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Alexander Rajko

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1 Introduction

Behavioural economics is a paradigm bringing together established economic methodology such as game theory and additional insights about human agency taken from psychology. Essentially, it accounts for a higher psychological realism, explicitly considering the psychological effects involved in economic decision making. Thus behavioural economics extends the standard assumptions of economics and increases the predictive power of economic models (Rabin, 2002). The basic idea for this interaction of disciplines dates back to H. A. Simon (1955).[1] By now, behavioural economics is an established field of economics in its own right with steadily growing importance for economics research (Rabin, 2002; Smith, 1991).[2] Regarding its methodological foundation, behavioural economics makes extensive use of experimental economics (Loewenstein, 1999).[3] Furthermore, it is also concerned with new economic theories accounting for behavioural effects and the analysis of empirical data.[4]

This thesis dwells upon *topics in behavioural economics: information and fairness*. It presents five studies, the first four focusing on the usage of information, the fifth being directed at fairness. The studies on information cover a broad range of topics in the economics literature. The first two are concerned with auction theory, i.e. the second-price sealed-bid and the English auction. The contributions in this thesis extend these formats with *information acquisition behaviour*. This behaviour has gained recent attention with some theoretical works, but has not been tested experimentally, yet. The first of the two studies focuses on instrumental information, i.e. the private value of an object at auction which is unknown to the bidder ex-ante. The value of this information can be assessed according to a rational choice model (Compte and Jehiel, 2007). The experimental results falsify the rational usage of information acquisition strategies and motivate the second study, where the *demand for non-instrumental information*, i.e. information which cannot be used for a rational bidding strategy, is analysed in a similar auction context. The third contribution addresses the game-theoretic framework of

[1] An anthology of behavioural economics is provided by Camerer and Loewenstein (2004).

[2] Some useful overviews on the field of behavioural economics can be found in Earl (1990), Smith (1991), Rabin (1998) and recently DellaVigna (2009). Etzioni (2011) provides an outlook on future directions for the evolution of the behavioural economics paradigm.

[3] Also field experiments become increasingly popular as a research approach in behavioural economics (e.g. Harrison and List (2004), Levitt and List (2009)).

[4] Camerer, Loewenstein and Rabin (2004) give a comprehensive overview on the current state of the art regarding research in behavioural economics.

global games (Carlsson and van Damme, 1993; Morris and Shin, 1998, 2003). In this framework, current theory makes opposing predictions for two kinds of uncertainty, i.e. risk and ambiguity (Ui, 2009). These theoretical predictions are put on test with an experimental analysis of *risk and ambiguity in global games* framed as a speculative attack game. The following study deals with *disclosing conflicts of interest*. Here the experimental design is based on an established theory for a game with strategic information transmission (Crawford and Sobel, 1982), where one player has a monetary incentive to deceive the other player. When this conflict of interest is made transparent, the extent of deceptive behaviour increases significantly. Finally, the last contribution immerses in the realm of economic fairness and the assumption of economic agents having social preferences (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999). In this regard, the standard approach of one-dimensional fairness is extended and *two-dimensional fairness in a real effort game* is studied. As a result, the exploitation of a self-serving bias effect, i.e. players distorting fairness norms to their own advantage whenever possible, is substantiated with experimental data. The remainder of this section gives a chapter by chapter overview on the main results, obtained within this thesis.

For chapter 2, which is joint work with Vitali Gretschko, one can state that information acquisition is crucial to almost all real world auctions, where one's valuation usually is fully unknown or at least very imprecise. This is already addressed by some of the recent theoretical work. We extend the literature, providing the first experimental test of auctions with explicit information acquisition in terms of acquisition strategies, timing and bidding behaviour. In doing so, we focus on the second-price sealed-bid and the English auction. Firstly, the main finding is excess information acquisition in both auction formats. For costs of information well above its rational value, a majority of subjects still chooses to acquire information. This effect is robust to learning and does not fade in the course of the experiment. Secondly, even if subjects make the optimal decision whether to purchase information; they prematurely buy in the English auction. In theory the English auction offers more information, as players can observe the drop out decisions of their competitors. However, in our experiment the subjects fail to use this advantage of the dynamic format. As a final result both informed and uninformed bidders significantly underbid in our experiment. Further it is shown how risk aversion also fails explaining the observed behaviour. However, assuming that bidders anticipate regret that they observe ex-post due to paying too much in an auction, we can extend the initial model and accommodate the experimental results.

In chapter 3 information acquisition behaviour in auctions is addressed from a different perspective. Already having established the effect of excess information acquisition, this work investigates if and how subjects acquire non-instrumental information. Therefore, a second-price sealed-bid auction is conducted, where all strategically relevant information is free but additional non-instrumental information is sold in the course of the auction. In this experiment, the two main effects found in the data are a significant acquisition of non-instrumental information and an underbidding effect, when scrutinising the bidding strategies. The acquisition of non-instrumental information goes along with the results from the previous study and supports some preference for confidence. Moreover, only after buying the non-instrumental information the bidding strategies exhibit a strong underbidding effect. This is a finding rarely observed in second-price auctions.

Chapter 4 is joint work with Christopher Zeppenfeld. In this contribution global games are studied experimentally. These games of incomplete information are very important, when it comes to applications such as market choices, refinancing company debts or speculative attacks. We use a model of global games based on maximising minimum expected utility, which predicts opposite effects for the behaviour in equilibrium under risk and ambiguity. This prediction is tested with an experiment based on the speculative attack game. We find that under both types of fundamental uncertainty subjects almost always play undominated switching strategies, i.e. they do not choose one strategy when they know that the other strategy yields a higher pay-off with certainty. However, only few adhere to unique cut-off values, where they would always switch their strategies from the safe to the risky action. Furthermore, when estimating cut-off values we find excess aggressiveness for subjects' behaviour, i.e. the risky action is chosen for much lower signals than the theoretical optimum predicts. This can be rationalised as a best response to the belief in others being overly optimistic and aggressive. Finally, our main finding regards the opposite theoretical predictions for risk and ambiguity. Accordingly, risk about the distribution of signals should improve the coordination in equilibrium, whereas ambiguity should reduce is. This must be falsified based on the experimental data, where we find no significant difference in estimated individual cut-off values or aggregate coordination between risk and ambiguity.

Chapter 5 is joint work with Axel Ockenfels and Roman Inderst. It presents an experimental study on disclosing conflicts of interest. Conflicts of interest occur in situations of advice giving between a client and a better informed advisor, as it is often found in the financial services industry. Economic theory captures this matter by means of principal-agent models or games of information transmission. We experimentally investigate how different degrees of transparency about a conflict of interest influence individual decision making in a game of strategic information transmission. It turns out that increasing transparency about the conflict of interest has a significant effect on both the deceptive behaviour of advisors and the strategic response behaviour of advisees. The advisors do deceive, but not to an extent which would be predicted by standard theory. Hence we find an overcommunication effect, where the messages sent between the two players still contain valuable information. Similarly, when knowing about a prevailing conflict of interest, advisees do not sufficiently discount the biased information they receive.

Economic fairness is scrutinised both theoretically and experimentally in chapter 6, in a joint work with Georg Gebhardt. The impact of fairness on economic decision making in terms of monetary distributions is already well established. However, in many real life encounters the issue of multi-dimensional fairness may play a crucial role, i.e. payment, effort and ability might all shape our perception of fairness to some extent. To formalise this notion, we extend a standard model of social preferences with working effort as a second non-monetary dimension. We then devise an experimental design to investigate the effect of two-dimensional fairness in a meaningful real effort game. With asymmetric endowments unfairness is induced in the initial distribution and allows us to study redistribution behaviour based on working times. The redistribution follows standard protocols of a dictator and an ultimatum game, respectively. The experimental data proves that our approach of two-dimensional fairness is feasible and subjects employ both dimensions to establish overall fairness. Hence, when providing more effort, the subsequent offers in monetary terms are steadily declining. With our theoretical model extension we can estimate the conversion factors for time and money for all subjects, assuming standard fairness parameters. Subsequently, the experimental data proves that in the dictator treatment subjects exhibit a self-serving bias distorting the usual fairness norms by taking a higher conversion factor, for their personal advantage.

The final chapter 7 summarises the main insights and pinpoints directions for further research.

2 Excess Information Acquisition in Auctions

2.1 Introduction

Auctions are one of the most important mechanisms for the efficient allocation of goods.[5] Procurement auctions, spectrum auctions, eBay auctions, google AdWords and many more are examples for market places, where goods worth billions of dollars are sold via auctions (Lucking-Reiley, 2000a; Varian, 2009). Their importance is also stylised by extensive scholarly work during the last decades. The theoretical literature has analysed various auction mechanisms in great detail and identified some principles such as the revenue equivalence theorem (Milgrom and Weber, 1982; Myerson, 1981). The experimental literature has brought to light some robust behavioural patterns such as overbidding in first price auctions with private values or the winner's curse in common value auctions (Charness and Levin, 2009; Harrison, 1989; Kagel and Levin, 1986). Nevertheless, the impact of information acquisition in standard auction formats has found little attention so far (Compte and Jehiel, 2007).

However, information acquisition is a matter with high relevance to auctions. For example, in spectrum auctions, additional information is usually acquired by means of technical research about the infrastructure, internal reports on future revenues or experts' opinions on the valuations of competitors. The same logic also applies to corporate takeovers, where ex ante the information about corporate valuations is unknown or at least unreliable. The typical large datasets from auction platforms cannot help explaining the effects of information acquisition, as these costs usually materialise outside the auction itself and hence cannot be observed on such platforms, even ex post. Therefore, we use a laboratory experiment directly attune to a rational choice model of auctions with information acquisition to study its effects on information acquisition behaviour and it dynamics. Our experiment implements two standard auction mechanisms, i.e. a second-price sealed-bid and an English auction with independent private values. Both formats are augmented with the opportunity of buying information about one's valuation at any time during the auction. Prior to their information acquisition subjects only know the distribution of their valuations, but not their precise value. Based on our exper-

[5] This chapter is joint work with Vitali Gretschko.

imental results we provide three new and robust insights into the behaviour in such auctions. Firstly, subjects follow excess information acquisition strategies compared to the predictions of the rational choice model. Secondly, in the dynamic format of the English auction subjects also fail to employ an optimal timing for their information acquisition and thus buy prematurely. Finally, in terms of the bidding strategies we find an underbidding effect, which is independent of the previous information acquisition decision. Furthermore, we proceed by extending the initial model with both risk aversion and anticipated regret. This yields that only anticipated regret can explain both excess information acquisition and underbidding.

The remainder of this paper unfolds in five sections. In section 2 the theoretical foundations are laid and the equilibrium properties for auctions with information acquisition are characterised. Next the experimental design and procedures are presented. Then section 4 discusses our experimental results. Section 5 shows that risk aversion and loss aversion cannot accommodate our results, but regret avoidance can. Finally the main insights are summarised and an outlook on subsequent research on auctions with information acquisition is provided.

2.2 Model and Equilibrium Properties

2.2.1 Related Literature

In the literature auctions with information acquisition are a relatively new branch. Hence, there are only few papers explicitly concerned with information acquisition in the context of auctions and to the best of our knowledge, there are no experimental or empirical studies. The theoretical work largely focuses on the comparison of different auction formats in terms of information acquisition strategies, revenues and efficiency. The second-price sealed-bid auction and the English auction are the most prominent formats when it comes to studying information acquisition in independent private value environments.

Matthews (1984) is the first to address the bidding and information acquisition strategies in a pure common value first-price sealed-bid auction. Thereby he focuses on the effects on efficiency and seller revenue. Hausch and Li (1993) extend this work by comparing first-price and second-price auction. They find that bidders shade their bids by the amount invested in information and thereby diminish seller’s revenue. Nevertheless, the second-price auction dominates the first-price auction in terms of revenue. Bergemann et al. (2009) are interested in the impact of endogenous information acquisition on efficiency in an interdependent value setting. They employ a mechanism design approach and show that increasing the degree of correlation between the valuations is diminishing the efficiency. For the case of affiliated valuations, Persico (2000) has shown that there should be more information acquisition in the first-price than in the second-price auction. Finally, if there is a private and a common value component, Hernando-Vecina (2009) argues not only revenue but also efficiency under information acquisition is higher in the English auction than in the second-price auction. One of the first works on information acquisition in independent private value auctions was conducted by Lee (1985), who shows that in a first-price auction the endogenous entry decisions are deterred by an increasing amount of information acquisition. Guzman and Kolstad (1997) find an equilibrium of a first-price auction when the cost of information acquisition is private information. Shi (2007) characterises the optimal auction based on the assumption that information can be acquired prior to the auction start. He shows that the optimal symmetric mechanism is a standard auction with a reservation price that depends on the ex ante mean valuation of the bidders.

Contrary to the models above, the following body of literature allows for information acquisition not only prior to the auction but also during the auction. The simplest way of modelling mid-auction information acquisition is to allow for multi-stage auctions as proposed by Engelbrecht-Wiggans (1988). He shows that allowing for multiple bidding rounds with bid disclosure increases the amount of information acquisition and thereby the revenue in a second-price auction. Parkes (2005) extends this idea in a general mechanism design setting and shows by computer-based simulations that the English auction achieves a higher allocative efficiency than a second-price auction. Rasmussen (2006) analyses the incentives to acquire information in an eBay style auction and shows that information acquisition might be a explanation for sniping and incremental bidding on eBay. Rezende (2005) finds an equilibrium of the English and the second-price auction in a very general informational setting. He shows that bidders in the English auction buy more information and place higher bids than in the second-price auction. This leads to higher revenue in the English auction if the number of bidders grows large. This result is driven by the fact that bidders may condition their information acquisition decisions on the observed price in the English auction. Contrary to that, Compte and Jehiel (2000, 2007) allow for the bidders to observe the remaining competitors in the English auction. They show in a setting where some of the bidders are perfectly informed and some of the bidders are perfectly uninformed that more information is acquired in the English auction than in the second-price auction and thereby the revenue is higher in the former if the number of bidders grows large. The results obtained by Compte and Jehiel (2007) are very intuitive and don't demand for sophisticated bidding strategies. Hence, their set up is well suited for a first experimental investigation of bidders' behaviour in auctions with information acquisition.

2.2.2 The Rational Expected Value Model and Equilibrium Properties

In the following we characterise the equilibrium behaviour in an English auction and in a second-price sealed-bid auction with one fully uninformed bidder. Our model is a special case of the model in Compte and Jehiel (2000).[6] More precisely, N risk neutral bidders are competing in an auction for an indivisible object. Each bidder i assigns a value of v_i to the object. The valuation is independently and identically distributed on $[0,100]$ according to the uniform distribution. Before the auction starts, all bidders but bidder 1 observe their private valuations. Bidder 1 is only informed about the distribution of the valuations.

In the second-price auction bidder 1 decides before the auction starts whether to learn his true valuation at price $c \in \mathbb{R}_+$. After the information acquisition decision all bidders simultaneously submit a bid for the object. The bidder with the highest bid wins the auction and pays the second highest bid to the auctioneer. In equilibrium it is a weakly dominant strategy for the informed bidders to bid their valuations.[7] If bidder 1 remains uninformed, his best reply to the bidding strategies of the other bidders is to bid $E[v_1]$. Hence, he will acquire information before the auction starts, if the expected utility of acquiring information is higher than the expected utility of not acquiring information:[8]

$$(1) \qquad E[\max(v_1, v^{(1)}) - v^{(1)}] - c \ \geq E[\max(\mathrm{E}[v_1], v^{(1)}) - v^{(1)}].$$

We summarise this finding in Proposition 1.

Proposition 1

In a second price auction bidder 1 acquires information if and only if

---

[6] In the following we draw upon the results of Compte and Jehiel (2000).

[7] Remember the auction for the informed bidders is identical to a the standard second price auction, thus for these bidders the standard results as in Vickrey (1961) also hold.

[8] Let $v^{(1)}$ denote the highest order statistic of n-1 independent draws from the uniform distribution between 0 and 100.

$$E[\max(v_1, v^{(1)})] - E[\max(\mathrm{E}[v_1], v^{(1)})] \geq c\ .$$

A bidder who is informed about his valuation bids according to $b = v_1$. If bidder 1 chooses to remain uninformed he bids according to $b = E[v_1]$.

Proof See Compte and Jehiel (2000) ■

In the English auction a price clock starts at a price of 0 and continuously increases. At each price p bidder 1 may decide whether to learn his true valuation at a cost $c \in \mathbb{R}_+$ and whether to stay in the auction or not. All other bidders only decide on whether to leave the auction at price p. Each dropping out decision is commonly observed by all bidders. The last bidder remaining in the auction receives the object at the price at which the last opponent dropped out. Again bidders who are informed about their valuation v_i have the weakly dominant strategy to drop out whenever $p = v_i$. Hence, it remains to determine when the only uninformed bidder 1 will acquire information. For this purpose two functions are defined:

$$H(p, c) \coloneqq E[\max(v_1, v_2) - v_2 | v_2 \geq p] - c \text{ and}$$

$$K(p) \coloneqq E[\max(E[v_1], v_2) - v_2 | v_2 \geq p].$$

The values $H(p, c)$ and $K(p)$ correspond to bidders 1's expected payoff when the current price is p and one other bidder remains active. At this point bidder 1 should decide to acquire information and remain active up to his true valuation or not to acquire information and drop out at $\min(p, E[v_1])$. Note that until only one other bidder remains, bidder 1 does not have to make this decision.

For a full characterisation of the equilibrium we define for each c:

(2) $$p^{**}(c) = \sup\{p \in [0,100] | H(p, c) \geq K(p)\} \text{ and}$$

(3) $$p^{*}(c) = \inf\{p \in [0,100] | E[\max(p, v_1) - v_1] - c \geq 0\}.$$

For illustrative purposes the two relevant price levels and the resulting information acquisition strategies are depicted in the following figure 2.1:

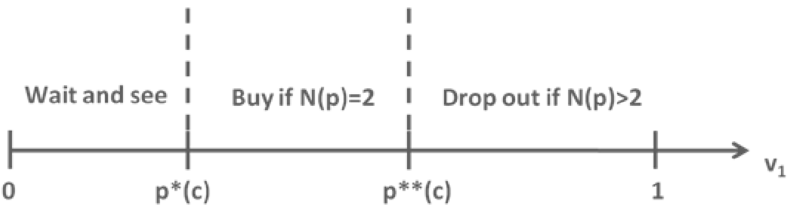

Figure 2.1: Equilibrium Information Acquisition Decision of Bidder

Here $p^{**}(c)$ is the highest p such that the expected payoff from buying information at p exceeds the expected payoff from not buying information, and $p^{*}(c)$ is the lowest p such that the information cost is lower than the expected loss from buying at a price above valuation. Finally, $N(p)$ denotes the total number of remaining bidders when the current price is p. Then the equilibrium behaviour is characterised as follows:

Proposition 2

If $\mathrm{N(p)} > 2$:

Bidder 1 does not acquire information and stays in if $\mathrm{p} < max\{\mathrm{p}^{*}(\mathrm{c}), \mathrm{E}[\mathrm{v}_1]\}$.

If $\mathrm{N(p)} = 2$ two cases are relevant:

1) $\mathrm{p}^{*}(\mathrm{c}) > \mathrm{p}^{**}(\mathrm{c})$:

   Then bidder 1 never acquires information and drops out at $\mathrm{E}[\mathrm{v}_1]$.

2) $p^{*}(c) < p^{**}(c)$:

   a) If $p \in [p^{*}(c), p^{**}(c)]$, bidder 1 acquires information and drops out immediately if $p > v_1$ and stays in the auction as long as $p < v_1$.

   b) If $p > p^{**}(c)$ he drops out at $\min\{E[v_1], p\}$.

   c) If $p < p^{*}(c)$ stays in and acquires information at $p^{*}(c)$ and drops out immediately if $p > v_1$ and stays in the auction as long as $p < v_1$.

   Bidders $2, \dots, N$ drop out when the price reaches his valuation, i.e., at $p = v_i$.

Proof See Compte and Jehiel (2000) ■

In the English auction it is sufficient for bidder 1 to know whether his true valuation is below the current price to avoid buying at an unfavourable price. As long as more than one competitor is still in the auction the probability of winning the object at the next price increase is 0. Hence, it is a dominant strategy not to buy information and to observe how strong the competition is. As soon as only one competitor is left in the auction, bidder 1 has to trade off the cost of information acquisition, the probability of winning and the risk of buying at an unfavourable price.

Having characterised the equilibrium behaviour in both auctions we can derive our hypotheses for the experiment. The first two hypotheses are concerned with the information acquisition decision of the uninformed bidder 1.

Hypothesis 1: If the cost of information acquisition is high, bidder 1 always refrains from information acquisition in the second-price auction. If the cost of information acquisition is low, bidder 1 always buys information.

Hypothesis 2: In the English auction bidder 1 never acquires information as long as more than one other bidder is active in the auction. If only one other bidder is left in the auction, the timing of the information acquisition is given by Proposition 2.

The next two hypotheses are making predictions regarding the bidding strategies of bidder 1.

Hypothesis 3: If bidder 1 learns his true valuation, he bids truthfully in the second-price auction and drops out of the English auction as soon as the price has reached his valuation.

Hypothesis 4: If bidder 1 remains uninformed in the second-price auction, he bids his expected valuation. If bidder 1 remains uninformed in the English auction he drops out at $\max\{p^{**}(c), E[v_i]\}$.

2.3 Experimental Design and Procedures

For our experimental design we followed the basic structure of the already characterised theoretical model.[9] Accordingly, in every auction group only one player did not know his valuation ex ante. The other players always had perfect information about their valuations. We chose groups of four players and, as we are only interested in the behaviour of the one uninformed player per group, we opted to implement the three remaining players as bidding robots. These robots were programmed to always bid their true valuation, which was also explained to the human subjects in the instructions. The uninformed human bidder only knew that his valuation is uniformly distributed between 0 and 100. Moreover, it was common knowledge that the valuations of the three bidding robots were drawn from the same distribution.

The main innovation of our experiment is the investigation of information acquisition in auctions. Hence we offered the human participants with unknown valuation to buy their valuation at a certain cost. This cost parameter was varied between the treatments in order to test our theoretical predictions. Following the main hypotheses low cost (c = 2) and high cost (c = 8)

[9] For the full experimental instructions refer to the appendix.

were chosen. Furthermore, for studying the timing of information acquisition behaviour in a dynamic environment, we used two different standard auction formats; the second-price sealed-bid auction as a static and the English auction as a dynamic format. The object at auction was not specified, and profits for the auction were calculated as one's valuation minus the final price and, if applicable, minus the cost for information acquisition. This gives the following 2 x 2 design for the four experimental treatment variations:

	Information Costs	
Auction Format	Low Cost	High Cost
2nd Price Auction	c = 2	c = 8
English Auction	c = 2	c = 8

Table 2.1: Experimental Treatments

In the second-price auction, information acquisition was only possible prior to the auction, i.e. before submitting the individual sealed-bid offer. During the English auction information could continuously be acquired. Here we implemented a pause button, enabling subjects to pause the price clock at any time to buy information and reflect upon this information. Thus we can rule out any time pressure effects, shaping the decision of buying information or quitting the auction.[10] The price clock was implemented to increase by 1 ECU every 2 seconds which is similar to other experiments on English auctions (Levin et al., 1996).

Overall, every auction format was repeated for 20 rounds and valuations for all players were redrawn every round. As feedback we provided the subjects with the information whether they won the auction, at what price the auction was won, what their final bid was and what the subject has won or lost in this round including the information costs. Hence learning one's valuation remained costly. If the human bidder won the auction we additionally gave feedback regarding his valuation, which was then known anyway, and made it easier to validate the profit calculations. As it was crucial for us that subjects did assess the real costs of information acquisition, we stressed in the instructions that all losses in experimental money must also be covered in real money after the experiment.

[10] Our design benefited from the implementation of bidding robots, so that pausing of an auction for the whole group of four players did not signal any additional information to the already fully informed and programmed robots.

The numerical predictions for the behaviour in our experiment are directly computed from the equilibrium characterisation in section 2. For the second-price auction we expect the following rational behaviour:

Treatment	**Information Acquisition**
2^{nd} Price Auction (low cost)	always
2^{nd} Price Auction (high cost)	never

Table 2.2: Predictions for Information Acquisition in 2^{nd} Price Auction

For the English auction we also expect no information acquisition in the high cost treatment, but can make a more sophisticated prediction regarding the timing of information acquisition in the low cost treatment.

Treatment	**Information Acquisition**	
	p*(c)	p**(c)
English Auction (low cost)	19.61	66.17
English Auction (high cost)	never	never

Table 2.3: Predictions for Information Acquisition in English Auction

According to the rational model information in this treatment is only acquired if the price clock is between 20 and 67.

The experimental sessions took place in April 2011 at the Cologne Laboratory for Economic Research (CLER). We had 30 subjects per treatment and 120 subjects overall participating. The average payment was 13.20€ including a guaranteed show-up fee of 2.50€. On average each subject participated in the experiment for 75 minutes. For the recruitment we used the online recruitment system ORSEE (Greiner, 2004) and the experiment itself was programmed with z-Tree (Fischbacher, 2007).

2.4 Experimental Analysis and Results

2.4.1 Excess Information Acquisition

First of all, we analyse the frequencies with which subjects harness information acquisition. For the high cost treatments, where the theoretical prediction was that information is never bought, the data in figure 2.2 shows that nevertheless subjects buy information in 59% respectively 51% of all auctions. This shows excess information acquisition compared to the theoretical prediction in both formats.[11]

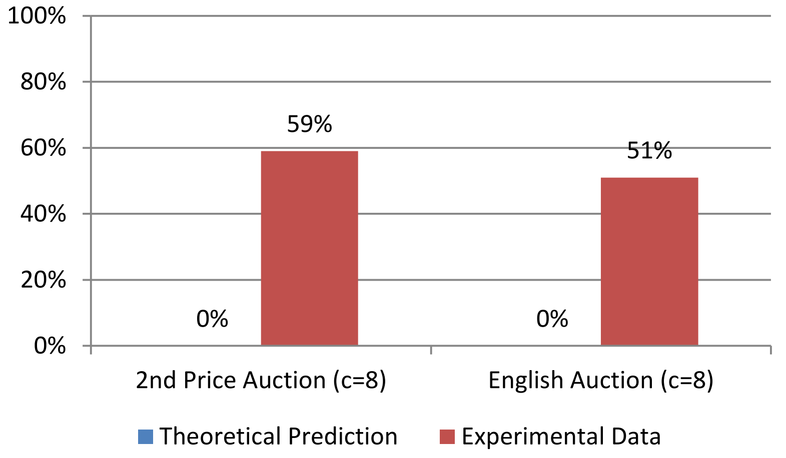

Figure 2.2: Frequency of Information Acquisition (High Cost)

Keeping in mind, that the optimal threshold for not buying information at all, was at information cost of 4.6, the subjects are obviously treating this cost differently than in our rational choice model. In the high cost treatment we chose deliberately high cost of $c = 8$ in order to make the decision of not buying information very clear. This strong effect of excess information acquisition offers an indication that the subjects overestimate the benefits of additional information. In section 5 of this paper, we will discuss different model extensions assessing this effect.

Having established the first effect, we compare the data from the two low cost treatments. Here the effect of excess information acquisition is also found in the dynamic format.[12] How-

[11] T-test: p-value < 0.0001.
[12] T-test: p-value < 0.0001.

ever, in the static format, we find less information acquisition than predicted.[13] Again both of the low cost treatments show information acquisition strategies in contrast with the fully rational, risk-neutral model.

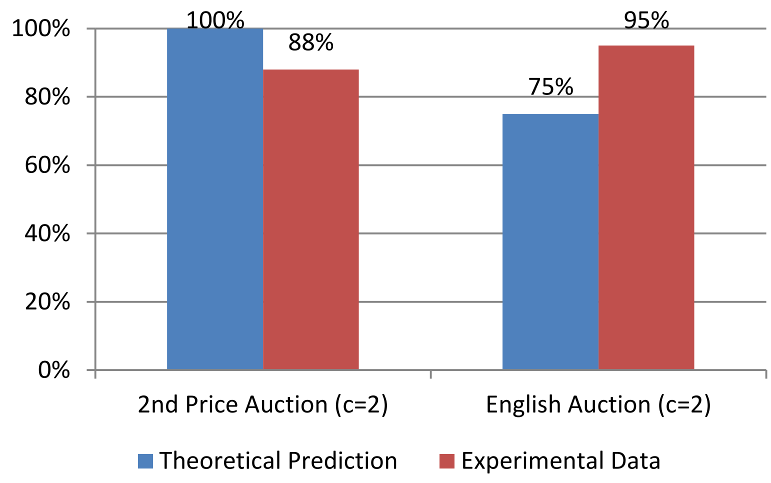

Figure 2.3: Frequency of Information Acquisition (Low Cost)

Figure 2.4 illustrates the robustness of these effects over the course of the auction rounds.

