest level of coordination obtained in round 4 – but slightly worse in round 2. Clearly, coordination in any round is far from that obtained in a pure strategy Nash equilibrium (last column of table 4.1).

It might be argued that the level of coordination is not the appropriate measure to look at, since payments for participants are based upon the relative ranking of the strategies they submit and not on the absolute number of points these strategies generate. However, there is an incentive for participants to maximize their number of points. Strategies that bear a cost in terms of points in order to do relatively well in one particular simulation by making the situation worse for the other four strategies in

⁶² Participants that missed a round were allowed to submit in later rounds. Dropping out in a round appears not to be related to success in the previous round: only participating in round 4 is significantly negatively correlated to the rank in round 3 (Wilcoxon rank-sum test with p-value of 0.0543).

that simulation, will hurt their performance relative to the 30 to 35 strategies that are not present in that simulation.

Table 4.1 also presents some results on the performance of participants and the strategies they submitted. The average number of points for the strategies varies from 31.12 and 32.29 between the rounds, which is very close to individual performance in the symmetric MSNE, for which the average number of points is 31.25 in 100 periods (note that the average number of points in a PSNE is 40). In fact, the proportion of strategies with an average number of points larger than 31.25 in round 1 to 5 is given by 23/42, 15/36, 17/34, 19/36 and 17/32, respectively, which is not significantly different from 50% (sign test). The symmetric MSNE therefore does well as a description of aggregate outcomes. It performs poorly at the individual level, however. The dispersion between payoffs generated by strategies, as measured either by the standard deviation of points, or by the range between the minimum and maximum number of points, is considerable, certainly when taking into account that each strategy was involved in more than 30,000 simulations of 100 periods.⁶³ These differences between the rules are therefore structural and there is substantial heterogeneity between the strategies in terms of their performance. This is consistent with the experimental literature on e.g. market entry problems reviewed in Section 2 which features substantial heterogeneity on the individual level, whereas the Nash equilibrium gives a good description of the aggregate outcome. Dispersion is much higher in rounds 2 to 5 than in the first round, suggesting that the strategies are more homogeneous in the first round and that heterogeneity increases after that. Moreover, the fact that the best strategies in round 2 and 5 earn more points than they would in a pure strategy Nash equilibrium suggests that these strategies (and possibly other strategies as well) are able to exploit some of the other strategies.

Strategies may be heterogeneous along different dimensions. For example, the strategies vary from very short (“never change”) to very long.⁶⁴ They also differ

⁶³ Although we simulated all possible combinations of strategies there is still some randomness in the average number of points, due to the first five random periods in each simulation and because strategies may be randomizing themselves. To check whether this randomness has an impact on the outcome we ran all simulations once more. This gives almost identical results: the correlation between the ranks in the two simulations turns out to lie between 0.998 and 1.

⁶⁴ One participant (participant 34) handed in strategies between 35 and 246 IF-statements in rounds 2 to 5. This participant ran about 5000 simulations with a 50%-change strategy and determined, by means of a computer program, for each possible history what the optimal (non-random) response would be. Note that this strategy, although quite creative, responds to strategies from the previous round, not taking into account that those will change as well. This procedure has given rise to one

substantially in how often they change colors. Some strategies never change colors, others change in about 95% of the periods. In all rounds but round 2 performance is negatively related to the propensity to change (Spearman rank correlation p-values smaller than 0.01).⁶⁵ We can see no consistent decrease in this tendency over the rounds, which suggests that participants do not learn that their strategies change too often and adapt them accordingly.

Improving a strategy on the basis of simulations against strategies from the previous round is not straightforward, since the other participants change their strategies as well. The adapted strategy may not perform as well in the new environment of strategies as expected. We consider two criteria that measure whether participants are successful in improving their strategies over the rounds. First, one would expect that the adapted strategy performs better than the old strategy when playing against strategies from the previous period, since the strategy can be tested (without limits) in that environment. Second, the new strategy should do better than the old strategy in the new environment. If it does not, the participant had better refrained from adapting the old strategy. Table 4.2 shows the results: over all rounds the new strategy is an improvement over the old strategy for the old environment in only slightly less than half of the cases. Only for round 4 a clear majority of strategies correspond to improvements over their old versions, when considering the old environment. The comparison of the old and new strategies in the new environment is more promising; but still in about only 60% of the cases the new strategy does better than the old one would have done, with round 4 giving the best results again.

<i>Round</i>	<i>The new strategy would have done better than the old strategy in the old environment</i>	<i>The new strategy does better than the old strategy would have done in the new environment</i>
2	38.89%	61.12%
3	53.57%	44.11%
4	73.33%	88.88%
5	23.33%	46.87%
Total	46.77%	60.87%

Table 4.2: The performance of the old and the new strategies in the old (column 2) and the new (column 3) environment

successful strategy (the winner in round 3), but not in the other rounds partly because the strategies are untidy and prone to mistakes (the strategy for round 2, for example, contained a mistake and never changed).

⁶⁵ This negative correlation between performance and the propensity to change is consistent with the results from computational models and laboratory experiments on the minority game (see Challet and Zhang, 1997, and Chmura and Pitz, 2006, respectively).

Besides the actual submitted strategies the experiment generates a wealth of web-server data, which may shed some light on how people tried to learn. On average participants tried out eight different strategies per round, and ran about 150 simulations with those strategies. More strategies are being tried in the first two rounds and the average number of strategies tried stabilized at around five strategies per participant in each of the last three rounds. We also considered the login behavior of participants in rounds 2-5: on average they logged in between 1 and 2 times in each of those rounds, and were logged in almost two hours in total in those rounds. The effects on the rank of the participant are ambiguous, however. Both the number of strategies used and the number of simulations ran have a positive effect upon the rank of the participant in round 4 only (i.e. higher rank, Spearman rank correlation p-values of 0.0052 and 0.0021, respectively).⁶⁶ Remarkably, the number of strategies used has a *negative* effect on performance in round 2 (Spearman rank correlation p-value of 0.0105). In none of the other rounds there is a significant effect. Also the number of times logged in has no significant effect in any of the rounds, the time logged in only has a *negative* effect on performance in round 2 again (Spearman rank correlation p-value of 0.0111). These results, combined with those from table 4.2, suggest that participants were on average not able to use the possibilities of the minority game website to improve their strategies substantially. On the other hand, some participants did succeed in improving their strategy, as can be seen from the tendency of the best strategies to generate more points over the rounds (see table 4,1).

An analysis of the decisions of participants to forego the fee of five euro in exchange for higher prizes suggests that participants were also unable to accurately predict the performance of their strategy. Participants choose higher prizes over the fee of five euro for about one third of all 180 submitted strategies. This decision was – as to be expected – positively correlated with the answer to the question from the questionnaire how confident the participant was about his strategy in rounds 2, 3, 4 and 5 (Mann-Whitney test p-values of 0.0016, 0.0110, 0.0013 and 0.0017), but remarkably only significantly positively correlated to the performance of the participant in round 4 (Mann-Whitney test p-value of 0.0088).

⁶⁶ We also considered, in the spirit of the analysis presented in table 4.2, the following. Let $S_i(R, T)$ be the score of the strategy of participant i in round R , when playing against the strategies the other participants submitted in round T . We checked whether the differences $S_i(R, R-1) - S_i(R-1, R-1)$ and $S_i(R, R) - S_i(R-1, R)$ are correlated with the number of simulations participants ran in that round. This is only (positively) significant for $S_i(3, 2) - S_i(2, 2)$, (p-value of 0.0059) suggesting that only in round 3 participants, on average, were able to improve their strategies by running more simulations.

Round	Prize	Strategy	Eq. Strat.	Cluster	Evolution
1	1	1-10		5	44.2
	2	1-40		4	13.4
	3	1-31		5	16.8
	4	1-26		5	29.6
	5	1-47		6	500
2	1	2-28	1-28	1	10.4
	2	2-29		1	10.8
	3	2-5	1-5	1	10.8
	4	2-4		5	54.8
	5	2-10	1-10	5	44.2
3	1	3-34		N.A.	N.A.
	2	3-10		5	68.6
	3	3-4	2-4	5	54.8
	4	3-11		5	59.4
	5	3-7	1-46	4	11.2
4	1	4-11	3-11	5	59.4
	2	4-35		5	28.8
	3	4-41		5	17.4
	4	4-4	2-4	5	54.8
	5	4-31		5	18.0
5	1	5-46	1-46	4	11.2
	2	5-38		4	12.2
	3	5-4	2-4	5	54.8
	4	5-31		5	24.4
	5	5-11	3-11	5	59.4

Table 4.3: The winners of the five rounds.

Strategy x-y is the strategy that participant y submitted in round x. The fourth column shows whether the strategy was already submitted in an earlier round (by the same or another participant). The fifth column gives the cluster in which the strategy is classified (see discussion below) and the sixth column gives the number of generations the strategy survives in an evolutionary competition between strategies (averaged over five evolutionary simulations) (see the discussion in the next section).

4.4.2. Classification of strategies

In this section we will study the strategies submitted in the experiment in a bit more detail. Over all the rounds participants submitted 180 strategies. However, only 110 out of those strategies are unique. This is because sometimes participants use the same strategies in two or more different rounds, and sometimes two different participants use the same strategy. Both instances are illustrated in table 4.3, which gives the five winners for each of the five rounds. The fourth column shows that each round contains at least one winner that was first submitted in an earlier period, by the same or a different participant. What is noteworthy that several strategies win a prize in different rounds, e.g. strategies 2-4 (winning in all four rounds in which it participated)

and 3-11 (winning in all three rounds in which it participated). Also, strategy 1-10 – together with its slight variation 3-10 – wins a prize in three rounds.

Obviously, the three participants submitting those strategies perform quite well in the experiment, participant 4 earned 260 euro, participant 10 245 euro and participant 11 235 euro. The only other participant who earned more than 200 euro is participant 46, (245 euro, he/she opted for the additional prize money in the final round, which already has higher stakes, and received the first prize round). All other participants earned 170 euro or less.

One could therefore argue that strategies 2-4, 3-11 and 1-10 (3-10) are the three most successful strategies submitted during the experiment. Table 4.4 gives a description of these strategies. Strategy 2-4 specifies to change only with a positive probability (and under some additional conditions) if the strategy is on the losing side for two consecutive periods. Strategy 3-11 presents a more extreme version of the same principle: it specifies to change (with certainty) only if the strategy lost in four consecutive periods. Finally, strategies 1-10 and 3-10 again tend to change when they lost for two consecutive periods. Moreover, these two strategies have two additional features, not shared with strategies 2-4 and 3-11, namely that they change with probability 0.5 when they lost in a group of five in the previous period, and they also change with a substantial probability (0.75 and 0.5, respectively) when they *won* (in a group of 2) in the previous period, and lost two periods ago in a group of 4. In general, all of these successful strategies are reluctant to change, but also make sure that they will not get stuck in a losing situation forever.

We tried to categorize all submitted strategies in different clusters. Of the 110 unique strategies we exclude the three very long computer generated strategies from participant 34 for the analysis in this and the next section (see footnote 18 for a discussion of those strategies). The main reason for this is that these are not the type of strategies that would typically be used by human decision makers and they are difficult to interpret (another minor practical reason is that these strategies increase computation time considerably).

