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Behavioral Aspects of Bargaining and Pricing

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PROEFSCHRIFT

ter verkrijging van de graad van doctor aan de Universiteit van Tilburg, op gezag van de rector magnificus, prof. dr. F.A. van der Duyn Schouten, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de aula van de Universiteit op

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door

SABINE KRÖGER

geboren op 14 maart 1975 te Berlijn, Duitsland.

PROMOTORES: Prof. Dr. Werner Güth
Prof. Dr. Jan Potters

für Gisela und Wolfgang

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Sabine Kröger,
Tilburg, November 11, 2003

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Chapter 1

Introduction

This thesis consists of five more or less independent papers which look at how institutions and individual preferences jointly influence human behavior and thereby determine economic outcomes such as prices and efficiency. Whereas institutions are observable rules, individual preferences are determined by personal characteristics and are often not directly observable. Institutional settings and preferences have many dimensions. This thesis focuses on four dimensions of preferences which are important for economic transactions: fairness considerations, trust propensities, risk attitudes, and time preferences. The impact of these preferences are studied in two economically relevant institutional settings: bargaining and markets.

Theoretical investigation helps to understand the strategic structure of a situation. By making assumptions about preferences and rationality of actors, the outcomes of the interaction can be analyzed and predicted. However, preferences often cannot be directly observed, nor can the rationality of actors. Economic experiments provide a method with which human behavior can be explored in controlled environments. Both theory and experiments are complements whose combination provides the analyst with an important tool to understand and study interaction in markets, bargaining, and other environments of economic interaction. All the chapters in the thesis are more or less based on this combinatorial approach:[1] A theoretical benchmark which controls

[1]Experiments in this thesis were conducted at the laboratory of Humboldt Universität zu Berlin, Berlin, Germany, and CentERdata, Tilburg University, Tilburg, The Netherlands. Financial support by the German Science Foundation, through National Research Center SFB 373, "Quantification and Simulation of Economic Processes," Humboldt Universität zu Berlin, CentER and CentERdata, EU–TMR Research Network ENDEAR (FMRX–CT98–0238), and Max–Planck–Institute for Research into Economic Systems, Jena, Germany, is gratefully acknowledged.

for certain preferences is subsequently tested with experimental data. Furthermore, preferences can be revealed by individual behavior in an experiment and be explained by the persons' observable characteristics.

We first provide an overview of the preferences under consideration as well as the economic environments in which behavior is studied. Then we explain briefly our methodological approach.

1.1 Preferences

1.1.1 Fairness

Many experiments suggest that bargainers may be concerned with more than just their own material payoffs in evaluating the outcome of bargaining. The prevalence of equal splits observed in bargaining experiments suggests that notions of fairness play a role in economic transactions. Other studies however suggest that seemingly 'fair' offers may be driven by the anticipated reaction of the opponent towards an unfair offer rather than by the intrinsic motivation to behave in a fair way. Therefore, it may be that fairness considerations of the person who has to accept or reject an offer, rather than the person who proposes the offer, determine bargaining outcomes.

A similar impact of fairness considerations in market contexts has been related to demand withholding of buyers, i.e., rejecting profitable offers, resulting in price decreases. Chapters 4 and 6 of the thesis investigate a bilateral monopoly and monopoly markets with incomplete and complete information about buyers' valuation for the product. An interesting result is, that buyers in both markets were willing to accept prices which grant them only one fourth of their valuation. Compared to results of bargaining experiments this is only half of the amount responders usually accept.

Chapter 3 shows that fairness principles explain behavior in a sequential bargaining situation involving risky joint profits.

1.1.2 Risk aversion

Risk neutrality would imply that people are indifferent between a safe option and a risky option when both yield the same expected payoff. What one actually observes is that even when the expected payoff of the safe option is slightly lower than the risky option, individuals prefer the safe option. Behavioral deviations from risk neutrality in situations involving risky outcomes led to the incorporation of risk aversion into the expected utility model. In chapter 3, we analyze a bargaining situation over risky joint profits and investigate risk aversion of subjects. Even though the theoretic solution with risk averse agents approximates the experimental outcome on average better than other theories, only 20% of the observed behavior can actually be explained by risk aversion. This suggests that even if risk aversion seems to capture average behavior quite well, a more detailed look reveals that its explanatory power is rather limited.

1.1.3 Impatience

Time preferences play a role when individuals have to trade off current for future consumption. Time preferences paly a role also for self control and commitment problems. One example of the latter is a durable goods monopoly. A monopolistic seller who offers a durable good and cannot commit himself to prices in future periods might suffer from the fact that buyers anticipate future price cuts and delay their purchase. In chapter 4, we model a bilateral two–period durable goods monopoly market and investigate how subjects react to induced time preferences. We allow for heterogeneity in time preferences, which imposes different degrees of bargaining power to seller and buyer in the sequential interaction. Our results suggest that subjects anticipate future profits well and react to short term problems in accordance with the theoretic prediction.

1.1.4 Trust

Trust and trustworthiness are basic components of human interaction and may depend on the context in which persons interact. In a bilateral bargaining situation the context is determined by the information bargaining parties possess: whether they interact repeatedly, who makes the proposal etc., and might influence whether one trusts or not, and whether one reciprocates in a trustworthy way. Trust and trustworthiness may also depend on own experiences and the socioeconomic background of people which might explain the variation of trust(worthiness) between people in the same context. To measure this basic trust(worthiness) propensity is the challenge taken up in chapter 2.

1.2 Institutions

The impact of the preferences mentioned above are analyzed in the context of a trust game, bilateral bargaining, auctions, and markets.

1.2.1 Trust Game and Bargaining

The trust game, also referred to as investment or gift exchange game, represents an institution in which no formal contracts are possible. The model is set up as follows: one person can send a certain amount to another person which will be multiplied by a number greater than one. The other person has the possibility to return money to the sender but can also take advantage of the situation and keep everything for himself. In many interactions individuals cannot rely on complete contracts or enforcing institutions. In these cases the presence of trust (and trustworthiness) helps to overcome opportunities to exploit the other party. In many instances, sufficient levels of trust and trustworthiness improve the odds of completing a transaction, making both parties better off. The trust game is used in chapter 2 to measure trusting behavior and trustworthiness.

In chapters 3 and 4, we consider sequential two–period bargaining situations. The basic bargaining model involves two players who make sequential offers over how to divide some amount of money. The information structure of these models is particularly important and is expressed by whether the amount over which to bargain is known to both or only one party. It differs further in the certainty with which the amount is realized, how costly a delay for each bargaining party is, and which bargaining party has the opportunity to make an offer. In chapter 3, the amounts to bargain over are separate by stages but additive and known to both parties which alternate with their offers, where the amount at the first stage is risky. In contrast in chapter 4, the amount to bargain over is not separable and only known to the person who has to accept or reject the offer, the costs of delay are private information and may differ between parties, furthermore, offers are made by one party only.

1.2.2 Markets: One–Sided Auctions and Posted–Offer Markets

Efficiency is a main concern in markets that we study. The difference with bargaining models is that in markets at least one side is characterized by competition. The distinctive feature of one–sided auctions and posted–offer markets is their pricing mechanism. In auctions the competitive side makes the offer in posted offer markets the monopoly side.

The institution studied in chapter 4 can also be used to investigate monopoly markets with privately known time preferences. The downward sloping demand curve is implemented by a single buyer with a privately known valuation randomly drawn from a commonly known uniform distribution.

In chapter 5, we focus on one–sided auctions (also known as procurement auctions) in which competition is on the sellers' side and the demand side is represented by a single buyer. Economically procurement auctions play an important role in the exchange of products. For instance, governments and public services in Germany are forced by law to call openly for tender bids to circumvent inefficient usage of taxes. The value of goods exchanged each year by public procurement auctions in Germany is approximately 250 billion Euro. We are interested in efficiency and profitability for the buyer in two different procurement auctions allowing for quality differences across products.