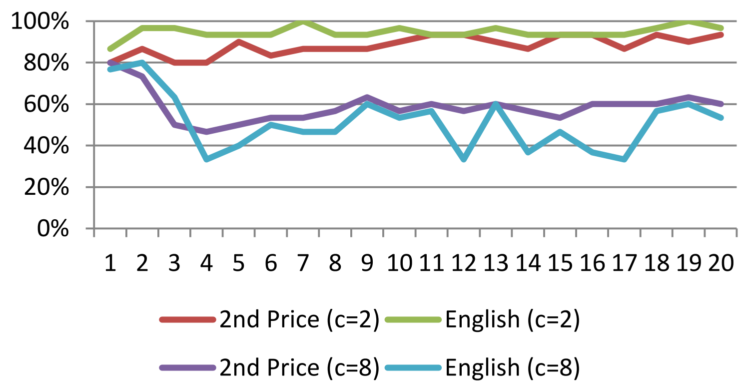

Figure 2.4: Development of Information Acquisition Frequencies

There is some adaptive behaviour during the first two rounds of the experiment and then aggregate behaviour converges to the already reported means in terms of information acquisi-

[13] T-test: p-value < 0.0001.

tion. The effect is likewise robust, when analysing the data for each individual subject, where we cannot find a significant amount of learning.[14]

2.4.2 Premature Information Acquisition

For the dynamic format, i.e. the English auction in our experiment, the theory does not only make predictions about the optimal frequencies of information acquisition, but also about the optimal timing of information acquisition. Therefore we first analyse the average price clock at time of information acquisition and compare it with the optimal value p*:

Treatment	Average Price Clock at IA	Prediction p*
English (c=2)	2 ECU	19.6 ECU

Table 2.4: Price Clock at Information Acquisition (English Auction)

We find premature information acquisition. In the low cost treatment, given our realisation of the random variable, where subjects should optimally buy at a price of at least 19.6 ECU, subjects on average buy almost immediately after the start of the auction at a price of 2 ECU. That means after observing the competitors' behaviour for at most 4 seconds and knowing that the auction could be paused at any time. Further, as long as there are at least two competitors remaining, the probability of the auction terminating at the next price step is virtually zero. Hence the pivotal information acquisition decision in the dynamic format should factor in the additional information of the number of competitors. This information allows the subjects to learn about the valuations of the competition at no cost and no risk. Table 2.5 corroborates the initial finding of premature information acquisition, as subjects fail to wait for the previous bidders to drop out before they decide on whether information should be bought. In the low cost treatment, where information should be bought in 75% of the auctions, when only one bidder is remaining, only 2.6% of information acquisitions can be classified as optimal in that sense.

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[14] See the appendix for details.

Treatment	Percentage of IA with 1 opponent remaining	Prediction	Percentage of IA with ≥ 2 opponents remaining	Prediction
English (c=2)	2.6 %	75 %	97.4 %	0 %
English (c=8)	20.2 %	0 %	79.8 %	0 %

Table 2.5: Number of Opponents at Information Acquisition (English Auction)

Interestingly, in the high cost treatments, where according to the rational model information acquisition should never occur, subjects who nevertheless engage in information acquisition do so after observing the competition longer and hence factoring in previous drop outs.[15]

2.4.3 Underbidding Behaviour

Next, we consider the bidding strategies employed by the subjects. First of all, the bidding strategies where the bidder chose to buy the information and thus had been perfectly informed. Here we can calculate the deviations between the bids and valuations, for all auction rounds where a subject has bought information.[16]

Treatment	underbidding	valuation bidding	overbidding
2nd Price (c=2)	35.9%	41.4%	22.7%
2nd Price (c=8)	40.9%	33.5%	25.6%
English (c=2)	51.6%	41.6%	6.8%
English (c=8)	54.5%	25.6%	19.9%

Table 2.6: Bidding Strategies with Information Across Treatments

Valuation bidding is defined as exactly bidding one's valuation, if informed. Respectively, underbidding is every bid under the valuation and overbidding every bid above the valuation.

[15] This finding is consistent with the price clock data from the high cost treatment, where the average price clock at information acquisition is at 12 ECU.

[16] For the English auction format we could only analyse auctions, where information had been bought and the previously uninformed bidder did not win the auction. In this format all auctions ended once the second last bidder dropped out, so that we could not observe the full bidding strategy of a winner. Nevertheless, under the conditions of information was bought and the player did not win, 57% (c=2) respectively 35% (c=8) of the English auctions still qualify for this analysis.

Table 2.6 shows that we do not find overbidding, but in fact underbidding in our subjects' behaviour. A result that is different from many the standard results received in second-price auctions (Cooper and Fang, 2008; Kagel and Levin, 1993).

Additionally, we consider the bidding strategies without information. As in the low cost treatment almost all subjects always bought the available information, this data is not reliable, as the remaining few observations are probably subject to a strong selection effect or mere errors.

Treatment	**Mean Bid**	**Prediction**
2nd Price (c=8)	40.6 (1.74)	=50
English (c=8)	36.7 (1.64)	>50

Table 2.7: Mean Bids without Information in the High Cost Treatments

However, for both high cost treatments we have an about 50% rate for information acquisition which is not due to learning or timing effects, but highly stable. Therefore we can report the mean bids for these treatments in table 2.7. Given our parameterisation, the optimal bid in the second-price auction should be 50 and in the English auction without information the optimal bid respectively time for dropping out should be between 50 and 66.2. On average, this must be strictly greater than 50. Again, the experimental results clearly depart from these predictions. In both treatments we find severe underbidding, which is difficult to explain on the grounds of our initial model.

2.4.4 Further Analyses

The robustness of our main results is further corroborated with the results of regression analyses. For the result of excess information acquisition we run logit regressions with random effects, separately for the two formats. These confirm that the round of the auction does not affect the information acquisition behaviour. Also the outcome of the previous round does not significantly influence the information acquisition behaviour, as one might argue.

For the bidding behaviour we run standard linear regression as reported in table 2.8. Here the dummy variable *high_cost* takes the low cost treatments as a baseline for comparison and shows significantly lower bids for the high cost treatments.

Dependent Variable: Bid	
Variables	**Model 1**
Intercept	7.50***
	(2.167)
High_Cost	-1.67**
	(0.771)
Dynamic_Format	-9.45***
	(0.823)
Valuation	0.61***
	(0.017)
Buy_Info	16.34***
	(1.304)
Round	0.07
	(0.071)
N observations	2400
Prob > F	0.000
MSE	20.14
R^2	0.456

Table 2.8: Linear Regression (robust standard errors)

Next, the dummy variable *dynamic_format* takes the second-price auction as a baseline and confirms that the average bids in the dynamic format are significantly lower. The variable *valuation* takes the valuation associated with each bid and shows a highly significant relationship between valuation and bid. Therefore, we can exclude arbitrary behaviour or simple mistakes as an explanation for the intriguing underbidding effect as already described. Also in line with our hypotheses and results we integrate a dummy variable *buy_info* into the regression model and find that bids are significantly higher, when subjects have acquired information about their valuation. This does not conflict with the fact that we find underbidding for both cases, with and without information acquisition. However, it indicates that the underbidding effect must be much more prevailing without prior information acquisition. Finally, the control variable for *round*, associating the round with every bid, is not significant. This is further proving that there is no trend or learning effect in our data.

2.5 Discussion

As the risk-neutral rational choice model presented in section 2 is inconsistent with the experimental results we discuss possible alternative explanations of the observed bidder behaviour. We consider risk aversion and anticipated regret as potential explanations for our data.

2.5.1 Risk Aversion

Our hypotheses for the experiment were derived from a model with risk neutral bidders. In what follows, we show that assuming risk aversion fails to explain the excessive information acquisition, observed in the high cost treatment of the second-price auction. Moreover, we can show that risk-averse bidders would acquire less information than predicted by the risk-neutral model which contradicts our experimental findings even more. As a consequence, we reject risk-aversion as an explanation for the observed data.

Suppose bidder 1 is risk averse with a concave utility function $u(x)$. If bidder 1 decides not to buy information his bid b^* will be lower than $E[v_1]$. To see this consider the maximisation problem of bidder 1 once he decided not to buy information.

$$\max_b \int_0^b \int_0^{100} u(v_1 - v^{(1)})\, dv_1 dv^{(1)}$$

The first order condition for this problem is:

$$\int_0^{100} u(v_1 - b^*)\, dv_1 = Eu(v_1 - b^*) = 0.$$

We know that $E[v_1 - 50] = 0$. By definition all risk-averse individuals dislike zero-mean risks. It follows $Eu(v_1 - b^*) \leq 0$. Hence the optimal bid must be $b^* \leq 50$.

Given the equilibrium behaviour of the informed bidders the decision whether to buy information or not is the choice between two random variables. If bidder 1 decides to buy information his pay-off is:

$$\tilde{x} - c = \max\{v_1 - v^{(1)}, 0\} - c.$$

If bidder 1 decides not to acquire information, the pay-off is:

$$\tilde{y} = \chi_{\{v^{(1)} \leq b^*\}}(v_1 - v^{(1)}).$$ [17]

The maximal c^*, for which a risk-neutral bidder would acquire information, is $c^* = E[\tilde{x}] - E[\tilde{y}]$. We will show that $E[u(\tilde{x} - c^*)] - E[u(\tilde{y})] \leq 0$.

---

[17] $\chi_{\{\cdot\}}$ denotes the indicator function.

Firstly, define $\tilde{x}_0 \coloneqq \tilde{x} - E[\tilde{x}]$ and $\tilde{y}_0 \coloneqq \tilde{y} - E[\tilde{y}]$. Then with $\pi(w, u, \tilde{x}_0)$ denote the risk premium of $\tilde{x}_0$ at wealth level w. It follows:

$$E[u(\tilde{x} - c^*)] - E[u(\tilde{y})] \leq 0$$

$$\Leftrightarrow E[u(\tilde{x} - E[\tilde{x}] + E[\tilde{y}])] - E[u(\tilde{y})] \leq 0$$

$$\Leftrightarrow \; E[u(\tilde{x}_0 + E[\tilde{y}])] - E[u(\tilde{y})] \leq 0$$

$$\Leftrightarrow \; E[u(E[\tilde{y}] - \pi(E[\tilde{y}], u, \tilde{x}_0))] - E[u(E[\tilde{y}] - \pi(E[\tilde{y}], u, \tilde{y}_0))] \leq 0$$

$$\Leftrightarrow \; \pi(E[\tilde{y}], u, \tilde{x}_0) \geq \pi(E[\tilde{y}], u, \tilde{y}_0).$$

For small risks we can use the Arrow-Pratt approximation and the last statement holds true whenever $Var[\tilde{y}_0] \leq Var[\tilde{x}_0]$. For the parameterisation of the experiment we get: $Var[\tilde{y}_0] \leq 1.4$ and $Var[\tilde{x}_0] = 1.\bar{6}$, whenever $b^* \leq 50$.

We can conclude that risk-averse bidders have a smaller willingness to pay for information than risk-neutral bidders in the second-price auction. Accordingly, the maximum willingness to pay for information of risk neutral bidders is such that $E[\tilde{x} - c^*] = E[\widetilde{y}]$, i.e. the expectations of the relevant random variables are equal but the pay-off from not buying information and bidding b^* is less volatile. Risk-averse subjects then prefer not to buy information.

2.5.2 Regret Avoidance

In the following, we put forward regret avoidance as a model extension in line with our results. The ex-ante unknown valuations in the two auction formats might cause regret in two ways. First of all, the uninformed bidder might bid too high and as a result experience a negative pay-off. Secondly, the bidder might bid too low despite having the highest valuation and thus foregoing a win, which also causes regret ex-post. Filiz-Ozbay and Ozbay (2007) as well as Engelbrecht-Wiggans and Katok (2007) show that the experience of regret depends on the feedback provided. The subjects in this experiment only learn their valuation if they buy information. Hence they can suffer regret from paying too much. Since they remain uninformed otherwise, they cannot experience ex-post regret from not having won an auction despite having a high enough valuation. Based on this feedback procedure, only regret from overpaying can arise.

If a bidder anticipates such regret, it is already established for other experimental auctions that he changes his bidding strategy accordingly (Engelbrecht-Wiggans and Katok, 2007, 2008;

Filiz-Ozbay and Ozbay, 2007). Furthermore, we show that he likewise adapts his information acquisition strategy. Therefore we adapt the model of Filiz-Ozbay and Ozbay (2007) to the auction set-up provided with the initial model and used in the experiment.

Second-Price-Auction

Assuming a bidder does not acquire his valuation and bids according to his expected valuation, then winner's regret in the second-price auction is defined in the following utility function for bidder *i*:

$$(4) \qquad u_i\left(v_i, b^{(1)}\right) = \begin{cases} v_i - b^{(1)} & if\ v_i \geq b^{(1)}\ and\ i\ wins \\ v_i - b^{(1)} - r(b^{(1)} - v_i) & if\ v_i < b^{(1)}\ and\ i\ wins \\ 0 & if\ i\ loses \end{cases}$$

Here $b^{(1)}$ is the highest bid of the competitors and $r(\cdot): \mathbb{R} \to \mathbb{R}_+$ is the regret function, which is assumed to be non-negative and non-decreasing. The informed bidders do not experience regret, as they have the weakly dominant strategy of bidding valuation. However, if a bidder chooses to remain uninformed his best reply b^* to the other informed bidders it the solution to the following:[18]

$$(5) \qquad \max_b \int_0^b \int_0^1 \left(v_1 - v^{(1)}\right) - \chi_{\{v_1 \leq v^{(1)}\}}(v_1) r\left(v^{(1)} - v_1\right) dF(v_1) dF^{N-1}\left(v^{(1)}\right)$$

The first order condition amount to:

$$(6)\left(\int_0^{b^*} (v_1 - b^*) - r(b^* - v_1) dF(v_1) + \int_{b^*}^1 (v_1 - b^*) dF(v_1)\right)(N-1) f(b^*) F^{N-2}(b^*) = 0$$

If r is strictly positive on a subset of $[0,50]$ with Lebesgue measure larger than 0, it directly follows that: $b^* < E[v_1]$ and further:

$$(7) \qquad \int_0^{b^*} \int_0^1 \left(v_1 - v^{(1)}\right) - \chi_{\{v_1 \leq v^{(1)}\}} r\left(v^{(1)} - v_1\right) dF(v_1) dF^{N-1}\left(v^{(1)}\right)$$

$$\leq E\left[max\left\{E[v_1], v^{(1)}\right\} - v^{(1)}\right]$$

A bidder will acquire information if the expected utility of acquiring information is higher than the expected utility of remaining uninformed:

---

[18] $\chi_M(\cdot)$ denotes the indicator function with $\chi_M(x) = 1\ if\ x \epsilon M$ and $\chi_M(x) = 0\ otherwise.$

(8) $$E\big[max\{E[v_1], v^{(1)}\} - v^{(1)}\big] - c$$

$$\geq \int_0^{b^*} \int_0^1 \big(v_1 - v^{(1)}\big) - \chi_{\{v_1 \leq v^{(1)}\}} r\big(v^{(1)} - v_1\big) dF(v_1) dF^{N-1}\big(v^{(1)}\big)$$

Comparing (8) and (1) with using inequality (7) has the following result on bidder's behaviour accounting for anticipated regret. In the second-price auction with regret, there is a cutoff $c^r > c$ so that bidder 1 acquires information if $c \leq c^r$. Hence he only dispenses with the information acquisition for a cost $c > c^r$. Consequently, he acquires information for costs above the threshold predicted in the initial standard model. This prediction is consistent with the result of excess information acquisition. Moreover if bidder 1 remains uninformed, he bids only $b^* < E[v_1]$. This explains the experimental data exhibiting an underbidding effect in the high cost treatments, where the bidders remain uninformed.

English Auction

The utility function for bidder *i* with regret in the English auction is defined as:

(9) $$u_i(v_i, p) = \begin{cases} v_i - p & if\ v_i \geq p\ and\ i\ wins \\ v_i - p - r(p - v_i) & if\ v_i < p\ and\ i\ wins \\ 0 & if\ i\ loses \end{cases}$$

Here p denotes the price at which the last competitor left the auction and the regret function $r(\cdot)$ is assumed to have the same characteristics as for the second-price auction above. As a result informed bidders have a weakly dominant strategy in the English auction and thus never experience regret.

As already argued, in the English auction the information acquisition decision is only made, if one competitor remains. To formalise the trade-off between the cost of information acquisition, the probability of winning and the risk of buying at an unfavourable price including regret we define analogously to section 2.2.2:

$$H_r(p, c) := E[\max(v_1, v_2) - v_2 | v_2 \geq p] - c \text{ and}$$

$$K_r(p) := \int_0^b \int_0^1 (v_1 - v_2) - \chi_{\{v_1 \leq v_2\}} r(v_2 - v_1) dF(v_1) dF(v_2 | v_2 \geq p)$$

$H_r(p, c)$ and $K_r(p)$ correspond to bidders 1's expected payoff when the current price is p and one other bidder remains active. For a full characterisation of the equilibrium we define for each c:

(10) $p_r{}^{**}(c) = \sup\{p \in [0,1] | H_r(p,c) \geq K_r(p)\}$ and

(11) $p_r{}^{*}(c) = \inf\{p \in [0,1] | E[\max(p, v_1) - v_1 + \mathrm{r}(\max(\mathrm{p}, \mathrm{v}_1) - \mathrm{v}_1] - c \geq 0\}$.

Here v_2 is the valuation of the last remaining competitor. Comparing (10) and (11) to the rational choice model in (2) and (3) using inequality (7) yields the following predictions for the English auction with regret. First, there exists $p_r^*(c) < p^*(c)$, $p_r^{**}(c) > p^{**}(c)$ and $b^* < E[v_1]$ so that with two competitors remaining bidder 1 does not acquire information but remains in the auction until $p < \max\{p_r^*(c), b^*\}$.

If there is only one competitor remaining and $p_r^*(c) > p^{**}(c)$, then bidder 1 also does not acquire information and bids until the price $E[v_1]$. However, if there is only one competitor remaining and $p_r^*(c) < p^{**}(c)$ and if $p \in [p_r^*(c), p_r^{**}(c)]$ then bidder 1 acquires information and bids up to $p = b^*$. Further, for $p > p_r^{**}(c)$ bidder 1 drops out and for $p < p_r^*(c)$ bidder 1 stays in the auction. This means that with $p_r^*(c) < p^*\ (c)$ and with $p_r^{**}(c) > p^{**}(c)$ the range of prices where information is acquired is extended. Accordingly, regret in the English auction can also explain the excess information acquisition. Moreover, regarding the bids with anticipated regret, uninformed bidders drop out before the price reaches their expected valuation which is consistent with our underbidding effect observed in the experiment. Finally, regret can also play a role in explaining the experimental result of premature information acquisition. Since $p_r^*(c) < p^*\ (c)$ not only more information is acquired with regret, it is also acquired at lower prices, i.e. at an earlier stage.

Estimation of the Regret Coefficient

Bringing together experimental data and the extended theory with anticipated regret we can estimate the regret. We assume a linear regret function:

$$r(x) = \begin{cases} \propto x & if\ x < 0 \\ 0 & otherwise \end{cases}$$

Solving equation (6) for $N = 4$ with valuations uniformly distributed on $[0{,}100]$, as used in the experiment we obtain:

$$b^* = \frac{\sqrt{1+\propto} - 1}{\propto} 100$$

With $\propto \rightarrow 0$ and thus no regret the optimal bid in the second-price auction without information approaches $b^* = 50$, as in the initial rational choice model. However with increasing regret $\propto$

increases and b^* decreases, i.e. with high anticipated regret bids of uninformed bidders decline. Based on our experimental results for the second-price auction with $b^* = 40.6$ we can estimate the regret coefficient as $\propto = 1.25$.[19] Putting this result into perspective we compare it to other auctions with regret. Here Filiz-Ozbay and Ozbay (2007) find $\propto = 1.23$ and Engelbrecht-Wiggans and Katok (2008) $\propto = 0.62$. Both results are in line with the order of magnitude we observe, corroborating that using anticipated regret to explain excess information acquisition as put forward in this contribution has also a solid empirical underpinning.

2.6 Conclusions and Outlook

We have provided the first experiment on auctions with information acquisition, studying how much information is acquired and how it is used. This setting is highly relevant to real world situations such as corporate takeovers, where valuations are usually unknown at first. We derived our hypotheses from a risk neutral rational choice model for auctions with the opportunity for information acquisition. The most crucial discrepancy between theory and our experimental results is the excessive information acquisition behaviour for high costs, where the price of information is not compensated by the average expected profit. This effect is very robust and prevails in both, the second-price sealed-bid and the English auction. In line with this finding, the experiment also showed that subjects in the English auction fail to account for the additional information provided by the number of competitors. This leads to premature information acquisition strategies. Finally, considering the bidding strategies of both informed and uninformed bidders we observe that subjects continuously and significantly underbid.

On the basis of our results, risk aversion as a standard model extension could not explain the behaviour we observed. However, adapting the initial theory with anticipated regret, i.e. the regret from overpaying, which was applicable in our setting, can accommodate the experimental results. It explains excess information acquisition and underbidding behaviour.

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[19] For the English auction we cannot directly estimate the regret coefficient, as the mean bids in this treatment only impose a lower bound on the bids. In this treatment the auction was ended, as soon as the last competitor dropped out, that the maximum bid could not be observed in these cases. However, based on the lower bound of bids we can estimate an upper bound for the regret coefficient of $\propto = 1.97$.

2.7 Appendix

2.7.1 Additional Results

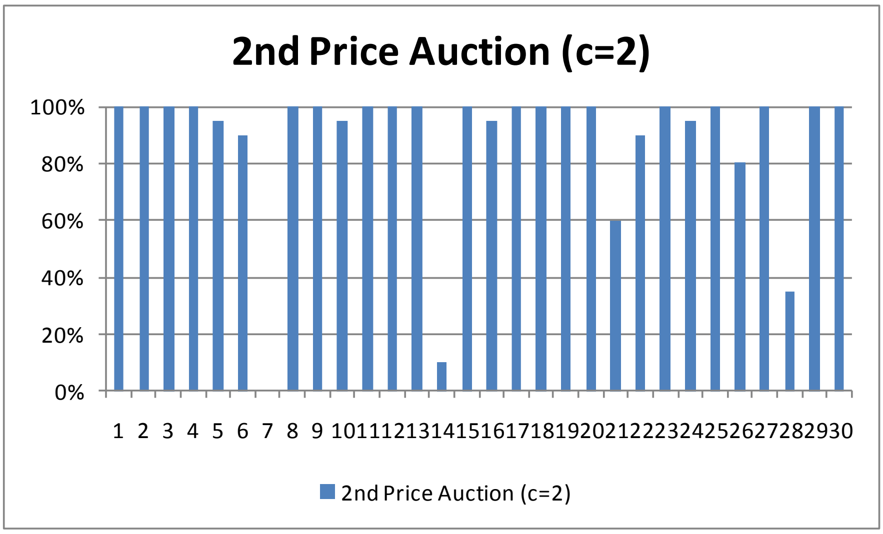


Figure 2.5: Individual Frequency of Information Acquisition (2nd Price Low)

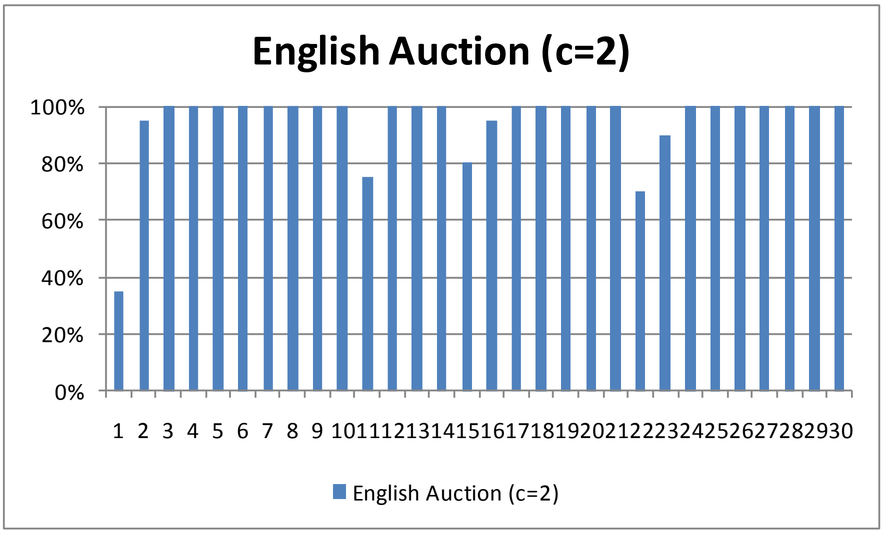


Figure 2.6: Individual Frequency of Information Acquisition (English Low)

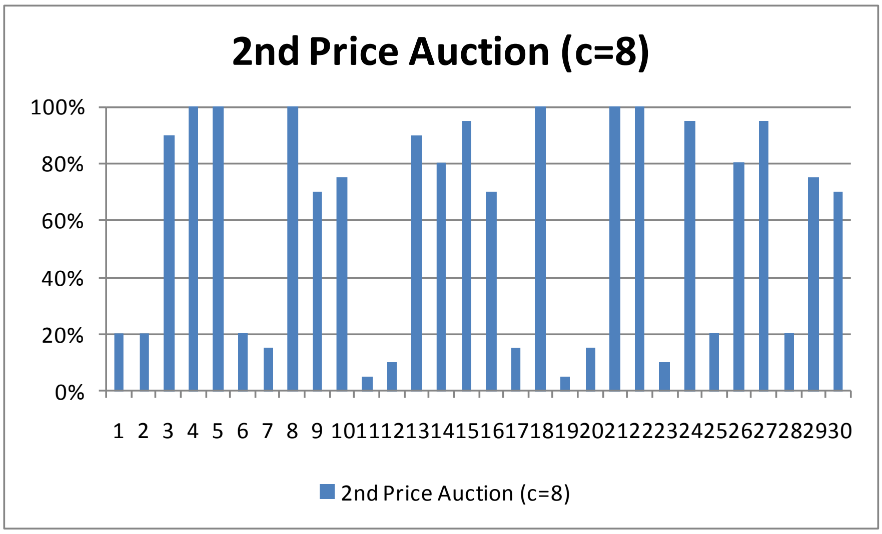


Figure 2.7: Individual Frequency of Information Acquisition (2nd Price High)

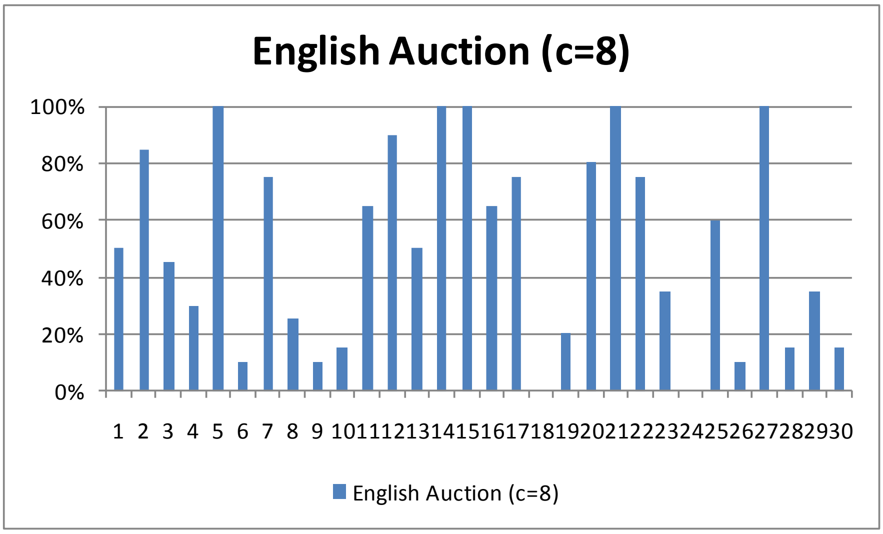


Figure 2.8: Individual Frequency of Information Acquisition (English High)

2.7.2 Instructions

Instructions

Welcome and thank you for participating in today's experiment. Please read the following instructions thoroughly. These are the same for all participants. Please do not hesitate to ask if you have any questions. However, we ask you to raise your hand and wait for us to come and assist you. We also ask you to restrain from communicating with other participants from now on until the end of the experiment. Please ensure that your mobile phone is switched off. Violating these rules can result in an exclusion from this experiment.