2-4	If you changed two periods ago and lost two periods ago and you did not change in the previous period and lost in the previous period, change with probability 1 Else if you lost in the last two periods in a group of three and you lost three periods ago, change with probability 0.8 Else if you lost in a group of 4 in the previous period and lost two periods ago but did not change two periods ago, change with probability 0.6
3-11	Only change (with probability 1) when you lost in each of the last four periods
1-10 (3-10)	If you lost in a group of 3 in the last two periods change with probability 0.5 Else if you lost in a group of 4 in the last two periods, change with probability 1 Else if you lost in a group of five in the previous period, change with probability 0.5 Else if you lost in a group of 4 two periods ago and won in a group of 2 in the previous period, change with probability 0.75 (0.5)

Table 4.4: Description of strategies 2-4, 3-11 and 1-10 (3-10).

Note that strategy 3-10 only varies from strategy 1-10 (both submitted by participant 10) in the probability of change in the last IF-statement (0.5 instead of 0.75).

We performed a cluster-analysis with the remaining 107 unique strategies. Figure 4.2 shows the resulting dendrogram. For this analysis we constructed a matrix of distances between strategies, calculated as follows. Strategies can use the history of the last 5 periods (outcome and whether they had changed colors in that period), which gives 100,000 possible histories. For every strategy the decision (probability of change) is calculated for each possible history. The distance between two strategies is then defined as the weighted average absolute difference between these probabilities.⁶⁷ Because not all histories are equally likely (a 5-0 outcome is less likely than a 3-2 outcome) the weights are based upon the distribution that would results from the symmetric mixed strategy Nash equilibrium.⁶⁸ We used the program multidendrograms⁶⁹ to draw the dendrograms, using the algorithm “joint between within” which both tries to minimize the distances within clusters and maximize the distances between clusters (Székely and Rizzo, 2005)

We find six clusters, labeled 1/6, 2/6 etc in figure 4.2, and on a higher level three clusters, labeled 1/3, 2/3 and 3/3. table 4.5 displays for each cluster the most central strategy, that is, the strategy with the minimum average distance to the other

⁶⁷ This is a continuous version of the Hamming distance.

⁶⁸ As discussed in Section 4.1 the symmetric MSNE leads to the outcomes 5-0 in 6.25%, 4-1 in 31.25% and 3-2 in 62.5% of the periods. This is very close to the numbers in the simulation discussed below, which are 5.63%, 30.87% and 63.50%, respectively.

⁶⁹ <http://deim.urv.cat/~sgomez/multidendrograms.php>, see also Fernández and Gómez (2008).

strategies in that cluster. The most central strategies (CS) give some impression of the kind of strategies in that cluster. CS1/6 and CS2/6 change often, the first typically after a losing period, for the second it is independent of the history of outcomes. The central strategy from cluster 3, CS3/6, is the one shot symmetric mixed strategy Nash equilibrium strategy (and therefore is also independent of the history of outcomes). CS4/6 seems to be a bit peculiar since it changes color after winning. However, this strategy might be quite sensible in an environment with many strategies that have the tendency to change after losing in the last period.⁷⁰ In fact, CS4/6 is the strategy that wins the fifth round (with double prizes) of the strategy experiment (see table 4.3). CS5/6 is the very simple strategy of never changing and CS6/6 only changes (with probability 0.5) after losing in the previous period.

The central strategies suggest that strategies from clusters 1 and 2 change relatively often, and strategies from cluster 5 and 6 relatively little, with the strategies from cluster 3 and 4 somewhere in between. In Section 4.4.1 we saw that, except in round 2, the propensity to change is negatively correlated to performance of the strategies, which suggests that, on average, strategies from clusters 5 and 6 should do well. To a certain extent this conjecture seems to be corroborated by the fourth column of table 4.3, which shows from which clusters the winning strategies in the different rounds originate. Two thirds of the winning strategies are from cluster 5/6, with the other winners typically coming from clusters 4/6 and 6/6, except for the second round where the first three prizes are for strategies from cluster 1. Note that the average number of points was lowest in round 2, and highest in round 4, where all winning strategies come from cluster 5.

⁷⁰ In the terminology of Selten et al. (2007) strategy CS4/6 can be classified as using a ‘contrary response mode’, whereas for example strategy CS1/6 uses a ‘direct response mode’.

Cluster	Most central strategy
1/6	1-5: <i>If you have lost the last period change with probability 0.8; if you win three times in a row change with probability 0.5</i>
2/6	3-36: <i>Always change with probability 0.9</i>
3/6	2-27: <i>Always change with probability 0.5</i>
4/6	1-46: <i>Only change when you won the last period.</i>
5/6	1-34: <i>Never change</i>
6/6	4-6: <i>When you lost the last period change with probability 0.5.</i>

Table 4.5: The most central strategy in each of the six clusters (the strategy with the minimum average distance to the other strategies in that cluster)

To further study these issues we ran 500,000 simulations of a 100-period minority game where, for each simulation, we randomly selected (with replacement) five strategies from the set of 107 unique strategies.⁷¹ For all strategies the average number of points over these simulations and the percentage of changes in these simulations are calculated. The second and third columns of table 4.6 show the average per cluster. The results are consistent with the discussion above. The change propensity is indeed very different between clusters; all differences are statistically significant with the exception of 4/6 versus 1/6 and 3/6. Changes are least frequent in 5/6 and 6/6. Furthermore, the strategies in clusters 5/6 and 6/6 earn more points than the strategies in the other clusters (Mann-Whitney tests, all p-values <0.001) and strategies in cluster 5/6 perform better than those in cluster 6/6 (p-value <0.001).

We have done additional simulations to understand the role that heterogeneity plays in the experiment. First we did simulations in a fully homogeneous setting, i.e. with five identical strategies. The fourth column of table 4.6 shows the averages per cluster. Average earnings in these homogeneous settings appear to be very low, even substantially lower than in the symmetric mixed strategy Nash equilibrium. This suggests that participants designed their strategies to exploit other strategies without taking into account that these strategies may be similar. For example, the central strategy of cluster 4/6 (1-46) “Change only when you won the last period” (i.e. the winner of the final round of the strategy experiment) will always lose after the first period in a homogeneous group. Note however that in the experiment strategies could only meet exact copies of themselves if other participants would submit the same strategy. Strategies from clusters 2/6, 5/6 and 6/6 perform significantly better in

⁷¹ The difference with the simulations that we ran to determine the ranking of strategies in the experiment are that strategies submitted in different rounds can now play against each other; not all combinations of submitted strategies are simulated, and random selection is with replacement.

homogeneous settings than strategies from the other three clusters.⁷² Still, strategies from all clusters benefit from at least some degree of heterogeneity and are not very well adapted to homogeneous environment.

To investigate this issue a bit further, we also ran additional simulations where all strategies were selected from the same cluster. Again, average performance of the strategies from clusters 2/6, 5/6 and 6/6 in these simulations is higher than that of the strategies from the other clusters (who on average still earn less than they would in the symmetric mixed strategy Nash equilibrium). Note that the strategies from cluster 5/6 do quite well when only playing each other (even better than when playing the full population of strategies), whereas strategies from clusters 1/6 and 4/6 seem to be particularly badly equipped to play against strategies that are similar to them. We will get back to this issue when we discuss the evolutionary simulations in the next section.

Cluster	Change (Sd)	Points (Sd)	Earnings in homogeneous simulations (Sd)	Earnings in simulations within cluster (Sd)	Evolution last generation alive (Sd)	N
1/6	54.20 (14.83)	31.20 (1.74)	16.28 (10.71)	20.47 (12.97)	11.27 (1.91)	14
2/6	71.34 (9.23)	30.33 (1.16)	27.97 (7.91)	32.79 (4.70)	9.89 (1.23)	16
3/6	44.66 (12.06)	30.70 (0.59)	23.95 (9.05)	30.31 (2.64)	11.44 (0.74)	33
4/6	46.43 (15.28)	30.80 (1.28)	12.94 (9.68)	23.89 (9.63)	11.72 (0.89)	13
5/6	17.00 (8.18)	34.46 (1.69)	27.59 (9.25)	36.60 (3.90)	105.42 (161.36)	18
6/6	33.99 (10.22)	32.58 (1.23)	25.54 (11.64)	32.38 (3.02)	95.74 (179.97)	13
Tot	44.16 (19.88)	31.59 (1.89)	23.02 (10.68)	29.92 (8.11)	37.27 (97.68)	107
K-W	0.000	0.000	0.000	0.000	0.000	

Table 4.6: Average characteristics of strategies per cluster

Column 2 and 3 display the percentage change and the average number of points per cluster in a simulation with all 107 unique strategies. Column 4 displays the average points in homogenous groups (5 identical strategies) and column 5 displays the average number of points in groups with only strategies from the same cluster. Column 6 displays the average last generation alive in an evolutionary simulation (see Section 5 for a discussion). The last row gives Kruskal-Wallis tests (p -values based on 2-sided tests).

⁷² We also studied whether the clusters differ in other aspects, like complexity, length of history used, etc, but found no consistent differences.

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In addition to studying success of strategies against all other strategies, or only against strategies from their own cluster (third and fourth column of table 4.6, respectively) we did one final simulation exercise with strategies from only two clusters, in order to learn about the interaction between those clusters. Figure 4.3 shows four of the comparisons between pairs of clusters.⁷³ The left top panel shows the interaction between clusters 1/6 and 4/6. We see that both types of strategies perform well if the majority of strategies are from the other cluster and vice versa. The central strategies for clusters 1/6 and 4/6 (see table 4.5) suggest a reason for this pattern. Strategies from cluster 1/6 typically have a propensity to change after losing while strategies from cluster 4/6 have a propensity to change after winning. The right top panel shows the interaction between clusters 1 and 5. Strategies in both clusters perform quite well (close to 40 points) when only 1 or 2 strategies from cluster 5/6 are present, but if there are 3 or 4 strategies from that cluster all strategies perform bad. The left bottom panel is an example where the composition matters much more for one cluster (6/6) than for the other cluster (4/6). Finally, the right bottom panel shows an example where the performance of both clusters is not very sensitive to the group composition.

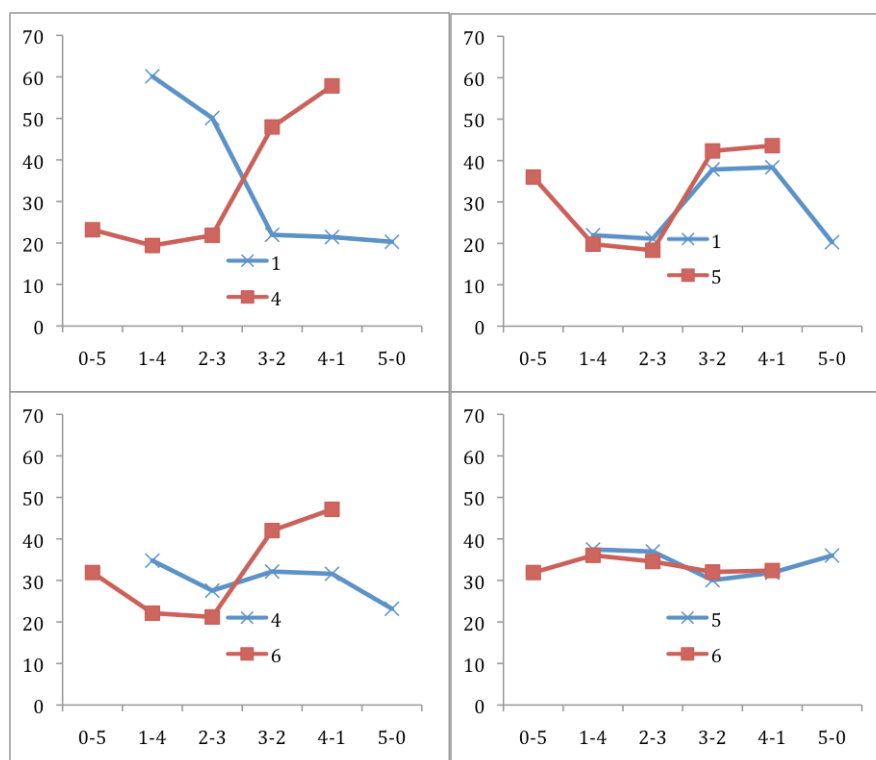


Figure 4.3: Interaction between strategies from two clusters. The horizontal axis displays the number of strategies from respectively the blue (cross) and the red (square) cluster; the vertical axis the average number of points.