In chapter 6 we study two posted price monopoly markets with and without copy protection for the information goods on the market.

1.3 Methodology

This thesis looks into the interplay between preferences and institutions, and their impact on bargaining and pricing by confronting theoretical benchmarks with actual human behavior. Choice data can be collected in three different ways: real life data, survey data, or experimental data. Information of public procurement procedures which are made publicly available to ensure monitoring and efficient tax usage, is an example of real life data. Information about bargaining is another example of publicly accessible real life data. One can, for instance, follow negotiations between employers and unions in the media. The advantage of real life data is that one captures actual behavior in those institutions. The drawback of this kind of data is that many variables of interest, such as costs for goods in the procurement auction or time preferences in bargaining situations, cannot be observed directly. In combination with survey data one could substitute most of the missing variables of interest by asking directly the persons involved. In procurement auctions sellers would have to be asked about their real production costs. In bargaining environments agents would have to reveal their preferences. In situations where private knowledge about own preferences or the cost structure leads to advantages in the bargaining or market situation, conducting questionnaires to collect data about these information is difficult. Many agents participating in bargaining or procurement auctions might be very reluctant to disclose their information due to strategic considerations.

This thesis follows the third approach and uses experimental data. The main advantage of economic experiments is that human decisions with real monetary consequences can be monitored in an environment under the experimenter's control. This allows isolation of specific aspects of complex real world situations, and at the same time observation of consequential rather than hypothetical choices. Predictions and assumptions usually made by theoretical models can thereby be tested, extended, and refined. Parametrization of experiments is usually chosen in a convenient way which allows either to distinguish theories or to be close to real life settings of the amounts to win or loose. In chapter 3 we present an experiment in which parameters were calibrated by real life data from the movie business industry.

Typically, experiments are conducted in a computer laboratory located at the researcher's university campus with a quite homogeneous sample of student subjects. The advantage of using students as subjects is that they are close to the laboratory and that their opportunity costs are rather low which requires less compensation. Homogeneous samples can successfully be used when background information is not the main interest of the study. This is the case in most chapters of this book. If one, however, is interested in certain questions where background characteristics may have an important influence on behavior, one might expect that results and conclusions drawn from student samples are not representative for the population at large. As we show in chapter 2 data panels open up the possibility to combine the strength of survey and experimental analysis. In this chapter members of a data panel participated in an experiment in order to investigate the determinants of trust and trustworthiness in a wide population sample. Data panels offer new opportunities to conduct an experiment with heterogeneous subject pools and use the information about participants' background characteristics when interpreting behavior. This combination is a promising area of experimental economics.

1.4 Summary of the different chapters

Chapter 2 combines an economic experiment with survey data to investigate determinants of trust and trustworthiness in the Dutch society. We contrast the inferences which can be made on the trust propensity using stated and revealed measures and we test for participation bias in our experiment. We find that middle aged and educated individuals trust relatively more but are relatively less trustworthy. The effect of age and religion on trust is shown to depend heavily on whether experimental or survey trust measures are used. We find no evidence of participation bias in any experimental decisions.

Chapter 3 investigates theoretically and experimentally sequential bargaining in risky joint ventures with additive stakes and alternating offers. Our example is the production of a movie that may give rise to a sequel, so actors and producers negotiate sequentially. To approximate the risk in real environments we calibrate parameters of the experiment with empirical data from the movie industry. We compare the predictions of alternative theoretical approaches based on different assumptions about the preferences of the bargaining parties. The game theoretic prediction (assuming risk averse actors) seems to explain the aggregate data best. Elicitation of individual risk parameters discloses inconsistencies with the theoretic assumptions questioning the predictive power of risk aversion. Equity theory seems to be a better explanation of the observed behavior. Bargaining parties seem to share risk in the sense that if only one party bears the risk the other party is willing to accept a lower share of the pie if (s)he is compensated later in case of a success.

In chapter 4, we model a durable monopoly market with privately known time preferences. The downward sloping demand curve is implemented by a single buyer with a privately known valuation randomly drawn from a commonly known uniform distribution. Therefore, the model under consideration is analogue to a two–period sequential bilateral bargaining game with private information about the costs of delay and asymmetric information about the amount to bargain over. We derive the closed–form solution and compare it to the experimentally observed behavior. Our results suggest that subjects anticipate future profits well and react to short term problems in accordance with the theoretic prediction.

Chapter 5 experimentally examines the efficiency and profitability for the buyer in two different procurement auctions allowing for quality differences across products. We compare the result of one treatment with more competition on the sellers' side (vector auction) to another treatment (half auction), reflecting actual procurement practice, namely: To organize an auction for the cheaper variant and then to bargain with the contractor about the additional cost of the higher quality variant. Our main hypothesis, that buyers will be better off when using the vector auction instead of the half auction, is confirmed in spite of its worst–case scenario (minimal competition by just having two potential sellers and ultimatum power of the buyer in the half auction).

Chapter 6 studies to what extent outcomes of information good markets are influenced by institutional settings. We ask, how products which are easy to replicate and redistribute (music, software etc.) are sold in a monopolistic market with and without copy protection. The implications of a theoretical model are compared to the outcome of an economic experiment. We observe demand withholding, i.e., buyers reject profitable offers. As a result of which prices are lower than predicted. Demand withholding depends on the institutional setting and results in welfare implications which are different than theoretically predicted.

Chapter 2

On Representative Trust

2.1 Introduction

It is increasingly argued that a nation's social capital can influence its economic performance. Although there is an ongoing debate over what constitutes social capital (Bowles and Gintis, 2002; Durlauf 2002), there seems to be a consensus that both average societal trust and trustworthiness are two important components. The transactions cost paradigm remains the traditional way of thinking about the mechanism by which both these components of social capital affect economic performance. When societal trust and trustworthiness levels are high, transactions costs are low which makes organizations and governments more efficient which ultimately leads to better economic performance.

The research on social capital started with the influential work of Putnam (1993) who found a strong correlation between measures of civic engagement and government quality across regions in Italy. The association of social capital with growth started with the work of Knack and Keefer (1997), and Zak and Knack (2001) who find that a one–standard deviation increase in the World–Value Survey (WVS) trust index increases economic growth by more than one–half of a standard deviation. La Porta, de Silanes, Shleifer, and Vishny (1997) find that across countries, a one–standard deviation increase in the same measure of trust increases judicial efficiency by 0.7 of a

[0]We thank Hanneke Dam, Marcel Das, and Corrie Vis of CentERdata for their support throughout the experiment. We are grateful to Oliver Kirchkamp, Arthur van Soest, Eric van Damme, Jan Magnus, Pierre–Carl Michaud, Wieland Müller, Karim Sadrieh, and Jan Potters for helpful comments. The results in this chapter were first formulated in Bellemare and Kröger (2003), "On Representative Trust," CentER Discussion Paper Series, No. 47, Tilburg University, Tilburg, The Netherlands.

standard deviation and reduces government corruption by 0.3 of a standard deviation. These empirical facts rest on the WVS trust index constructed by drawing in each participating country a random sample of participants who are asked to answer, amongst others, the following question

WVS trust question Generally speaking would you say that most people can be trusted or that you cannot be too careful in dealing with people?

1.) Most people can be trusted.

2.) You have to be very careful.

3.) I do not know.

The WVS reports for each country the percentage of responders who indicated that "Most people can be trusted".