You will be able to earn money during this experiment. The amount of your payout depends on your decisions. Each participant will receive his payout individually in cash at the end of the experiment. You will receive 2.50 € as a show-up fee for your presence as well as the sum of payouts from each round. Possible losses will at the end of the experiment be set against the show-up fee (if you accumulated losses on top of that, you will be required to pay these in cash at the end of the experiment). During the experiment payouts will be stated in the currency "ECU" (Experimental Currency Unit). 10 ECU are equivalent to 1 Euro (10 ECU = 1 EUR). The experiment consists of 20 payout relevant rounds.

Course of a Round (Treatment: 2nd Price Auction)

During this experiment you will take part in an auction of a fictional good. You will be bidding in a group of four with three other participants. These three participants are pre-programmed bid robots. Their exact functioning will be described in more detail in the following.

<u>Information prior to the Auction:</u>

The fictional good is of different value for each bidder. Therefore prior to each round the valuation is determined for each participant. This valuation is between 0 and 100 ECU and each number has the same probability. However, during this auction you do not have any information about your valuation at first. Nevertheless, at the cost of 2 ECU/ 8ECU you can at any time acquire knowledge of your exact valuation. By contrast, the bid robots know their exact valuation of the fictional good. Their valuation, just as your own valuation, is between 0 and 100 ECU and each number has the same probability. The three bid robots will always have different valuations.

Profits and Losses during the Auction:

All bidders simultaneously make an offer for the fictional good. The bidder with the highest offer wins the auction. The price for the fictional good is set at the amount of the second highest bid. The winner of the auction has to pay this price for the good. If multiple bidders make the same offer during one round, then the winner is randomly determined. (Please note: You will not be able to revoke an offer or buy any information, once an offer has been submitted.)

The payout for the winner of an auction is calculated from his previously, randomly determined valuation of the good minus the price at the end of the auction. (Please note: You will incur a loss if your offer is higher than your valuation of the good. Losses will at the end of the experiment be set against the show-up fee. However, if you accumulate losses on top of that, you will be required to pay these in cash at the end of the experiment.) Additionally, if you have bought information at the cost of 2 ECU/ 8 ECU, then this amount will be deducted from your profit or entered as loss.

Feedback after an Auction Round:

At the end of an auction round you will be informed, whether you won the fictional good with your bid. Additionally, you will be informed about the second highest bid and therefore the price of the fictional good as well as your individual profit for this round.

Course of a Round (Treatment: English Auction)

During this experiment you will take part in an auction of a fictional good. You will be bidding in a group of four with three other participants. These three participants are preprogrammed bid robots. Their exact functioning will be described in more detail in the following.

Information prior to the Auction:

The fictional good is of different value for each bidder. Therefore prior to each round the valuation is determined for each participant. This valuation is between 0 and 100 ECU and each number has the same probability. However, during this auction you do not have any information about your valuation at first. Nevertheless, at the cost of 2 ECU/ 8ECU you can at any time acquire knowledge of your exact valuation. By contrast, the bid robots know their exact valuation of the fictional good. Their valuation, just as your own valuation, is between 0 and

100 ECU and each number has the same probability. The three bid robots will always have different valuations.

Profits and Losses during the Auction:

The auction begins at 0 ECU for the fictional good. The bid will increase every 2 seconds by 1 ECU. A price clock indicates the current bid in ECU during the auction. You will also be able to see at any time of the auction how many bidders are still active and you will be able to buy information on your exact valuation. You can pause the price clock at wish by clicking the button "Pause/ Continue". All participants automatically continue bidding until they leave the auction round by clicking the button "Quit" on their screen. The auction ends automatically once only one bidder is left active. The last active bidder wins the auction and has to pay the last price on the price-clock, i.e. the price when the second last bidder dropped out. If multiple bidders quit simultaneously, then the winner of this round is randomly determined.

The payout for the winner of an auction is calculated from his previously, randomly determined valuation of the good minus the price at the end of the auction. (Please note: You will incur a loss if your offer is higher than your valuation of the good. Losses will at the end of the experiment be set against the show-up fee. However, if you accumulate losses on top of that, you will be required to pay these in cash at the end of the experiment.) Additionally, if you have bought information at the cost of 2 ECU/ 8 ECU, then this amount will be deducted from your profit or entered as loss.

Feedback after an Auction Round:

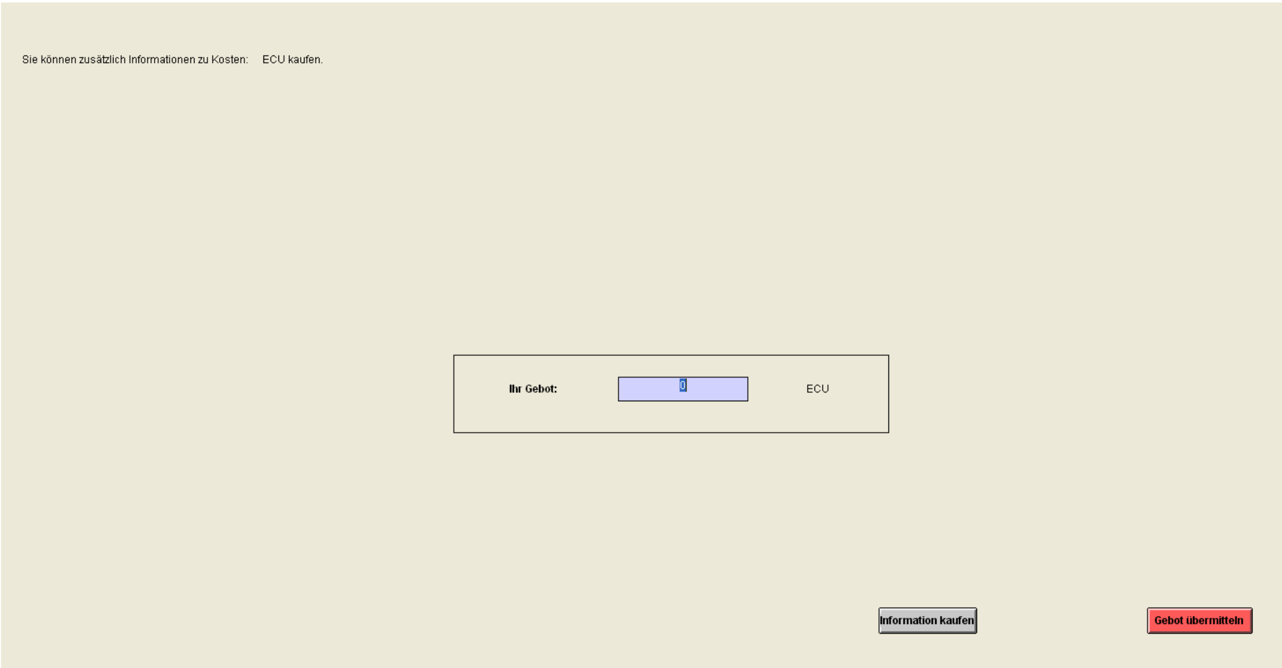
At the end of an auction round you will be informed, whether you won the fictional good with your bid. Additionally, you will be informed about the second highest bid and therefore the price of the fictional good as well as your individual profit for this round.

End of the Experiment

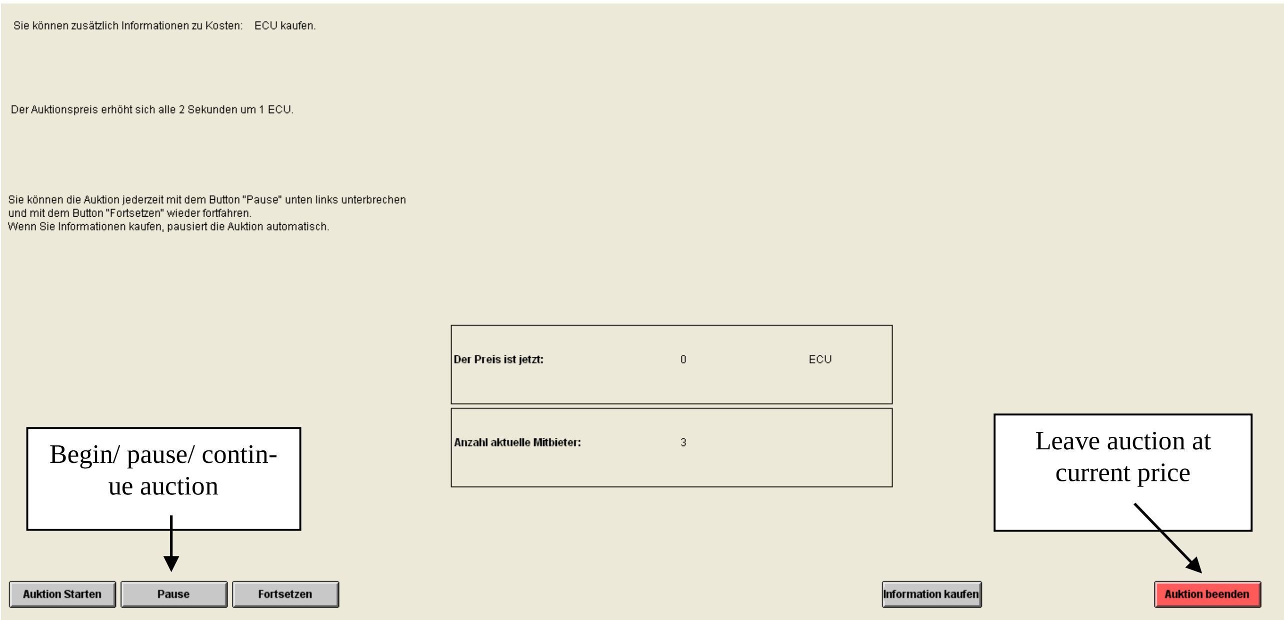
All auction rounds of this experiment are payout relevant. After completion of all 20 auction rounds, your payouts for each round as well as your overall result will be presented to you in a summary on your screen. After that we will ask you to fill in a short questionnaire concerning the experiment.

Please raise your hand, if you have any further questions.

Screenshot Example (Treatment: 2nd Price Auction):



Screenshot Example (Treatment: English Auction):



3 Demand for Non-Instrumental Information in Auctions

3.1 Introduction

Economic theory provides clear predictions regarding the usage of information and the formation of strategies in auctions (Milgrom and Weber, 1982; Rasmussen, 2006; Vickrey, 1961). The second-price sealed-bid auction is the most studied and best understood format, both from a theoretical and empirical point of view. However, it still maintains puzzles such as overbidding, regret and spite motives which constantly show that human bidders are not adhering to all theoretical predictions. Some departures from strict rationality in the context of auctions have already been covered by the existing literature (Ariely and Simonson, 2003; Lucking-Reiley, 2000b; Ockenfels and Roth, 2006), but the provision of additional non-instrumental information has not been studied in this context, yet. Information is non-instrumental when it cannot directly be used in the formation of a rational bidding strategy.

This contribution aims at studying the value of information in auctions. Previous works have relied on the rational choice framework calculating a monetary pay-off of information with a strategic or instrumental value for the bidders (Compte and Jehiel, 2007; Persico, 2000). In this line of research, first experiments have already shown that subjects deviate from the rational choice model, when assessing the value of information in auctions (Gretschko and Rajko, 2011). Considering these new insights, where the additional information is essentially overvalued, the related supplementary work with non-instrumental information can help organising and reviewing current results. Non-instrumental information should not change a decision from a standard rational point of view, but it might reduce uncertainty about the realisation of a decision as it updates some irrelevant priors. For simple decision lotteries, Eliaz and Schotter (2007, 2010) argue that decision makers may derive some benefit from non-instrumental information. If that also applies to auctions, this is would be a starting point for the explanation of the valuation of additional information in such games. In particular the understanding of motives for the excess information acquisition behaviour found by Gretschko and Rajko (2011) can be improved. Hence an experiment is designed which extends the second-price sealed-bid auction, with an information acquisition option. Here, according to standard theory non-instrumental information should not be assessed or valued at all by the bidders. However, the results show that on the contrary, there is a positive demand for non-instrumental information. This demand is significantly dependent only on the costs of information and not on the group size of competing bidders. Further, with non-instrumental information the individual bids are characterised by underbidding behaviour.

The remainder of this chapter unfolds as follows. Section 2 combines literature on non-instrumental information, auctions with information acquisition and standard experimental results in second-price auctions. On these grounds some hypotheses are derived. The experimental design and procedures are presented in section 3. In section 4 the main results are discussed. Finally, section 5 gives a short conclusion and outlook on future research.

3.2 Related Literature and Hypotheses

Investigating the use of non-instrumental information in auctions embarks upon a very recent line of research. Up to now, the experimental literature on auctions has mainly envisaged deviations from equilibrium play in terms of the bidding strategies such as overbidding. The antecedent question of how the information provided in an auction scenario is processed has not been explicitly addressed.

Nonetheless, the idea of studying the impact of non-instrumental information on economic decision making has already found profound evidence in a different context. Foremostly, Eliaz and Schotter (2007) have demonstrated that players buy non-instrumental information in a standard one player decision experiment consisting of various lotteries. Moreover, it has been theoretically and experimentally validated that players can derive some additional utility from the perceived increase in confidence, simply when having more information for making a decision (Eliaz and Schotter, 2010). According to their model, anticipated wins are also part of the utility function. Simultaneously to these advances, the literature on auction theory started to deal with the process of information acquisition in auctions (Compte and Jehiel, 2007; Persico, 2000). However, this literature assumes that the information to be acquired is instrumental for the formation of a bidding strategy. Bringing this problem to the laboratory, Gretschko and Rajko (2011) already confirmed that players acquire and use this information. Expanding on these results, this study investigates how participants of an auction respond to the costly provision of non-instrumental information.

Here the information to be acquired does not have any impact on the bidding strategies, according to the standard models widely accepted and used in auction theory. The non-instrumental information in this context is the highest valuation of the competing bidding agents in a second-price sealed-bid auction. This additional information is costly but not strategically useful, as there is a weakly dominant strategy of precisely bidding one's valuation independent of the valuations of others. Consequently, this information should never be bought. Nevertheless, based on the findings of Eliaz and Schotter (2007, 2010), it is expected that in an auction set-up there is also a positive demand for non-instrumental information.

Hypothesis 1: There is positive demand for non-instrumental information.

Two previous studies have already used non-instrumental information in auctions and found first evidence for such a demand. However, these studies focus on bidding strategies and on finding an explanation for overbidding, rather than analysing the actual information acquisition behaviour (Andreoni et al., 2007; Cooper and Fang, 2008). Cooper and Fang (2008) find that non-instrumental information is bought in experimental second-price auctions. However, they do not systematically vary the costs of information acquisition and the group structure. Varying the information cost allows testing an important hypothesis. If there is a systematic effect based on players' reasoning, then there should be significantly less information acquisition in the high cost treatments.

Hypothesis 2: There is less demand for non-instrumental information at a higher cost.

As already argued, in the second-price-auction truthfully bidding one's valuation is a weakly dominant strategy. With the non-instrumental information acquisition option, this prediction still holds. However, the most pivotal experiment on bidding strategies by Kagel and Levin (1993) finds more over- than underbidding. Furthermore, Cooper and Fang (2008) and Andreoni et al. (2007) report a significant impact of non-instrumental information on players' bidding strategies. In fact, both studies find overbidding in their experimental data. Hence it is expected, that the bidding strategies are systematically affected by the non-instrumental information.

Hypothesis 3: Non-instrumental information leads to overbidding.

Finally, there has not been any variation of group composition in previous experiments. However, Kagel and Levin (1993) have already found that under- and overbidding effect in standard auctions are susceptible to group size, i.e. the amount of under- and overbidding increases with the number of bidders. As a consequence, the next hypothesis addresses the number of bidders in an auction.

Hypothesis 4: Deviations from valuation bidding increase with the number of bidders.

3.3 Experimental Design and Procedures

The experimental design aims at the utilisation of additionally provided non-instrumental information in a standard second-price sealed-bid auction.[20] The item being auctioned was a hypothetical good associated with independent private values for the bidders.[21] These private valuations were known to the bidders at the beginning of each auction round. Overall, this auction game was played for 20 rounds. The profits were calculated as the winner's valuation minus the final price and, if applicable, minus the costs of information acquisition. In the experiment each human subject was matched in a group with one respectively three bidding robots. These robots were pre-programmed to play the weakly dominant strategy of the second-price auction, i.e. bidding their true valuation. Knowing that one was competing with a rational bidding robot rather than a human agent, there was no social interaction in this experiment. This systematically rules out any confusion due to uncertainty or strategic reasoning about other human players.

Overall, all relevant information for the bidding strategy is provided in this auction. Nevertheless, an information acquisition feature offering additional non-instrumental information at a cost of c=2 or c=8 was additionally provided.[22] As non-instrumental information, the private value of the one competing bidding robot with the highest valuation was chosen.

	Information Costs	
Group Size	Low Cost	High Cost
2 players	c = 2	c = 8
4 players	c = 2	c = 8

Table 3.1: Experimental Treatments

Using different cost parameters, the demand for information can be controlled. Information acquisition was only possible prior to submitting the sealed-bid offer. In addition, the group size was varied in order to assess whether the non-instrumental information is valued differently, when there is only one competitor and hence the valuations of all bidders are implicitly

[20] For the full experimental instructions refer to the appendix.

[21] The distribution of the private values is a uniform distribution ranging from 0 to 100, and the private values are redrawn for every round and for all bidders.

[22] Based on the valuation profiles drawn in this experiment, the cost of c=2 came down to 10% of overall profits for the human subjects and c=8 meant a 40% dispense of overall profits.

known ex ante when information acquisition is chosen. Overall, the experiment yields the 2 x 2 design for the four treatments as depicted in table 3.1. The feedback procedure was the same for all treatments. Subjects learned each round whether they won the hypothetical good, the final price of the good, their bid as a reminder and the overall profit or loss for this auction including the costs of information acquisition if applicable. In order to have the costs of information acquisitions assessed in a realistic manner, it was stressed in the instructions that all losses in the experiment must be paid to the experimenter after the experiment.

The experimental sessions were conducted in November 2011 at the Cologne Laboratory for Economic Research (CLER). 30 subjects per treatment and 120 subjects overall participated with average payments being 11.83 € including a guaranteed show-up fee of 2.50 €. Subjects stayed in the laboratory for about 55 minutes. The recruitment was organised with the online recruitment system ORSEE (Greiner, 2004) and the experiment itself was programmed with z-Tree (Fischbacher, 2007).

3.4 Experimental Analysis and Results

3.4.1 Excess Acquisition of Non-Instrumental Information

The first two hypotheses are based on the fact, that it is always a weakly dominant strategy to bid one's valuation in a second-price auction. Hence the instrumental value of knowing the highest valuation of one's competitors is zero. The demand for this non-instrumental information must be zero according to both standard auction theory and models for auctions with information acquisition. But in fact, as exhibited in figure 3.1, the average amount of information acquisition is significantly higher than zero, in all four treatment variations.[23] So in all treatments subjects do acquire non-instrumental information at a substantial cost. Also as hypothesised, the demand for non-instrumental information is higher in the two low cost treatments.[24] The size of competition, however, being either one or three bidding robots, does not make a significant difference to the acquisition behaviour.[25]

[23] T-test: p-value < 0.0001 for all four treatments.
[24] T-test: p-value < 0.0001 for comparing n=2 and n=4 across costs.
[25] T-test: p-value = 0.62 for comparing the low cost treatments and p-value = 0.35 for the high cost treatments.

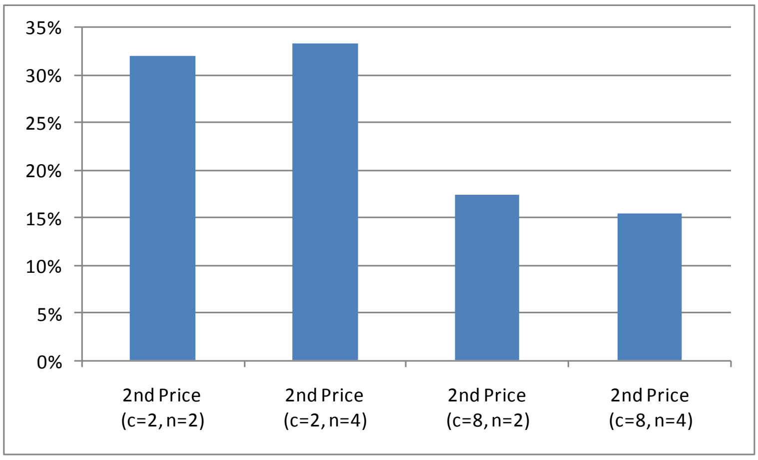


Figure 3.1: Non-Instrumental Information Acquisitions (all rounds)

Furthermore, when considering the demand for non-instrumental information for the individual auction rounds, it becomes evident that there is some immediate learning effect in the first five rounds (figure 3.2).

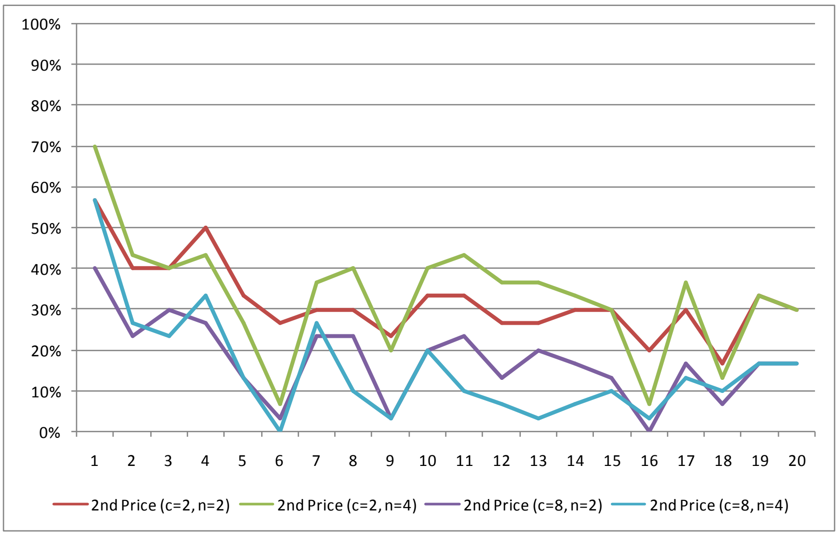


Figure 3.2: Development of Information Acquisition Frequencies

After the initial rounds, however, the demand for non-instrumental information stabilises for all treatments. Even though there is some learning effect in the course of the experiment, the main effect of excess acquisition of non-instrumental information does not vanish.[26]

[26] The contingencies on different rounds and the fact whether the previous round was won are analysed further with a fixed effect regression model in the following sections.

One can consider the possible objection that some players drive the effect especially in the high cost treatments and most players act rational by not buying information at all. Considering the data in table 3.2, 17-37% of players do not buy any information and thus act in accordance with the standard theory. 23-37% of players just buy the non-instrumental information occasionally, i.e. once or twice. However, the majority of players (33-60%) buy the information more than twice which indicates that this is a repeated conscious decision.

Treatment	Percentage of players buying no info	Percentage of players buying ≤ 2	Percentage of players buying > 2
c=2, n=2	17%	30%	53%
c=2, n=4	17%	23%	60%
c=8, n=2	37%	30%	33%
c=8, n=4	23%	37%	40%

Table 3.2: Player Types for Information Acquisition

Altogether, this means that subjects acquiring non-instrumental information must derive some utility or confidence from this information which they deliberately and repeatedly buy.

3.4.2 Bidding Strategies

In the following, the bidding behaviour of players is characterised in terms of over- and underbidding.[27] First, all bidders who have not previously acquired information are considered. Then, all bidders who had the additional non-instrumental information available are discussed. Regarding the bidding strategies without information acquisition, valuation bidding prevails, with only slight differences between under- and overbidding (see table 3.3). Over all treatments approximately 50% of the subjects exactly bid valuation, which is in line with a recent study by Garratt et al. (2012) finding under- and overbidding of comparable magnitude.

[27] Valuation bidding is defined as bidding exactly one's valuation. Underbidding is every bid under the valuation and overbidding every bid above the valuation, irrespective of the size of this deviation.

Treatment	underbidding	valuation bidding	overbidding
c=2, n=2	29.7%	57.4%	13.0%
c=2, n=4	19.8%	64.0%	16.3%
c=8, n=2	36.6%	36.8%	26.7%
c=8, n=4	24.7%	46.9%	28.4%

Table 3.3: Bidding Strategies without Non-Instrumental Information Acquisition

More striking are the bidding strategies subjects employ after they have acquired the non-instrumental information (see table 3.4). Whereas the literature on standard second price auctions is usually concerned with resolving the overbidding phenomenon (Cooper and Fang, 2008), the data on bidding strategies with non-instrumental information from this experiment reveals a much stronger underbidding effect. Across all four treatments the average underbidding with information, i.e. after information acquisition, is 73%, whilst valuation bidding only occurs in 12% and overbidding in 15% of the cases.

Treatment	underbidding	valuation bidding	overbidding
c=2, n=2	68.2%	21.9%	9.9%
c=2, n=4	73.0%	12.5%	14.5%
c=8, n=2	76.2%	9.5%	14.3%
c=8, n=4	75.3%	3.2%	21.5%

Table 3.4: Bidding Strategies with Non-Instrumental Information Acquisition

With regard to hypothesis 3, in this experiment there is strong underbidding of subjects, who acquire additional information. This underbidding effect is similar to the results obtained by Gretschko and Rajko (2011). However, the effect is even more manifest in this experiment where with information acquisition only 11.8% of auctions exhibit valuation bidding in the second-price auction. Gretschko and Rajko (2011) have still found 37.5% of auctions to comply with valuation bidding. Thus the non-instrumental information strongly affects bidding strategies in a manner rarely found in the experimental auction literature and hypothesis 3 is rejected.

The generally strong impact of underbidding in this experiment is also fortified by the size of underbidding. Whilst without information both over- and underbidding are small in size, for the auctions with non-instrumental information the effect is severe, as illustrated in table 3.5 (standard errors are in parentheses).

Treatment	Mean Bid	Mean Valuation
c=2, n=2	52.67 (2.14)	68.76 (1.78)
c=2, n=4	59.81 (1.60)	72.22 (1.39)
c=8, n=2	52.42 (3.05)	75.84 (1.69)
c=8, n=4	60.69 (2.77)	77.55 (1.93)

Table 3.5: Mean Bids for Underbidding with Non-Instrumental Information

So when deciding to underbid, subjects do not only deviate by small amounts from their true and known valuation, but underbid by 17.2 on average. In order to put this number into perspective, the amount of underbidding found in the same treatments, but without knowledge of the non-instrumental information, was only 0.63. Thus bidding strategies were strongly affected by the non-instrumental information. Finally, the actual cost parameter of the information has little impact on the height of discounting bids.

With reference to hypothesis 4, in this experiment there is only partial support. When considering all auctions unconditional of information acquisition there is no difference in valuation bidding between the two low cost treatments.[28] However, for the high cost treatments there is a significant effect where valuation bidding increases with the number of bidders.[29]

3.4.3 Further Analyses

This section uses fixed effect regression models to control for additional influences which might affect the results previously obtained. By choice of the regression model one also controls for unobserved heterogeneity between the individual subjects.

First, regressions are run to analyse the information acquisition behaviour (see table 3.6).[30] In this regard, the previous results have already established that subjects acquire significantly

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[28] Mann-Whitney Test: p-value = 0.772.
[29] Mann-Whitney Test: p-value = 0.003
[30] All regression models are with standard errors in parentheses. Moreover, standard OLS regressions yield the same overall results for both dependent variables.

more information than theory predicts and that they even do more so, when the costs for information acquisition are lower.