⁷³ Figures of the other 11 combinations of two clusters are available from the authors upon request.

4.5. Evolutionary competition between submitted strategies

In this section we consider an evolutionary competition between the strategies submitted by the participants to determine which of those strategies will eventually survive. The analysis in Section 4.2 on the different clusters already allows us to formulate some conjectures about the final distribution of surviving strategies. For example, the fifth column of table 4.6 suggests that the evolutionary competition will not eventually result in an environment with only strategies from cluster 1/6 (or only from cluster 3/6 or only from cluster 4/6). On the other hand, Figure 4.3 suggests that it is in principle possible that evolution results in coexistence of strategies from clusters 5/6 and 6/6 (or coexistence of strategies from clusters 1/6 and 4/6).

We model the evolutionary competition between strategies as follows. In the first generation every strategy i has the same *weight* $w(i,1) = \frac{1}{N} = \frac{1}{107}$. In every generation g we run 2000 simulations of 100-period minority games. In each of these games five strategies are randomly selected (with replacement), where the probability of selecting strategy i equals its weight $w(i, g)$. For each strategy i we determine the average number of points it earned, averaged over all simulations that it was part of. We denote this average by $P(i, g)$. We also determine the average number of points earned by all strategies, averaged over all simulations, and denote this by $M(g)$. After each generation of 2000 simulations the weights of the different strategies are updated on the basis of how well they did as compared to the whole population of strategies. This updating is formalized as follows: $\bar{w}(i, g+1) = (1 + \lambda [P(i, g) - M(g)])w(i, g)$ where λ is a positive parameter which measures *selection pressure*. Note that if a strategy performs better than the average strategy in a generation, its weight increases ($\bar{w}(i, g+1) > w(g, i)$). If $\bar{w}(i, g+1) < 0$, which happens if a strategy performs much worse than average, its weight is set to $\bar{w}(i, g+1) = 0$ and the strategy becomes extinct. The same thing happens if a strategy was selected in none of the 2000 simulations of a generation (which is only likely to happen if its weight is very small to begin with).

The final weights are then determined as $w(i, g+1) = \frac{\bar{w}(i, g+1)}{\sum_j \bar{w}(j, g+1)}$ to make sure that

the weights sum up to one again.

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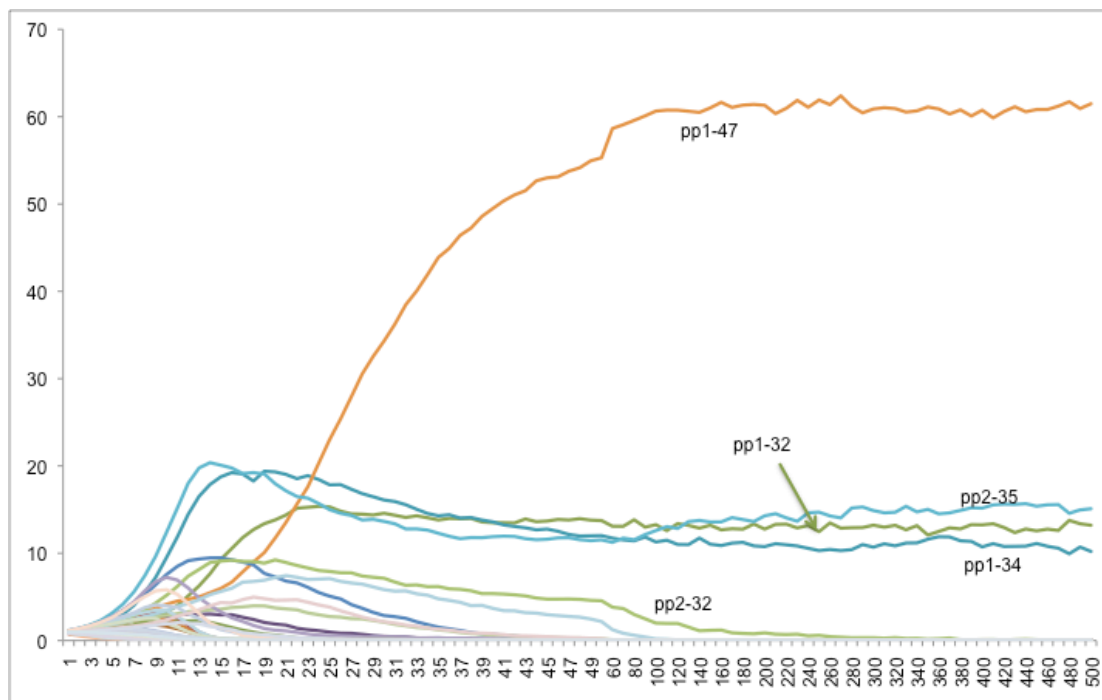


Figure 4.4: Evolutionary analyses with a starting population of 107 strategies. On the horizontal axis the generation (after generation 50 in steps of 10), on the vertical axis the percentage of the population, averaged over five evolutionary simulations.

We ran this evolutionary simulation five different times with the same 107 strategies and for 500 generations, with the parameter λ set equal to 0.05.⁷⁴ Figure 4.4 shows how the weight of each of the 107 strategies, averaged over the five simulations, evolves over the 500 generations. The outcome of this evolutionary simulation is very robust: in all five simulations the same four strategies survive for 500 generations with more or less the same weights in the last generation. In only one of the simulations another strategy (2-32) survives for the first 500 generations, but its weight in generation 500 is very small (0.11% of the population), and it seems likely that this strategy would die out if the evolutionary simulation would run longer.

⁷⁴ A low value of λ leads to a very slow evolution and long simulation times, while a large value increases the role of ‘bad luck’. The parameter value we chose is relatively low: even the worst performing strategies take at least 8 generations to die out. Moreover, for higher values of λ some weights may become negative, which does not happen in our case.

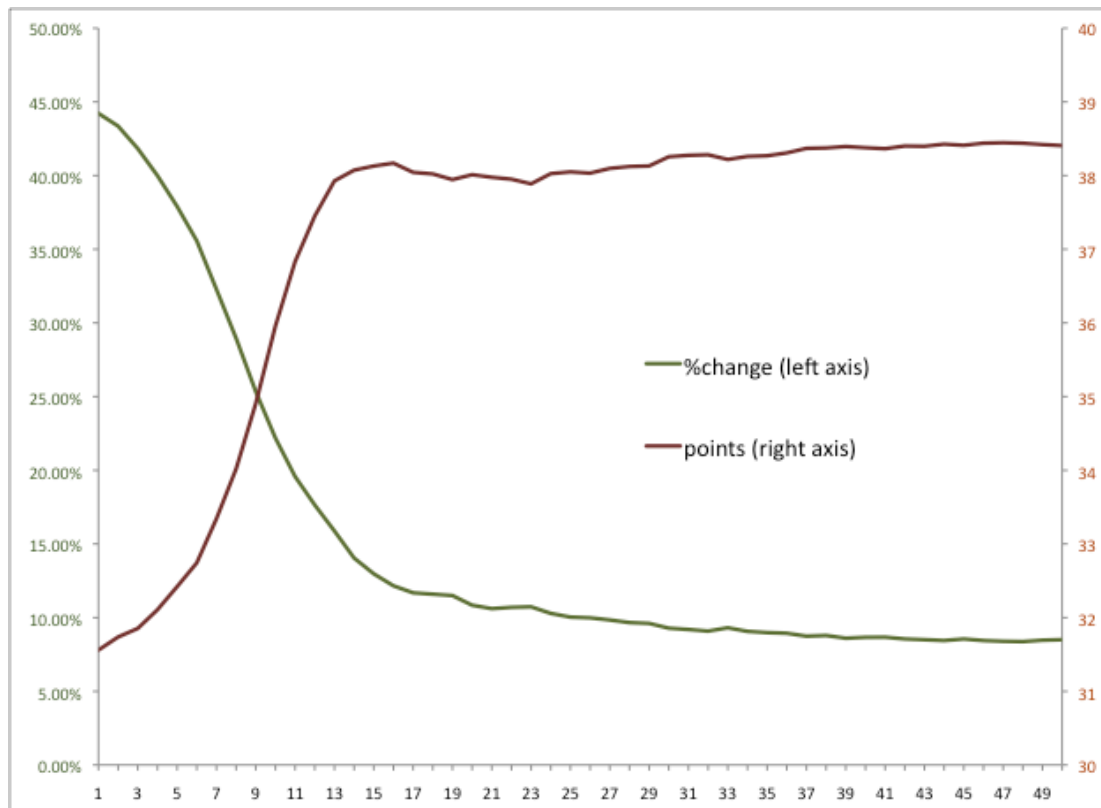


Figure 4.5: Development of average points earned and propensity to change during the evolutionary analysis

Generations are on the horizontal axis, the left vertical axis (green line) is the average percentage that a strategy changes and the right vertical axis (red line) the average number of points of a strategy in a game of 100 periods, again averaged over the five evolutionary simulations.

Although there is little evidence that, over the course of the strategy experiment, coordination on the optimal outcome increases (see the discussion in Section 4.1) coordination grows considerable, on average, in the evolutionary simulations.⁷⁵

Figure 4.5 shows, for the first 50 generations, the average number of points and the average percentage that strategies in the population change color (these levels remain more or less constant after the first 50 generations). The average number of points clearly increases over the generations, from about 31.5 (which is very close to the average number of points under the symmetric mixed strategy Nash equilibrium, which equals 31.25) to 38.5 points (which approaches the maximal possible average number of points of 40 that is obtained under any pure strategy Nash equilibrium). After 50 generations the efficient distribution (with two players in the minority) occurs in 92.66% of the periods, whereas an inefficient outcome with one (zero)

⁷⁵ This is reminiscent of the result from evolutionary simulations in computational models of the minority game, where efficiency also becomes quite large (see e.g. Li et al, 2000ab, Sysi-Aho et al., 2005).

player in the minority occurs in 6.97% (0.37%) of the periods, and these numbers remain more or less constant from then on (for the first generation these numbers are 63.63%, 30.85% and 5.52%). Particularly the worst outcome (with all players making the same decision) is quite rare now, occurring only once every 270 periods, as opposed to about every 18 periods in the mixed strategy Nash equilibrium.

Figure 4.5 suggests that the substantial increase in coordination is directly related to a decrease in the percentage with which strategies in the population change colors, which goes from about 45% when all 107 strategies are present in the population, to around 8% when only the four survivors remain. This relationship between change and number of points may be caused by the fact that predictability, and thereby coordination, increases in an environment with strategies that rarely change, provided that at least some of these strategies condition on the history.