Because of the strong correlation between measures of social capital and economic growth, and since age and income distributions of many Western societies are predicted to evolve over time in very alarming ways (Gruber and Wise, 2001; Gottschalk and Smeeding, 1997), it becomes relevant for policy makers to investigate how average trust and trustworthiness in their population are shifted as a result of changes in age, education, income, and past life experiences of individuals. To perform these measurements, two essential features must be combined: 1) to have a random sample drawn from a country's population, and 2) being able to measure trust and trustworthiness with little error. The motivation for the first condition follows from the law of large numbers, whereas sample average trust and trustworthiness are consistent estimates of their population counterparts. The second requirement follows from the fact that the estimated parameters we make inferences on will, in general, be biased if trust and trustworthiness are measured with error (Bound, Brown, and Mathiowetz, 2001).

The empirical literature has up to now been unable to meet both requirements simultaneously. The empirical methods employed so far to analyze individual trust are survey questionnaires and economic experiments. Alesina and La Ferrara (2002) use answers to the WVS trust question to investigate determinants of trust in the United States. The main advantage of their approach is that it allows to make population inferences by observing the behavior of a randomly drawn sample of individuals from that population. The main drawback is that researchers run the risk of collecting answers to a vague and hypothetical question which can create a discrepancy between someone's answers and his actual behavior. Thus, part of the variation in responses may be attributed to differences in interpreting who compromises ”most people”, differences in what is meant to trust someone etc. Moreover, variations in response may also arise because individuals do not answer truthfully to the question.

Economic experiments have the virtue of countering the effects associated with survey data by observing the actual behavior of individuals placed in a context which is under experimental control. These experiments offer an attractive alternative to surveys, given one can design an experiment which captures the essential features of trust and trustworthiness which are of interest. The seminal experiment of Berg, Dickhaut, and McCabe (1995) (hereafter BDMc) remains today the main experimental design used to test for the presence of trust and trustworthiness (more details on the game are given later). The general results of the BDMc game are that people place trust in others, but it is ambiguous whether or not this trust pays. These results have since been shown to be robust to several framing effects (Ortmann, Fitzgerald, and Boeing, 2000) and role reversals (Burks, Carpenter, and Verhoogen, 2003). The main drawback of these types of experiments is that subjects are generally drawn from homogenous pools of university students. These subjects lack the required variation in background characteristics to measure how these variables influence trust and trustworthiness.

The analysis of determinants of trustworthiness is even less documented than trust and relies mostly on analyzing responders' behavior in trust games such as the BDMc game. Hence, this form of analysis suffers from the same drawbacks outlined above. Our little knowledge of the determinants of trustworthiness at the population level is source of concern as recent research has argued that trustworthiness could be ”the” economically relevant component of social capital to understand the process of economic development (Francois and Zabojnik, 2002).

This chapter makes three important contributions. First, we combine the strengths of survey and experimental methods in a straightforward way by having a random sample of the Dutch population play a computerized version of the BDMc trust game. This allows us to touch on several related issues. First and foremost, we add to the scarce body of knowledge on determinants of trust and trustworthiness by estimating age, education, and other life experience effects from experimental trust responses. The key results are that the age and education level of subjects influence trust and trustworthiness in very different ways. Specifically, we find that there is an inverted U shape relation between trust and education, and trust and age, while both relationships are U shaped with respect to trustworthiness. The later finding contrasts with some of the existing relationships found in the social capital literature. The second advantage of combining survey and experimental methods is that we are able to test the assumption of parallelism between the lab and the field. Up till now, this assumption has generally been tested with newspaper experiments (e.g., see Bosch–Domènech, Montalvo, Nagel, and Satorra (2002) for a survey of newspaper experiments) and internet experiments (e.g., Lucking–Reiley, 1999). Because newspaper readers or internet users are not generally representative of a nation's population, these mediums prevent population inferences which are the primary concern of this chapter. Three noteworthy experiments have recently been run with representative samples. Harrison, Lau, and Williams (2002) use a random sample of the Danish population to investigate the heterogeneity in individual discount rates. Hey (2002) used the CentERpanel of Tilburg University (more on this panel later on) to have a random sample of the Dutch population play an experiment on decision making under risk and uncertainty. Fehr, Fischbacher, Rosenbladt, Schupp, and Wagner (2002) report about a "preliminary analysis" (p. 529) of a "first implementation" (p. 528) of an interview based trust game with the German Socio–Economic Panel.

The second contribution of the chapter builds on the seminal work of Glaeser, Laibson, Scheinkman, and Soutter (2000) (hereafter GLSS) who, among other things, invalidate the use of survey trust measures on the basis that they do not predict well trust responses in the lab. We show that the predictive power of survey trust is strongly related to the sample used and the experimental design. The immediate consequence of this is that experimenters can influence the predictive power of survey trust by appropriately choosing subjects and experimental designs. This leads us to conclude that the predictive power of survey trust questions is insufficient to validate or invalidate their use. We propose a new approach to compare both trust measures, which consists of testing whether survey and experimental approaches are equally informative on the determinants of trust. Our evidence shows that the effect of some background characteristics, especially religion and education, can change dramatically when using stated rather than experimental trust responses.

Finally, by giving our subjects the choice to participate and by observing the characteristics of those who refuse to do so, we are in the unique position to test for participation bias in our experiment. If participants have for example above average taste for gambling and risk, or higher cognitive abilities than non–participants, and these unobservable attributes are correlated with the outcomes that are measured in the lab, a participation bias will be present. Because experiments typically do not observe non–participants, tests of participation bias in experiments are very limited. Eckel and Grossman (2000) find some evidence on the presence of participation bias in a classroom experiment by comparing responses of student volunteers and "pseudo"–volunteers. Their results are specific to the game they use (a dictator game) and hold only for student populations. In this chapter we provide the first full fledge test of participation bias in experimental economics. We do not find any evidence suggesting that the trust and trustworthiness behavior of participants in our experiment differ in any way from that of randomly selected subjects.

The remainder of the chapter is organized as follows. Section 2.2 describes the design of the experiment, the experimental procedure, and our sample. Section 2.3 discusses the empirical results on trust. Findings on trustworthiness are presented in section 2.4. Section 2.5 reports results for participation bias while section 2.6 discusses and concludes.

2.2 The Experimental Design and the Sample

The recruitment of our subjects was made by CentERdata, the survey research institute of Tilburg University in the Netherlands. The main activity of CentERdata is to manage and carry out panel surveys through a telepanel: the CentERpanel (hereafter CP), consisting of approximately 2000 representative Dutch households. Every Friday, CP's household members receive a questionnaire which they are asked to fill in at any time between Friday and Tuesday of the following week. This questionnaire is filled at home either on a computer or on a television set which is connected to a set–up box linking the household to the CentERdata server. In order to keep the sample representative of the Dutch population, low income households without a computer or a television set are given the necessary equipment in order to complete the weekly questionnaire.[1]

There are many reasons why the CP is an attractive medium to conduct experiments. First, it gives us access to a representative sample of a population, which is the key feature of this study. Second, because participants answer questions on a computer or a television set, we are able to replicate as closely as possible the environment of a laboratory experiment, which simplifies comparisons of our results with those of the existing literature. Third, because participants communicate with CentERdata, the experiment is double blind as participants were told that they will be anonymously matched and that their identities would not be revealed to the experimenters. Finally, as CentERdata reimburses the weekly telephone costs for answering the questionnaire by crediting CentERpoints (1 CentERpoint = 0.01 Euro) to their private bank accounts four times a year, our participants are already familiar to payment in fictitious currency. This allows us to use CentERpoints as the experimental currency unit and reimburse our participants in a very convenient way.

Our design closely follows BDMc.[2] A sender[3] S and a responder R were both endowed with 500 CentERpoints. S could send money to R from his endowment. We discretized the choice set of S to 11 amounts $y^E \in \{0, 50, ..., 450, 500\}$. The amount S sent was doubled by the experimenters and added to R's endowment. We measured responses using the strategy method by which R was asked to return an amount to S, contingent on each of the 11 possible amounts he might receive from S. The response which corresponded to the actual decision of S was chosen to be the effective action and determined the payoff of both participants. After all participants made their decisions, S and R players were randomly matched and payoffs were computed based on the decisions of the pair. The final payoffs were computed as follows: S received the initial 500 CentERpoints reduced by the amount sent y^E plus the amount received from R, while R received his initial endowment of 500 CentERpoints, the amount sent by S multiplied by 2 minus the amount returned to S.