Dependent Variable: Buy_Info		
Variables	**Model 1**	**Model 2**
Intercept	0.055***	0.074***
	(0.016)	(0.018)
Valuation	0.003***	0.002***
	(0.000)	(0.000)
First_Rounds (1-5)	0.114***	0.070***
	(0.015)	(0.015)
Won_Previous_Round		-0.020
		(0.013)
N observations	2400	2280
Prob > F	0.000	0.000
R^2	0.05	0.04

Table 3.6: Fixed-Effects Regression on Information Acquisition

According to the first model the information acquisition behaviour is significantly higher in the first five auction rounds as the significant coefficient of the dummy variable *first_rounds* shows. Of course, also the *valuation* has a small, but significant impact on the information buying behaviour. The second model further corroborates the robustness of the results, as the success of winning in the previous round has no significant impact on the information acquisitions.

Dependent Variable: Bid			
Variables	**Model 1**	**Model 2**	**Model 3**
Intercept	6.272***	3.861***	3.176***
	(0.817)	(1.029)	(1.098)
Valuation	0.883***	0.869***	0.866***
	(0.126)	(0.013)	(0.013)
Max_Valuation_Competition		0.052***	0.066***
		(0.014)	(0.013)
Buy_Info	-10.360***	-10.485***	- 10.271***
	(1.044)	(1.042)	(1.079)
First_Rounds (1-5)	-3.174***	-3.517***	-2.213**
	(0.763)	(0.766)	(0.776)
Won_Previous_Round			-0.132
			(0.631)
N observations	2400	2400	2280
Prob > F	0.000	0.000	0.000
R^2	0.65	0.65	0.68

Table 3.7: Fixed-Effects Regression on Bids

Next, the main results regarding bidding behaviour are also analysed with fixed-effects regressions (see table 3.7). Model 1 starts with the valuation of the subjects as an independent variable and confirms its highly significant impact. Also the previous result of severe underbidding is verified with the regression, where the coefficient buy_info for buying information is significant and highly negative. This substantiates the robustness of the underbidding effect found here. Moreover, the base model shows that the bidding behaviour in the first 5 rounds is different from the remaining auction rounds. Model 2 additionally controls for the highest valuation of the competing bidding robots, i.e. the non-instrumental information. This indicates a significant effect on the bids, but the effect is relatively small in its magnitude. With model 3 it can be shown that it has no impact on bids, whether the previous round was won.

3.4.4 Discussion

This experiment relates to two new strands of literature, i.e. non-instrumental information and information acquisition in auctions. With respect to the first one, the experimental data gives credence to the basic insight by Eliaz and Schotter (2007) that non-instrumental information matters in economic decision making. In the applied scenario of an auction rather than a lottery choice experiment, the basic result persists.

Putting the experimental results into perspective with Gretschko and Rajko (2011) helps shedding light on their results, as it not only confirms that information acquisition is not in line with rational choice models, but that it is robust even in this very simple set-up. Hence this experiment confirms the substance of the puzzle of excess information acquisition behaviour and also explains some of the results from Gretschko and Rajko (2011). In particular, when considering the magnitudes of information acquisition in the two experiments, based on very similar and thus comparable designs, the strategic information acquisition for high costs showed a 55% excess acquisition of information, whereas the non-instrumental information only exhibited 18%. Hence the motives underlying this behaviour and driving the results for non-instrumental information acquisition, such as a preference for confidence, could also account for some of the puzzle of excess acquisition behaviour for strategically relevant information. Additionally, the weak underbidding effect found by Gretschko and Rajko (2011) can be reproduced with this experiment. In fact, this effect is even 60% stronger in the non-instrumental information experiment at hand.

3.5 Conclusions and Outlook

This contribution has extended recent work on auctions with information acquisition, which has given a first indication of departures from rational choice in this context. In order to improve the understanding of the behaviour in such auctions, the literature on non-instrumental information was taken up and the demand for non-instrumental information in a second-price auction was investigated.

The experimental design extended a second-price sealed-bid auction with the opportunity to acquire non-instrumental information. Two main results were found. First, there is a substantial demand for non-instrumental information, supporting the notion of a preference for confidence. Secondly, the bidding strategies that players use when having acquired the additional information deviates from most other auction experiments. Bidding strategies hardly depend on the number of bidders. Nevertheless, the acquisition of non-instrumental information subsequently leads to substantial underbidding; both in terms of frequency and amount of underbidding. Altogether, the acquisition and processing of information in auctions is a promising field for further research. The experimental results in this area are very strong, and yet, there is little theoretical work fully explaining the observed behaviour.

3.6 Appendix

3.6.1 Instructions

Instructions

Welcome and thank you for participating in today's experiment. Please read the following instructions thoroughly. These are the same for all participants. Please do not hesitate to ask if you have any questions. However, we ask you to raise your hand and wait for us to come and assist you. We also ask you to restrain from communicating with other participants from now on until the end of the experiment. Please ensure that your mobile phone is switched off. Violating these rules can result in an exclusion from this experiment.

You will be able to earn money during this experiment. The amount of your payout depends on your decisions. Each participant will receive his payout individually in cash at the end of the experiment. You will receive 2.50 € as a show-up fee for your presence as well as the sum of payouts from each round. Possible losses will at the end of the experiment be set against the show-up fee (if you accumulated losses on top of that, you will be required to pay these in cash at the end of the experiment). During the experiment payouts will be stated in the currency "ECU" (Experimental Currency Unit). 20 ECU are equivalent to 1 Euro (20 ECU = 1 EUR). The experiment consists of 20 payout relevant rounds.

Course of a Round (Treatment: 2nd Price Auction)

During this experiment you will take part in an auction of a fictional good. You will be bidding in a group of two/four with one/three other participants. These one/three participants are pre-programmed bid robots. Their exact functioning will be described in more detail in the following.

<u>Information prior to the Auction:</u>

The fictional good is of different value for each bidder. Therefore prior to each round the valuation is determined for each participant. This valuation is between 0 and 100 ECU and each number has the same probability. Only your valuation is known to you. At the cost of 2 ECU/ 8ECU you acquire the highest valuation of the competing bidding robots.

<u>Profits and Losses during the Auction:</u>

All bidders simultaneously make an offer for the fictional good. The bidder with the highest offer wins the auction. The price for the fictional good is set at the amount of the second highest bid. The winner of the auction has to pay this price for the good. If multiple bidders make the same offer during one round, then the winner is randomly determined. (Please note: You will not be able to revoke an offer or buy any information, once an offer has been submitted.)

The payout for the winner of an auction is calculated from his previously, randomly determined valuation of the good minus the price at the end of the auction. (Please note: You will incur a loss if your offer is higher than your valuation of the good. Losses will at the end of the experiment be set against the show-up fee. However, if you accumulate losses on top of that, you will be required to pay these in cash at the end of the experiment.) Additionally, if you have bought information, then this amount will be deducted from your profit or entered as loss.

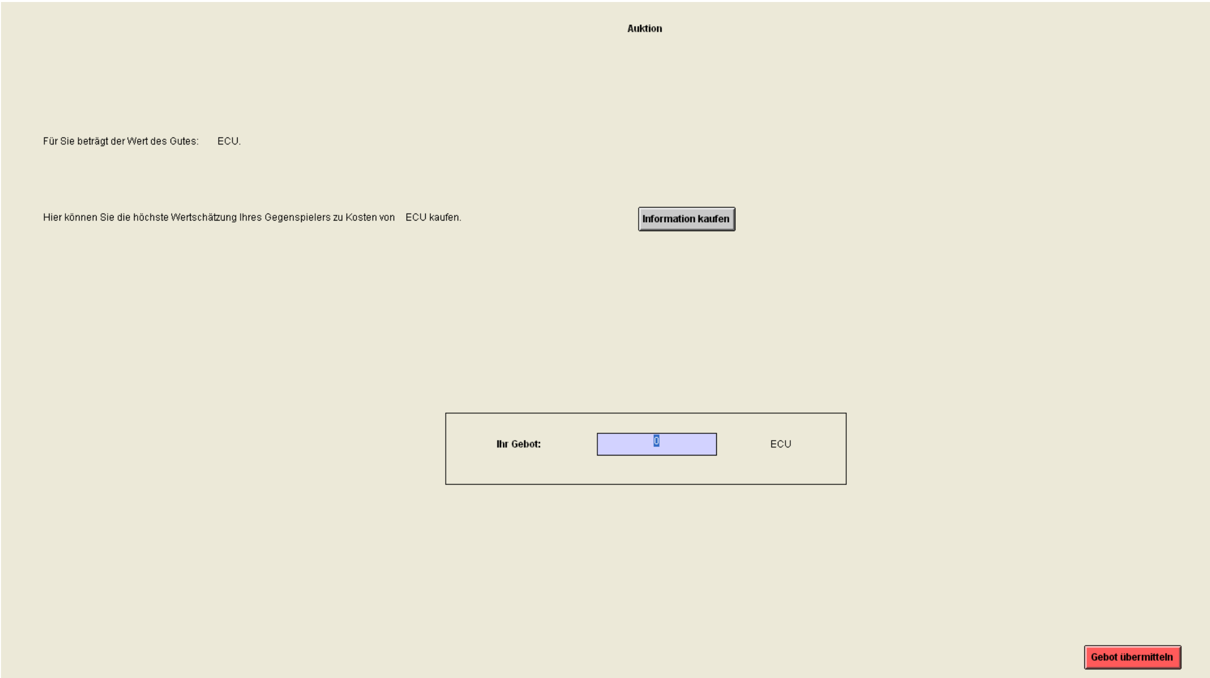
Feedback after an Auction Round:

At the end of an auction round you will be informed, whether you won the fictional good with your bid. Additionally, you will be informed about the second highest bid and therefore the price of the fictional good as well as your individual profit for this round.

End of the Experiment

All auction rounds of this experiment are payout relevant. After completion of all 20 auction rounds, your payouts for each round as well as your overall result will be presented to you in a summary on your screen. Please raise your hand, if you have any further questions.

Screenshot Example (Treatment: 2nd Price Auction):



4 Risk and Ambiguity in Global Games

4.1 Introduction

Economic interactions, in which strategic complementarities require coordination among groups of players, do not automatically result in efficient outcomes.[31] Assuming common knowledge of the payoff structure and rationality among agents, coordination games typically have multiple equilibria with self-fulfilling features complicating the analysis and derivation of possible policy implications. Bank runs (Diamond and Dybvig, 1983) and speculative attacks (Obstfeld, 1996) are canonical examples, where common knowledge results in indeterminacy of coordination outcomes. The theory of global games accounts for the fact that players often have some private information and relax the common knowledge assumption by perturbing the payoffs associated with according actions (Carlsson and van Damme, 1993; Morris and Shin, 1998, 2003). Some important work on global games assumes both public and private information regarding the actual game that is played (Heinemann et al., 2004). This important assumption can be extended to having variations of risk with respect to signal precision or even ambiguous information about one's own signal precision, i.e. multiplicity of priors. Accounting for ambiguity is a meaningful extension for global games, as it often applies in real life scenarios. Here one would rarely know the exact precision of information, nor would one have reliable knowledge about the underlying distribution of signals. Accordingly, risk and ambiguity are a central theme for some recent theoretical advancement of global games (Ui, 2009). We take these theoretical predictions and experimentally investigate a global game framed as a speculative attack under two different types of uncertainty, i.e. risk and ambiguity.

In particular we assess three main hypotheses with our experimental design. Firstly, we confirm the consistent use of undominated switching strategies, but the rare commitment to unique cut-off values, as a robust finding in global games. Our second hypothesis confirms excess aggressiveness in terms of individual subjects' behaviour. We also show that this excess aggressiveness can be explained as a rational best response given certain expectations

[31] This chapter is joint work with Christopher Zeppenfeld.

regarding the other players' behaviour. Finally, we take our experimental results to falsify the prediction from the model of Ui (2009), that there are opposite effects on overall coordination for increasing risk and ambiguity.

Section 2 discusses the theoretical and experimental literature on global games. More importantly, it also outlines the theory of global games and its extensions for ambiguity. This yields the equilibrium predictions and the comparative statics for our experiment. Section 3 presents our experimental design and procedures. Section 4 discusses the main results. Finally, section 5 provides a short summary.

4.2 Global Games Theoretical Predictions and Hypotheses

Coordination games often assume common knowledge of all payoffs and rationality of all players, resulting in multiple equilibria. However, many real life situations exhibit strategic complementarities and players who have some private information are uncertain about the actions of other players working towards the same end. This applies to bank runs, debt crises or speculative attacks. The theory of global games (Carlsson and van Damme, 1993; Morris and Shin, 1998, 2003) avoids the common knowledge assumption by perturbing the payoffs of associated actions. Instead of observing the true state of the world, players receive a noisy private signal. Hence, the term global games accounts for the fact that the actual game is drawn from a class of possible games and players can only observe the actual game with some noise. The main advantage global games entail is that they have a unique equilibrium, which is in general dominance solvable.

In the following we derive the theoretical equilibrium predictions and comparative statics for the speculative attack game under both risk and ambiguity. Based on this we can determine the parameterisation of the game for our experimental design.

4.2.1 The Speculative Attack Game

We adopt the speculative attack game of Morris and Shin (1998) based on the game of complete information by Obstfeld (1996). A central bank faces a continuum of speculators who independently decide whether or not to attack a fixed exchange rate (peg). Attacking means short selling of a unit of currency, which involves transaction costs (c). Whether the currency can be successfully devalued depends on both the state of fundamentals of the underlying economy (θ) and the aggregate mass of attackers (l). In the first stage of the game nature selects a state θ and the central bank will abandon the peg if the aggregate mass of speculators short-selling is equal or exceeding some critical value $\Psi(\theta)$. We will denote this as the hurdle function $\Psi: \mathbb{R} \rightarrow [0,1]$ which assigns a critical proportion of agents needed for a devaluation to every possible state of fundamentals. We assume that $\Psi(\theta)$ is decreasing in θ. Further, $\Psi^{-1}(0) = \overline{\theta}$ is a state above which the safe action is dominated by the speculative action, i.e. attacking is always successful no matter what the other agents do. On the other hand, if the economy is sufficiently strong, the central bank will maintain the peg no matter the size of the aggregate speculative attack. Then we obtain $\Psi^{-1}(1) = \underline{\theta}$ below which the safe action dominates such that attacking cannot be successful.[32] The players have a simple binary choice between a speculative option (attacking) and a safe option (not attacking). This game can then be solved by using global games with its standard assumptions.

We will consider action $a = 1$ as “attacking” the currency and action $a = 0$ as refraining from doing so. Further we assume that the payoff of successful speculation is the actual (but unknown) state of fundamentals while the agent receives zero if he chooses $a = 1$ but the attack fails. The payoff $u(a, l, \theta)$ depends on the chosen action, the aggregate mass of speculators and the actual state of fundamentals. The resulting payoff structure reads

$$u(1, l, \theta) = \begin{cases} v(\theta), & l \geq \Psi(\theta) \\ 0, & l < \Psi(\theta) \end{cases}$$

and

$$u(0, l, \theta) = v(c)$$

where $v(\cdot)$ is a standard utility function.[33]

4.2.2 The Game with Risk and Ambiguity

In the following we build on Ui (2009) to extend global games to risk and ambiguity. According to Ui (2009) we assume that $u(1, l, \theta)$ is monotonically increasing in l and θ, whereas

[32] In order to have a coordination problem, we require that $\underline{\theta} < \overline{\theta}$.

[33] We do not impose any restrictions on the functional form of v(·) besides non-satiation and non-negativity.

$u(0, l, \theta)$ is monotonically decreasing in l and θ.[34] We will now follow Morris and Shin (1998) incorporating risk into the speculative attack model thus transforming it into a global game. The term risk refers to the fact that agents do no longer observe the actual state but receive an individual private signal $x = \theta + \varepsilon$.[35] Besides the uniform prior of the state, we assume a uniform distribution of noise over $[-\xi, \xi] \subset \mathbb{R}$. Here it is crucial that the distribution of state and noise are common knowledge among agents. The actual state lies in a ξ-surrounding around the idiosyncratic signal. Accordingly, any signal another agent j can receive is at most 2ξ away from agents i's signal, because all signals are included in a ξ-surrounding around the actual state. Based on their signal, agents can decide on their action $a \in \{0,1\}$. In the incomplete information global game, with a unique prior on the signal's precision, a switching strategy is based on a cut-off value $\kappa \in \mathbb{R}$, where a rational player switches from action 0 to action 1:

$$s(x|\kappa) = \begin{cases} 1, & x > \kappa \\ 0, & x \leq \kappa \end{cases}.$$

Morris and Shin (1998) show, that this strategy survives the iterated elimination of strictly interim-dominated strategies. More importantly, there is one unique switching point κ^* constituting a Bayesian Nash equilibrium. The utility of action 1 weighted by the conditional probability of success must be equal to the utility of the safe action in equilibrium. The switching equilibrium has the unique cut-off value κ^* above which rational agents attack and the unique state θ^* above which attacking is successful.

In reality, however, information is often vague in a sense which is fundamentally different from pure risk. In fact, the real state of the world is often subject to ambiguity rather than to risk. Knight (1921) distinguishes risk as objectively known probabilities in contrast to ambiguity as unknown probability distributions. With the Ellsberg Paradox the response to ambiguous situations or tasks has been experimentally tested and since then ambiguity aversion in simple lottery experiments is a well-established fact (Camerer and Weber, 1992; Ellsberg, 1961; Maccheroni et al., 2006). From a theoretical perspective this is usually captured by

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[34] The standard formulation of global games by Morris and Shin (2003) uses the weaker assumption that only the utility gain of choosing action 1 over action 0 is monotonically increasing. However, the above formulation is necessary to incorporate multiple priors into the model, since agents can assess the actions with different priors (Ui, 2009).

[35] For ease of notation we omit the subscripts for individual players *i*.

models with multiple priors such as MaxMin Expected Utility (Gilboa and Schmeidler, 1989) or Choquet Expected Utility (Schmeidler, 1989). In terms of global games with ambiguity an agent does not know the precision of his signal with certainty. Hence ambiguity can be incorporated into global games on the basis of MaxMin Expected Utility (MMEU) and assuming that agents minimise the expected utility over different conceivable distributions and then make decisions which maximise the corresponding utilities (Ui, 2009). Accordingly, each player has a set of conditional probability distributions Ξ with cardinality higher than one. We term this game as a global game with multiple priors and index the conditional posteriors $f_\xi(\theta|x)$ by $\xi \in \Xi$.

4.2.3 Equilibrium Properties and Hypotheses

The equilibrium structure can be depicted with the following two graphs. Here, the states $\underline{\theta}$ and $\overline{\theta}$ are the bounds of the state space for which rational choices are determined from the outset. In the first illustration one sees that between $\kappa^* - \xi$ and $\kappa^* + \xi$ the proportion of agents choosing the risky option is linearly increasing, due to the uniform distribution of the noise term.

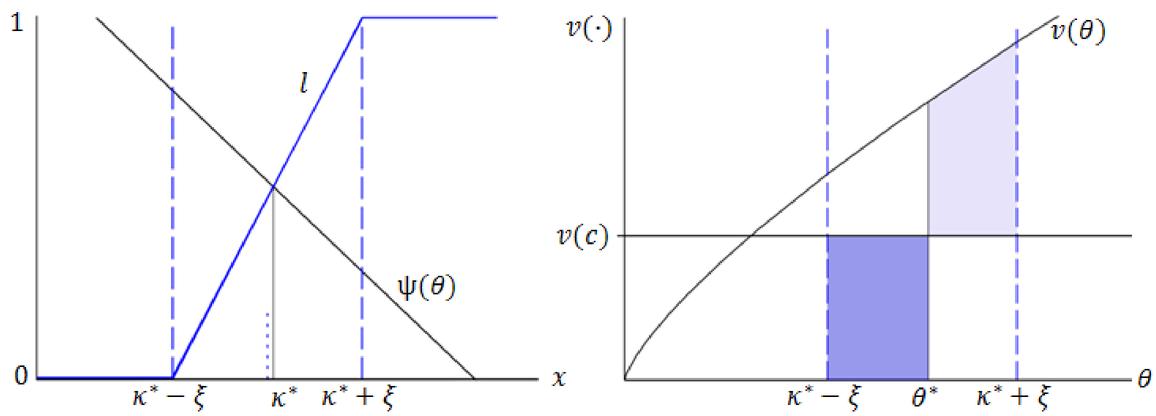

Figure 4.1: Optimal Cut-off Values and Equilibrium Structure

In equilibrium the expected utility for the marginal agent from choosing action 0 between $\kappa^* - \xi$ and θ^*, and the expected utility from choosing action 1 between θ^* and $\kappa^* + \xi$ must be the same to pin down the unique switching state θ^*. This is illustrated by the two shaded areas in the second illustration above.

Next, we discuss the equilibrium properties under ambiguity instead of risk. Here agents do not know the precision of their signals. They only know that $\varepsilon \sim U[-\xi, \xi]$ is uniformly distributed with precision $\xi \in \Xi = \left[\underline{\xi}, \overline{\xi}\right] \subset \mathbb{R}$. Hence $\underline{\xi}$ gives the highest precision of signals, as it induces the smallest support. Analogously, $\overline{\xi}$ implies the lowest precision of signals. As for the case of risk we have to limit the state space for ambiguity in order to avoid the spill over of signals. Hence, it must hold that:

$$\underline{\xi} < \overline{\xi} < \left\{\frac{1}{4}, \frac{\underline{\theta}}{2}, \frac{1-\overline{\theta}}{2}\right\}.$$

According to MMEU-preferences, we look for the cut-off value that minimises the chance of success for the speculative action. A higher cut-off will impair coordination and hence the maximum cut-off value $\overline{\kappa}^*(\Xi) \equiv \max_{\xi \in \Xi}\{\kappa^*(\xi)\}$ minimises the expected utility of action 1. If only few people are required for a successful coordination, prospects for speculation are good and thus the minimal precision should be considered under ambiguity aversion. Conversely, if the chances for a successful coordination require many agents to coordinate, the maximum cut-off is determined by the maximum precision, as the prospects for speculating are already bad.

Hypothesis 1: Players use undominated switching strategies under risk and ambiguity.

Furthermore, recent experiments have shown that subjects' behaviour exhibits excess aggressiveness regarding the individual cut-off points for switching from one strategy to the other, at least for risk (Fehr and Schurchkov, 2009; Heinemann et al., 2009). Following these results it is investigated whether this behaviour is consistent for global games under ambiguity.

Hypothesis 2: Players are excessively aggressive under risk and ambiguity.

Finally, we are particularly interested in the impact of varying both kinds of uncertainty. Hence the comparative statics are considered for the global game outlined so far. We deal with a single-prior game for risk and a multiple-prior global game for ambiguity; in both cases a theoretically unique prediction for the equilibrium can be derived.[36] For the equilibrium under risk, θ^* decreases with the imprecision of the signal. If the hurdle function indicates a

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[36] For details how the comparative statics are derived refer to the original results in Ui (2009).

low critical mass in θ^*, then increasing imprecision of signals raises the equilibrium cut-off. Hence, the optimal cut-off value κ^* decreases in the support of the noise and more coordination is facilitated.

For ambiguity the set of conceivable supports of noise Ξ is enlarged (i.e. $\Xi_1 \subseteq \Xi_2$) and the proportion of agents to receive a signal above $\overline{\kappa}^*(\cdot)$ decreases. Therefore, the optimal cut-off value κ^* increases in the support of the noise and reduces overall coordination.

Hypothesis 3: Risk and ambiguity have opposite effects on coordination in a global game.

The increase of risk and ambiguity respectively is implemented with the appropriate change of parameters in our experimental design. Based on the support of the noise term, in our setting, for increasing risk the equilibrium cut-off values will decrease and hence more choice of the risky option and more coordination are expected. For increasing ambiguity, the cut-off values increase and hence less choice of the risky option and less coordination are expected.

4.2.4 Related Experimental Literature

There is already some literature which discusses the experimental investigation of global games. One main contribution is Heinemann et al. (2004) who investigated the difference between public and private information in the global games setting. They prove that subjects play switching strategies with undominated cut-off values, as the theory predicts. Also they control comparative static predictions and confirm that the estimated subjects' thresholds increase with the payoff of the safe action and the number of players. Anctil et al. (2010) conduct a first experiment analysing the role of increasing information quality in global games based on the creditor coordination game. However, their set-up allows for multiple equilibria, which impedes a test of comparative statics. More importantly, none of the previous global games experiments incorporate both risk and ambiguity. The first work focusing on this aspect is Kawagoe and Ui (2010). Overall, they find that increasing information quality in terms of reducing risk regarding the private signals results in more choices of the safe action. Thus they establish the comparative statics predictions for comparing two degrees of risk. Moreover, their ambiguity treatment shows more choices of the safe option than their risk treatments. However, their experimental design compares one ambiguity and two risk treatments, so that they cannot test the comparative statics predictions for ambiguity, as pinpointed in this chapter. Another distinction is that for the one ambiguity treatment, they only consider two conceivable supports, whereas we use different sets of supports of the ambiguous noise term making the decision situation more complex and more realistic.

4.3 Experimental Design

The focus of our experimental investigations is directly derived from the theoretical considerations which yield different predictions for the unique equilibrium under risk and ambiguity. Accordingly, we need two treatments to vary the kind of informational uncertainty. Moreover, we are interested in the comparative statics for changing the degrees of risk respectively ambiguity. Overall, we have a 2 x 2 design with the four treatments low risk (LR), high risk (HR), low ambiguity (LA) and high ambiguity (HA).

The experiment itself followed the standard protocol of most global games experiments based on the speculative attack game (Cabrales et al., 2007; Cornad, 2006; Heinemann et al., 2004).[37] Subjects had a list of ten different signal numbers and correspondingly ten independent decisions to make between a safe action A and a risky action B for every round.[38] The decisions were displayed in random order to avoid any order effects in the application of switching strategies. Every experiment lasted for 15 rounds, giving us 150 decisions per subject. The state space was drawn from a uniform distribution between 20 and 180, the values 0 to 20 and 180 to 200 were not considered in order to avoid a spill over to the state space, given our parameterisation.

The only difference between the risk and ambiguity treatments is the information provided to the experimental subjects. While in the risk treatments subjects knew the exact precision of their signal, this information was not given in the ambiguity treatments. Here subjects only got the information that the actual precision (the support of the noise term) was contained in a given closed interval. Accordingly, in the risk treatments it was common knowledge that the signals were either within 10 or 16 around the true state. In the ambiguity treatment it was known that the precision of the signal was either between 8 and 16 in the low ambiguity treatment or between 3 and 19 in the high ambiguity treatment. All integer values in between

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[37] For the full experimental instructions refer to the appendix.

[38] Please note that the action 0 from the theory part is denoted as A for the experiment. Accordingly, action 1 from the theory is denoted as action B.

were possible. Both intervals were deliberately chosen so that the average which might be taken as a focal point was not the actual value of the signal precision.

Treatment	Set of conceivable precisions	Actual Precision	Subjects knew that $\boldsymbol{\theta}$ was contained in...
Low Risk (LR)	$\xi \in \{10\}$	10	$[x-10, x+10]$
High Risk (HR)	$\xi \in \{16\}$	16	$[x-16, x+16]$
Low Ambiguity (LA)	$\xi \in [8, 16]$	10	$[x-\xi, x+\xi]$
High Ambiguity (HA)	$\xi \in [3, 19]$	16	$[x-\xi, x+\xi]$

Table 4.1: Parameterisation of Noise Term across Treatments

In global games experiments there is no standard group size. However, it is imperative to have more than two players in one group, because with only two players the global games solution does not differ from a risk dominant solution. For our experiment, we chose groups of six players. To reach our objective of analysing both the difference in risk and ambiguity, which theory directly predicts and the difference in varying degrees of risk and ambiguity, which theory addresses with comparative statics, we chose a between-subjects design. This departs from much of the existing experimental literature on global games, which implement within-subject designs. However, for our purpose it is eminent to have different subjects play the different parameter settings. Finally, we deliberately omit the provision of feedback between single rounds, which is sometimes found in related experiments. This is crucial for our experimental design to prevent the dissolution of ambiguity due to resampling.

The experimental sessions were conducted in February 2010 at the University of Cologne Laboratory for Economics Research (CLER). The experiment was implemented using z-Tree (Fischbacher, 2007) and the subjects were recruited via ORSEE (Greiner, 2004). In total we had 120 subjects participating in the experiments, most of them with a background in economics or business administration. Subjects were matched in groups of six players and the matching remained constant over the course of the experiment. Every subject received a show-up fee of 2.50 € and the average pay-off was 16.97 € with sessions lasting about 1 hour and 20 minutes.