Let us now study in some more detail which strategies survive the evolutionary competition, and what they look like. The survivors, all from clusters 5/6 and 6/6 are described in table 4.7. It is interesting to compare the survivors of the evolutionary competition with the winners of the five rounds in the strategy experiment, given in table 4.3. Several things are noteworthy. Out of the four survivors of the evolutionary competition, only strategy 1-47, the clear winner of the evolutionary competition, won a prize in the strategy experiment (fifth place in the first round). In the last column in table 4.3 we indicated the average number of generations the winners of the strategy experiment survive in the evolutionary competition. Most winners become extinct within the first 20 generations (with the winners from clusters 1/6 and 4/6 dying out first).⁷⁶ The best performing strategies in the strategy experiment (strategies 2-4, 1-10 (3-10) and 3-11) survive on average for about 50 generations and are therefore also relatively successful in the evolutionary competition.⁷⁷ Other successful strategies in the evolutionary competition are 2-32, that becomes extinct only after 349 generations, strategy 4-1 (143 generations) and 5-10 (66 generations).

⁷⁶ Strategy 2-27, which is the central strategy of cluster 3/6 and corresponds to the symmetric mixed strategy Nash equilibrium, does not perform very well. It does not win in any round of the strategy experiment, and it becomes extinct in each of the five evolutionary simulations in generation 15 or earlier.

⁷⁷ Note that strategy 3-10 survives substantially longer than strategy 1-10 (on average 68.6 versus 44.2 generations). The only difference between the two strategies is that the former changes color with probability 0.5 (instead of probability 0.75) if the strategy lost in a group of four two periods ago and won in a group of two in the previous period (see table 4.4). This suggests that the last condition of strategies 1-10 and 3-10 diminishes the evolutionary fitness of these strategies.

1-47	If you won the last period: don't change Else if you lost the last period in a group of 3 or 4: change with probability 0.2 Else if you lost one or both of the periods -2 and -3: change with probability 0.6
2-35	Only change (with probability 1) when you lost the last period in a group of 5
1-32	Only change (with probability 1) when you lost the last period in a group of 4 or if you won in period -3 in a group of 1.
1-34	Never change

Table 4.7: The four surviving strategies after 500 generations.

Apparently, strategies 2-4, 1-10 (3-10) and 3-11 perform quite well in an environment where there are still many other strategies, possibly by exploiting the less successful strategies. However, when those less successful strategies disappear, these three winners cannot feed off them anymore and are overtaken by the strategies that eventually win the evolutionary competition and that were not that successful when all strategies were still present. To understand this phenomenon better table 4.7 shows the four strategies that survive 500 generations of evolutionary competition.

Strategy 1-47 is the clear winner of the evolutionary competition, with a proportion of the population of more than 60%. This strategy does not disturb a winning situation, is not too eager to change after losing (and does not change if it lost in the previous period in a group of five, implicitly predicting that enough other strategies will change in that situation). Finally, it prevents staying in a losing situation forever. In contrast, 1-34 is more conservative and never changes. This strategy can never become a dominating strategy in a population because it does relatively bad when meeting itself (in a homogeneous group its expected earnings are 31.25 points, see table 4.8). In fact, it earns less than the other surviving strategies in last generations; which indicates it is still decreasing in strength. We have run simulations for all possible group compositions with the four survivors and solved for the population equilibrium by finding a distribution for which all active strategies earn the same average number of points (this equilibrium is given in the sixth column of table 4.8). We find that strategy 1-34 would eventually die out, and the population fraction of strategy 2-35 (change only when the last period was lost in a group of 5) would increase. The average number of points earned in the population equilibrium is slightly higher than that in generation 500 (38.47 versus 38.40).

Strategy	Cluster	Population proportion after 500 generations	Earnings	Earnings Homogeneous	Population equilibrium	% Change
1-47	6/6	61.5%	38.43	36.8	60.1%	14.8%
2-35	5/6	14.6%	38.98	31.3	24.5%	4.1%
1-32	6/6	13.5%	38.24	21.7	15.4%	28.8%
1-34	5/6	10.4%	37.76	31.3	0%	0%
Overall			38.40		38.47	

Table 4.8: Characteristics of the four surviving strategies.

Cluster refers to section 4.4.2 and figure 4.2. The population proportion (earnings) are the average in generations 491-500 in the 5 evolutionary simulations. The earnings solo are the average earnings of that strategy in homogenous groups. The population equilibrium displays the population proportions in the equilibrium is which all strategies have the same expected earnings. The final column displays the percentage of periods the strategy changes color.

Let us now compare the strategies of the survivors of the evolutionary competition (table 4.7) with those of the best performing strategies in the strategy experiment (table 4.4). In general, the latter set of strategies shows a high level of inertia: these strategies typically change colors after they lost at least two consecutive periods, and not even always then. This turns out to be quite profitable in an environment where many strategies change color immediately after losing. However, these strategies are too reluctant to change in an environment where evolutionary competition has driven out the strategies that change often. In such an environment strategies that change a bit quicker (like the first three strategies in table 4.7) do better, and therefore eventually win the evolutionary competition.

To test robustness 25 additional evolutionary simulations are run, each with a random selection of 50 of the original 107 strategies.⁷⁸ In these simulations between 2 and 7 strategies survive up to generation 500. In 24 of the 25 simulations all survivors are from the clusters 5/6 and 6/6, the exception is the only one with 7 survivors where there is one survivor from cluster 3/6 and one from cluster 4/6, with the other five from clusters 5/6 and 6/6. The four surviving strategies from the original simulation all do very well: 1-47, 1-32 and 1-34 are survivors in all simulations in which they are involved; 2-35 survives in 10 of the 13 (77%) simulations in which it is involved. All

⁷⁸ The only other difference with the original evolutionary simulations is that now in a generation only 1000, instead of 2000 minority games (each of 100 periods) are simulated. Since the number of strategies is less than half the number for the original evolutionary simulation, this implies that (at least in the beginning of the evolutionary simulation, when all weights are equal) each strategy is involved on average in the same number of simulations as in the original evolutionary simulation.

other strategies perform worse with the exception of strategy 2-32, which is very similar to 1-32 and survives in 10 out of 12 (83%) of the simulations which it was involved in. In these 25 additional simulations the average number of points increases and the average propensity to change decreases over generations, like in figure 4.5 for the original evolutionary simulations. The average number of points in generation 491-500 is slightly less than in the original simulations; 37.12 versus 38.40 and the average propensity to change is higher 12% versus 8%.

The evolutionary simulations discussed this far all suggest that as evolution proceeds the surviving strategies will change color less, on average. This, of course, also has to do with the environment: in an environment where other strategies change more often than not, a good strategy might be also to change more often. To illustrate this point we ran 5 simulations without the strategies of clusters 5/6 and 6/6. In the first generation the average number of changes is about 52%. This increases to about 85% in generation 50 (see figure 4.6). Average earnings also increase: to an impressive 39 points around generation 20 and finally stabilize around 37.8 points. A final remarkable aspect of these simulations is that it takes a longer time to weed out bad strategies: strategies become extinct in generation 15 (instead of generation 7).

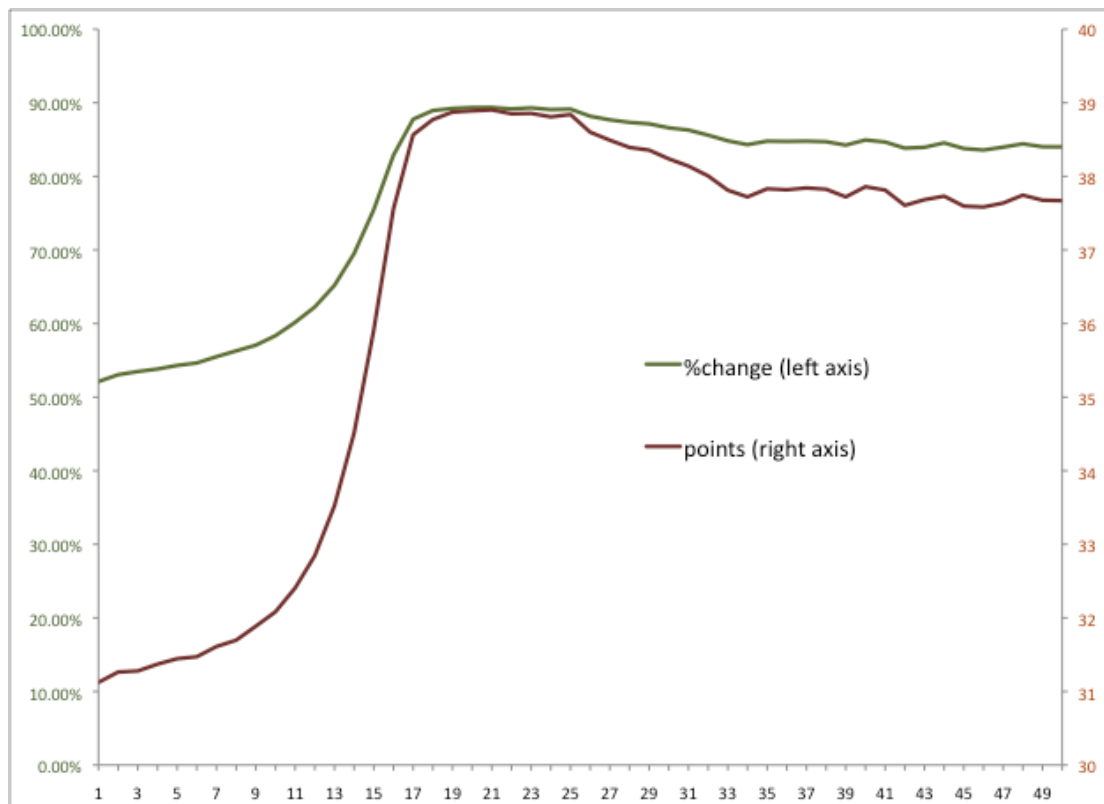


Figure 4.6: Evolutionary simulations with strategies of cluster 5/6 and 6/6 excluded. On the left axis change (green line) and on the right axis (red) earnings.

4.6. Conclusion

In this paper we used an internet based strategy method experiment to explicitly elicit the strategies employed in the minority game. Participants could try out their strategies on the minority game website by simulating them against strategies submitted by the other participants in the previous round. This allowed them to adapt their strategies in the direction they believe will be successful. We believe this is not only a novel aspect of the experimental design, but also relevant for many applications where decision makers, for example traders in financial markets, have the possibility to employ technological tools to try to improve their decisions.

We find that the aggregate behavior of the submitted strategies is close to that implied by the symmetric mixed strategy Nash equilibrium. However, there is considerable heterogeneity between the submitted strategies. Remarkably, participants do not structurally succeed in improving their strategies over the five rounds, and hence the amount of learning seems to be limited, although the minority game website provides ample opportunities to learn. We aim to examine the causes of participant's inability to improve performance in future experiments.

A cluster analysis revealed that the submitted strategies can be divided in six distinct groups. The central strategies in each cluster give an idea of the types of strategies in each group. We find both lose shift (1/6) and win shift (4/6) types of strategies. The most central strategy in another cluster (6/6) also only changes after losing, but with a low probability. The central strategies of the three other clusters are independent of the history and either change very often (1/1), half the time as in the symmetric mixed strategy Nash equilibrium (3/6), or never (6/6). Success of a strategy is negatively correlated with how often the strategy changes sides. The most successful strategies come from cluster 5/6 and have a tendency to only change sides after being on the losing side for two consecutive periods or more.