The strategy method was chosen to overcome the difficulty of having CP members play together in real time. This method has several additional advantages. First, it facilitates data acquisition as the complete strategy plan for all 11 possible amounts received is elicited. Second, as our game may seem complex to some subjects, the strategy method requires that people thoroughly familiarize themselves with the ramifications of all choices, so that we do not retrieve data from uninformed subjects.[4]

Under the assumption that both players maximize their monetary payoffs, the Nash equilibrium of the game is for S to send nothing to R, as R's dominant strategy is to return nothing to S. Hence, observing increasing positive amounts sent is interpreted as evidence that people increasingly trust others. Likewise, observing increasing amounts returned is taken as evidence of the existence of increasing trustworthiness. It is important to stress that repeated game effects, retaliation strategies, and game experience effects are deliberately excluded by our experimental design. Thus, one can think of the current design as measuring the basic trust propensity of an individual at a given point in time.[5]

S and *R* were additionally asked to state their beliefs about their partners' action. These questions were asked after both players made their decisions in order to circumvent the possibility that belief elicitation induces non–cooperative behavior when asked before the play of the game (Croson, 2000). We elicit senders' beliefs with two questions. The first question asks to state how much they think *R* will return to them. The second question asks them to state what they think the average *S* player will send. The latter question is intended to capture behavior directed towards some social norm. Responders on the other hand simply had to state how much units they thought of receiving from senders. This concluded the experimental part of the session.

All players were then asked to answer two survey questions. The first question asked players to state their average experience with trust

Lifetime trust experience question In the past, when you trusted someone, was your trust usually rewarded or usually exploited?

(Always rewarded) 1 , 2 , 3 , 4 , 5 , 6 , 7 (Always exploited).

This question will be used to test for the presence of state dependent behavior whereby differences in past experiences with trust may lead to different experimental decisions.

The second question was the WVS trust question presented in the introduction. This question will allow us to compare the inferences on trust which can be made using stated and revealed decisions. Contrary to GLSS, our subjects answered the WVS trust question after having made their experimental decisions. This has the benefit of not framing the experimental decision as one involving trust. The disadvantage is that answers to the WVS trust question may be influenced by the experimental decisions which were made before, which would complicate the comparisons of our experimental and survey trust measures. In the next section, we develop a simple econometric model which allows to test for the presence of misreported answers to the WVS trust question, and examines to which extent misreporting was influenced by the experimental decision. As will be shown, we do not find evidence suggesting that our sequence of tasks influenced answers to the WVS question.

Two weeks after the experiment, each participant received feedback information on the outcome of the game and their final payoff which was later credited to their CentER bank accounts. The experiment was conducted in two sessions, in the 31st and the 36th weeks of the calendar year 2002. Individuals contacted had to read an opening screen informing them that they were selected to participate in an experiment conducted jointly by a team of university researchers. A detailed description of the game followed with the mode of payments. Each person was informed that conditional on their participation, they would be randomly matched to one of the roles. The role was revealed to panel members once they had agreed to participate. We contacted 541 panel members from which 42 declined to participate.[6] Of the 499 panel members who completed the experiment, 276 were *S* players and 223 were *R* players.

Table 2.1 gives the description of the variables and descriptive statistics of the 541 household members contacted for senders, receivers and non–participants. The means of most variables are relatively identical across non–participants, senders, and responders. 63.7% of the persons contacted were heads of households and most players either had a secondary or vocational training degree. Catholics and protestants are the two most important religious communities in the sample and their relative weights in the three participation categories are very similar. Two notable differences across the three groups concern work propensity and age. Non–participants are on average 10 years older than both senders and responders. This age effect is also reflected in a higher labor market retirement frequency and lower labor work participation.

Despite all the advantages of using the CP, one limitation is that panel members were not constrained to complete the experiment in a limited amount of time, giving them the opportunity to seek advice in order to make more informed choices. As a result, a high game time would be an indication of collective decision making. CentERdata keeps track of the time taken to complete the questionnaires from the time subjects log in the CentERdata network. Table 2.1 also displays some statistics on completion times for both types of players and non–participants. As expected, non–participants have the lowest participation time in the experiment, with a median time slightly greater than a minute. The median time taken by *R* players is greater than that by *S* players, a result primarily due to the fact that *R* players made their decisions using the time–intensive strategy method. Since the majority of subjects took less than 10 minutes to complete the experiment, it seems unlikely that collective decision making is present in the data.

2.3 Results on Trust

The distribution of amounts sent in the experiment is shown in figure 2.1. The two distinctive features of this distribution are 1) the majority of subjects send positive amounts 2) the distribution is heavily skewed to the left, with a mode at 5, the equal split category. The shape of this distribution is familiar to that usually found in lab–experiments with student samples (BDMc; Ortmann, Fitzgerald, and Boeing, 2000) but differs greatly from that of GLSS, which was heavily skewed to the right, with most subjects sending the maximal possible amount. We will try to reconcile the differences between GLSS and our data below.

We assume that senders have a continuous unobserved latent trust propensity T_i^*. This propensity is heterogeneously distributed in the population according to

$$T_i^* = \boldsymbol{x}_i^{\boldsymbol{t}\prime} \boldsymbol{\beta} + \varepsilon_i^{\boldsymbol{t}} \tag{2.1}$$

where $\boldsymbol{x}_i^{\boldsymbol{t}}$ is a vector of observed characteristics of sender i, β is a vector of unknown slope parameters, and ε_i is a random term capturing unobserved heterogeneity across individuals. Our experimental trust measure y_i^E is ordinal and discrete. The ordered probit model is adequate to analyze this type of data (Maddala, 1983). However, the ordered probit model requires a sufficient amount of observations in each discrete category to estimate nuisance threshold parameters. As can be seen from figure 2.1, categories 300 to 450 CentERpoints have very little observations. In our empirical application, we merge these categories and estimate an ordered probit model with eight categories.

The first 2 specifications of table 2.2 present ordered probit regressions. The first specification uses as regressors a standard set of background characteristics supplemented by reported life experience with trust (TRUSTEXP), subjects' beliefs about the amount they expect to be returned to them (STHINK), and the average amount they expect other senders will send (SMEANS). The second specification omits beliefs.[7]

In both specifications, we do not find gender (GENDER) of subjects to influence trust. This contrasts with the earlier findings in the social capital literature which showed that women are less involved in organizations (Glaeser, Laibson, and Sacerdote, 2002) and that women trust less than men (Alesina and La Ferrara, 2000). We also find that family size (HSIZE), whether an individual is retired from the labor force (RETIRED) or is not working (WORK) do not correlate with trust.

The estimated age effect is robust and of similar magnitude across both specifications. Both age parameters are significant. Based on specification 1 and holding other factors constant, trust is seen to increase until the age of 30, beyond which it starts to decline. This reconfirms the inverted–U shape pattern usually found in the social capital literature (Putnam, 2000; Glaeser, Laibson, and Sacerdote, 2002) although those studies report that social capital reaches a high at 45 years of age. Education also has an inverted–U profile. We find that individuals with secondary and technical training are more likely to trust than subjects with either low education levels (the omitted category) and subjects with university degrees. Comparisons of specification 1 with specification 2 show that the education relationship is also robust to inclusions of beliefs. Glaeser, Laibson, and Sacerdote (2002) find a positive relation between education and organizational membership but do not report results which would indicate the presence of non–linearities. Subjects' religious beliefs were classified in three categories, protestants, catholics, and atheists. We find no evidence that either catholics or protestants trust differently than atheists (the omitted category), which squares with results found by Alesina and La Ferrara (2002).