4.4 Experimental Analysis and Results

4.4.1 Undominated Switching Strategies

The theory of global games implies that subjects employ switching strategies, i.e. that they choose the safe action for low states and the speculative action for high states with one cut-off value where they switch their strategy. A strategy is dominated whenever the subject opts for an action although he knows that the other action yields a higher payoff with certainty. In our setting the dominant strategy is to choose the risky speculative action B, if $x > \overline{\theta} + \xi$ or to choose the safe action, if $x < \underline{\theta} - \xi$. The usage of switching strategies is well established by previous experiments on global games (Duffy and Ochs, 2009; Heinemann et al., 2004). We extend this finding into decision situations with risk and ambiguity.

Treatment	**Usage of undominated switching strategies**
Low Risk (LR)	98.89%
High Risk (HR)	98.44%
Low Ambiguity (LA)	96.75%
High Ambiguity (HA)	97.53%

Table 4.2: Equilibrium Cut-offs and Individual Estimates

Here 97.07% of the strategies in the risk treatments and 98.64% of strategies in the ambiguity treatments are consistent with such undominated switching strategies.[39] The frequencies with which these strategies are played do not differ significantly between treatments.[40]

4.4.2 Excess Aggressiveness and Best Response Behaviour

Based on the fact that most subjects employ switching strategies, we analyse whether these are based on unique cut-off points, i.e. the equilibrium prediction of the global game solution. Overall, only 30% of players under ambiguity and only 23% of the players under risk have a unique cut-off point. Regarding our aim of comparing subjects' behaviour and equilibrium

[39] For the ambiguity treatments, this is an overall average over both respective extremes of conceivable precisions.

[40] Using the Mann-Whitney Test, independent of the assumed interval of the support we find in the ambiguity treatments for the smallest possible support p = 0.86 and for the highest possible support p = 0.49. Furthermore, there is no significant difference between the risk treatments.

predictions, that imposes the problem that overall only 27% of the subjects always behave consistently with a unique cut-off. We proceed in the following by estimating cut-off values for all players.[41] Then we compare the estimated individual cut-offs with the equilibrium cut-offs for all treatments.[42] We find that the estimated individual cut-offs ($\hat{\kappa}$) are significantly smaller than the equilibrium predictions.[43] Hence, subjects are overly aggressive in their choices of the risky action as stated in hypothesis 2.[44]

Treatment	Precision	$\boldsymbol{\kappa}^*$	$\langle\hat{\boldsymbol{\kappa}}\rangle$	min	max	$\boldsymbol{\sigma_{\hat{K}}}$ (between)
LR	$\xi = 10$	89.1	72.14	42.78	105.0	17.32
HR	$\xi = 16$	87.1	72.11	25.06	122.13	17.74
LA	$\xi = 8$	89.7	71.47	33.75	140.69	22.94
	$\xi = 16$	87.1				
HA	$\xi = 3$	91.2	71.64	40.0	114.35	18.74
	$\xi = 19$	86.0				

Table 4.3: Equilibrium Cut-offs and Individual Estimates

In case of the ambiguity treatments this holds for any precision of the signal the subjects might have assumed in the given range. The actual mean cut-off points, as observed in our data set for those players having a unique cut-off, are all significantly lower than the equilibrium predictions.[45] This is consistent with the data from Kawagoe and Ui (2010).

To further analyse excess aggressiveness, we compare the aggregate mass of players choosing the risky action B with the equilibrium prediction for all possible signals. This gives us six graphical depictions for all treatments (see figure 4.2). The red lines represent the experimental data and the blue line the predicted equilibrium play. This behaviour illustrates that subjects play more aggressively than predicted by the theory, as many switch for private sig-

[41] We estimate individual cut-offs via logistic regression. Since subjects with unique cut-offs exhibit complete data separation we test for robustness of the estimates via the penalised maximum likelihood estimation (MLE) proposed by Firth (1993). However, the difference between ordinary MLE and penalised MLE estimation is only 0.23 units on average.

[42] In the ambiguity treatments we calculate the equilibrium cut-offs for the minimum and maximum support of the noise term for both these treatments.

[43] Two sided t-test: p-value < 0.01.

[44] Estimated values are indicated by $\hat{}$ and mean values by $\langle\cdot\rangle$.

[45] Two sided t-test: p-value < 0.01.

nals lower than the optimal theoretical cut-off. The robustness of our results regarding excess aggressiveness and the similar finding by Kawagoe and Ui (2010) suggests that this kind of behaviour is independent of the functional form of uncertainty. However, according to the definition of a best response in global games, the behaviour of economic agents depends essentially on their beliefs about other players' beliefs, i.e. higher order beliefs (Fehr and Schurchkov, 2009; Morris and Shin, 2003).

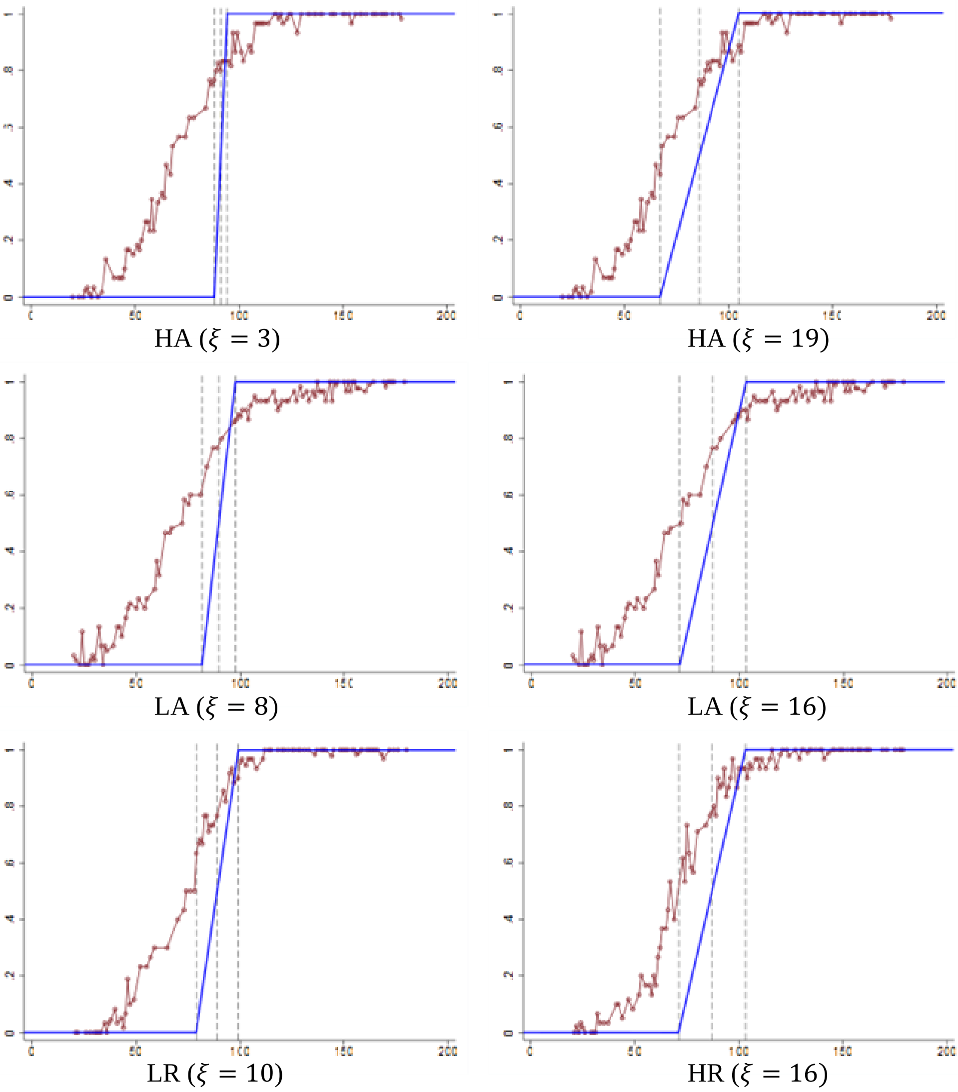

Figure 4.2: Aggregate Mass of Players choosing the risky action B

Hence, given that subjects have overly aggressive or optimistic beliefs about the other players, i.e. judging the common cut-off values smaller than the theoretical optimum in equilibrium, the results of excess aggressiveness can still be rationalised.

To test this hypothesis we employ two related measures for best response behaviour. First, we use the averages of estimated individual cut-offs ($\langle\hat{\kappa}\rangle$) and calculate the best responses on this basis ($\widetilde{K}_1$), given a linear utility function for every subject. This is done for every treatment by:

$$\frac{1}{2\xi}\int_{\theta^*(\langle\widetilde{\kappa}\rangle)}^{\widetilde{K}_1+\xi}\theta d\theta = c.$$

Secondly, we determine the highest state up to which coordination always failed and the lowest state from which onwards coordination was always successful. The centre of this interval serves as our measure for the threshold of a group. We then take the estimated group thresholds ($\hat{\theta}$) from our data set and compute the best responses on this basis ($\widetilde{K}_2$) for every group:

$$\frac{1}{2\xi}\int_{\hat{\theta}}^{\widetilde{K}_2+\xi}\theta d\theta = c.$$

This allows us to classify the strategies played in our experiment as best responses across all four treatments and yields the following results.

	Mean share of best responses		**Standard deviation**	
Treatment	$\widetilde{\boldsymbol{\kappa}}_{\boldsymbol{1\tau}}$	$\widetilde{\boldsymbol{\kappa}}_{\boldsymbol{2m}}$	$\widetilde{\boldsymbol{\kappa}}_{\boldsymbol{1\tau}}$	$\widetilde{\boldsymbol{\kappa}}_{\boldsymbol{2m}}$
Low Risk	92.38 %	92.58 %	0.0509	0.0440
High Risk	90.47 %	90.76 %	0.0703	0.0623
Low Ambiguity	89.27 %	90.33 %	0.0966	0.0923
High Ambiguity	89.49 %	89.44 %	0.0623	0.0616

Table 4.4: Mean Share of Best Responses

Both measures exhibit evidence for our hypothesis, showing that across all treatments about 90% of all decisions can be classified as rational based on a simple best response model with overly optimistic beliefs. Therefore overly optimistic beliefs in the speculative attack game, become a self-fulfilling prophecy.

4.4.3 Opposite Effects of Risk and Ambiguity

Regarding the comparison of risk and ambiguity in global games, we set out from a theoretical result derived by Ui (2009). Accordingly, an increase of risk should lower the cut-off points, whereas an increase of ambiguity should raise the cut-off points. Hence, determining the equilibrium behaviour for these two kinds of uncertainty, risk should theoretically improve coordination in global games and ambiguity should diminish it. Regarding risk, the experiment of Kawagoe and Ui (2010) found that decreasing risk, i.e. providing a more precise signal, increases the relative frequency of safe actions. However, for their results they did not provide statistical tests on the differences of the individual cut-offs, but employ logistic regressions on individual choices. We in fact focus on the individual cut-offs as a more fundamental and robust representation of individual behaviour in our speculative attack game. The following figure shows the estimated individual cut-offs across all four treatments:

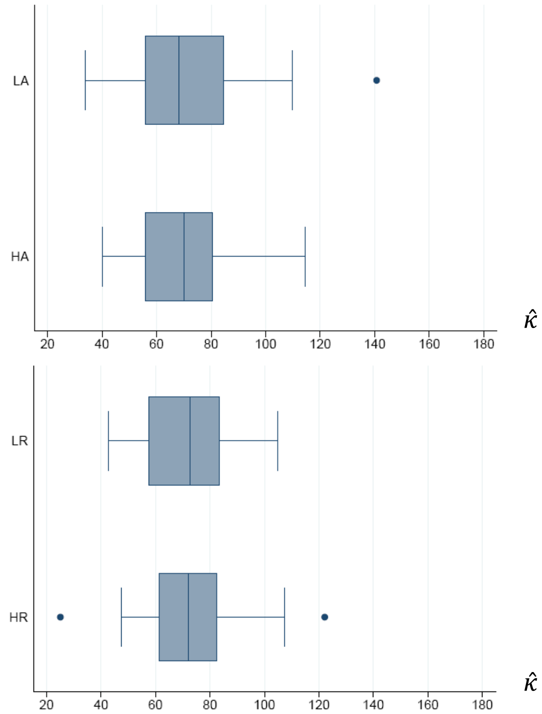

Figure 4.3: Estimated Individual Cut-off Values (Boxplot)

For both risk and ambiguity the two graphs are much alike. We can infer that there is no significant difference for either variation.[46] First, regarding risk this means that the results from Kawagoe and Ui (2010) are not robust, when comparing the distributions of individual cut-offs. Secondly, the distributions of cut-offs show that the predictions of the comparative statics do not hold for either kind of uncertainty. When increasing the degree of risk respectively ambiguity, theory predicts an increase respectively decrease of cut-offs for the individual switching strategies. We initially find no evidence for these opposite effects in our data. However, this might be due to the high variance of individual cut-offs as already discussed by the figure above.

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[46] Comparing LA and HA the Kolmogorov-Smirnov test has p = 0.958, for LR and HR the Kolmogorov-Smirnov test yields p = 0.594.

Hence, we additionally analyse individual behaviour based on logistic regressions with random effects. In doing so, we can directly estimate individual probabilities to choose the risky action B in our experiment, whilst controlling for several variables. The most central independent variables directly corresponding with our design are the *Signal* x subjects receive and a dummy variable for the high risk treatment *HR* respectively the high ambiguity treatment *HA*. Accordingly, the low risk and ambiguity treatments serve as the baseline. Furthermore, *Sex* is a standard control variable we elicited from the additional questionnaire. Then *Round* controls for time effects in the course of the experiment, whereas *1st Round* only controls for a difference in behaviour in the very first round, where subjects might still adapt to the experimental set-up. Finally, the dummy *Unique* κ flags all subjects which made their choices based on a unique cut-off value for all rounds of the experiment. Similarly, *Never Dom* is a dummy variable for subjects who never played dominated switching strategies. For the ambiguity treatments this is computed, given the highest conceivable support. Based on these variables, four models are tested in terms of the choice of the action.

According to the theoretical prediction increasing risk from the low risk to the high risk treatment should increase the probability to choose the speculative action B.

Dependent Variable: choice between action A and B				
Variables	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Intercept	-11.988***	-11.701***	-10.637***	-10.689***
	(0.311)	(0.437)	(0.450)	(0.453)
Signal x	0.163***	0.163***	0.166***	0.167***
	(0.004)	(0.004)	(0.004)	(0.004)
HR	-0.081	-0.084	-0.019	0.0378
	(0.255)	(0.269)	(0.267)	(0.277)
Sex		-0.212	0.016	-0.017
		(0.303)	(0.286)	(0.294)
1st Round		0.304	0.304	0.306
		(0.256)	(0.258)	(0.259)
Round		-0.023	-0.024	-0.024
		(0.014)	(0.014)	(0.015)
Never Dom			-1.795***	-1.998***
			(0.339)	(0.354)
Unique κ				0.730**
				(0.332)
σ_μ	2.547	2.551	2.543	2.529
ρ	0.664	0.664	0.663	0.660
Wald χ^2	2219.52	2216.86	2191.65	2177.61
$p > \chi^2$	0.000	0.000	0.000	0.000
N observations	9000	9000	9000	9000
n clusters	60	60	60	60

Table 4.5: Logistic Regression for Risk Treatments (Random Effects)

As a first result it is confirmed that the signal x always has a significant effect on the probability of choosing action B rather than A (see table 4.4). Also we find significant effects for the variables *Never Dom* and *Unique* κ. Thus in the risk treatments subjects who never played dominated switching strategies are less likely to choose the risky action B. Conversely, all subjects with a unique cut-off point are more likely to choose the risky action based on our experimental data. Further, all four models indicate that, there is virtually no effect for increasing risk, when considering the *HR* dummy variable with the low risk treatment as a baseline. This contrasts the theoretical predictions as well as the initial findings on risk in global games from Kawagoe and Ui (2010).

Applying the same regression techniques to ambiguity (see table 4.5), we even find significant evidence against the initial theoretical prediction, i.e. with increasing ambiguity, as indicated by the *HA* dummy variable, the individual probability to choose the speculative action B increases significantly.

Dependent Variable: choice between action A and B				
Variables	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Intercept	-8.971***	-9.886***	-11.036***	-11.034***
	(0.223)	(0.283)	(0.438)	(0.442)
Signal x	0.118***	0.120***	0.122***	0.122***
	(0.002)	(0.002)	(0.003)	(0.003)
HA	0.279	0.416*	0.542**	0.538***
	(0.239)	(0.220)	(0.262)	(0.262)
Sex		1.121***	0.967***	0.932***
		(0.217)	(0.243)	(0.252)
1st Round		0.590**	0.596**	0.596**
		(0.250)	(0.252)	(0.252)
Round		0.010	0.010	0.010
		(0.013)	(0.428)	(0.013)
Never Dom			1.243***	1.184***
			(0.360)	(0.368)
Unique κ				0.255
				(0.314)
σ_μ	2.276	2.297	2.274	2.277
ρ	0.612	0.616	0.611	0.612
Wald χ^2	2483.64	2558.80	2432.87	2420.57
$p > \chi^2$	0.000	0.000	0.000	0.000
N observations	9000	9000	9000	9000
n clusters	60	60	60	60

Table 4.6: Logistic Regression for Ambiguity Treatments (Random Effects)

This finding entails that in our experiment subjects behaved in an ambiguity affine manner, because for this kind of uncertainty the comparative static effect depended directly on the assumption of ambiguity aversion. Furthermore, ambiguity affinity is in some way consistent with the robust observation of excess aggressiveness in experimental global games. If subjects are overly aggressive or believe others to be overly aggressive and hence the common cut-off to be significantly lower, ambiguity loving behaviour further amplifies the choice of the speculative action for smaller signals. Considering the other significant effects in the regression, we find again that the signal is highly significant for predicting the choice between actions A and B, as already found in the risk treatments. More interestingly, we find a strong gender effect where males are more likely to choose the risky action B under ambiguity.

The actual round of the game is not relevant for the decision in both the risk and the ambiguity set-up. However, for risk we assert a strong effect of the first round on decision outcomes. Finally, we also find that under risk the subjects never using dominated switching strategies are also more frequently choosing action B. As for the analysis of cut-offs, the regression of individual probabilities to choose the risky action, does not support the theoretical predictions taken from Ui (2009).

4.5 Conclusions

In this paper we have investigated the effects of risk and ambiguity in global games. Even though theory predicts an opposite effect on coordination for these two different kinds of uncertainty, we do not find this effect in the experimental data.

First, we corroborated a central result from related experiment on global games, showing that subjects almost always (98%) employ switching strategies. However, the cut-offs they use for this strategies are only rarely unique, i.e. only for 27% of the players. Theory predicts a unique equilibrium of cut-offs for any given parameterisation of the global game. However, when estimating the cut-off values for each subject our experimental data revealed a high variance. Secondly, we also detected excess aggressiveness in subjects' behaviour, which is consistent with many other experiments on global games. Further, we demonstrated that the excess aggressiveness we find can be explained as a best response strategy based on the belief that other subjects behave overly optimistically. This is also supported by our data on the estimated individual cut-off values which are below the equilibrium predictions.

Finally, we tested the predictions of a theoretical model differentiating risk and ambiguity with respect to signal precision. This theory estimated that an increase in risk from our low to high risk treatment would result in lower individual cut-offs, more play of the speculative action and thus more coordination. In contrast, for an increase from the low to high ambiguity treatment, it predicts the opposite effect of higher individual cut-offs, less play of the speculative action and thus less coordination. Although a previous experiment by Kawagoe and Ui (2010) gave first evidence for the effect regarding risk, we could not confirm the theory in both regards. Regarding the estimated cut-offs between treatments we do not find a significant difference. A further analysis of individual probabilities with a logistic regression even suggests that if there are effects for the comparative statics, they might be diametrically opposed to the theoretical predictions. This means that subjects in this sort of game are eventually ambiguity affine. With regard to this final result, further work is necessary to elicit why subjects exhibit both excess aggressive and ambiguity affine behaviour in this kind of coordination game.

4.6 Appendix

The instructions have been the same across all treatments. We only changed the paragraph about what subjects know about the precision of their signal. The following version is translated from German and the according different paragraphs are exemplary given for the LR and LA treatment as the HR and HA treatments were modified accordingly.

Instructions

Welcome and thank you for participating in today's experiment! Please read the following instructions carefully; they are identical for all participants. Please do not communicate from now on. If you have any question, raise your hand and we will come to your place and help you. Furthermore, please switch off your mobile phone. Violating these rules can result in an exclusion from this experiment.

You can earn money in this experiment. The amount depends on your decisions as well as on the decisions of the other participants. At the end of the experiment, you individually receive your payoff in cash. Your payoff consists of a show-up fee of 2.50 EUR plus the sum of payoffs of the single rounds. In the experiment, the currency "ECU" (Experimental Currency Unit) will be used for your payoff. 1000 ECU equal one EUR (1000 ECU = 1 EUR).

At the beginning of the experiment you will be randomly matched with 5 other participants. You will never know who these participants are. You will interact with these participants for the whole course of the experiment as a group of 6 people; the composition of your group does not change. In total, the experiment consists of 15 payoff-relevant rounds with 10 individual decisions each. In each decision you have the choice between two options A and B. Before the first round, you will have to answer 8 comprehension questions at the computer.

Course of a round

There are totally 10 decisions between options A and B in each round. Your payoffs depend on your decision as well as on a number Y which is unknown to you. You will just receive a more or less precise hint about Y. Before we present you the decision options, we will begin with a characterization of the unknown number Y and the according hint.

- The computer randomly chooses an integer Y which lies between 20 and 180, i.e. Y can take the values 20, 21, 22, . . . 179 and 180. Each number is equally probable.

- For each decision, the computer chooses a new Y. [Information given in the LR treatment with $\xi = 10$]

- The number Y is identical for every participant, but is not told to anybody. Instead, every Participant will receive a hint about Y.

  All hints are integers and lie between Y – 10 and Y + 10. Every number between Y – 10 and Y + 10 is equally probable. For example, if you receive the number 80 as your hint, you know that Y lies between 80 – 10 and 80 + 10. And every number between 80 – 10 and 80 + 10 is equally probable. [Information given in the LA treatment with $\xi \in [8,16]$]

- The number Y is identical for every participant, but is not told to anybody. Instead, every participant will receive a hint about Y. All hints are integers and lie between Y – e and Y + e. Every number between Y – e and Y + e is equally probable. For example, if you receive the number 80 as your hint, you know that Y lies between 80 – e and 80 + e. And every number between 80 – e and 80 + e is equally probable. No participant knows the value of the number e, but all know that e lies somewhere between 8 and 16. How probable the single values of e are is not known by anybody. This means that you do not know how precise the hint number informs you about Y. Every participant only knows his own hint number. One's own hint number is drawn independently of the hint numbers of the other participants according to a random process. Hence your own hint number usually differs from the hint numbers of the other participants in your group.

Based on your hint number you now decide between the options A and B:

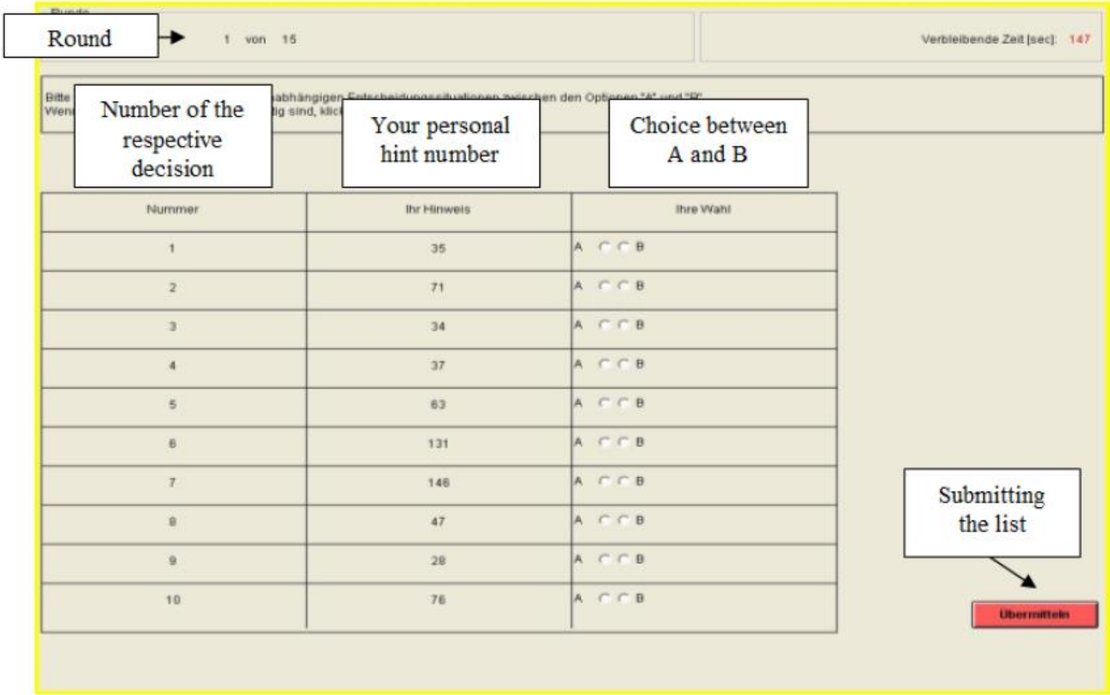
- <u>If you decide for option A</u>, you receive 40 ECU. The value for option A is always the same.

- <u>If you decide for option B</u>, you can reach a payoff of Y. However, this depends on how many of the other participants in your group have also chosen B in this decision. Option B is the more probable to yield a payoff of Y the more participants choose B. Furthermore: the higher the unknown number Y, the less participants are needed for option B to yield a payoff. The following table shows how many players are required given a certain value of Y for option B to yield a payoff of Y. (Every participant also gets this table on a separate sheet.)

If the *unkown number Y* lies within the following interval:	…then the *minimum number of participants* (including yourself), who have to choose option B for this to yield a payoff of Y is:
40 bis 59	6
60 bis 79	5
80 bis 99	4
100 bis 119	3
120 bis 139	2
140 bis 180	1

In order to determine the required number of participants for success of option B, the table is based on the formula: $8-\frac{Y}{20}$. This means that for, e.g. Y = 76 a minimum of $8-\frac{Y}{20}=8-\frac{Y}{20}=4.2$, i.e. 5 or more participants in your group have to choose option B for this to yield a payoff of Y = 76 ECU. The minimum number of participants is always rounded up and there is always at least 1 participant required.

Sample Screen for the decisions:



- After you have made your 10 decisions, please click on „Submit" to send your list.
- When all participants have submitted their 10 decisions, the round is over. Subsequently, a new round begins.

Exemplary calculation of payoffs

<u>Example 1:</u>

Suppose in this example the unknown Y = 81 has been chosen by the computer. [Remember: no participant knows the true value of Y, you always just receive a hint number]. According to the table, for Y = 81 there have to be at least 4 participants (or more) choosing option B for this to yield a payoff of Y = 81 ECU. Suppose, for example, 2 participants choose option A and 4 participants choose option B. Those 2 participants who have chosen option A each receive 40 ECU. Since 4 participants have chosen B, option B yields a payoff of Y ECU. Therefore, each of those 4 participants is credited Y = 81 ECU.

<u>Example 2:</u>

Suppose in this example the unknown Y = 81 has been chosen by the computer. [Remember: no participant knows the true value of Y, you always just receive a hint number]. According to the table, for Y = 81 there have to be at least 4 participants (or more) choosing option B for this to yield a payoff of Y = 81 ECU. Suppose, for example, 4 participants choose option A and 2 participants choose option B. Those 4 participants who have chosen option A each receive 40 ECU. Since 2 participants have chosen B, this option does not yet yield a success at Y = 81. Therefore, those 2 participants who have chosen option B receive 0 ECU for this decision. After all 15 rounds have been played, the payoffs of all decisions of all rounds will get summed up to a final payoff. In addition to your final payoff you will get the show-up fee of 2.50 EUR.

Questionnaire

At the end of the experiment, we will hand out a short questionnaire. The stated data therein, along with all other collected data, will be made anonymous. We would like to ask you to fill out the questionnaire.

5 Disclosing Conflicts of Interest

5.1 Introduction

Information and its reliability and completeness are central elements in economic decision making.[47] However, people frequently lack full information or decisions have to be made on the ground of asymmetric information as in principal-agent conflicts (Benabou and Laroque, 1992; Inderst and Ottaviani, 2009). This is especially the case whenever a decision maker does not have the necessary expertise, as for example a patient compared to his doctor or an individual investor compared to his financial advisor. Thus in many economic and social situations, people obtain advice from professional experts who are better informed or more proficient. However, when the interests of an advisor and a client are not perfectly aligned, a conflict of interest shapes their relationship. This is a major impediment to perfect information transmission and the realisation of efficient outcomes.