The experiment shows that the restrictions placed on strategies in many evolutionary simulations of the minority game do not allow for the kind of strategies people actually use. Importantly we find that people use mixed strategies and condition their actions on more detailed histories than just winning or losing. We therefore perform an evolutionary simulation with the 107 unique strategies gathered in the experiment.

Although the most successful strategies from the actual strategy experiment perform relatively well in the evolutionary competition, they do die out eventually, to the benefit of some other strategies that survive the evolutionary competition and that are slightly less reluctant to change. The intuition for this is that the initially successful strategies do quite well in an environment where all submitted strategies are present, because they profit from strategies that change often. When those strategies become extinct, however, the initially successful strategies are not very well suited to the new strategic environment and other strategies take over. Evolutionary competition leads to a fast and dramatic improvement in coordination.

In conclusion we find that there is substantial heterogeneity in the strategies people use in the minority game and many strategies condition on quite specific histories and use randomization. Participants on average fail to improve their strategies between rounds despite the possibilities offered by the minority game website. It appears that as a result there is very little coordination and aggregate outcomes resemble the mixed strategy equilibrium. If bad strategies are allowed to go extinct in an evolutionary simulation the remaining strategies do achieve much higher levels of coordination. The surviving strategies are reluctant to change, thereby enhancing stability, but they do change, with a small probability, when a bad situation lasts too long. This appears to be the key to their coordination.

4.7. Appendix A: Experimental Instructions

(Translated from Dutch. Original Dutch instruction available upon request)

The minority game

The minority game is played with 5 players, each of which chooses either **red** or **blue**. Players who selected the color selected by the **smallest** number of players earn one point. Other players earn nothing. Then a new round is started and everyone decides to change color or not. The game is repeated a large number of rounds with the same player.

In the experiment the decision isn't made directly by you, but by a strategy devised by you. How this exactly works is explained below.

Conditions

We use computer code consisting of so called "IF statements" that look like this: **IF (condition) { RETURN number ; }** With these you can determine when you will change or not. Your strategy can consist of multiple if statements.

Condition The condition in your if statement is either **true** or **false**. In the condition you can use the history of the previous 5 rounds. Per round the number of players with your color (including you) and whether you changed color can be used. The table

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below shows the codes for these events. In construction your conditions you can use arguments. **These arguments are: and/or (OR), and (AND), negation (!), equality (==), smaller than (<), larger than (>), brackets ().** You can use these arguments by clicking on them. To use the arguments ==, > and < you should view the events as variables which have the value 1 if they are true and 0 if they are false. You can add or subtract conditions using + and - (this is an example, you can do anything.)

Below you will find a number of examples of IF statements. These are only examples and not necessarily smart strategies.

Example 1 (OR argument)

IF (\$W1[2] OR \$C[5])

means "if I won 2 periods ago with 1 player (including myself) choosing my color **and/or** if I changed color 5 periods ago."

Example 2 (AND argument and negation !)

IF (\$L3[4] AND ! \$C[2])

means "if I lost 4 periods ago with 3 players (including myself) choosing my color **and** I did not change 2 periods ago."

Example 3(inequality >)

IF (\$C[1]+\$C[2]+\$C[3] > \$W2[1]+\$W2[2]+\$W2[3])

means "if I in the previous 3 periods changed color **more often** than I won with 2 players (including myself) choosing my color in those same periods "

Example 4(equality == and negation !)

IF (\$C[3] == ! \$W2[5])

means "if I changed 3 periods ago **and** I did not win with 2 players (including myself) choosing my color 5 periods ago **or** if I did not change 3 periods ago **and** I did win with 2 players (including myself) choosing my color 5 periods ago."

Number Your IF statement always ends with "{ RETURN getal ; }". The number you fill out here determines what happens if your condition is **true**. A 1 means you will change color, a 0 that you will not and a number between 0 and 1 means that you will change color with that probability.

Example 5 (number between 0 en 1)

**IF (\$L5[4]== \$W2[1]) {
RETURN 0.64;
}**

means "if I lost 4 periods ago with 5 players (including myself) choosing my color **and** I won 1 period ago with 2 players (including myself) choosing my color, **or** both are not true, I will change color with a probability of 64%. "

You can also use 1 as a condition. 1 means "always true".

Example 6 (condition that is always true: 1)

**IF (1) {
RETURN 0.4;
}**

means "independent of the history I will change color with a probability of 40%."

Strategy

Your strategy can consist of multiple IF statements. In that case the statements are reviewed in the order in which you wrote them down. If a condition in an IF statement is true, subsequent IF statements are ignored. (For those with programming experience: they can be considered ELSEIF statements). If one of your IF statements is fulfilled it is assumed that you will not change color.

Example 7 (multiple IF statements)

```
IF ($C[5] AND $W2[1] ) {
RETURN 1;
}
IF ($L3[2] ) {
RETURN 0.5;
}
```

means "if I changed color 5 periods ago **and** I won with 2 players (including myself) choosing my color in the previous period, I will change color. If that is not true, but I have lost with 3 players (including myself) choosing my color 2 periods ago I will change color with a 50% probability. Otherwise I do not change."

Example 8 (multiple IF statements)

```
IF ($W1[4] OR ($C[1] AND $L4[3] ) ) {
RETURN 0.2;
}
IF (1) {
RETURN 0.7;
}
```

means "if I won with 1 player (including myself) choosing my color 4 periods ago, **and/or** I changed color in the previous period and I have lost with 4 players (including myself) choosing my color 3 periods ago, then I change color with a probability of 20%. In all other cases I change color with a probability of 70%." During the experiment you can either click on all the codes you may need while making a strategy or write them down yourself. You can also cut (ctrl x), copy (ctrl c), paste (ctrl v) and undo things (ctrl z), or redo things that you undid (ctrl y).

Simulations and results

Participants' earnings in every round depended on the place of their strategy in the ranking of strategies. In order to determine a ranking of strategies all possible combinations of strategies are considered in a simulation. Each simulation starts with 5 rounds where each player chooses red or blue with equal chance. This way a random history is created. Then 100 rounds are played with the same combination of strategies. For the history it is assumed that you didn't change color in the first round. The 5 random rounds don't count towards a strategies score. For each simulation the number of points scored by each strategy is recorded. The final score is the average score over all simulations a strategy was involved in. On this basis a ranking is determined. Using this ranking earnings were determined according to the following table:

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	Rounds 1, 2, 3 and 4	Round 5
Best strategy	€75	€150
Second place	€60	€120
Third place	€45	€90
Fourth place	€30	€60
Fifth place	€15	€30
All other strategies	€0	€0

Testing strategies

After writing a strategy you can test it in simulations against four random strategies from round 5 of the experiment. In the experiment subjects could test their strategies in simulations against four random strategies from the previous round. In the first round they could try their strategies against a set of preprogrammed strategies. Subjects were told this could only help them to determine whether their strategy worked as intended, not whether it was a good strategy. The reason for this, it was explained is that when the ranking is determined their strategy will be playing against strategies made by other players. Like the subjects in the experiment you can try as many different strategies and as many simulations as you want.

Example of a screen to formulate a strategy (translation from Dutch). The buttons had context dependent help (showed in the red box).

Formulating the strategy

Strategy

IF statement AND OR () negation ! == > < + - clear

	Changed	Number of players with your color				
		Win		Lose		
Previous period:	SC(1)	\$Win[1]		\$Lose[1]		
		\$W1[1]	\$W2[1]	\$L3[1]	\$L4[1]	\$L5[1]
2 periods ago:	SC(2)	\$Win[2]		\$Lose[2]		
		\$W1[2]	\$W2[2]	\$L3[2]	\$L4[2]	\$L5[2]
3 periods ago:	SC(3)	\$Win[3]		\$Lose[3]		
		\$W1[3]	\$W2[3]	\$L3[3]	\$L4[3]	\$L5[3]
4 periods ago:	SC(4)	\$Win[4]		\$Lose[4]		
		\$W1[4]	\$W2[4]	\$L3[4]	\$L4[4]	\$L5[4]
5 periods ago:	SC(5)	\$Win[5]		\$Lose[5]		
		\$W1[5]	\$W2[5]	\$L3[5]	\$L4[5]	\$L5[5]

Lost last period with 5 players with my color

```

IF (condition) {
  RETURN number;
}
ELSE {RETURN 0;}(This is always added: when none of the conditions is met you will not change color)

```

to simulation

Click [here](#) to view the complete instructions.

Example of a screen after a strategy is tried out by the participant

Simulations

with strategy:

```

IF ($L4[1]) {
RETURN .2 ;
}
IF ($L5[1]) {
RETURN .3 ;
}
ELSE {RETURN 0;}

```

Period	Self	Others	Changed	Result
101	B	<i>RRRB</i>	0	W2
102	B	<i>RRBB</i>	0	L3
103	B	<i>RRBB</i>	0	L3
104	B	<i>RRBB</i>	0	L3
105	B	<i>RRRR</i>	0	W1

Results 100 periods					
Win			Lose		
W1	W2	L3	L4	L5	Points
4	26	38	31	1	30

New simulation

Other strategy

This is my strategy! (To submit the definite strategy for that period, not working in the demo)

If you do not want to test any other strategies at this time and do not want to register a final strategy you can log out. Do not forget to register a strategy before the deadline, otherwise you can not make money during this round.

Log out

Example feedback of round by email (translated from Dutch)

Dear [name participant]

The simulations for round [round number] are finished and these are the results:
Your strategy finished on place [rank] and your earnings in this round are [earnings].

The general results

Rank	Login name	Average number of points
1	Chung-Lin KWA!	39.873644
2	gemer92	38.148873
3	Kees	37.256570
4	Witchy	36.612166
....		
35	A-town	27.996524
36	capital P	27.888102

After logging in on www.creedexperiment.nl/minor/login.php you can run simulations with your strategy of round [round number] and 4 other strategies also from round [round number]. After that you can try out new strategies against 4 strategies of round [round number] and after that submit your final strategy for round [round number + 1].

Best regards,

Jona Linde, Joep Sonnemans en Jan Tuinstra

5. Nudge: a Lullaby?*

5.1. Introduction

There is ample evidence that people have limited cognitive resources and that as a result they often rely on heuristics when making decisions (see e.g. Kahneman & Tversky, 2000). As a consequence their decisions can deviate from rational choice and go against their own interest. Preference reversal (Lichtenstein & Slovic, 1971) and inconsistent time preferences (Frederick, Loewenstein & O'Donoghue, 2002) are well known examples of this kind of behavior.

Knowledge of behavioral biases can be used to influence choices. Thaler and Sunstein (2003) and Camerer et al. (2003) propose to use our knowledge of biases to change the environment in which the choice is made, the “choice architecture”, in an attempt to promote “better” decisions without changing incentives. They call their approach libertarian or asymmetric paternalism.⁷⁹ A feature of the choice architecture specifically designed to improve decisions is called a “nudge”.

Numerous studies validate the effectiveness of this approach (Thaler and Sunstein, 2003). A problem with libertarian paternalism is that although inconsistent behavior shows that people sometimes act against their own interest it does not tell us what decision is optimal. For example, we know that the presence of irrelevant alternatives can influence which product people choose to buy (Huber, Payne & Puto, 1982), but this does not tell us which product they would have bought in the absence of behavioral biases. As a result libertarian paternalism has received its share of criticism focusing on the justification for influencing choices in a certain direction (e.g. Mitchell (2005) and Sugden (2008)).