Both belief variables, STHINK and SMEANS, have positive effects on trust and are highly significant. These results indicate that senders who expected to receive more sent more, and senders who thought other senders would send more increased their amount sent. The former result captures expectations of the subjects.[8] The latter result can be interpreted as a social norm as individuals tend to partly emulate what they expect others to do. In order to asses the contribution of beliefs to the empirical model, we computed a likelihood–ratio test comparing specifications 1 and 2. The test value of 232.12 (5% χ^2 critical value of 5.99) indicates that apart from being statistically significant, beliefs substantially improve the predictive fit of the model.

Finally, we asked subjects to report their lifetime experience with trust (TRUST-EXP). This was done to investigate whether a form of state dependance existed, whereas an individual who is dissatisfied with his experiences with trust is less likely to trust in the experiment.[9] Our results show that the state of trust individuals perceive themselves to be in does not significantly correlate with experimental trust.

2.3.1 Comparing Experimental and Survey Trust Measures

In this section, we sequentially address the following questions: 1) Do answers to the WVS trust question predict well experimental trust? 2) Do survey and experimental trust measures provide the same information on the determinants of trust? It is important to stress that both questions are complementary. The first question is relevant if we are interested in predicting experimental trust responses. However, it leaves open the reasons of what drives the possible correlation between both variables. The second question asks whether we can extract the same information from both set of measures.

Prediction

The question of whether or not answers to the WVS trust question predict experimental trust has been addressed by GLSS. Running a linear regression of experimental trust of answers to the WVS trust question and a set of covariates, they find that answers to the WVS do not significantly explain their experimental trust outcomes. The main explanations given for this result are that the WVS question is vague, hypothetical, and likely to be misreported. To investigate which of these explanations is more relevant, we first follow GLSS by adding answers to the WVS trust question (WVS) as a regressor in our experimental trust equation. This corresponds to specification 3 of table 2.2. Contrary to GLSS, we find that answers to the WVS trust question do significantly explain experimental trust. Furthermore, all other parameter estimates of the model are robust to the inclusion of this variable.

How can we rationalize the finding that answers to the WVS trust question predicts well our experimental trust and not that of GLSS? In our view, the two elements explaining the differences between both studies are the amount of background information on subjects which is available, and differences in experimental designs. The role of each of these factors can be better understood by analyzing the following system of linear equations [10]

$$y_i^E = \boldsymbol{x}_i^{\boldsymbol{t}\prime}\boldsymbol{\beta}^E + \xi y_i^{WVS} + \varepsilon_i^E \quad (2.2)$$

$$y_i^{WVS} = \boldsymbol{x}_i^{\boldsymbol{t}\prime}\boldsymbol{\beta}^{WVS} + \varepsilon_i^{WVS} \quad (2.3)$$

Equation (2.2) is the linear version of equation (2.1), where y_i^{WVS} denotes answers to the WVS question, ε_i^E captures the unobserved determinants of experimental trust and ξ measures the predictive power of answers to the survey trust question. The second equation relates answers to the WVS trust question to $\boldsymbol{x}_i^{\boldsymbol{t}}$ and an unobserved component ε_i^{WVS}. It is important to point out that the result which we present below will hold in general for any other measure or linear combination of survey trust and social capital measures which are used as predictors. Substituting (2.3) in (2.2) and rearranging yields

$$y_i^E = \boldsymbol{x}_i^{\boldsymbol{t}\prime}\boldsymbol{\beta}^{E+} + \xi\varepsilon_i^{WVS} + \varepsilon_i^E \tag{2.4}$$

where $\beta^{E+} = \beta^E + \xi\beta^{WVS}$. From equation (2.4) we see that the value of ξ is identified from the variation between y_i^E and ε_i^{WVS}. Partialling out the covariates $\boldsymbol{x}_i^{\boldsymbol{t}}$, it is straightforward to show that for the linear model, the probability limit of the estimated predictive effect $\widehat{\xi}$ is

$$p\lim\ \widehat{\xi} = \xi + \left(\frac{\boldsymbol{V}\left(\varepsilon_i^E|\boldsymbol{x}_i^{\boldsymbol{t}}\right)}{\boldsymbol{V}\left(\varepsilon_i^{WVS}|\boldsymbol{x}_i^{\boldsymbol{t}}\right)}\right)^{1/2} \boldsymbol{Cor}\left(\varepsilon_i^E, \varepsilon_i^{WVS}|\boldsymbol{x}_i^{\boldsymbol{t}}\right) \tag{2.5}$$

where $V\left(\varepsilon_i^E|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$ and $V\left(\varepsilon_i^E|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$ are the variances of ε_i^E and ε_i^{WVS} conditional on $\boldsymbol{x}_i^{\boldsymbol{t}}$ and $Cor\left(\varepsilon_i^E, \varepsilon_i^{WVS}|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$ denotes the correlation between both unobserved components for a given set of covariates.

The impact of the amount of background information available and the experimental design on the estimated predictive power of survey trust measures emerge from equation (2.5). First, the amount of information on the characteristics of subjects which is available to experimenters will play an important role, as any omitted characteristics remotely correlated with survey and experimental trust will be captured by ε_i^E and ε_i^{WVS}. The higher the number of common unobserved characteristics, the higher $Cor\left(\varepsilon_i^E, \varepsilon_i^{WVS}|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$ will be. It is important to note that a stronger correlation will amplify the differences in estimates of $\widehat{\xi}$ between studies which have different variance terms $V\left(\varepsilon_i^E|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$ and $V\left(\varepsilon_i^{WVS}|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$. One such omitted factor is subjects' history of traumatic experiences which has been shown to be correlated with trust (Alesina and La Fererra, 2002), but which is not observed by GLSS nor the present study. We investigated the amount of correlation between ε_i^E and ε_i^{WVS} in our data by jointly estimating equations (2.2) and (2.3), conditioning on the set of background characteristics corresponding to specification 4 in table 2.2.[11] The estimated value of $Cor\left(\varepsilon_i^E, \varepsilon_i^{WVS}|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$ was 14.8%, significant at the 1% level. To get an idea of the impact an omitted variable can have on the estimated correlation between unobserved components, we removed lifetime experience with trust (TRUSTEXP) from the list of regressors. As conjectured, the estimated correlation increased from 14.8% to 17.28%.[12]

Second, differences in experimental design will directly influence the predictive power of survey trust measures via ξ, the common component between both trust measures, and via $V\left(\varepsilon_i^E|\boldsymbol{x}_i^{\boldsymbol{t}}\right)$, the variance of the experimental design. The common element shared by two trust measures ξ is expected to be higher when both measures are tailored to capture the same type of trust. The WVS question explicitly elicits trust towards strangers. In our experiment, senders and receivers were strangers as their identities were never disclosed to the other party. On the other hand, subjects in GLSS were not strangers, as their identity was deliberately revealed amongst subject pairs in order to investigate the role of social connections.[13] We suspect that the fact that subjects knew each other served as a behavioral discipline device, with subjects acting more kindly than they would if paired against strangers. This may well account for the high concentration of offers around the maximal amount sent reported by GLSS. Because our experimental design makes us more likely to capture WVS type trust than the experimental design of GLSS, we expect ξ to be relatively higher in our experiment. Finally, if whether subjects knew each other or not influences the distribution of responses, this design feature will also affect the variance term $V\left(\varepsilon_i^E|\boldsymbol{x}_i^t\right)$. Data of GLSS show that the unconditional variance in amounts sent is low, with 71% of their senders sending the maximal amount. In our experiment, the variance in the amount sent is much more dispersed which, from (2.5), implies that we should indeed expect higher values of $\widehat{\xi}$.[14]

The framework above demonstrates that the predictive power of survey trust measures is intimately linked to the information experimenters posses on their subjects, and the choice of the experimental design. Because most of the factors are to some extent under the experimenters' control, prediction is not a suitable basis to compare experimental and survey trust. As the relevant policy exercise concerns measuring the effect of changes in the background characteristics on trust and trustworthiness, it seems more interesting to compare both measures on the basis of whether or not they carry the same information on the determinants of trust. This is something on which prediction has little to say.