Our investigations address this general problem of information transmission. The conclusions drawn here can then be transferred to application such as conflicts of interest for financial advice. In this realm financial advisors are usually incentivised differently for different products; this prevents them from making optimal decisions for their customers. Moreover, in such a scenario customers might not be aware of conflicts of interest, which affect the strategic responses to their received advice. Sender-receiver games can be used to model conflicts of interest and their disclosure. In essence, disclosure is meant to prevent the advisor from giving biased advices. Accordingly, legislators, regulators and many academics regard mandatory disclosure of conflicts of interests as the best policy advice.

With our experiment we investigate the effects of transparency, when disclosing conflicts of interest. We use a standard cheap-talk sender-receiver game in line with much of the existing theoretical and experimental literature (Cai and Wang, 2006; Crawford and Sobel, 1982; Sanchez-Pages and Vorsatz, 2007). The sender can be perceived as bank advisor and is the only one fully informed about the true state of the world. This information is then transmitted to a receiver, who has to make a (financial) decision on this basis. However, the sender can

[47] This chapter is joint work with Axel Ockenfels and Roman Inderst. It is based on the diploma thesis of Julian Conrads (Conrads, 2009), with whom the data was jointly gathered and an earlier working paper version of this chapter was written.

but must not transmit the true state of the world as his message. According to our hypotheses and previous evidence we expect the messages to be highly dependent on the conflict of interest we induce for the sender and even more so by the degree of transparency with which the receiver is informed about the prevalence of this conflict. Therefore we introduce three different treatments, one with intransparency about the conflict of interest, one with a clear probability distribution for senders' having a conflict of interest and a final one where the conflict of interest is disclosed to the receiver. We find that disclosing the conflict of interest matters as it diminishes the informativeness of the game. Nevertheless, sender behaviour is characterised by overcommunication and receivers are discounting biased advice insufficiently.

The remainder of this chapter unfolds as following: section 2 presents some related literature and the basic game theoretical structure as the basis for our experimental study. Section 3 describes our experimental design and procedures. Then section 4 presents and analyses the main results. Section 5 gives a short discussion and finally the last section concludes the findings and gives an outlook on further extensions of our experiment.

5.2 Related Literature and Hypotheses

5.2.1 Related Literature

In this section we give a short overview of the related experimental literature on information transmission games as well as on disclosing conflicts of interest.

The first experimental investigation of Crawford and Sobel's information transmission game was conducted by Dickhaut et al. (1995). It has corroborated the theoretical predictions, and in particular proven that increasing the bias *b*, leads to senders gradually transmitting less information.[48] However, transmitting less information is still different from the equilibrium prediction of not transmitting any information; hence, an "overcommunication effect" is found. This effect was also confirmed by Cai and Wang (2006) for a similar strategic information transmission game.[49]

[48] For a survey on experiments regarding information transmission and cheap talk confer to Crawford (1998).

[49] Other recent experimental studies of information transmission games can be found in Cai and Wang (2006),Sanchez-Pages and Vorsatz (2007) and Wang et al. (2010).

Coming to the second point at hand, there is yet little literature specifically dedicated to studying disclosure in information transmission games. Disclosing conflicts of interest was first experimentally assessed by Cain et al. (2005). Their experiment was based on a principal-agent model, where the agents had financial incentives to exaggerate their advice. They chose an experimental design where the first treatment disclosed the induced conflict of interest, whereas the second treatment did not disclose it. However, in the disclosed conflicts of interest treatment, principals were not able to sufficiently discount the agents' reports. The agents did also not achieve higher payments in the undisclosed treatment as one would expect. These results are a first indication that increasing transparency does not necessarily increase efficiency under conflicts of interest. Koch and Schmidt(2010) replicate these main results in a different experimental setting. Another recent study of de Meza et al. (2007) on behalf of the Financial Service Authority (FSA) in the United Kingdom also showed only small evidence that mandatory information disclosure matters.

5.2.2 Theoretical Predictions and Hypotheses

We base our investigations on a simple information-transmission game, where a sender and a receiver are interacting. Generally, in this class of games the sender has better or more reliable information, but the receiver has to take a decision which determines the payoff for both players. Thus we have a dynamic game with incomplete information partioned into three general stages. First a state of the world s_i is drawn from $S = \{s_1, \ldots, s_I)$ with a probability distribution $p(s_i)$.[50] Secondly, the sender in the game is allowed to convey a message m_j from $M = \{m_1, \ldots, m_J)$ to the receiver. Finally, the receiver chooses an action a_k from $A = \{a_1, \ldots, a_K\}$. He is free to choose any action, but his only source of knowledge about the true state of the world comes from the received message.

In this paper we follow the model of Crawford and Sobel (1982) which specifies the following utility functions U_S for the sender and U_R for the receiver:

$$U_S(s, a) = -[a_i - (s_i + b)]^2$$

$$U_R(s, a) = -(a_i - s_i)^2$$

[50] Each state must have a positive probability and all individual probability must add up to 1.

This continues the basic setup and introduces the central parameter *b*, which constitutes the conflict of interest in the sender's utility function. In this game sender strategies can be either pooling or separating.[51] The equilibrium analysis yields, that there are no fully separating equilibria but only partial pooling equilibria (Crawford and Sobel, 1982). Therefore, unless the sender's and receiver's interests are identical, the cheap-talk game with a conflict of interest has no equilibrium in which the sender accurately reports the true state of the world, except for incidentally telling the truth. Hence, if the conflict of interest becomes transparent there should be no communication in this game.

Our analyses of potential overcommunication are following Cai and Wang (2005), who mainly use correlation analysis between states, messages and actions to assess the information flow in this game structure. The most important correlation is that between states and actions (CorrSA). This is the measure for the overall flow of information throughout the full game and can then be broken down into a part of the sender and a part of the receiver. The correlation between states and messages (CorrSM) shows us how much information a message conveys, if the correlation were 1 then every time the true state of the world is communicated to the receiver via the message. Correlations between messages and actions (CorrMA) serve as an indicator for the receivers' trustworthiness; again if the correlation were 1 then the receivers would fully trust every message.

For the intransparent treatment (T1) and semi-transparent treatment (T2) the theoretical prediction is that for $b=0$, we should also observe: CorrSM=CorrMA=CorrSA=1. Moreover, for $b=2$ we should find CorrMA=CorrSA=1, but CorrSM=0 in the intransparent treatment. And for $b=2$ in the semi-transparent treatment we expect CorrSM=CorrMA=CorrSA=0, as the players know about the possibility of the conflict of interest and thus cannot trust each other.

Concerning our disclosed treatment (T3), if there is no conflict of interest, i.e. $b=0$, perfect communication is possible in the game. The separating equilibrium yields full information, where it always holds that state=message=action. However, with a conflict of interest theory predicts the opposite. In the resulting babbling equilibrium the receiver has not faith into the

[51] The pooling strategy is characterised by sending the same message independent from the state drawn by nature. The separating strategy on the other hand means sending different messages depending on the drawn state. So both pooling and separating equilibria possible in this game. In the basic model the value of *b* is common knowledge.

messages and always chooses the action in the middle of the set *M*.[52]Regarding our analyses we expect most importantly to find CorrSA=1 in the no conflict of interest cases and CorrSA=0 in the cases with a disclosed conflict of interest. Based on the theoretical predictions we derive two basic hypotheses.

Hypothesis 1: For all treatments T1-T3 introducing the conflict of interest reduces information transmission (CorrSM) and reveivers' trustworthiness (CorrMA) across.

Hypothesis 2: Considering the conflict of interest under the different transparency conditions, we expect that the informativeness (CorrSA) gradually decreases when increasing the degree of transparency. Especially, in the disclosed treatment there should be no exchange of information.

Regarding the adaption of behaviour we make two further hypotheses, one for the sender and one for the receiver.

Hypothesis 3: In the disclosure treatment, senders lie more frequently and to a higher extent as in the intransparency treatments T1 and T2.

Hypothesis 4: With conflicts of interest are disclosed, a receiver is rationally less credulous and thus discounts the received messages more. So we expect receivers to increase their discounting of messages with increasing transparency.

5.3 Experimental Design and Procedures

Based on the existing experimental literature, there is still no consistent theory explaining the interplay of information transmission and disclosing conflicts of interest. Therefore we devise a new experimental design to study the effects when increasing transparency levels. For three different transparency levels we observe the behaviour of a potentially biased advisor and that of his client.

In the experiment we use a partners matching with pairs of one sender and one receiver. The roles of the individual subjects are also kept constant throughout the experiment in order to facilitate learning and reputation building. To avoid framing effects we opted to use the neu-

[52]Given the parameterization chosen in the experimental design of S=M=A={1, 2, 3, 4, 5}, this would mean receivers would always opt for action 3.

tral labels “participant A” for senders and “participant B” for receivers, rather than the technical terms sender and receiver or the applied terms advisor and advisee[53], which we sometime refer to for our interpretations.[54]

The game itself follows the three stage protocol as described in the previous section with S=M=A={1, 2, 3, 4, 5}:

1) Senders learn the “true state of the world” *s*. This is drawn from *S* according to a uniform distribution.[55]
2) Next senders choose which message *m* they transmit to the receivers.
3) Finally, after having learned about their message receivers can decided on which action *a* to take.

This game is played for 10 identical rounds with the same matched pairs of players. For understanding the games incentive structure it is important to bear in mind that payoffs for senders and receivers are determined by the congruence of state of the world and receivers’ actions. We use the same payoff functions as established in a related study by Wang et al. (2010):

Sender: $\Pi_S = 110 - 10|s + b - a|^{1.4}$

Receiver: $\Pi_R = 110 - 10|s - a|^{1.4}$

The integral component here it he bias *b,* which can be *B={0, 2}* with equal probability. For *b=0* there is no conflict of interest and payoff incentives are the same for both player types. However, with *b=2* the conflict of interest comes into play and the sender has an incentive to provoke higher values for the action, than the true state of the world is. The existence of the bias can be change with every of the 10 rounds, but is implements that in the end 50% of decision are played with a bias and 50% without a bias. Based on this set-up we implemented the three treatments T1-T3 as already described in the previous section. Here the wording of the subtle differences in transparency was crucial for implementing the experiment.

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53 For a full account of this decision also refer to Wang et al. (2010).

54 For the full experimental instructions refer to the appendix.

55 Note that the distribution and state space of S are common knowledge for both sender and receiver, only the individual instances are private information for the senders.

First of all, senders obtain the true state of the world and the value of the bias in all treatments. Further both the possibility of a bias, i.e. a conflict of interest, and its value of being 2, when it exists is common information also for receivers. Then in the intransparent treatment (T1) receivers do not get any information about the existence of a bias. In the semi-transparent treatment (T2), the receiver learns that for each round that is played there is a 50:50 chance of being opposed to a biased sender. The disclosed treatment (T3) communicates the bias not only to the sender but also to the receiver, thus there is no uncertainty left. Altogether, the degree of uncertainty is decreased from T1 to T3. The only feedback provided to the players is their payoff after every round. Based on that information receivers can only infer what the true state of the world has been in the previous round. In order to avoid reference point effects, when it comes to payoff all payments, where assigned in Experimental Currency Units (ECU). Furthermore, we used a questionnaire subsequent to the experiment so elicit what subjects believed about the other subject's decision making behaviour.

The experiment was conducted in February 2009 at the Cologne Laboratory for Economic Research (CLER). The subjects were seated randomly and there was no opportunity for communication. The instructions explained all possible actions and payoffs of senders and receivers so that payoffs could be looked up in a table instead of being calculated. Moreover, in order to ensure that every participant understood the game-structure we conducted a short quiz before the start of the experiment, which was filled out correctly by all participants. Overall, 96 subjects participated. About 50% of the participants were studying economics or business administration; the other half came from other fields of academic studies. All subjects received a guaranteed show-up fee of 2.50 Euro and on average the participants earned 14.40 Euro with sessions lasting for approximately 1.5 hours. For the recruitment we used the online recruitment system ORSEE (Greiner, 2004) and the experiment itself was programmed with z-Tree (Fischbacher, 2007).

5.4 Results and Discussion

5.4.1 Main Experimental Results

For assessing our two first hypotheses we consider the effect of introducing a conflict of interest into the three different information treatments. Here we obtain the following results, as presented in table 5.1:

Treatment	Sender		Receiver	
	CorrSM (*b=0*)	**CorrSM (*b=2*)**	**CorrMA (*b=0*)**	**CorrMA (*b=2*)**
Intransparent (T1)	0.946	0.616	0.809	0.600
Semi-transparent (T2)	0.921	0.686	0.812	0.773
Disclosed (T3)	0.995	0.443	1	0.395

Table 5.1: Effects of Conflict of Interest

Regarding the first hypothesis, as expected there is almost perfect information transmission without the bias across all treatment variations. This confirms the prediction based on the model from Crawford and Sobel (1982) that correlations between states and message are one. Moreover, we can confirm that shifting from the non-biased to the biased-setting the information transmitted by the sender drops. Nevertheless, there is still a positive correlation between states and messages. Therewith we can corroborate similar results, finding an "over-communication phenomenon" (Cai and Wang, 2006; Wang et al., 2010). More importantly, without a prevailing conflict of interest the disclosed treatment gives the most truthful messages, whereas with a conflict of interest the disclosed treatment gives the least truthful messages. These tendencies are still in line with the theory, but the correlation of states and messages in T3 being 0.443 and thus significantly different from T1 (5% level) , T2 (1% level) and zero (1% level) necessitates further discussion.[56] Possible explanations are that strategic considerations limit the amount of truth distortion or senders feel morally obliged to lie only within certain boundaries. This discussion is taken up in the next section with a more detailed analysis of sender behaviour.

For the receiver responses, we find that they also approach or equal one in the setting without a conflict of interest. However, as the bias is implemented, the credulity decreases. Receivers trust in the senders' messages is much higher in the two non-disclosure treatments.[57] When conflicts of interest are disclosed, a receiver is less credulous on a sender's biased message. More importantly, senders are more successful in misleading receivers when conflicts of interest are not made transparent. The change in the intransparent treatment, shifting from the

[56] All significances based on two-sided Mann-Whitney tests.
[57] However, the difference of CorrMA is only significant at a1% level between T2 and T3 (two-sided Mann-Whitney test).

non-biased to biased setting, is insignificant. The same holds for the second treatment, where we would have expected some adjustment of the receivers' actions, because now they were well informed about the possibility of bias. In the disclosed treatment we observe a significant drop of following behaviour, which is in line with our predictions. But still there is a correlation of 0.395 which is significantly different from the theoretic prediction of 0. Hence in the disclosed and semi-transparent treatment receivers are overly credulous.

Coming to the second hypothesis we compare the general efficiency of information transmission for the different treatments. We find that without a conflict of interest, there is no significant difference in terms of information transmission as predicted.

Treatment	**CorrSA, *b=0***	**CorrSA, *b=2***
Intransparent (T1)	0.729	0.451
Semi-transparent (T2)	0.780	0.620
Disclosed (T3)	0.995	0.198

Table 5.2: Information Transmission across Treatments

However, with the conflict of interest there is less information transmission in the disclosed treatment than in the two other treatments. In fact the correlation of 0.198 shows that information transmission converges to zero in this treatment. Unfortunately, the treatment differences we find are not statistically significant.[58] However, the related study by Cai and Wang (2005) reports similar results for a bias of 2, backing the robustness and reproducibility of the presented results.

5.4.2 Results of Sender Behaviour

After having established the main insights for the overall game, we now focus on hypothesis 3 and a more detailed analysis of the sender behaviour. Based on these findings we have a closer look at strategic elements of deception. Table 5.3 shows the high rates of deception in all treatments, where there is a conflict of interest. First of all we can state, that there is a trend towards less deception, when increasing the degree of transparency. Thus full disclosure of

[58] Based on two-sided Mann-Whitney tests.

conflicts does increase the frequency of lying as one might suspect.[59]Therefore the frequency of lying can still not explain the behaviour in all biased treatments.

Treatment	Frequency of deception
Intransparent (T1)	78%
Semi-transparent (T2)	69%
Disclosed (T3)	67%

Table 5.3: Frequencies of Deceptive Messages in Percent when *b=2*

Next we consider the extent of exaggeration, which is defined as the difference between a sender's message and the true state of the world (see figure 5.1).Here we find that increasing the transparency level also significantly increases the extent of the lies.[60] In particular, for T1 the extent of exaggeration is 1.22 and for T3 it is 1.79.[61]Therefore not the frequency of lies but their size drives the overall results for deception in the disclosed treatment.

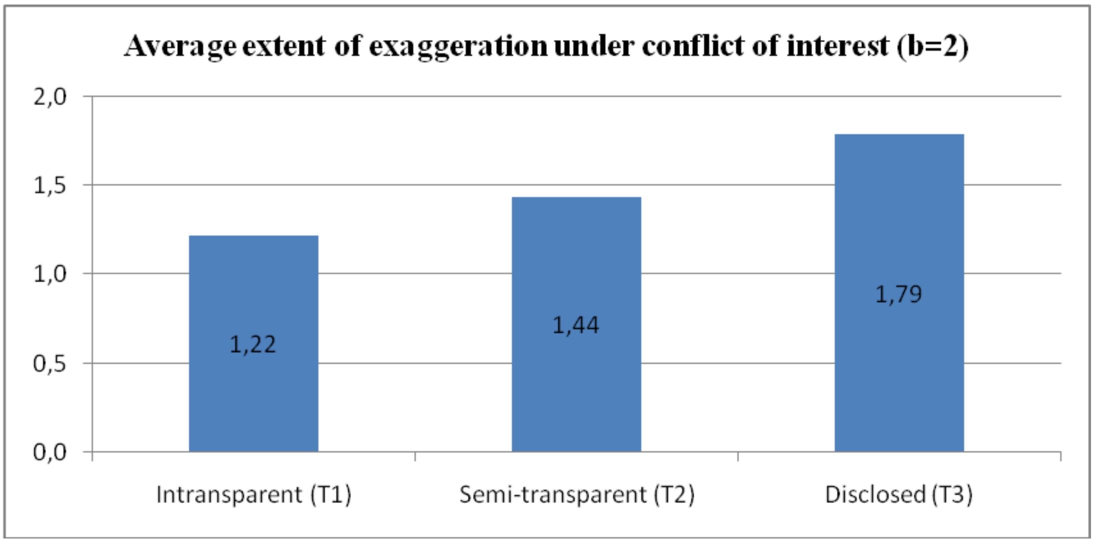

Figure 5.1: Average Extent of Exaggeration (b=2)

Nevertheless, the average extent of exaggeration is still below the theoretical prediction of 2. So from a theoretical perspective our experiment shows that senders do lie less often and to a

[59] However, the differences between the three treatments are again not statistically significant using a two-sided Mann-Whitney Test.
[60]At the 5% level with based on a two-sided Mann-Whitney test.
[61]Again the differences for the other treatments are not statistically significant.

less higher degree than predicted. Thus hypothesis 3 is only partially confirmed, senders feel more morally licensed to distort the truth under disclosure, but they still overcommunicate.

5.4.3 Results of Receiver Behaviour

Next we discuss the data concerning receiver behaviour. Analogously to frequency and extent in the sender's lying, we can break down the receiver's following behaviour into frequency and extent of discounting. For the frequencies, the data from table 5.4 shows the same behaviour across all three treatments, where messages are followed in 50% of the decisions receivers make. 50% is also the ex ante probability one would expect for accidentally following an honest message. However, the main question is whether receivers have learned to distinguish trustworthy from deceptive messages in the game. Therefore we compare the frequencies of following honest and dishonest messages. In the two non-disclosed treatments there is little deviation from the 50% probability of following an honest or dishonest message.

Treatment	**Overall message following**	**Following honest message**	**Following dishonest message**
Intransparent (T1)	51%	51%	47%
Semi-transparent (T2)	47%	62%	45%
Disclosed (T3)	51%	77%	27%

Table 5.4: Frequencies of Message Following

However, in the disclosed treatment we find 77% of the receivers following honest messages, which is significantly different from T1 at the 10% level.[62] Overall, while the degree of transparency is increasing from T1 to T3, a gradual increase in the frequencies of following honest messages is observed. Apparently, subjects can make use of the additional information they are provided with in this regard. Looking at the receiver's behaviour regarding dishonest messages proves that in the disclosed treatment the receivers also follow dishonest messages less frequently. The difference for following a dishonest message is significant for both transparency treatments compared to the disclosed baseline treatment T3; on a 1% level for T1 and

[62] Based on two-sided Mann-Whitney test.

10% level for T2.[63] Hence receivers' propensity to distrust in the disclosed setting also has a negative impact on their following behaviour.

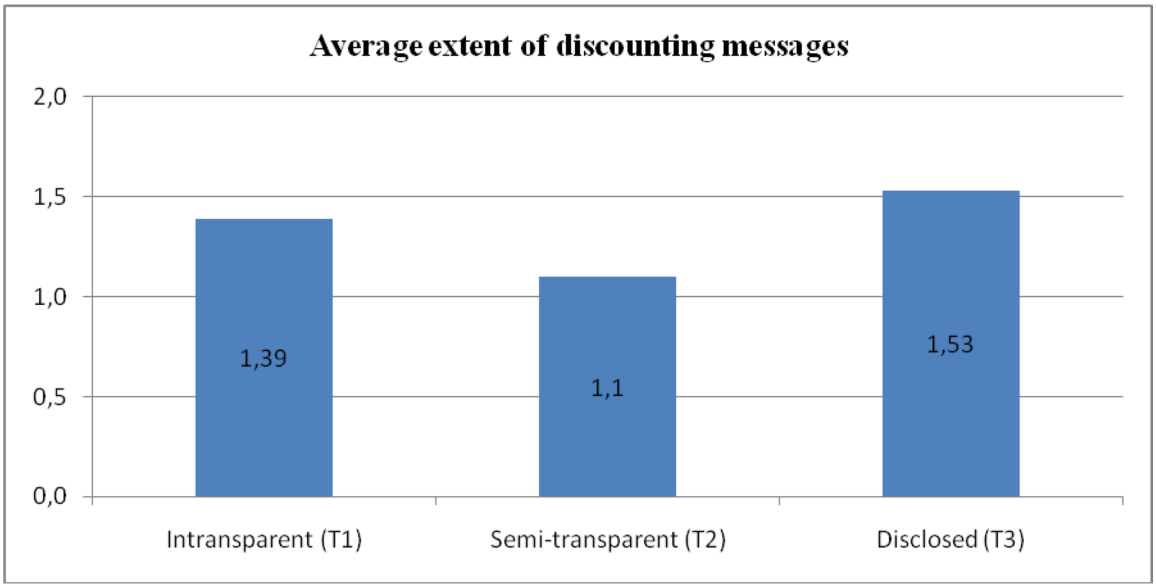

Figure 5.2: Average Extent of Discounting Dishonest Messages

As the analysis of frequencies has shown, the receivers often do not follow a sender's message; this means they mistrust their given advice. Figure 5.2 illustrates the average extent of discounting dishonest messages for all treatments. Here, one would expect a trend analogous to figure 5.1, where the increasing degree of transparency gradually pushes the extent of discounting. However, this is not reflected in the data. Generally, the disclosed treatment yields the highest discount rates, but there are no significant differences between the treatments. Receivers are unable to find the right extent for discounting messages. In neither treatment the theoretical optimum of discounting by the full bias of 2 is realised. Thus hypothesis 4 cannot be confirmed, as receivers' do not sufficiently adapt their discounting behaviour, when informed about the conflict of interest.

5.4.4 Behaviour Type Analysis

Also our data can be discussed in the light of different behavioural types among the subjects. This assumes that people have different levels of sophistication due to their reasoning capabil-

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63 Based on two-sided Mann-Whitney test.

ities.[64] Applying a definition of behaviour types based on Crawford, we can classify our subjects for each treatment.[65] In this definition three levels of sophistication are distinguished. The lowest level of sophistication type players are not playing strategically, i.e. do not use any additional information on the other players incentive structure, they only report the truth or follow the message they receive. They are called *trusters* as for the senders and *believers* for the receivers. The next level is adjusting his action to maximize his own pay-off whilst assuming the other player is not behaving strategically but according to one of the types previously described. These players are called *liar* or *inverter*. Showing the highest level of sophistication there are the *"sophisticated liar"* and *"equilibrium inverter"* types. These types anticipate the basic strategic reasoning of a liar or inverter and respond by further adjusting their strategy, i.e. an inverter always send the message M=5 and the equilibrium inverter always chooses the action A=3, irrespective of what message he receives.

A given subject can be mapped to one behaviour type, if the subject's behaviour is consistent with that type for more than 60% of his decisions. About 75% of all participants can be classified. For the senders we get the following classification results:

Type name	**Level of sophistication**	**Number of classifiable senders per treatment**		
		T1 "intransparent"	**T2 "semi-transparent"**	**T3 "disclosed"**
Truster	L0	0	1	3
Liar	L1	12	9	2
Sophisticated Liar	L2	0	1	7
N/A		4	5	4
% classified		75%	68.8%	75%

Table 5.5: Behaviour Type Classification for Senders

Comparing the three transparency treatments, we find that in the two intransparent treatments the simple liar is the dominating player. But in the fully disclosed treatment senders respond to the fact that their bias is publicly known and become more sophisticated liars.

64 See Costa-Gomes, Crawford and Broseta (2001) and Costa-Gomes and Crawford (2006).

65 For the formal definition of the behaviour types refer to appendix 5.6.2.

These results are consistent with the overcommunication effect. Due to a lack of sophistication senders transmit too much information compared with the theoretical prediction. Regarding the receivers, the following classifications can be made:

Type name	Level of sophistication	Number of classifiable receivers per treatment		
		T1 „intransparent“	**T2 „semi-transparent“**	**T3 „disclosed“**
Believer	L0	6	6	3
Inverter	L1	4	1	7
Equilibrium Type	L2	3	3	0
N/A		3	6	6
% classified		81.3%	62.5%	62.5%

Table 5.6: Behaviour Type Classification for Receivers

Here the overall degree of sophistication is lower than for the senders. Especially, there is a lack of *equilibrium type* players in the disclosed treatment. This finding is consistent with the already obtained result that receivers are more credulous to senders' messages in the non-disclosure settings.

5.5 Conclusions and Outlook

This study is motivated by the classical principal-agent conflict as for example in giving financial advice. Our experimental design takes up an established theory with some previous experimental studies and introduces different degrees of transparency as a new feature. Thus with our different treatments we test the effect of intransparency (T1), semi-transparency (T2) and full disclosure (T3) on conflicts of interest in a basic information transmission game.

Overall, the increase in transparency had a significant effect on the behaviour with respect to conflicts of interest. It increases senders' lying behaviour, although they do not lie up to the theoretical optimum. This confirms the existence of an overcommunication effect. Receivers are alert to distrust messages, but they do not discount enough. Furthermore, there is no significant difference between the intransparent and semi-transparent treatment. Apparently, subjects in our experiment did not experience the mere possibility of a bias as salient enough to adapt their behaviour. Finally, full disclosure of conflicts does not prevent a majority of advisors from deceiving their clients. In the two non-disclosure treatments some fairness concerns must have prevented senders from inflating their advices to a higher extent. In fact, none of the receivers and only few senders adopted the equilibrium strategy when conflicts were disclosed.

Guiding future research, in contrast to previous studies such as Cai and Wang (2006), we introduced a partner-matching into the experimental design. However, we could not find any trends for reputation building. So maybe extending the partner-matching to a game of more than ten periods can facilitate studying reputation effects. Another extension with highly realistic application could be offering a second advisors opinion, competition for honesty between the advisors might also have a strong effect on their diligence in communicating the true state of the world even with a prevailing conflict of interest. Potentially only adding the second advisor even without taking his advice can improve the overall communication in such games.