*This chapter is based on De Haan and Linde (2011).

⁷⁹ In the rest of this paper we use the term libertarian paternalism to refer to this approach.

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We do not aim to take a side in this philosophical debate but consider a somewhat different, one might say more practical, possible concern with libertarian paternalism. We ask whether the most popular nudge, a good default option⁸⁰, affects performance in subsequent, similar decision situations.

Behavioral Economics research has amply illustrated that people are more likely to choose the option presented as the default or the option they currently possess (e.g. Samuelson and Zeckhauser, 1988). This tendency is known as the status quo bias and commonly explained by loss aversion (Kahneman, Knetsch & Thaler, 1991). So far the assumption underlying libertarian paternalism is that the status quo is a constant behavioral tendency that can be used to influence decisions. However the choice architecture may not only change decisions but also more deeply influence the choice process. A behavioral tendency like the status quo bias might have an ‘endogenous’ component.

If people receive a nudge in the form of a good default, the choice heuristic to stick with the default option performs well. This may reinforce the use of this heuristic. This reinforcement may make the person more likely to choose the default in similar decisions in the future, even if she is no longer being nudged but faces a random, or possibly even a bad, default. This would hurt her performance.

A nudge can also hurt later decisions because a person who faces good defaults becomes ‘spoiled’. If the time comes that the defaults are no longer good options, the consumer might to some degree not have learned how to choose for herself. She may even feel resistance towards putting effort into a task that used to be easy, just ‘pick the default’, even if she realizes that the nature of the default has changed. In a very recent experiment, Caplin and Martin (2011) find behavior that can be interpreted as becoming spoiled. Their participants put less effort in a choice task when provided with a relatively helpful nudge. They do not however look at what happens to performance if the nudge would disappear again.

A good default may however also improve performance in subsequent similar decisions without a nudge. A good default draws people’s attention to good options which may teach a consumer what a good option looks like. This knowledge may help her make better decisions in the future, even when she is no longer nudged.

⁸⁰ Thaler and Sunstein (2003): “The most common nudge is a default option that the choice architect believes is a good choice for the decision maker.” For the use of a default as a nudge see for example Madrin and Shea (2001), Johnson and Goldstein (2003) and Benartzi and Thaler (2004).

To test whether a good default affects performance in subsequent decisions we developed an experimental task with an unequivocal best choice which is nevertheless hard to find. Participants face this task for 50 rounds. In the first 25 rounds participants in the “nudge” treatment receive a nudge in the form of a good default. Participants in the control group on the other hand receive a random default. In the second 25 periods both groups receive a random default. Any difference in performance between participants in the nudge and control treatments in these second 25 periods reveals the effect of a nudge on subsequent behavior. In section 2 we describe the experimental design in detail.

The effect of a nudge on subsequent decisions is not only of academic interest. In real life decisions a good default may be followed by a worse default for three possible reasons⁸¹. A first reason is commercial interests. Many purchases require several separate decisions (e.g. buying a car, a computer, or a plane ticket). Companies may try to lure consumers into a false sense of security by providing good defaults for the first decisions, but malicious defaults later on (e.g. first recommending economy class and direct routes but later also expensive flight insurance).

A second reason why good defaults may be followed by less helpful ones is that for some decisions, ‘good’ defaults are easier to provide than others. This happens in one of the most prominent examples of libertarian paternalism, a default enrollment in pension plans (Madrin & Shea, 2001). Saving something, and therefore participating, is probably optimal for the large majority of employees, but there is far more heterogeneity in how much too safe. Setting a ‘good’ default savings rate is therefore far more difficult (Choi et al., 2003). As a result the default savings rate is probably a less helpful default than the default enrollment choice.

A third reason for following good with random defaults is legal limitations. Courts may view libertarian paternalism as unwarranted government intervention. In their book “Nudge” Thaler and Sunstein (2008) discuss a program implemented in Maine (USA) to provide Medicare users with a good default health care program. Legal challenges have contributed to the failure of this project to spread to other states. Similar legal challenges may cause libertarian paternalism programs that are already in use to be discontinued. If that happens a person used to helpful defaults may face suboptimal defaults in the future.

⁸¹ We do not claim to provided an exhaustive list, but we do believe that these three reason are probably the most prevalent.

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For these reasons we believe it important, both in order to expand our insight in the use of decision heuristics and to design optimal libertarian paternalism policies, to explore the effect of a good default on subsequent decisions. Section 5.2 elaborates on our experimental design. Section 5.3 presents the results and section 5.4 concludes.

5.2. Design

The experiment was computerized with php/mysql and conducted at the CREED laboratory of the University of Amsterdam. A total of 88 participants participated in the experiment, half of them assigned in the control treatment and half of them to the nudge treatment. At the beginning of the experiment participants read the instructions on the computer at their own pace. They then received a summary of the instructions on paper. After reading the instructions, participants had to correctly answer some questions to test their understanding of the instructions.

All participants in the experiment performed the same set of 50 multi-attribute choice tasks. The difference in treatments consisted only of a difference in the nature of the default in the first half of the experiment. Performance in the second half, when all participants face the same task and the same default reveals the effect of being nudged on subsequent decisions. Below we first discuss the choice task and then the difference in the default between the two treatments.

5.2.1. Task

Each round participants chose one option from a list of six. The information on which to base this choice was presented in the form of a table. Each option consisted of a number of points in 6 categories, each with a different weight value. The weight values were 6, 5, 4, 3, 2 and -1. The category with a weight of -1 was labeled the price of an option. These categories and their weights, but not the points, were the same for each choice task. An option generated an amount of credits equal to the sum of the points in each category multiplied by the weight of that category. The tasks were randomly generated under the conditions that each option generated between 70 and 230 point and that the best option generated at least 10 points more than the second best option. An example of a task is shown in figure 5.1.⁸²

⁸² This task can be seen as a choice between different products, each with a different price and different qualities. The category weights represent the relative importance of different types of characteristics

Choice Task 48						
TIME: 39 BONUS: 20						
Choices	Weight=6	Weight=5	Weight=4	Weight=3	Weight=2	Price
<input type="radio"/> Option 1	12	8	10	4	3	102
<input type="radio"/> Option 2	1	13	11	28	19	138
<input type="radio"/> Option 3	4	9	29	39	13	122
<input checked="" type="radio"/> Option 4	5	20	49	7	13	271
<input type="radio"/> Option 5	28	5	13	21	12	109
<input type="radio"/> Option 6	42	22	6	4	3	348
<input type="button" value="Make choice"/>						

Figure 5.1: An example of the choice task as presented to the participants. Option 4 is the default option in this example.

On top of the credits generated by the chosen option participants received a bonus, starting at 20 credits and decreasing by 1 credit every two seconds the participant used to make a decision. The maximal time a participant could take to choose was 40 seconds. 20 credits is a small amount compared to the gains that could be made by making a better decision.⁸³ The bonus can be seen as an opportunity cost of spending time on the task. After a participant made her decision she had to wait till the time for this round expired before moving on to the next round. In addition there was a 5 second waiting time between rounds.

Without the bonus participants who have already decided could have waited until the full 40 seconds were over without any costs. With the bonus, as soon as a participant has decided, she would want to enter her choice to save on the bonus. We implement this bonus for two reasons. Firstly, in order to have a measure of search effort in the form of time spend on the task. Secondly, to ensure that we know when participants actively choose an option and when they were forced into a decision because time ran out.

Participants performed this task for 50 rounds. All participants faced the same 50 tasks but there were 6 different orders in which the tasks were presented. The order was counterbalanced between treatments⁸⁴. At the end of the experiment one round

and the points the quality of the product in that characteristic. In that sense the problem is similar to many everyday decision problems, from buying a phone to choosing a medical insurance or an investment plan. This type of task has previously been used by Kalayci and Potters (2011).

⁸³ For example the difference between the best and the second best option was always at least 10 credits.

⁸⁴ The counterbalancing procedure also ensured that each group as a whole faced the same tasks in the first and the second half of the experiment. Due to a small software error two participants had to be excluded, one in the control treatment and one in the nudge treatment. This affected the counterbalancing slightly, as these two participants had different orders. Leaving out two random participants with these orders in the other treatments does not materially affect our results.

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was randomly selected⁸⁵. The number of credits earned in that round determined the participant's earnings. Each credit was worth 10 eurocents.

5.2.2. Default and treatments

One of the six options in the table was given a different color and was preselected when participants were presented with the task. This option was the default option. In the instructions it was labeled the recommended option without further specifying why it was recommended. If participants did not select one of the other options, the default automatically became the choice of the participant for that round if the time limit ran out before the participant entered a choice.⁸⁶ If a participant chose an option different from the default option a smaller version of the table was shown, containing only the chosen and the default option. They were then asked if they wanted to stay with their original choice or switch to the default.⁸⁷

The experimental design consisted of two treatments. Both treatments were identical, except for one aspect. In the 'control' treatment, the default option was determined randomly for each task. In the 'nudge' treatment, the default option for the tasks a participant faced in the first 25 rounds of the experiment was the option with the highest value. The tasks faced by participants in the 'nudge' treatment in the rounds 26 till 50 had the same random defaults as in the control treatments.

5.3. Results

Because it was in the participants' best interest to choose the option that would yield maximum earnings, we use the value of the chosen options as a performance measure.⁸⁸ We will report the main treatment effect in the first subsection and explore the different possible explanations in the second subsection. All tests reported are two-sided and, unless otherwise specified, performed at the individual level.

⁸⁵ The same round was selected for all participants in a session but because of the different task orders that was a different task for different participants.

⁸⁶ This happened only 161 times out of 4400 i.e. in 3.66% of all decisions.

⁸⁷ People switched a total of 99 times out of 2577 i.e. in 3.8% of all initial non-default choices.

⁸⁸ We leave the bonus payment the subjects received for the speed of their decision out of the analysis for now as this is not the point of focus. Adding the bonus does not significantly change the results. In fact participants in both treatments spend an average of 19.4 seconds per task.

5.3.1. Treatment effect

The main question our experiment tries to answer is the effect of having received a nudge on performance when that nudge has disappeared. Table 5.1 answers this question. We find that in the second half of the experiment, when all participants faced the same random defaults, participants in the control treatment chose an option worth 5.72 points more on average than participants in the nudge treatment.⁸⁹ A Wilcoxon rank-sum test shows that this difference is marginally significant ($p=0.081$).

While we are mainly interested in what happens when the default is no longer optimal, we would expect the nudge to be helpful in the first half of the experiment. Table 5.1 indeed confirms that providing people with a good default helps them make better decisions. As the first row of table 5.1 shows nudged participants chose an option worth 18.52 points per round more in the first half of the experiment. That is a substantial and highly significant difference, according to a Wilcoxon rank-sum test.

The regression in table 5.2 below confirms the main treatment effect. This regression controls for several demographic variables, high school math level and grade as a proxy of skill and time used in the first 10 rounds as a proxy of effort. As time spend can be influenced by the treatment we take the time spend during the first ten rounds as an exogenous measure of effort.⁹⁰ Controlling for these variables in the regression, the treatment effect becomes significant at a 5% level.