Informational Content

Measuring trust and trustworthiness using experiments is difficult when the target group is a nation's population. A more accessible alternative consists of surveys, which are easily found for a handful of countries. The methodological question is whether or not the effects of background characteristics on trust can be measured equally well using either experimental or survey data. If the effects measured were the same, the relative accessibility of survey measures would weaken the need for conducting experiments with representative samples.

By observing subjects' decisions in the trust game and their answers to the WVS trust question, we are in the unique position to compare how different the inferences on the determinants of trust can be when researchers use a popular survey question rather than experimental methods to measure trust. Specification 4 in table 2.2 reports results from a probit regression of the answers of our senders to the WVS trust question on their background characteristics. The differences with the experimental estimates are quite remarkable. We find that catholics and protestants trust others less than individuals without religious beliefs. This is in sharp contrast to the results from the experimental data where religious effects were totally absent. The second major difference concerns the education pattern. We do not find any effect of education on survey trust while we have found that a significant inverted–U relationship related experimental trust to education. Another notable difference is the effect of reported lifetime experience on trust. The effect is positive and significant when using the survey trust measure while it has an insignificant impact on experimental trust.

Despite these differences, there are some notable similarities. The inverted–U shape effect of age on trust remains when using survey trust. The number of children, subjects income, gender, and work status also remain insignificant using the survey measure.

To test whether the differences across measures significantly outweigh the similarities, we compare all effects simultaneously. In appendix 2.A.1, we propose a simple minimum distance test which compares the difference between all parameter estimates of the probit model and the ordered probit model. We computed the test statistic based on specifications 2 and 4 of table 2.2. The value of the test is 21.78, significant at the 10 percent level. This indicates that the differences driven by the changes in the religious and education effects are strong enough to reject the null hypothesis that the informational content of both trust measures is identical.

2.3.2 Explaining Differences between both Measures

The preceding section has shown that effects of background characteristics on trust are not robust to the type of measure used. We already mentioned that these differences can be attributed to the fact that the WVS question is vague and hypothetical or that it is misreported at the individual level. Recent developments in econometrics give us ways to test and control for misreporting of a discrete endogenous variable. If the difference between both experimental and survey trust measures are entirely due to misreporting, there is scope for using the WVS question and appropriately correct for misreporting. Furthermore, because our senders answered the survey question after having played the trust game, there is the possibility that senders may have given answers to the WVS question which looked coherent with their play in the game. This would add to the amount of misreporting in our answers to the WVS question. This section tests for general misreporting and misreporting due to the sequence of play.

We define $\alpha_{10}\left(y_i^E\right)$ as the probability that a subject answers 1 ("YES") to the WVS trust question when his truthful answer would be 0 ("NO"). This event occurs when, for example, generous senders are more inclined to state that they trust others rather than truthfully answering that they don't. Similarly, we define $\alpha_{01}\left(y_i^E\right)$ as the probability that subject answers 0 ("NO") to the WVS trust question when his truthful answer would be 1 ("YES"). This probability captures events such as senders who are more likely to state that they do not trust others when they would have answered the opposite, had they been truthful. We allow for the fact that the amount of misreporting may depend on the experimental decision y_i^E by assuming the probabilities have the following logit form

$$\alpha_{10}\left(y_i^E\right) = \frac{\exp\left(\theta_0^{10} + \theta_1^{10} y_i^E\right)}{\exp\left(\theta_0^{10} + \theta_1^{10} y_i^E\right) + 1}$$

$$\alpha_{01}\left(y_i^E\right) = \frac{\exp\left(\theta_0^{01} + \theta_1^{01} y_i^E\right)}{\exp\left(\theta_0^{01} + \theta_1^{01} y_i^E\right) + 1}$$

where $\left\{\theta_0^{10}, \theta_0^{01}, \theta_1^{10}, \theta_1^{01}\right\}$ are unknown parameters to be estimated. Some special cases are of interest. If $\theta_1^{10} = \theta_1^{01} = 0$ and the constant terms θ_0^{10} and θ_0^{01} are large, misreporting is random in the population of senders and is not affected by the preceding experiment. If in addition θ_0^{10} and θ_0^{01} are small, the misreporting probabilities are small indicating that senders truthfully answer the WVS question. Incorporation of these probabilities in a likelihood equation is a straightforward application of the results of Hausman, Abrevaya, and Scott–Morton (1998) and can be found in appendix 2.A.2 of this chapter.

Specification 5 of table 2.2 presents regression results for the probit model with misreporting. Both θ_1^{10} and θ_1^{01} associated with senders' experimental decisions y_i^E are not significantly different from zero, indicating that the experimental decision did not lead senders to systematically misreport their true answer to the WVS trust question. To test for overall misreporting, we computed a log–likelihood ratio test comparing the probit model with and without misreporting (specifications 4 and 5).[15] The test value of 5.38

with 10% critical value of 7.02 does not reject the null hypothesis of no misreporting. Both sets of results suggest that senders in our experiment truthfully answered the WVS trust question, which means that allowing for the possibility that subjects misreport their answers is not sufficient to reconcile both measures. This suggests that the WVS trust question captures a notion of trust which differs from the notion of trust captured in our economic experiment.

2.4 Results on Trustworthiness

We follow GLSS and measure trustworthiness as the return ratio, defined as the amount returned divided by the amount available to return. In our experiment, the available amount to return equals the amount received multiplied by two, plus the experimental endowment of 500 CentERpoints. Responders were asked to play the strategy method by which they decide how much they will give back for each of the 11 possible amounts they can receive from the sender. This implies that we observe a sequence $\{y_a^R \in [0,1] \,|a \in \{0, 50, ..., 500\}\}$ for each responder, where y_a^R denotes the return ratio when receiving a CentERpoints from the sender. The main advantage of the return ratio is that it is automatically scaled, which controls for the fact that receivers can send more simply because the total available amount increases with a. Figure 2.2 shows a boxplot of the return ratio for all 11 possible amounts. The thick line inside each box represents the median, the top and bottom of the boxes represent the 75th and 25th percentiles while the top and lower whiskers represent respectively the maximal and minimal values of the distribution.[16] The two important features of this figure are that the ratio 1) monotonically increases and is concave in the amounts received, 2) a significant fraction of the responders return nothing (especially in low categories) while practically no responder returns the entire possible amount.

Did it pay to trust? Figure 2.3 presents density estimates of senders returns of investments in trust.[17] Each line represents the estimated distribution of returns for a given number of CentERpoints sent. If responders return to senders exactly what they sent, the return on investment is 0. If responders do not return anything to the senders, the return is -1. Apart from the distribution of returns when 50 CentERpoints are sent, all distributions have roughly the same shape. The common finding in laboratory trust experiments is that trust barely pays, as responders return to senders what they have sent (Camerer, 2003). Our results reconfirm these findings. We find that the median return on investment is slightly above 0 for every amount sent. Furthermore, the probability of getting nothing back from a receiver (a return ratio of -1) is not zero. The individual level analysis of the return ratio is based on the following Tobit model (Amemiya, 1984)

$$y_{ai}^{*R} = \boldsymbol{x}_i^{\boldsymbol{r}\prime}\boldsymbol{\eta} + \gamma_1 a + \gamma_2 a^2 + \varepsilon_i^{\boldsymbol{r}} \quad i = 1, 2, ..., N \tag{2.6}$$

$$y_{ai}^{R} = y_{ai}^{*R} \quad \text{if } y_{ai}^{*R} > 0 \tag{2.7}$$

$$= 0 \quad \text{if } y_{ai}^{*R} \leq 0 \tag{2.8}$$

Equation (2.6) describes the unconstrained trustworthiness propensity of responders. This propensity is modelled as depending on a vector of background characteristics $\boldsymbol{x}_i^{\boldsymbol{r}}$, an unobservable component $\varepsilon_i^{\boldsymbol{r}}$, and a vector of unknown population parameters η. The quadratic form in a is added to capture the increasing monotonicity in amounts returned observed in the data.[18] Equations (2.7) and (2.8) describe the censoring rule which allows responders with extremely low trustworthiness propensities to return nothing with positive probability.