5.6 Appendix

5.6.1 Additional Results

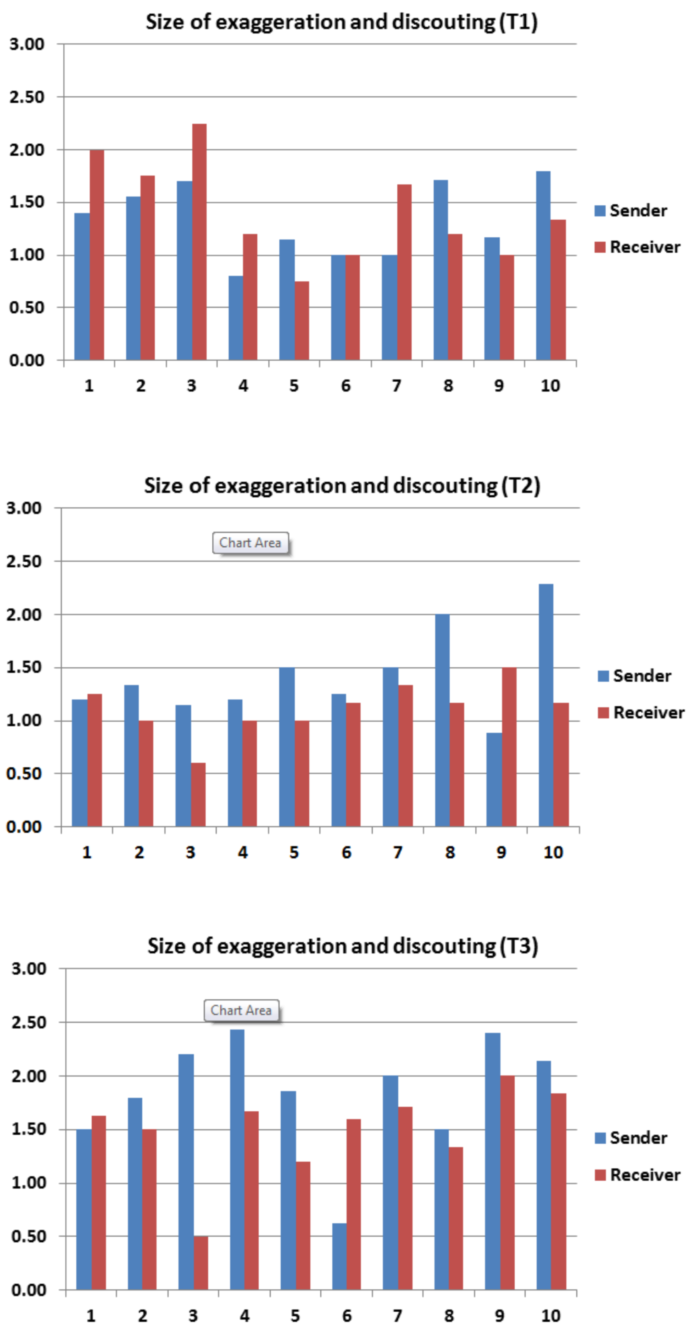


Figure 5.3: Average Sizes of Exaggeration and Discounting over rounds

5.6.2 Behaviour Type Definitions

Type name	Level of sophistication	Sender's message, given *s*				
		s = 1	***s = 2***	***s = 3***	***s = 4***	***s = 5***
Truster	L0	1	2	3	4	5
Liar	L1	3	4	5	5	5
Sophisticated Liar	L2	5	5	5	5	5

Table 5.7: Classification definition of behaviour types for senders (b=2)

Type name	Level of sophistication	Receiver's action, given *m*				
		m = 1	***m = 2***	***m = 3***	***m = 4***	***m = 5***
Believer	L0	1	2	3	4	5
Inverter	L1	1	1	1	2	3
Equilibrium Inverter	L2	3	3	3	3	3

Table 5.8: Classification definition of behaviour types for receivers[66]

[66] Please note that this definition can only partially be applied in T3, where also the receivers have full information about *b*.

5.6.3 Instructions

Below we show the instructions translated from German:

Instructions

Welcome to today's experiment and thank you for your participation. Please read the following instructions carefully; the instructions are the same for all participants. If you have a question, please do not hesitate to raise your hand, we will personally help you.

You can earn money in this experiment. How much money you earn depends on your decisions and the decisions of the other participants. At the end of the experiment you will be paid in cash. The payoff consists of your accumulated earnings from the respective rounds plus a show-up fee of 2.50€. In the experiment we will use "ECU" as currency. 100ECU stands for 1€ (100ECU=1E).

From now on, please do not communicate with other participants. Please also ensure that your mobile phone is switched off. Violating these rules can result in the exclusion from the experiment. All interactions in this experiment will take place through the computers.

The experiment consists of ten payoff-relevant rounds. At the beginning of the experiment you will be randomly matched with another participant with whom you will interact for the first ten rounds. One participant will be assigned the role of "participant A" the other one as "participant B". You will be informed about your role at the beginning of the experiment. You will maintain your role during the whole experiment. Your decisions and payments are anonymous.

Procedure of one period

- At the beginning of each round the computer randomly determines a random number Z. This random number can take the values 1, 2, 3, 4 or 5. Only participant A will be informed about the randomly generated value of Z.
- After participant A has received the information about Z, he sends a message to participant B: "The random number I have received is X". For X, participant A can choose every value out of the values 1, 2, 3, 4 and 5, independently of the randomly generated value of the random number.

- After participant B has received the message about the value of the random number from participant A, he decides for an action. Participant B can choose for an action out of the actions 1, 2, 3, 4 and 5.

Calculation of the income from one period

The income from each period for participant A and participant B depends on the random number Z and on participant B's action. Participant B's income is higher when his action is closer to the random number. In comparison to participant B, participant A can be subject to a difference of interest I. In case of I=2, participants A's income is higher when participant B's action is closer to the random number plus the value of the difference of interest. In case of I=0, there is no difference of interest and participant A's income is higher when participants B's action is closer to the random number.

T1 "intransparent": At the beginning of each period, participant A will be informed about the value of the difference of interest. Participant B however receives no further information about the value of the difference of interest.

T2 "semi-transparent": At the beginning of each period, participant A will be informed about the value of the difference of interest. Participant B is however just informed that the probability for I=2 is 50 percent.

T3 "disclosed": At the beginning of each period, participant A as well as participant B will be informed about the value of the difference of interest.

The exact incomes, which occur for participant A and participant B in case of no difference of interest, can be seen in the following table:

Table of incomes for participant A and participant B without difference of interest:

Possible actions of the receiver

Possible values of the random number Z		action=1	action=2	action=3	action=4	action=5
	Z=1	110	100	84	63	40
	Z=2	100	110	100	84	63
	Z=3	84	100	110	100	84
	Z=4	63	84	100	110	100
	Z=5	40	63	84	100	110

Without the difference of interest (I=0), participant A as well as participant B achieves the highest possible payoff when the difference between the random number and participant B's action is as small as possible. For example, if the random number is 2 and participant B's action is also 2, both players each achieve the income of 110 in this period.

When the difference of interest exists, participant A's income is higher when participant B's action is closer to the random number plus the value of the difference of interest. The following table shows participant A's and participant B's income per period when participant A is subject to the difference of interest (I=2). The respective left value in each possible concourse of random number and action represents participant A's income and the right value participant B's income, respectively.

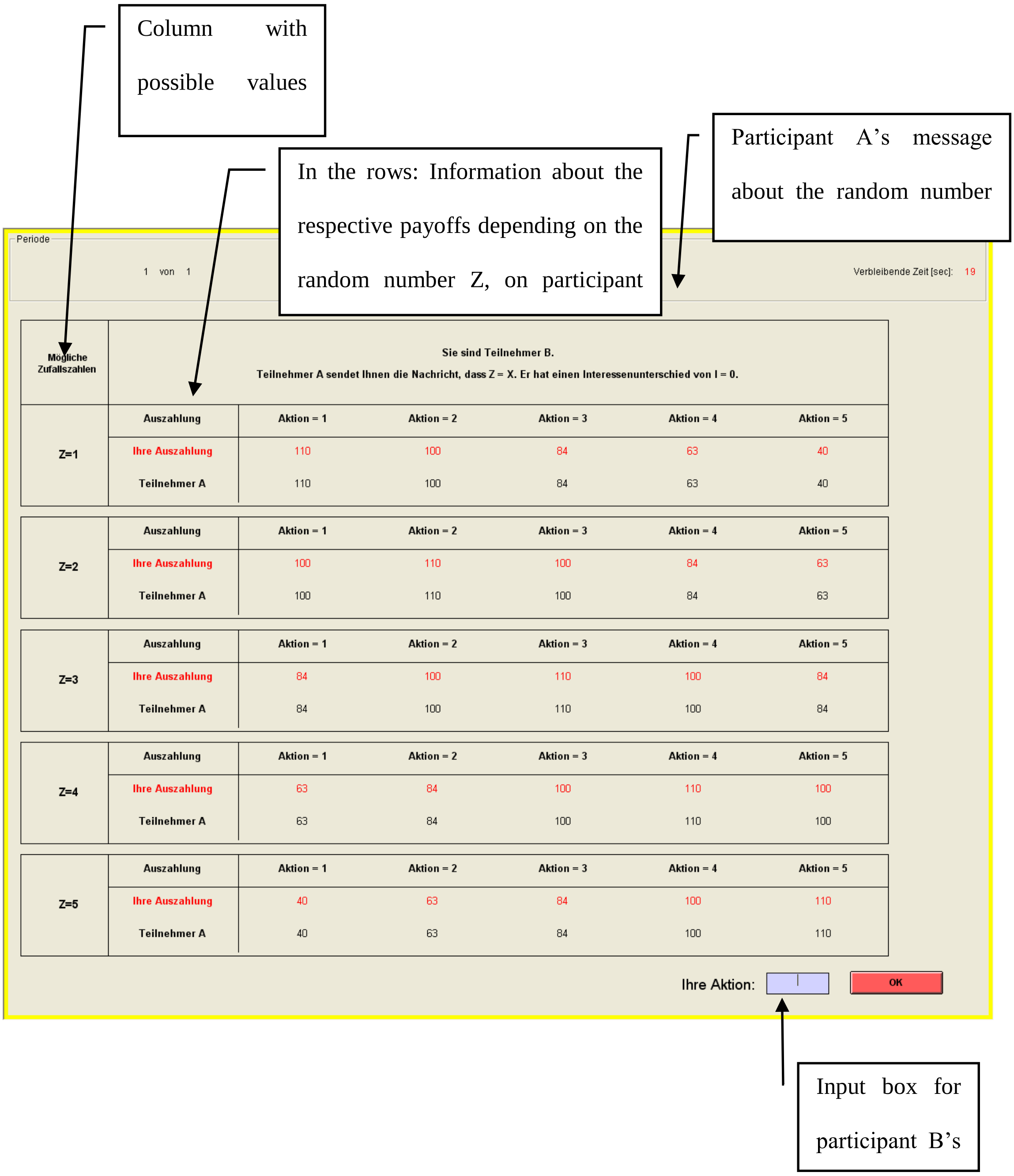
Table of incomes for participant A and participant B with difference of interest:

Possible actions of the receiver

Possible values of the random number Z		action=1		action=2		action=3		action=4		action=5	
	Z=1	84	110	100	100	110	84	100	63	84	40
	Z=2	63	100	84	110	100	100	110	84	100	63
	Z=3	40	84	63	100	84	110	100	100	110	84
	Z=4	15	63	40	84	63	100	84	110	100	100
	Z=5	-13	40	15	63	40	84	63	100	84	110

Here, participant A's income takes the highest possible value when participant B's action is equal to the random plus two. Participant B still achieves the highest possible income when his action matches the value of the random number. For example, when the random number takes the value of two, participant A's highest possible income occurs when participant B decides for the action=4. However, participant B's highest possible income occurs when he decides for action=2.

Example: Screenshot for participant B



6 Two Dimensional Fairness in a Real Effort Game

6.1 Introduction

The impact of fairness on economic decision making is already well established in the literature on various distribution, market and other games (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999).[67] The currently existing models essentially formalise fairness in terms of a one-dimensional function of monetary pay-offs. However, in many real life encounters, the issue of multi-dimensional fairness may play a crucial role, i.e. payment, effort and ability might all shape our perception of fairness to some extent. We concentrate on the fairness of wages as prime example, since Akerlof and Yellen (1990) found that workers reduce efforts, when their wage decreases with reference to what they find to be the fair wage. Furthermore, when considering the relation of working effort and payments, as a matter of fact we know that people in jobs with high wages also typically work long hours (e.g. investment bankers, lawyers and management consultants). This phenomenon is so striking that we have pervasive evidence even with macro level data. Aguiar and Hurst (2008) for example, find that the relative wage increase for well educated employees over the last decades is accompanied by a large relative increase in working hours.

We study the general relationship between working effort and monetary payments in a labour economics setting. Therefore we devise a real effort game to investigate fairness along these two dimensions. In our design, subjects participate in a meaningful real effort task with two player types. Players A receive 12€ and players B 2€ as a fixed payment for the same task, which they can pursue as long as they want. Overall, they have only two decisions to make. Firstly, they can decide how long they work, whilst they are informed about the working time and payment of a matched partner. Secondly, they can redistribute respectively bargain over the difference in fixed payments. With regard to the two pivotal dimensions of fairness we extend a standard model of social preferences with an additional conversion factor for time and money, allowing us to derive predictions for the offers and acceptance thresholds in the redistribution stage.

[67] This chapter is joint work with Georg Gebhardt.

Regarding working times, we find results consistent with the existing literature on gift-exchange games. For monetary transfers, our aggregate data is also consistent with standard results from the dictator and ultimatum game literature. However, when combining the two dimensions, we can estimate the conversion factor for time and money. From this new perspective, we are able to expose a self-serving bias effect, substantiating that the high endowment players A distort their conversion factor to justify lower offers.

Section 2 discusses the related theoretical and experimental literature. Section 3 presents our experimental design and procedures. Section 4 introduces the extension of a social preferences model with a conversion factor for time and money. Then section 5 delivers our experimental results in terms of working times and monetary transfers. Finally, the main insights are summarised in section 6.

6.2 Related Literature

To the best of our knowledge, there is no literature on multi-dimensional outcome fairness, yet. Hence, we start with a theoretical and experimental analysis studying the impact of two-dimensional fairness based on time and money. Nevertheless, this general idea relates to existing literature on social and fairness norms, where other non-monetary dimensions of fairness such as “spectator status” have already been discussed (Croson and Konow, 2009; Offermann, 2002). More importantly, there is some literature showing that economic agents tend to distort some social norms to their own advantage, thus exhibiting a self-serving bias (Babcock and Loewenstein, 1997; Babcock et al., 1995). Moreover, our experimental design also relates to the standard literature on dictator and ultimatum games, which can also be taken to verify the robustness of our main results (Forsythe et al., 1994; Güth et al., 1982). Finally, the literature on social preferences (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999) can be integrated in our theoretical derivation of hypotheses and help organising our results. Here the importance of equity norms to explain bargaining behaviour and how people perceive fairness in terms of pay-off distributions is already well-established. However, the interrelation of pay-offs and working effort is not explicated in these models.

6.3 Experimental Design and Procedures

We conducted a real effort game with two different types of players.[68] One type receives a fixed payment of 12€ (A), the other receives 2€ (B). Therewith we induced initially unfair monetary payments. Then both player types were asked to perform a real effort task. Here, the payment for both was common knowledge and both had to perform the same task without any restrictions on their effort provision. The effort game literature stresses a crucial difference between hypothetical tasks and real effort tasks (Brüggen and Strobel, 2007; Garcia-Gallego et al., 2008). Here real effort tasks are generally taken more seriously and accordingly more working effort is provided. Even stressing this effect, we chose a meaningful real effort task, in order to avoid that subjects would quit the experiment prematurely. As the task we used questionnaires from a previous classroom experiment and asked the subjects to type these questionnaires into a computer mask to facilitate further scientific usage. In this experiment, we focused on extra working efforts, which are provided voluntarily. Hence, we did not stipulate exogenously given working times. Moreover, in order to avoid any obvious reference points, we scheduled the experiment for 2 hours, which was clearly too long for compensating the opportunity costs of the students, considering our fixed payments. Also we provided an affluence of 153 questionnaires per subject, in order to avoid that the subjects simply processed all questionnaires.[69] These questionnaires were the same for all subjects and always had the same order.

After being randomly matched to another player and being assigned a role as player A or player B, the experiment was structured into two essential parts. In the first stage, the real effort task was executed and subjects were continuously informed about their own working time and their partner's working time. The players of a pair started at exactly the same time. Moreover, the two different fixed payments were always salient and common knowledge. We deliberately omitted any other information, such as the number of questionnaires typed in or accuracy. Therefore, working time and payment were the only two relevant dimensions of information, which could be observed. In addition, we implemented a pop-up box, which notified one player as soon as the other player decided to finish his real effort task. This pop-up

[68] For the full experimental instructions refer to the appendix.

[69] Additionally, in the instructions, we explicitly stressed that we did not expect that all questionnaires were used.

had to be confirmed, so that we could be certain all players realised when the associated partner finished his effort stage. After this notification, the remaining players were free to carry on with their task, but the finished players had no opportunity to return to the real effort stage. In the second stage of the game the payments could be re-distributed based on the working time of the two players. Here, we implemented our three treatment variations as follows:

Treatment	1) Effort Stage	2) Redistribution Stage
Baseline (T1)	yes	no
Dictator (T2)	yes	Dictator Game
Ultimatum (T3)	yes	Ultimatum Game

Table 6.1: Experimental Treatments

In the baseline treatment (T1), both player types immediately leave after they have provided their effort. Hence, this treatment is a simple gift-exchange game between experimenter and subjects, with no interaction. In the dictator treatment (T2), we conduct a dictator game, where the high payment players A have the opportunity to balance the unequal payment, i.e. they can transfer a share (0 to 10€) of their excess earnings to their matched partner. In the ultimatum treatment (T3), we conduct an ultimatum game, where the high payment player A can make a bargaining offer (0 to 10€) to the low payment player B, who can accept the offered contribution or reject and perish the surplus for both players. The standard ultimatum game protocol presupposes the responder to be still present, when he receives his offer. But as we wanted to avoid any externalities regarding work motivation in our design, we decided to capture the responder's behaviour with the strategy method (Selten, 1967). Hence, each player was independent of the working status of his partner and could always leave the experiment, once he decided to finish the working task. Also in deviation from common experimental designs, we framed the decisions in this one-shot game in Euros (€) rather than experimental currency units (ECU) to make the difference in payoffs immediately accessible.

The experimental sessions took place in February and November 2010 at the Cologne Laboratory for Economic Research (CLER). In order to avoid the subjects influencing each other's behaviour when leaving the room, we invited four groups of 8 subjects for each session, which entered the laboratory in waves with a time delay of 15 minutes for each wave. Hence, the groups remained big enough to maintain anonymity between subjects, but by having various groups in different stages of the experiment at any time, whenever someone left the laboratory it became unclear, how long this particular individual had previously worked. The subjects were seated randomly and there was no opportunity for communication. Subject payments were organised with payment receipts to ensure that subjects could immediately leave the laboratory, when they decided to finish the experiment. In exchange for the payment receipt, the subject could collect the cash payment during the next weeks. We had 64 students per treatment and 192 students overall participating. The average payment was 9€ including a guaranteed show-up fee of 2.50€. On average each subject participated in the experiment for 50 minutes. For the recruitment we used the online recruitment system ORSEE (Greiner, 2004) and the experiment itself was programmed with z-Tree (Fischbacher, 2007).

6.4 Model of Tow-Dimensional Fairness and Hypotheses

In the following three hypotheses are outlined, based on an extended standard model for social preferences. The first hypothesis for our experiment comes straightforward from the literature on gift-exchange games. In the baseline treatment we provide no opportunity for redistribution or interaction. Hence, fairness considerations do not apply between subjects, but rather between the individual subject and the experimenter giving the endowment (Akerlof, 1982; Fehr et al., 1993). Here, we expect that subjects take the endowment as a gift and provide working effort in exchange. The players A with the much higher endowment of 12€ will work harder than the players B with the cheap endowment of 2€.

Hypothesis 1: Players A provide higher working efforts than players B.

To derive further hypotheses for our experimental design, we formalise our notion of two-dimensional fairness based on the model of outcome-fairness by Fehr and Schmidt (1999). With initial endowments A and B we induce inequality in our basic set-up. Accordingly, x_i and x_j represent the initial monetary distributions from either player's perspective. With t_i and t_j we denote the players final working times. Moreover, the fairness parameters α, β are taken from the basic Fehr and Schmidt model. Here α is the loss of utility from inequality to one's disadvantage, β is the loss of utility from inequality to one's own advantage. To incorporate the new aspect of two-dimensional fairness, we extend the model with a conversion factor for the exchange between time and money γ_i. On this basis we can make predictions how different effort levels weigh off with monetary redistributions. The resulting utility function is:

$$U_i(x_i, x_j, t_i, t_j) =$$

$$x_i - \alpha \cdot \max\{x_j - x_i - \gamma_i(t_j - t_i), 0\} - \beta \cdot \max\{x_i - x_j - \gamma_i(t_i - t_j), 0\}$$

One of our implicit assumptions is that both player types assess fairness in terms of both dimensions and not solely based on monetary outcomes. This can also be tested with our experimental data.

Hypothesis 2: Both dimensions of fairness are reflected in the subjects' behaviour.

Next the most innovative hypothesis directly building on the model extension is derived in some more detail. In the dictator game we have a player A in the dictator role and a passive player B receiving a monetary transfer. If $\beta > 0.5$, then the proposer offers the equal split. For players with $\beta < 0.5$ the resulting offer is by definition always zero. Hence, for the dictator we can predict the following offer defined as y_A:

$$y_A = \begin{cases} 0, if\ \beta < 0.5\ \vee\ \gamma_A(t_A - t_B) > 10 \\ \frac{1}{2}[12 - 2 - \gamma_A(t_A - t_B)], if \beta > 0.5\ \wedge\ \gamma_A(t_A - t_B) \leq 10 \end{cases}$$

The additional assumptions regarding the conversion factor γ above are necessary, to deal with cases, where the monetary equivalent of working time exceeds the initially induced surplus of 10€. In these cases, offers cannot be negative, but must be zero. In the ultimatum treatment we can determine what players B are minimally willing to accept. For the responder B, we obtain the following solution:

$$y_B = \begin{cases} 0, if\ \ \gamma_B(t_A - t_B) > 10 \\ \frac{\alpha}{1 + 2\alpha}[12 - 2 - \gamma_B(t_A - t_B)], if\ \gamma_B(t_A - t_B) \leq 10 \end{cases}$$

Again, as an additional assumption we have to demand that $\gamma_B(t_A - t_B) \leq 10$, otherwise the offer constituted by the additional working effort would exceed the surplus of 10€. In these cases, offers cannot be negative, but must be zero.

In order to ultimately compare the behaviour of redistribution in both treatments, one must rely on some distribution of the standard fairness parameters α and β, specifying the offer and acceptance threshold. For this purpose, we take the established distributions from Fehr and Schmidt (1999). First, for the dictator game treatment, we assume that 60% of all players have $\beta < 0.5$ and 40% have $\beta > 0.5$. Then one can calculate the expected offer of the average player as:

$$\hat{y}_A = 0.4 \cdot \left[\frac{10 - \gamma_A(t_A - t_B)}{2}\right] = 2 - 0.2\gamma_A(t_A - t_B)$$

For the ultimatum treatment we adopt existing estimates for α as provided by an aggregate study from Fehr and Schmidt (1999). Based on several studies on ultimatum games, they find 4% of $\alpha = 0.125$, 25% of $\alpha = 0.75$ and 71% of $\alpha = 2$.[70] With this parameterisation, we account for the different strategic situation in the ultimatum game regarding fairness and can further specify the acceptance threshold of players B in the ultimatum treatment as:

$$\hat{y}_B = \left(0.04 \cdot \frac{1}{10} + 0.25 \cdot \frac{3}{10} + 0.71 \cdot \frac{9}{20}\right) \cdot [10 - \gamma_B(t_A - t_B)] = 3.99 - 0.399\gamma_B(t_A - t_B)$$

This shows that the base rate of acceptance from player B is twice as high as the base rate of an offer of player A in the dictator treatment. The same holds for the decline of the slope, when player A provides more working effort. Having obtained this solution for the acceptance threshold, we can relate the two treatments and the behaviour of both player types. Taking the partial derivative for both treatments we obtain:

$$\frac{\partial y_A}{\partial(t_A - t_B)} = -0.2 \cdot \gamma_A$$

$$\frac{\partial y_B}{\partial(t_A - t_B)} = -0.399 \cdot \gamma_B$$

Next, we take the above results and determine the relation between the two conversion factors in both treatments as:

$$\gamma_A \approx 2\gamma_B$$

If behaviour in our setting of two-dimensional fairness is not influenced by a self-serving bias of players A, then we must also find the predicted $\gamma_A = 2\gamma_B$ in the experimental data. By contrast, if we were to find $\gamma_A > 2\gamma_B$, then subjects would exhibit a self-serving bias effect when deciding on the redistribution of the surplus 10€.

Hypothesis 3: There is no self-serving bias effect: γ_A is equal to $2\gamma_B$.

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[70] According to the model, the α directly translate into acceptance thresholds for the offer in terms of the overall surplus, which can be offered. Here $\alpha = 0.125$ equals an offer of 10%, $\alpha = 0.75$ equals an offer of 30% and $\alpha = 2$ equals an offer of 45%.

6.5 Experimental Results

The following experimental analysis is structured along the two dimensions of fairness. First, we consider behaviour in terms of working times provided in the real effort stage. Then we analyse the monetary transfers from the redistribution stage. Finally, we combine both dimensions, which allow us to make inferences about the trade-off between monetary transfer and working effort. This can also confirm our self-serving bias hypothesis.

6.5.1 Working Times in the Real Effort Task

First, we consider our data in terms of working efforts. The following table depicts the average behaviour of the two player types:

Treatment	**Player A (12€)**	**Player B (2€)**
Baseline (T1)	45.0 min	28.8 min
Dictator (T2)	44.1 min	29.6 min
Ultimatum (T3)	41.0 min	35.2 min

Table 6.2: Working Effort across Treatments

For both the dictator and the ultimatum treatment, we find no significant changes regarding average working times for the two player types.[71] On average, each type provides the same effort levels in the effort stage independent of the nature of the subsequent distribution stage. Moreover, the differences between player types are what we expected from the literature on gift-exchange games and in line with our first hypothesis. Working times for players A are significantly higher in all treatments.[72]

6.5.2 Monetary Offers in the Redistribution Treatments

In the following we focus on the second stage of the experiment, where we have varied the redistribution protocol and have observed different monetary transfers. In this stage the difference of working time between the two players in each pair becomes relevant, as the subjects

[71] A pairwise analysis of treatments using a Mann-Whitney test yields, that the largest difference, i.e. the working time of player B in the baseline and in the ultimatum treatment still has a p-value of = 0.295. Overall, we find no significant differences when comparing the treatments.

[72] The difference in the dictator treatment is significant on a 1%-level, the differences in the baseline and ultimatum treatment are significant on a 5%-level.

may decide to condition their proposal and acceptance on the surplus or shortcoming in working time of their partner.

Treatment	Mean Offer in €
Baseline (T1)	-
Dictator (T2)	1.2€
Ultimatum (T3)	4.1€***

Table 6.3: Mean Offers across Treatments

But first, only considering the mean offers by players A from table 6.3, we basically reproduce standard results from the literature on bargaining games. Accordingly, the mean offers in the dictator treatment are significantly lower than in the ultimatum treatment.[73]

More interestingly, the monetary offers can be put in perspective with the difference in working time in minutes. For the dictator game treatment we can plot figure 6.1 illustrating the offers proposed by player A conditional on the difference in working effort.

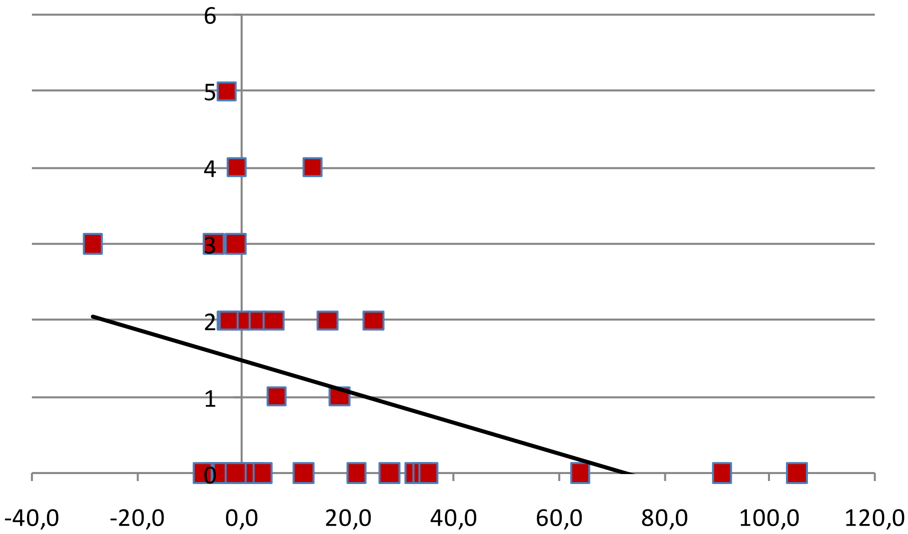


Figure 6.1: Surplus in Working Time (A) and Offers in Euro (T2)

Here we can see that most players type A work longer than their partner, whereas only a few players type A work shorter than their partner. Analogous to the dictator treatment we can plot how the difference in working time affects the offers in the ultimatum game treatment:

[73] T-test: p-value = 0.000.