	Control treatment	Nudge treatment	p-value of a Wilcoxon rank-sum test
first half	173.0 (14.1)	191.5 (11.6)	0.000
second half	174.8 (15.1)	169.1 (17.5)	0.081
p-value of a Wilcoxon signed rank test	0.398	0.000	

Table 5.1: Average value of the chosen option split over treatment and first and second half of the experiment (standard deviations at the individual level between brackets).

⁸⁹ For comparison the average value of an option for a person who always chooses the best option is 71.21 points higher than those of a person who chooses randomly.

⁹⁰ Time spend in the first ten rounds strongly correlates with time spend in later rounds (Spearman correlation coefficient is 0.6346 and $p\text{-value}<0.01$) Using time spend in the entire experiment or only the second half yields the same qualitative results

Dependent variable: average value of the chosen option in the second half of the experiment	Coefficient	p-value
constant	147.82	0.000
treatment (1=nudge treatment)	-6.62	0.047
time used in first 10 rounds	0.97	0.000
male	11.54	0.001
age	0.01	0.848
studies economics	1.75	0.664
Dutch	-5.56	0.213
math grade	0.20	0.860
math level	3.51	0.393
adjusted r-squared	0.22	

Table 5.2: Treatment effect when controlling for demographic variables and proxies for effort and skill.

Result 1: Participants who had previously been nudged chose worse options than participants who had not previously been nudged.

In the first few rounds after the default changes from the best to a random option participants might not have noticed that the default is no longer helpful. If that would have been the case, the treatment effect might be an artifact of the first few rounds of the second half. However, as figure 5.2 shows, the treatment effect persists throughout the second half of the experiment. In almost every round average earnings were higher for the control than for the treatment group and this difference does not show a tendency to decline.

5.3.2. Causes

As mentioned in the introduction there are two possible causes for the treatment effect. Firstly participants who have received a helpful nudge may have come to rely on default more even though it was no longer helpful. Secondly participants in the nudge treatment may have become spoiled and therefore unwilling to put in effort.

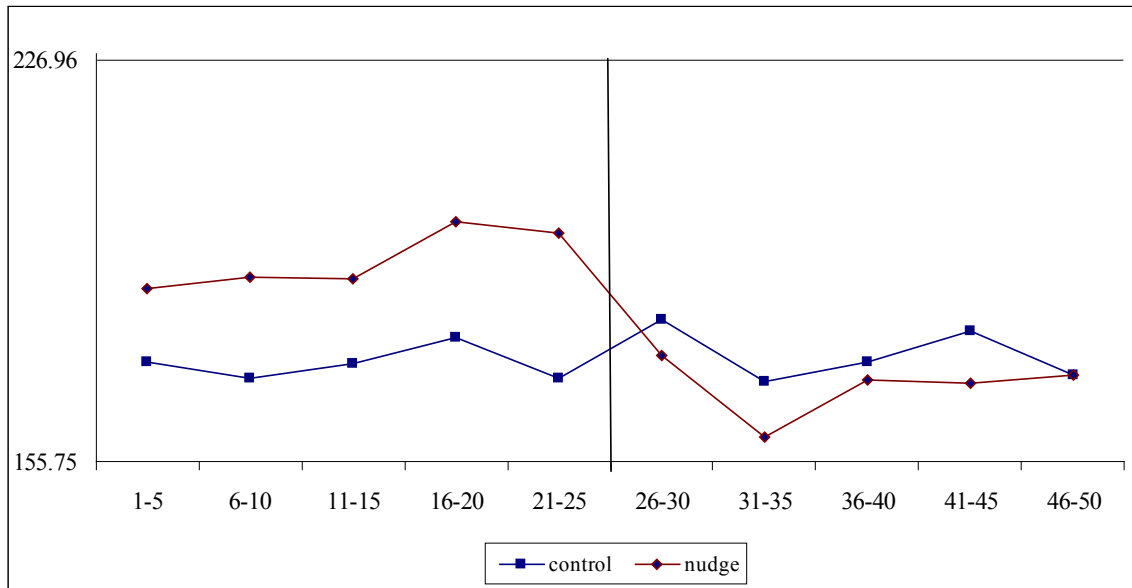


Figure 5.2: Average value of the chosen option aggregated over 5 rounds split between the nudge and control treatments.

The bottom of the graph corresponds to the expected value over the all 50 tasks for a person who chooses randomly (155.75), the line at the top to the average value of the chosen option for a person who always chooses the best option (226.96).

Reliance on the nudge

If placing greater trust in the default is indeed the cause of the treatment effect we should find that also in the second half of the experiment participants in the nudge treatment selected the default option more often than participants in the control treatment. As table 5.3 shows this was indeed the case. In the second half of the experiment nudged participants were 11.6 percentage points more likely to pick the default than participants from the control group even though they faced the exact same default. Figure 5.3 also illustrates this effect.

	Control treatment	Nudge treatment	p-value of a Wilcoxon rank-sum test
first half	30.0%	65.3%	0.0000
second half	33.9%	45.5%	0.0039
p-value of a Wilcoxon signed rank test	0.0511	0.0000	

Table 5.3: Percentage of default choices per round split over treatment and first and second half of the experiment.

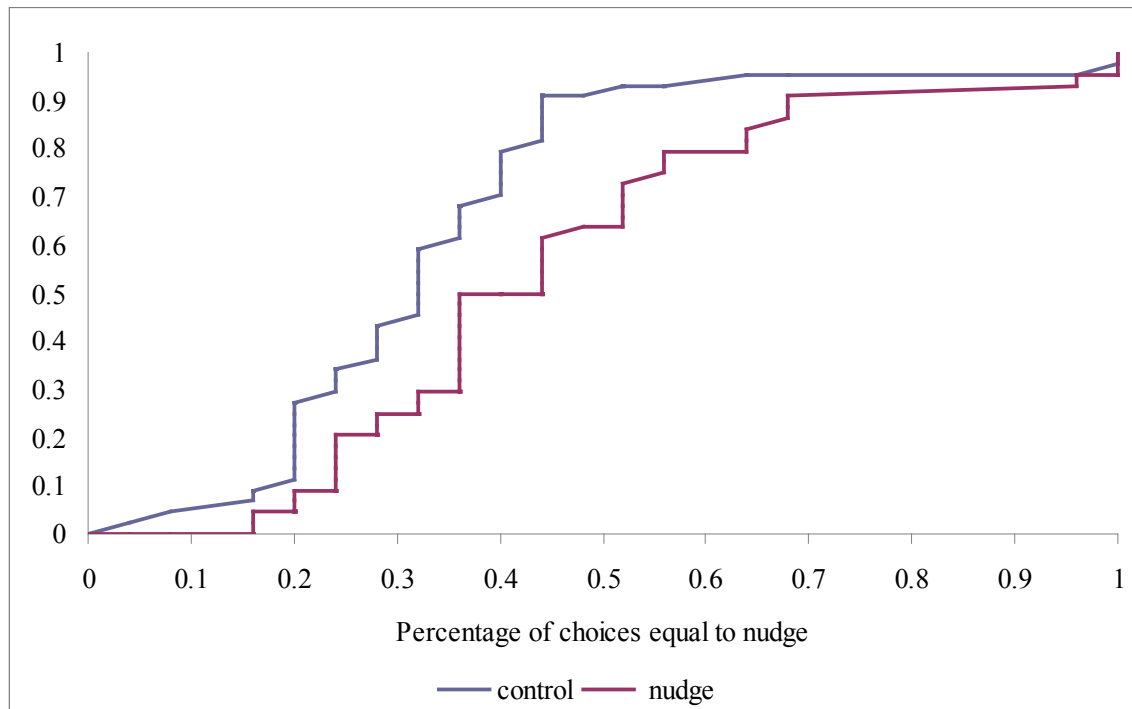


Figure 5.3: Cumulative distribution of participant's tendency to rely on the default option in the second half of the experiment for both treatments.

Result 2: Participants who have previously been nudged choose the default option more often than participants who have not.

The likelihood of default choices in the first half provides further evidence that participants in the nudge treatment indeed came to rely more on the default than the participants in the control treatment. In both treatments participants chose the default more often than the expected random percentage of 16.7% (rank-sum p-values <0.001), but nudged participants were significantly more likely to do so (rank-sum $p=0.000$). This in itself does not provide evidence for a greater trust in the default in the first half of nudge treatment. Participants may also have chosen the default more often in the nudge treatment because it was the best option which they could have chosen anyway, regardless of it being the default or not. However we find that in the first half participants in the nudge treatment were also more likely to choose the default than participants in the control treatment were to choose either the default or the best option (65% vs. 57%, rank-sum $p=0.0025$). We therefore conclude that participants in the nudge treatment came to trust the default more than participants in the control treatment in the first half.

Although this shows that nudged participants were more likely to choose the default, it does not necessarily explain their worse performance in the second half. They could, for example, have chosen the default more often, but only when the default provides relatively good earnings. Conversely their worse performance may also be due not only to choosing the default more often, but also to choosing the default when it is a relatively bad option. The regression in table 5.4 shows that neither effect is present. Participants in the nudge treatment are neither more nor less sensitive to the relative value of the default option; they just follow the nudge more often.

Result 3: Participants who have previously been nudged are no less sensitive to the difference in value between the default and the best option.

Effort

We now turn to the second possible explanation of the treatment effect: nudged participants putting in less effort. In opposition to this hypothesis we find that participants in the nudge treatment spend on average slightly more time in the second half of the experiment than participants in the control treatment: 21.7 versus 20.9 seconds. This difference is however far from significant (Wilcoxon rank-sum test $p=0.81$). Nudged participants clearly do not appear to be less willing to put in effort.

Dependent variable: choice equals the default in the second half	Coefficient	p-value
constant	0.618	0.000
treatment (1=nudge treatment)	0.113	0.008
difference in value between the default option and the best option	-0.004	0.000
interaction between treatment dummy and the difference in value between the default option and the best option ^a	0.000	0.678

Table 5.4: Linear regression examining the effect of the treatment, the difference in value between the default option and the best option and their interaction on the likelihood of choosing the default option in the second half of the experiment. Each choice is used as an independent observation, standard errors have been adjusted by treating each participant's choices as a cluster.

^a The interaction term was normalized to prevent multi-colinearity issues with the dummy variable for treatment.

Dependent variable: average value of the chosen option in the second half	coefficient	p-value
Constant	156.99	0.000
treatment (1=nudge treatment)	-6.87	0.043
time used in the first 10 rounds	0.85	0.007
Interaction treatment and time used in the first 10 rounds ^a	-0.24	0.585

Table 5.5: Interaction between effort and the treatment effect.

^aThe interaction term was normalized to prevent multi-collinearity issues with the dummy variable for treatment.

Result 4: Participants who have previously been nudged do not put less effort into the task than participants who have not.

Effort might however interact with the treatment effect in another way. We saw that nudged participants came to trust the default more and therefore perform worse. It seems that this is less likely to occur for those participants who put more effort into the task as these would be more likely to realize that the default no longer provides guidance. We therefore explore interaction effect between our proxy for effort, time used in the first ten rounds and a treatment dummy (table 5.5).⁹¹ This regression shows that there is no significant interaction, although the effort proxy on itself does have a significant positive effect. Participants identified as putting in more effort are no less likely to perform worse due to having been nudged.

Result 5: Participants who (initially) put more effort into the task are no less likely to perform worse due to having been nudged.

Gender effect

As we can see from the regression in table 5.2, there is a significant effect of gender on the earnings in the second half. Although we did not hypothesize any gender effect, we feel compelled to analyze the effect further given the size and significance of the effect in the regression.