The estimation results are presented in table 2.3. The first specification includes standard background characteristics of the responder, reported trust experience, their beliefs about what they expect to receive from the sender (RTHINK),[19] and responders' answers to the WVS trust question. The second specification removes the WVS trust answers while the third specification adds answers to the trust question and responders' beliefs. We compare the first three specifications using log–likelihood ratio tests. The extended specification which includes interaction terms is clearly preferred to the first two specifications.[20] Accordingly, our analysis below will focus on the results of the extended specification.

As could be seen from the raw data in figure 2.2, amounts returned monotonically increase and are concave in a, the amounts received. This is also reflected in the Tobit estimates, where the first order term γ_1 is positive and the second order term γ_2 is negative, both significant at the 1% level. The life cycle evolution of reciprocity is captured by the parameters of RETIRED, AGE, AGESQ, and the three interaction terms. The change in the trustworthiness propensity which follows from a change in age is given by (standard errors in parenthesis)

$$\frac{\partial R_{ia}^{*}}{\partial \text{AGE}_i} = 2 \times \underset{(0.000017)}{0.000032}\ \text{AGE}_i - \underset{(0.0005)}{0.0014}\ \text{WVS}_i - \underset{(0.0000)}{0.0003}\ \text{RTHINK}_i \tag{2.9}$$

We first solve for the turning points, which we define as the age around which the sign of the derivative changes, and compare them with those of trust found in the previous section. Because of the interaction terms, computation of the turning points requires that we fix the values of WVS and RTHINK. We can get an overall picture by evaluating equation (2.9) at the sample means of WVS and RTHINK. We find that trustworthiness reaches its lowest level when individuals reach the age of 30 years, and increases beyond that. These results differ remarkably from the life cycle evolution of trust discussed in the previous section. There, we found that trust increases until the age of 30 and decreases beyond that. The last section of the chapter discusses a possible explanation of this result. We next evaluated the age turning points for those who report trusting others (WVS=1) and those who do not (WVS=0). The age profile of individuals who state they do not trust others reaches a low at 21 years of age, while it reaches a low at 43 years of age for those who declare trusting others.

In the previous section, we found that the relationship between trust and education was inverted U shape, with subjects without a secondary degree and those with university degrees displaying relatively less trustful behavior. The relation between education and trustworthiness is very different. Less educated subjects (the omitted category) return significantly more than educated subjects, all degrees confounded. Moreover, the parameter estimates suggests a U shape relationship, with individuals with university degrees being more trustworthy that subjects with technical education degrees.

The effect of gender also distinguishes trust from trustworthiness. While gender was found to have no impact on trust, we find here that men return on average significantly less than women.

One of the interesting findings of GLSS was that answers to the WVS trust question did not correlate with experimental trust but correlated rather well with the return ratio. In our experiment, we also find that subjects who trust more others are also more trustworthy. However, some of the issues raised in section 2.3.1 concerning the predictive power of stated trust questions may also apply here, although it is less clear from the results in this section whether the underlying process determining trustworthiness and trust are as similar.

We have shown in the previous section that subjects' beliefs were important determinants of trust. Here, beliefs of responders also play an important role in determining trustworthiness. Responders who believed they would receive more had higher average return ratios. To gain some insights on the importance of beliefs on trustworthiness, we estimated our extended specification omitting beliefs. Specification 4 in table

2.3 shows the result of this regression. The only notable change is that trustworthiness of those who report trusting others continues to decline with age while it no longer declines for those who report not trusting others. A log–likelihood ratio test (value of 85.88, significant at the 1% level) confirms that omitting beliefs substantially lowers the predictive fit of the model.

We end by noting that some individual characteristics have no effect on trustworthiness. This is the case of subjects' income, whether they work or not, their retirement status, religion, and their lifetime trust experience. Interestingly, none of these characteristics were found to explain experimental trust.

It is well known that the Tobit model is sensitive to the distributional assumption placed on the unobserved component (Newey, 1987). An alternative estimator which relaxes most distributional assumptions of the Tobit model is the Symmetrically Trimmed Least Squares estimator (STLS) of Powell (1986). Contrary to Tobit, the semiparametric STLS estimator does not require normality and is robust to (bounded) heteroscedasticity of unknown form in ε_i^r. All our specifications were estimated using the STLS estimator (results are presented in table 2.4). Hausman (1978) specification tests never reject the null hypothesis of normality and homoscedasticity of the error terms on which the Tobit model rests.

2.5 Results on Participation

The major impediment experimenters must overcome to test for participation bias is that they generally do not observe non–participants. In our experiment, we observe both the decision to participate and the characteristics of non–participants. This allows us to address the following issues 1) whether observed or unobserved factors are more important determinants of participation and 2) if participation is based on unobservable characteristics, are these related to experimental outcomes? The first question addresses the current belief in experimental economics which suggests that unobserved factors such as preferences for risk and money are more likely to explain participation in the experiment than the observed characteristics of subjects (Camerer and Hogarth, 1999). The second question directly touches the issue of participation bias, by which actions of participants are not representative of the population at large.

The most natural framework to approach both questions is that developed by Heckman (1978). We model participation as being driven both by observed and unobserved factors, the latter having the potential to affect the outcomes of interest and cause participation bias. Let $d_i \in \{0, 1\}$ be an indicator of participation in the experiment and let an individuals' unobserved latent propensity to participate be

$$d_i^* = \boldsymbol{x}_i^j \boldsymbol{\delta} + \theta \, \mathrm{RATIO}_i + \varepsilon_i^d \quad \text{for } j = \boldsymbol{r}, \boldsymbol{t}$$

where $\boldsymbol{x}_i^j$ is the conditioning vector entering the trust and reciprocity models, ε_i^d is an unobservable determinant of participation assumed to be drawn from a $N(0, 1)$ distribution, and $(\boldsymbol{\delta}, \theta)$ are unknown parameters. A general feature of these models is the requirement of a valid exclusion restriction for nonparametric identification of the participation bias. In practical terms, we need a variable which affects participation but does not directly affect either experimental and survey measures used in this chapter. Our exclusion restriction is the variable RATIO, which is computed as proportion of questionnaires completed by panel members in the three months which preceded our experiment. This variable directly measures the participation propensity of subjects when participation is uncorrelated with financial outcomes.[21] The dependance between the experimental outcomes and the participation decisions is captured by the amount of correlation between ε_i^d and the unobservable components determining trust (see equation (2.1)) and trustworthiness (see equation (2.6)). We replicated the estimations of sections 2.2 and 2.4 by separately estimating an ordered probit, a binary probit model, and a Tobit model, along with the participation decision. We allow for potential participation bias by letting ε_i^d be correlated with the unobserved component of each experimental decision.

Most of the parameters entering the systematic part of the participation propensity were insignificant, confirming the conjecture that participation is mostly explained by unobserved characteristics of subjects. Results for trust and trustworthiness are virtually identical to those reported in the text. All but one parameter entering the trust propensity was significant (see text above).[22] One notable exception was income which has a positive and significant effect on participation, which rules out participation based on low opportunity costs. The presence of participation bias can be determined by testing the statistical significance of the correlation coefficients between ε_i^d and the unobserved components of the trust and trustworthiness measures used in this chapter. We find that none of the three correlations are significant at the 10% level, a clear indication that the unobserved characteristics determining participation in the experiment do not correlate with the experimental decisions.