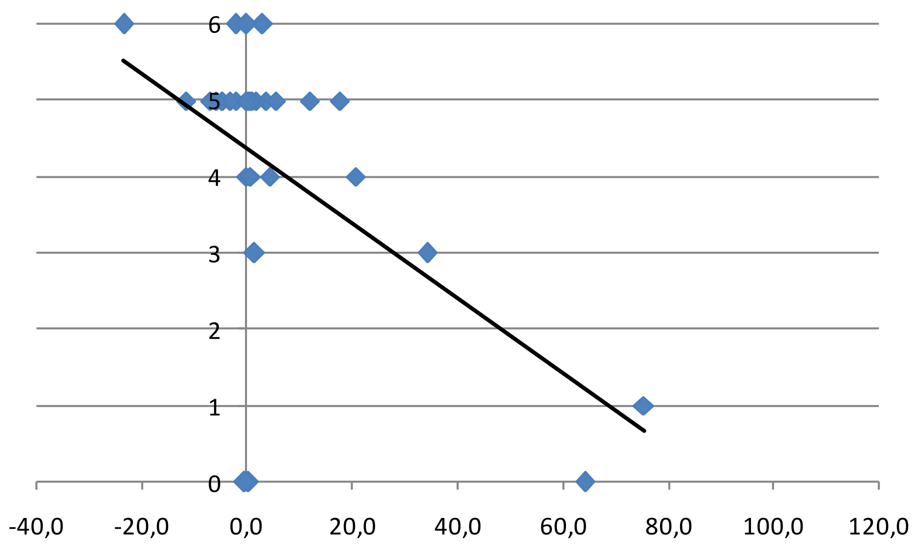


Figure 6.2: Surplus in Working Time (A) and Offers in Euro (T3)

Here, the data nodes are centred on a significantly higher mean. Also we find some very fair or even altruistic offers ≥ 5 €. Overall, the data on offers in the two redistribution treatments proves that subjects base their decisions on both dimensions of fairness. Both slopes decline, showing that working time is converted into lower offers, giving credit to our notion of two-dimensional fairness as postulated in the model.[74]

6.5.3 Self-Serving Bias Effect

Finally, in light of the differences in working time we can process how the two player types evaluate their own working surplus, when making an offer or acceptance in the redistribution stage. Dwelling upon the results from section 4, we thus compare the dictator offers of players A with the acceptance thresholds of players B in the ultimatum treatment. For this analysis, we only take positive differences in working time into account. Only in these cases, the proposer could make an offer after having observed the relevant working time of his partner with certainty. For this constellation, we can plot the proposal and acceptance rates depending on the surplus of working time as in figure 6.3.

[74] To substantiate this finding we run linear regressions for both treatments and find that in the dictator treatment we have a coefficient of -1.22 (p-value = 0.001) and in the ultimatum treatment of -2.94 (p-value = 0.000) for the declining slope when taking the offers as the dependent variable. For details refer to appendix 6.7.1.

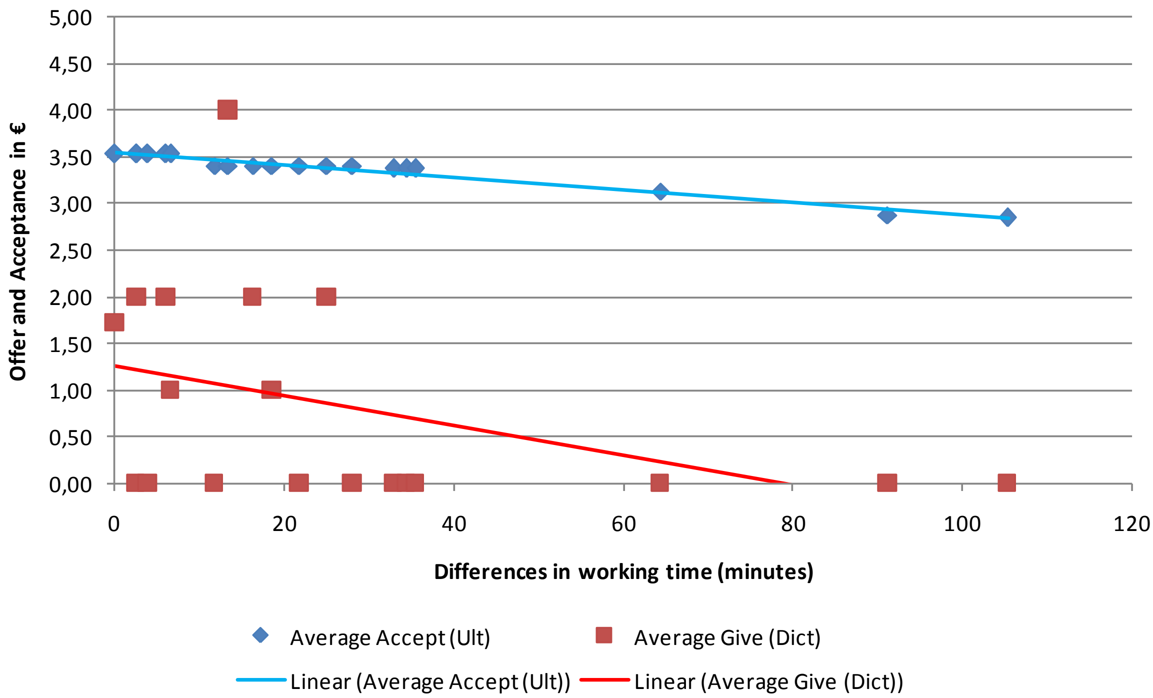


Figure 6.3: Comparison of Offers and Acceptance Thresholds

Due to the strategy method elicitation of acceptance behaviour in the ultimatum game, we are able to determine the responses of players B for the same working times as players A have provided in the dictator game. Based on the full specification, we can conduct this for every offer and for every difference in working times. Therefore, to connect both treatments we take the real observed working differences in the dictator treatment (red line) and the computed minimal acceptance thresholds in the ultimatum game, given exactly these working times (blue line). This provides a clean comparison of subjects' behaviour between the two treatments and for both player roles. In order to establish a self-serving bias effect we must find that:

$$\frac{1}{2}\frac{\partial y_A}{\partial (t_A - t_B)} > \frac{\partial y_B}{\partial (t_A - t_B)}$$

First, we determine the slopes of the decline in offers and acceptances for both players as:

$$\frac{\partial y_A}{\partial (t_A - t_B)} = -0.94$$

$$\frac{\partial y_B}{\partial (t_A - t_B)} = -0.38$$

On this basis we can calculate the conversion factors γ_A and γ_B:

$$\gamma_A = -5 \frac{\partial y_A}{\partial (t_A - t_B)} = 4.69$$

$$\gamma_B = -2.5 \frac{\partial y_B}{\partial (t_A - t_B)} = 0.94$$

We find that for the subjects' behaviour in our experiment, γ_A is significantly higher than $2\gamma_B$.[75] Hence we assert the prevalence of a self-serving bias effect for the players A. With this effect, these players exaggerate the value of their own working time to justify very cheap offers after providing some more effort. In fact they value their own working effort, as the basis for their offers, five times higher than the working effort of the receiving player is valued.

6.6 Conclusions

This paper has provided a first theoretical and experimental advance towards two-dimensional fairness. Therefore we have devised a standard real-effort game with a meaningful task and asymmetric payments of 12€ and 2€ for two player types, allowing for a redistribution of the surplus 10€. Further we appended it with different treatment variations regarding the redistribution protocol and communicated working times towards the whole experiment, so that there were two dimensions to assess final outcome fairness; the differences in working time and the monetary redistribution. To substantiate our claims we extended a standard model of social preferences with our notion of two-dimensional fairness. This gives us clear predictions about the equilibrium behaviour of both player types.

As a first result, we have reiterated that working times follow the typical behaviour of a gift-exchange game. Across treatments, all players A with the higher endowment work significantly longer than their counterparts. When putting the offers of players A in perspective with the differences in working time between both players, we confirm that our basic grasp of two-dimensional fairness is reflected in the data. In both treatments, there is a robust trend proving that offers decline as the additional working effort increases. Therefore the players must weigh off the two dimensions of effort and payment. Finally, with recurrence to our theoretical model, we take the experimental data in order to estimate the average conversion factors of time and money for both player types. In contrast to the theoretical prediction, these factors

[75] Mann-Whitney Test: p-value = 0.039.

are significantly different, which leads us to the conclusion that players do not only assess fairness according to two dimensions, but are also subject to a self-serving bias in the dictator treatment. This bias distorts the conversion factor to their own advantage, justifying cheap offers.

6.7 Appendix

6.7.1 Additional Results

For the offers in the dictator treatment, we run a linear regression with robust standard errors:

Dependent Variable: Offer Player A	
Variables	
Intercept	1.48***
	(0.304)
Differences Working Time	-1.22***
	(0.349)
N observations	32
Prob > F	0.002
MSE	1.399
R^2	0.147

Table 6.4: Linear Regression Profits Player A (robust standard errors)

We find that there is a highly significant dependence between the decline of offers and the difference in working time, which here again are expressed in hours.

The same regression model can be applied to the ultimatum treatment:

Dependent Variable: Offer Player A	
Variables	
Intercept	4.37***
	(0.305)
Differences Working Time	-2.94***
	(0.495)
N observations	32
Prob > F	0.000
MSE	1.618
R^2	0.265

Table 6.5: Linear Regression Profits Player A (robust standard errors)

Again the regression results show, that the relationship between offers from player A and the differences in working time is highly significant.

6.7.2 Instructions

Instructions

Welcome and thank you for participating in today's experiment. You will be able to earn money during this experiment. You will receive 2.50 € as a show-up fee. The following instructions will outline how you can earn more. There will be new groups of people coming into the laboratory room during today's session. Please do not let this disturb you.

Please read the following instructions thoroughly. These are the same for all participants. Please do not hesitate to ask if you have any questions. However, we ask you to raise your hand and wait for us to come and assist you. We also ask you to restrain from communicating with other participants from now on until the end of the experiment. Please ensure that your mobile phone is switched off. Violating these rules can result in an exclusion from this experiment.

Course of the Experimental Task (Baseline Treatment)

During this experiment you are expected to type in data from a questionnaire into the computer. This data is intended for scientific research. The exact proceedings will be explained in the following part *work instructions* on the next page. During the experiment you will be paired with a second participant, who is working on the same questionnaires. You will therefore be assigned the role of either participant A or participant B. Your payoff depends on the role you are randomly assigned in the beginning. **Participants A will receive 12 €** for their work and **participants B 2 €.** All participants stay anonymous during the entire experiment.

For your own reference and to be able to check your payoff you will be able to see the following on your computer screen: how long you have already been working and if the participant you have been paired up with is still working. You additionally receive a message, when the other participant stops working. It is up to you to decide how many questionnaires you type in to the computer.

Once you have stopped working on the questionnaires you will be given a summary of the work duration of both participants.

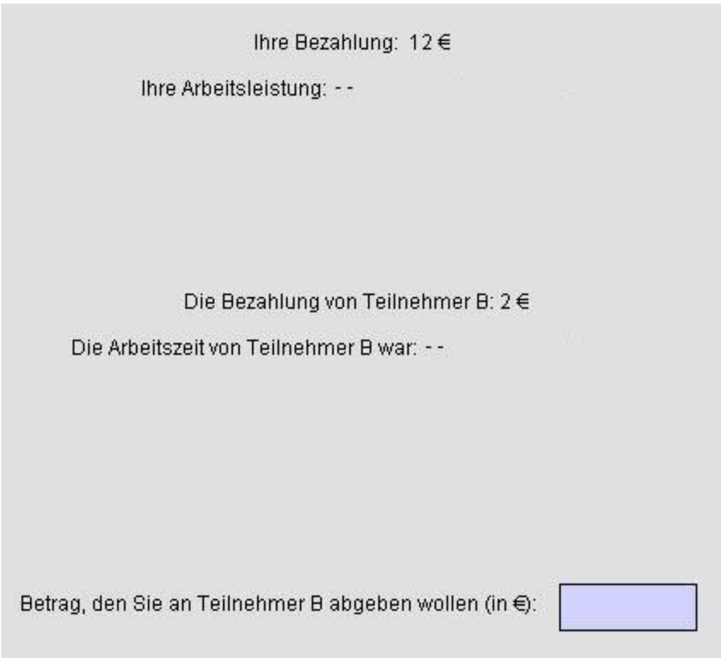
You will receive your final payoff (plus the 2.50 € show-up fee) in cash on presentation of your payout slip during the following days. Please do not forget to take the payout slip on your desk with you after finishing the experiment.

Course of the Experimental Task (Dictator Treatment)

During this experiment you are expected to type in data from a questionnaire into the computer. This data is intended for scientific research. The exact proceedings will be explained in the following part *work instructions* on the next page. During the experiment you will be paired with a second participant, who is working on the same questionnaires. You will therefore be assigned the role of either participant A or participant B. Your payoff depends on the role you are randomly assigned in the beginning. **Participants A will receive 12 €** for their work and **participants B 2 €.** All participants stay anonymous during the entire experiment.

For your own reference and to be able to check your payoff you will be able to see the following on your computer screen: how long you have already been working and if the participant you have been paired up with is still working. You additionally receive a message, when the other participant stops working. It is up to you to decide how many questionnaires you type in to the computer.

Once you have stopped working on the questionnaires you will be given a summary of the work duration of both participants. Participant A can then decide to transfer an amount between 0 and 10 € of his payoff to participant B (only integers). For this purpose participant A will see the following screen:



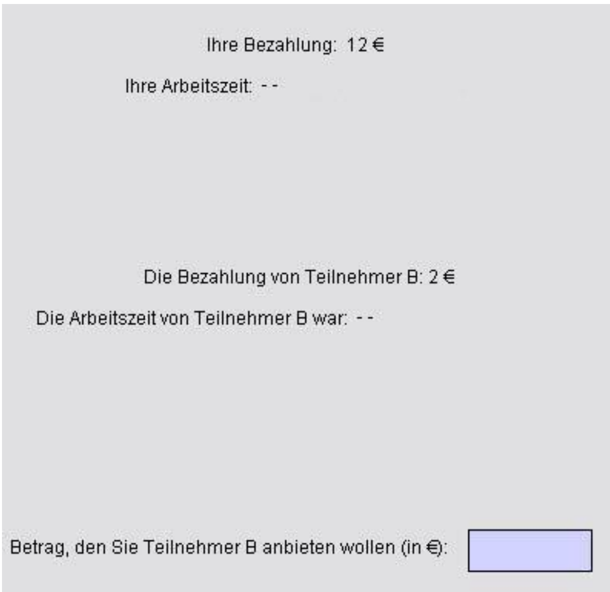
You will receive your final payoff (plus the 2.50 € show-up fee) in cash on presentation of your payout slip during the following days. Please do not forget to take the payout slip on your desk with you after finishing the experiment.

Course of the Experimental Task (Ultimatum Treatment)

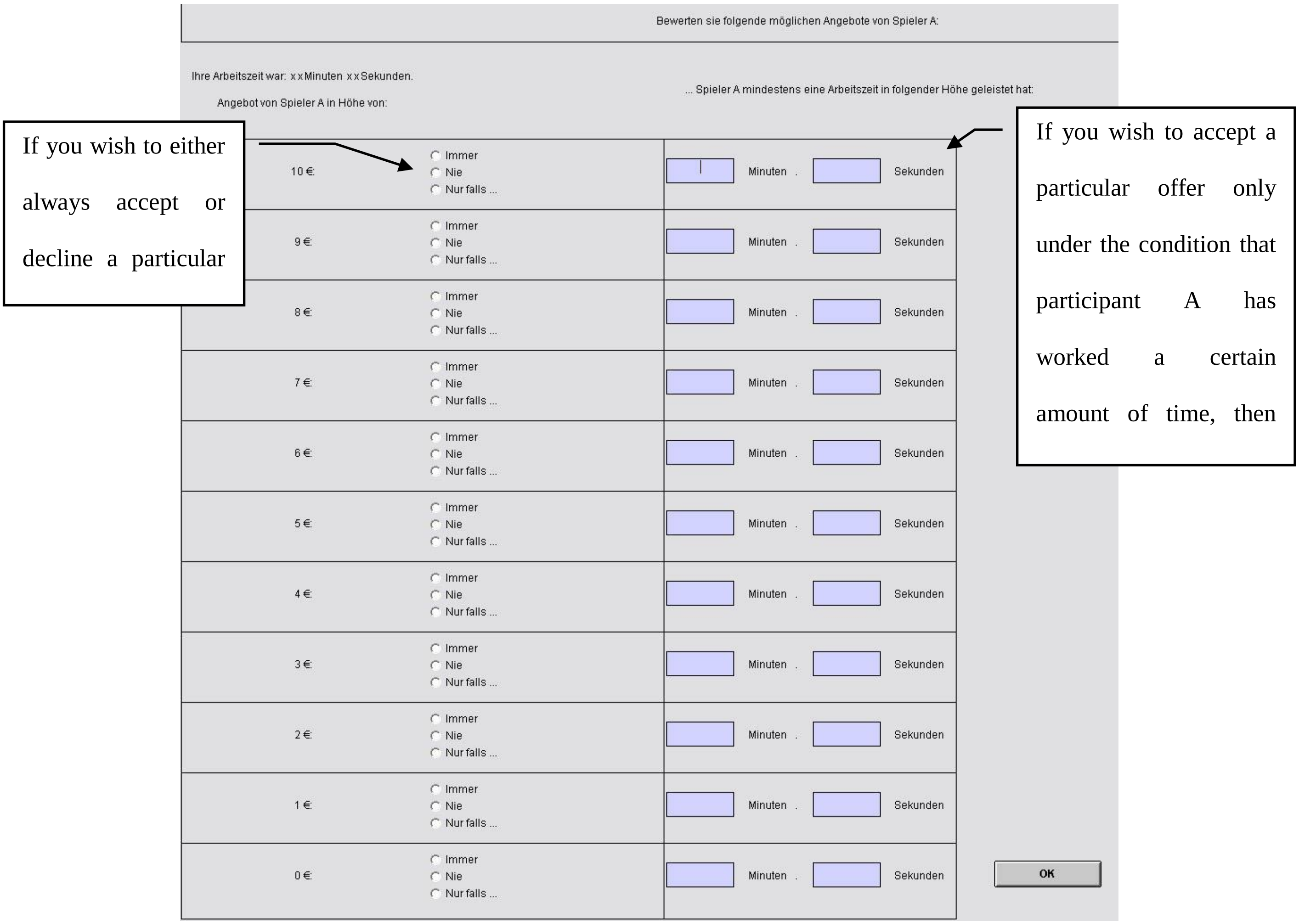
During this experiment you are expected to type in data from a questionnaire into the computer. This data is intended for scientific research. The exact proceedings will be explained in the following part *work instructions* on the next page. During the experiment you will be paired with a second participant, who is working on the same questionnaires. You will therefore be assigned the role of either participant A or participant B. Your payoff depends on the role you are randomly assigned in the beginning. **Participants A will receive 12 €** for their work and **participants B 2 €.** All participants stay anonymous during the entire experiment.

For your own reference and to be able to check your payoff you will be able to see the following on your computer screen: how long you have already been working and if the participant you have been paired up with is still working. You additionally receive a message, when the other participant stops working. It is up to you to decide how many questionnaires you type in to the computer.

Once you have stopped working on the questionnaires you will be given a summary of the work duration of both participants. Participant A can then decide to transfer an amount between 0 and 10 € of his payoff to participant B (only integers). For this purpose participant A will see the following screen:



Participant B can determine in the following input screen, which offers from participant A he would accept:

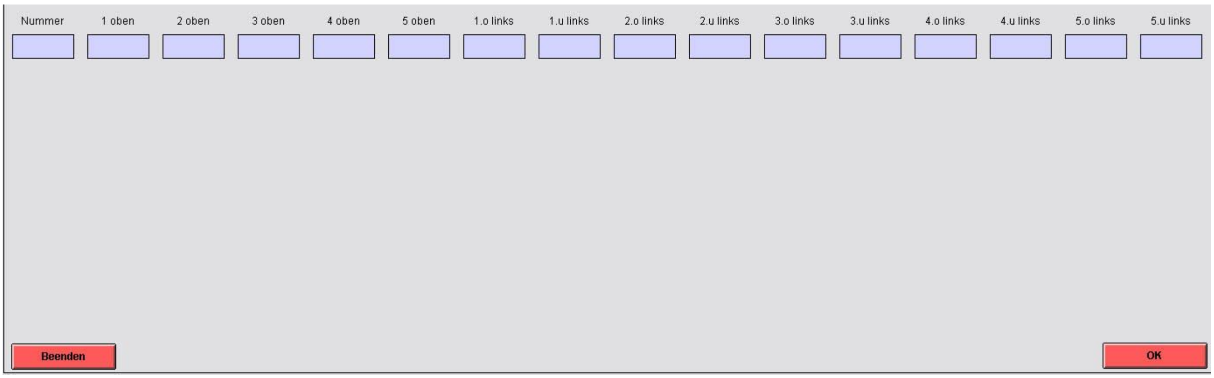


Participant B can here determine, whether he always accepts a certain offer, never accepts or whether he will accept it under the condition that participant A (12 €) has worked a certain amount of time. If participant A has made an offer, that participant B will accept, then the 10 € payoff difference will be allocated according to the offer of participant A. If however participant B declines the offer, then the 10€ will not be divided and both parties receive a payoff of 2 €.

You will receive your final payoff (plus the 2.50 € show-up fee) in cash on presentation of your payout slip during the following days. Please do not forget to take the payout slip on your desk with you after finishing the experiment.

Work Instructions

A stack of questionnaires is placed on each desk. These questionnaires were filled in by other students during an earlier classroom experiment. These data needs to be digitalised for evaluation. It is your task to type in the information into the form on your computer screen. The data will then be automatically transferred into an Excel-sheet. The participants of the classroom experiment were asked to answer 15 questions. These questions were labelled 1, 2, 3, 4, 5, 1.o, 1.u, 2.o, 2.u, 3.o, 3.u, 4.o, 4.u, 5.o and 5.u. The participants were asked to give estimates in percentage points for each question. The following illustration is a screen shot of the data entry form:



You are kindly asked to convey first of all the **number in the right hand corner of the questionnaire** into the box "number" on the screen for each questionnaire you process. After that please convey **only the percentage figures of the left column** of the form. Thus enter only the numbers for the categories "top" and "left" into their respective boxes on the screen. Please do not enter the percentage sign into the form. Please click OK after entering all the information. The data is thereby saved and transferred to the Excel-sheet. You can then continue with the next questionnaire. We very much appreciate your work and kindly ask you to be accurate with the input of the data. You are by no means required to process all questionnaires during this experiment. You can stop working any time by clicking the "Finish" button in the bottom left hand corner of the screen. You cannot return to the data entry form after quitting.

Please click OK on your screen, once you have read these instructions, and wait for the other participants. A countdown will start, once all participants are ready. You will then be told whether you are participant A (12 €) or participant B (2 €) and you can then also start working.

7 Conclusions

This thesis encompasses five contributions on information and fairness in behavioural economics. Regarding information the following issues have been examined: information acquisition in two different auction formats, the value of non-instrumental information is a second-price sealed-bid auction, coordinative behaviour in a global game under two different kinds of uncertainty and deceptive behaviour in an information transmission game with a conflict of interest. For fairness, multi-dimensional fairness was embedded in a model of social preferences and self-serving bias behaviour was detected.

In chapter 2, a very important scenario for real world applications of auctions was investigated, i.e. auctions with information acquisition. In this set-up information acquisition to discover one’s private valuation for a good at auction is costly. The uncertainty about one’s private valuation can occur for all kinds of goods from old books or CDs to corporate takeovers or spectrum auctions. The costs can be envisioned as the external costs of an expert opinion determining the valuation or as the internal cognitive cost for discovering one’s true valuation. As this question is difficult to analyse with empirical or field data, we designed an experiment exactly testing the theoretical predictions of a rational choice model. Starting with the predictions for a second-price sealed-bid and an English auction with different cost parameters, we found that subjects buy excess information as they fail to assess the correct value of this instrumental information. In line with this main result, we find further departures from the rational theory, such as premature information acquisition in the English auction and underbidding in both formats. Extending the initial model with anticipated regret we can explain why subjects still buy information above the rational model’s threshold level and why they underbid, when not knowing their valuation.

The clear results from chapter 2, refuting the rational theory for auctions with information acquisition, have raised a further question guiding the research for chapter 3. Here the subjects’ behaviour dealing with non-instrumental information is scrutinised. Using a simple auction set-up subjects are provided with the opportunity to acquire the highest valuation of their competitors at a certain cost. This is non-instrumental information in the second-price sealed-bid auction at hand. As a result the excess information acquisition behaviour is confirmed and is extended to non-instrumental information. Further, the bidding strategies exhibit underbidding behaviour, which is striking given that the players, had full information about their own valuation. Overall, the experiments on auctions with information acquisition prove that human agents assess the information provided in such a context very different from the predictions of standard rational choice models. Considering the strong evidence both these contributions provide, this is an area where more experimental work is promising.

With chapter 4 a specific theory is put on test, introducing risk and ambiguity as two kinds of uncertainty into a global game, framed as a speculative attack. First, we confirm that almost all subjects employ switching strategies. Next, the data refutes the theoretical predictions that subjects play unique equilibrium cut-offs. In fact, we find a high variance in the cut-offs of individual players. Also we find excess aggressiveness in the individual subjects' decisions, which is consistent with previous experiments on global games and can be rationalised assuming that players have beliefs about others playing overly aggressive strategies. Most importantly, we investigate the two different kinds of uncertainty regarding the signals in this game. We cannot confirm the comparative statics theoretical predictions of an opposite effect for risk and ambiguity. The experimental data shows no difference between risk and ambiguity for the estimated cut-off values of individual player and also no difference for the aggregate coordination behaviour. As this kind of coordination game continuously generates overly aggressive behaviour, one could further test whether global games also produce ambiguity affine behaviour with additional experiments.

In chapter 5 we have studied a game of strategic information transmission and how players exploit and respond to incentives for deception, especially when these are disclosed. Without a conflict of interest there is no difference in subjects' behaviour for different degrees of transparency. With a conflict of interest, increasing transparency has a significant effect on subjects' behaviour. The advisors send deceptive messages with a high frequency and increase their extent of lying the more the conflict of interest is made transparent. Nevertheless, advisors do not use maximally deceptive messages and thus overcommunicate. By contrast, advisees account for the conflict of interest by discounting the messages they receive. However, they do not discount the messages sufficiently, even given their own beliefs about the degree of deception from their counterparts. Explaining such behaviour, fairness concerns for the advisees might play a role.

Chapter 6 was directed at fairness in behavioural economics. Based on an established theoretical model, we make a first step towards the investigation of multi-dimensional fairness. This is again a question difficult to address with empirical data. Therefore, we start this question with a small extension of a social preferences model, introducing a conversion factor to weigh off different dimensions of fairness, i.e. monetary pay-off and working effort. Then we devise a real effort experiment to obtain clear observations about the two dimensions of fairness at hand. We confirm that people perceive both dimensions of fairness and involve them in their decision making. More importantly, we are able to demonstrate a self-serving bias effect where players distort the conversion factors to their own advantage. In this regard, there is a lot of experimental work remaining, testing the interplay and accessibility of additional fairness dimensions.

Overall, this thesis has investigated how information is used in various economically relevant contexts, such as auctions, speculative attack games and strategic advice giving. It has also demonstrated how the notion of economic fairness can be expanded to accommodate additional dimensions such as working effort.

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