⁹¹ We take time taken in the first ten rounds because this time in the second half could be affected by the treatment. Table 5.2 shows that putting more effort into the task indeed improves performance.

	Control treatment	Nudge treatment	p-value Wilcoxon rank sum test
first half, females	173.7 (14.2)	190.0 (10.8)	0.000
second half, females	168.3 (15.1)	166.6 (17.9)	0.942
p-value Wilcoxon signed rank test	0.046	0.002	
first half, males	172.4 (14.3)	192.3 (12.5)	0.000
second half, males	179.7 (13.3)	170.9 (17.7)	0.028
p-value Wilcoxon signed rank test	0.007	0.000	

Table 5.6: Average earnings per round, split between treatments, first and second half of the experiment and males and females.

When we split the results by gender an interesting pattern emerges. Table 5.6 below shows again the average earnings per round for the first and second 25 rounds, but now separately for men and women. For the first half of the experiment, the picture for men and women is similar. Again, having a good default helps very much. But in the second half of the experiment, we see no difference for females but only a significant treatment difference for men. The reason for this difference between treatment effects appears to be that in the control treatment men improve performance in the second half compared to the first half while women in the control treatment perform worse in the second half.

Result 6: Having been nudged leads to a significantly worse performance for males but not for females.

5.4. Conclusion

In this study we showed that providing people with a nudge in the form of a good default can affect performance in subsequent decisions. Participants in our experiment who faced a good default in the past came to rely on the default more than other participants, leading to worse decisions when the default was no longer helpful. We therefore conclude that nudging people not only changes choices, but also seems to affect the choice process, possibly leading to worse decisions when circumstances change.

While this is a single experiment and further studies should assess the robustness of this phenomenon, we do believe this result provides a note of caution to

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policymakers attempting to improve decisions using a nudge. When implementing a policy it is important to consider possible changes to the policy in the future and the effect a policy has on people's general attitude toward other choices. Of course we certainly do not argue that policy makers should never engage in libertarian paternalism. In fact our experiment showed that the nudge we provided helped participants make better decisions. Realizing the effects on subsequent decisions may however be an extra element to consider while designing public policies and could also provide a ground to regulate certain business practices.

Stepping away from direct policy implications our experiment provides interesting behavioral insights. We find that, although putting more effort into the task did improve the performance, our main treatment effect still persists even if we control for effort. On top of that we see that those who put in more effort do not 'suffer' less from the negative effect of having previously faced good defaults. Furthermore, the fact that we observe a significant treatment effect for males but not for females is an intriguing finding.

In conclusion our results show that being nudged can affect subsequent decisions and that the choice environment faced influences the extent to which people rely on certain choice heuristics. We hope that these findings induce further research into the wider effects of choice-architecture.

5.5. Appendix A: Experimental Instructions

General instructions

Welcome to this experiment on decision-making. Please read the following instructions carefully.

When everyone has finished reading the instructions and before the experiment starts, you will receive a handout with a summary of the instructions.

During the experiment you will be asked to make a number of decisions. Your decisions will determine your earnings. Your earnings will be privately paid to you in cash at the end of the experiment.

The experiment will consist of 50 rounds. In each round you will face a choice task with which you can earn credits. The choice task will be explained on the next page. At the end of the experiment **one of the rounds will be randomly selected**. Your payoff will be determined by the amount of credits you earned **in that round**. For each 10 credits you earn, you will receive 1 euro.

Instructions: choice task

In each round you have to select one option from a list. An example is shown below. Each option generates a number of credits equal to the

sum of the points in each column multiplied by the weight of that column minus the price of the option:
Number of credits generated by an option = 6*points in column 1 + 5*points in column 2 + 4*points in column 3+ 3*points in column 4+ 2*points in column 5 - price.

Choice Task X						
TIME: 40 BONUS: 20						
Choices	Weight=6	Weight=5	Weight=4	Weight=3	Weight=2	Price
<input type="radio"/> Option 1	18	10	37	26	21	240
<input type="radio"/> Option 2	29	14	24	1	45	342
<input type="radio"/> Option 3	15	49	13	6	19	234
<input type="radio"/> Option 4	20	0	30	20	10	170
<input type="radio"/> Option 5	22	32	19	18	30	279
<input checked="" type="radio"/> Option 6	34	43	2	25	25	320

So for example, in the list above, option 4 will generate:
 $20*6+0*5+30*4+20*3+10*2-170=150$ points.

You can think of this choice task as choosing which product to buy from a set of similar products. The products have different characteristics and prices which determine the value of each product.

Instructions: making a choice

Choice Task X						
TIME: 40 BONUS: 20						
Choices	Weight=6	Weight=5	Weight=4	Weight=3	Weight=2	Price
<input type="radio"/> Option 1	18	10	37	26	21	240
<input type="radio"/> Option 2	29	14	24	1	45	342
<input type="radio"/> Option 3	15	49	13	6	19	234
<input type="radio"/> Option 4	20	0	30	20	10	170
<input type="radio"/> Option 5	22	32	19	18	30	279
<input checked="" type="radio"/> Option 6	34	43	2	25	25	320

Choosing an option

- You can select an option by clicking on the radio button next to the option.
- The selected option only becomes your choice if you press the "Make choice" button or when the time limit runs out.

Time limit

- For each choice task you will have a maximum of 40 seconds to decide.
- If you make a choice before the time expires you will earn a bonus.
- This bonus starts at 20 credits but decreases with 1 credit every 2 seconds until you have made your choice.
- The remaining time and the bonus are depicted at the top of the choice task screen.
- After making your choice you will go to the waiting screen for the remaining seconds and 5 extra seconds.

Default option

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- At the start of the round, one of the options will be the default option. This option is selected at the start of the round and the option row is shown in green.
- If you do not select any of the other options the default option will automatically become your choice when the time expires or when you press the "Make choice" button.
- If you make your choice before the time has expired and you do not choose the default option you will be asked whether you want to stick with your original choice or whether you would prefer the default option. The time keeps running until you choose either your original choice or the default option. If the time runs out you stick with your original choice.
- The default option can be seen as a recommendation to buy a certain product.

We would like to remind you that at the end of the experiment one round is randomly selected. Your earnings will be determined by the amount of credits you earned in that round. Remember, for each 10 credits you earn, you will receive 1 euro.

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Samenvatting (Summary in Dutch)

Menselijk gedrag is fascinerend en in mijn ervaring altijd weer net iets ingewikkelder dan gedacht. Hoe mooi dat ook is, het maakt het bouwen van economische modellen erg ingewikkeld. Menselijk gedrag vormt immers de basis van elke economisch model. Bestaande modellen gebaseerd op perfecte rationaliteit geven overduidelijk geen correcte beschrijving van werkelijke keuzes, maar zodra de veiligheid van volledige rationaliteit verlaten betreden we de “wildernis van de beperkte rationaliteit”. Gedragseconomisch onderzoek probeert daarin een weg te vinden en specialisatie is daarbij onontkoombaar. Er zijn immers onnoemelijk veel soorten beslissingen waarbij vele verschillende beslisprocessen meespelen.

Helaas brengt deze specialisatie ook nadelen met zich mee. Gespecialiseerde modellen zijn maar beperkt toepasbaar en juist het combineren van methodes kan nieuwe inzichten verschaffen. Het onderzoek gepresenteerd in dit proefschrift combineert daarom oude ideeën en methodes om nieuwe inzichten te krijgen in menselijk gedrag.

Hoofdstukken 2 en 3 rapporteren beide experimenten waarin zowel sociale preferenties als risico een rol spelen. Hoofdstuk 2 onderzoekt of een sociaal referentiepunt, de verdiensten van een relevante ander, risicovolle keuzes net zo beïnvloeden als andere referentiepunten. Dat blijkt niet het geval. Wanneer alle uitkomsten onder het sociale referentie punt liggen maken mensen meer risicomijdende keuzes dan wanneer alle uitkomsten boven het sociale referentie punt liggen. Bij andere referentiepunten gebeurt juist het omgekeerde.

Hoofdstuk 3 bekijkt een andere manier waarop sociale preferenties risicovolle keuzes kunnen beïnvloeden. We testen verschillende sociale preferentie modellen in een experiment waarin mensen de verdiensten van anderen niet kunnen veranderen. We vergelijken keuzes in situaties met en zonder de mogelijkheid tot sociaal vergelijken. Sociale preferentie modellen die uitgaan van preferenties over uitkomsten voorspellen dat de introductie van sociaal

vergelijken leidt tot andere keuzes, maar de geobserveerde keuzes zijn vrijwel hetzelfde. Deze uitkomst is wel in overeenstemming met sociale preferentie modellen gebaseerd op het volgen van regels zoals een voorkeur voor procedurele eerlijkheid.

Hoofdstuk 4 combineert twee methodes die niet vaak samen gebruikt worden, de strategiemethode en evolutionaire simulaties, om het zogenaamde “minderheidspel” te bestuderen. Dit spel is een gestileerde weergave van veel relevante economische situaties. Proefpersonen leveren via internet een strategie om het spel voor het te spelen. Vervolgens worden deze strategieën geanalyseerd door het hen in een evolutionaire simulatie tegen elkaar op te laten nemen. We vinden onder andere dat mensen vaak expliciet randomiseren terwijl veel bestaande simulaties die mogelijkheid uitsluiten. Strategieën die succesvol zijn in de evolutionaire simulatie bereiken een hoog niveau van efficiëntie.

Hoofdstuk 5 behandelt een nieuwe vorm van economisch beleid, libertair paternalisme. Bij libertair paternalisme probeert de beleidsmaker mensen betere keuzes te laten maken zonder hun keuzes te beperken. De beleidsmaker maakt gebruik van bekende gedragspatronen om de negatieve gevolgen van andere gedragspatronen tegen te gaan. Er wordt bij het evalueren van libertair paternalistische maatregelen impliciet van uit gegaan dat deze maatregelen wel tot andere keuzes leiden, maar geen gevolgen hebben in keuzesituaties die de beleidsmaker niet wil beïnvloeden. Er bestaat echter bewijs dat (recente) ervaringen invloed hebben op het maken van keuzes. Wij passen dat idee toe op libertair paternalisme door te kijken wat het gevolg is van libertair paternalistische maatregelen op latere beslissingen.

Specifiek kijken we wat er gebeurt als mensen die gewend zijn geholpen te worden door libertair paternalistische maatregelen niet meer geholpen worden. We zien dat zulke mensen slechtere beslissingen nemen dan mensen die nooit zijn geholpen. De reden is dat ze keuzes blijven maken op een manier die goed werkt in de omgeving met libertair paternalisme, maar niet in de nieuwe omgeving.

The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus University Rotterdam, University of Amsterdam and VU University 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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Economic experiments have challenged traditional assumptions about, and increased our understanding of, human behavior, resulting in more realistic models. Specialization, both in terms of methods and of situations studied, has contributed considerably to the success of this research program. At the same time specialization makes predicting behavior difficult when many different elements play a role.

This dissertation combines four papers which try to relieve this problem by combining ideas from several specialization. The focus lies on linking ideas from social preferences with ideas on decision making under risk. Chapter two shows that a social reference point can influence risky decisions in unexpected ways. Chapter three uses individual risky decisions to distinguish between different social preference models.

The last two chapters combine other ideas. Chapter four combines the strategy method with evolutionary simulations to study the minority game. Chapter five applies the idea that prior experiences can influence decisions to libertarian paternalism.

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