2.6 Discussion and Conclusions

The literature has identified trust and trustworthiness as important factors of economic performance and growth. Understanding the determinants of these at the societal level is important yet, not well documented. The majority of the existing empirical evidence relies on one of two complementary methodologies. Survey methods on one hand collect responses of heterogeneous samples, at the expense of having to rely on hypothetical and self–reported measures. On the other hand laboratory experiments offer the possibility to collect data on the actual behavior of subjects at the expense of collecting this data for a very special subset of the population of interest.

This chapter presented results from a computerized experiment whose participants were randomly drawn from the Dutch population. This approach allowed us to combine the strengths of experiments and survey data collection methods.

One of the key findings of this chapter is that background characteristics of subjects, mainly their age and education levels, do play an important role in determining trust and trustworthiness, although they affect trust and trustworthiness in very different ways. Our results reconfirmed the existing inverted U shape relation between trust and age, with trust increasing until the age of 30 and decreasing beyond that. On the other hand, we find that the relation between trustworthiness and age is U shaped, with trustworthiness decreasing until the age of 30, and rising again beyond that point. This raises the question of why do the young and elderly trust less but are more trustworthy than middle aged individuals? One explanation is that individuals who trust the most take for granted that the average individual in society will do the same. Hence, when someone places trust in them, they are less likely to be surprised by this action and will not place a premium as high on rewarding trust as would individuals who trust others less.

The education patterns are also very different. We find an inverted–U shape relation between education and trust, and an U shape relationship between education and trustworthiness. Such opposite influences of background characteristics are puzzling insofar as it is typically assumed that trust and trustworthiness go hand in hand, which would suggest that both are determined in similar ways. Reconciling the age and education patterns of trust and trustworthiness is an interesting topic for future research.

An additional contribution of the chapter is that we provided a new way to compare experimental and survey trust measures. The literature has up till now assessed the validity of survey trust questions by testing whether or not they predicted well experimental trust. One of the main messages of the chapter is that this method of validation has been given too much attention, primarily because the predictive power of survey measures is intimately linked to the sample used, the amount of background information available on the subjects, and the experimental design. Our analysis has shown that by carefully selecting samples and designs, experimenters increase their odds of finding either a low or high predictive power of the survey trust measure. Thus, despite that contrary to the existing literature our survey trust measure predicts well trust in our experiment, we do not take this as evidence validating the use of survey trust questions. It is important to note that the problems with prediction are only relevant if predicting experimental trust with survey trust is what experimenters are trying to achieve. In general, prediction is useful if applied to an object which has a clear interest in being predicted well. Trust measured in an experiment is an abstract quantity whose main purpose is to extract from it useful characteristics of the population under study. Viewed in this way, it is not clear whether the emphasis on predicting its value by other trust measures is in general warranted.

As our chapter focuses on investigating the determinants of trust and trustworthiness, it follows quite naturally that a more convincing comparison of both measures could be achieved by comparing whether experimental and survey measures have the same informational content. On these grounds, the differences between both trust measures are stark. We found that education has an inverted–U shape relation with experimental trust while it does not correlate at all with answers to the WVS trust question. In contrast, religion correlates well with answers to the WVS trust question but not at all with experimental trust.

We have also examined possible explanations for the differences between both measures. The two prominent explanations are that subjects either misreport their answers to the WVS question, or that the question simply captures a different notion of trust than the one which is captured in the experiment. We do not find any evidence suggesting that these differences are due to subjects misreporting their answers to the survey trust question. As surveys remain more accessible than nation–wide laboratory experiments, it seems worthwhile for researchers interested in making cross–country comparisons to design new survey questions which will narrow the gap with experimental measures.

Finally, this chapter made one of the first tests of participation bias in an economic experiment. We have not found any evidence suggesting the presence of participation bias in our experiment. In our view, this is a reassuring finding for experimental economics.

Appendix to Chapter 2

2.A Econometric appendix

2.A.1 Minimum Distance Test

Define σ^{WVS} and σ^{E} respectively as the standard deviation of ε^{WVS} and ε^{E}. Under the assumptions of the parametric models, the probit estimator provides consistent estimates of β^{E}/σ^{E} while the ordered probit model provides consistent estimates of β^{WVS}/σ^{WVS}. The comparison of both ratios is complicated by the fact that the experimental variance is partly under experimental control (see discussion in section 2.3.2). It is possible to get ride of σ^{E} by normalizing say the kth component to 1, which is equivalent to dividing the parameter vector by the kth component β_k^{E}/σ^{E}. Under this normalization,[23] the following transformed vector $\left\{\frac{\beta_1^E}{\beta_k^E},...,\frac{\beta_{k-1}^E}{\beta_k^E},1,\frac{\beta_{k+1}^E}{\beta_k^E},...,\frac{\beta_K^E}{\beta_k^E}\right\}$ is independent of σ^{E}. One can perform a similar division for parameter estimates from the probit model on the WVS trust question and obtain a second vector of parameters, this time independent of σ^{WVS}. In what follows, we use ξ^{E} and ξ^{WVS} as shorthand notations for the vectors of the ordered and binary probit models excluding the constant and the normalized kth term.[24] Under the null hypothesis that the effects of background characteristics are the same, both sets of estimates would equal each other.

Our test statistic has the following familiar quadratic form

$$N\left(\widehat{\boldsymbol{\xi}}^{E}-\widehat{\boldsymbol{\xi}}^{WVS}\right)'\boldsymbol{W}^{-1}\left(\widehat{\boldsymbol{\xi}}^{E}-\widehat{\boldsymbol{\xi}}^{WVS}\right) \tag{2.10}$$

where W represents the covariance matrix of the difference between both parameter vectors. Below we show that the test statistic above follows a chi square distribution with $K-2$ degrees of freedom.

We briefly sketch here a minimum distance test for observable characteristics. We will use $\boldsymbol{E}_N$ to denote sample expectations and $\boldsymbol{E}$ for corresponding population expectations. The estimated parameters solve

$$\begin{aligned}\widehat{\boldsymbol{\xi}}^{E} &= \arg\max_{\xi} \boldsymbol{E}_N\left[L^{E}(\boldsymbol{\xi})\right]\\ \widehat{\boldsymbol{\xi}}^{WVS} &= \arg\max_{\xi} \boldsymbol{E}_N\left[L^{WVS}(\boldsymbol{\xi})\right]\end{aligned}$$

where $L^{WVS}(\boldsymbol{\xi})$ denotes the binary probit likelihood function and $L^{E}(\boldsymbol{\xi})$ denotes the likelihood function of the ordered probit where the threshold parameters have been concentrated out. We will use the notation $L_{\boldsymbol{\xi}}(\boldsymbol{\xi})$ and $L_{\boldsymbol{\xi}\boldsymbol{\xi}'}(\boldsymbol{\xi})$ to respectively denote the scores and the matrix of second derivatives of each function. It follows that

$$\sqrt{N}\begin{pmatrix} \widehat{\boldsymbol{\xi}}^{E}-\boldsymbol{\xi}_0 \\ \widehat{\boldsymbol{\xi}}^{WVS}-\boldsymbol{\xi}_0 \end{pmatrix} = -\left\{\begin{pmatrix} \left\{\boldsymbol{E}_N\left[L^{E}_{\xi\xi'}\left(\overline{\boldsymbol{\xi}}\right)\right]\right\}^{-1} & \mathbf{0} \\ \mathbf{0} & \left\{\boldsymbol{E}_N\left[L^{WVS}_{\xi\xi'}\left(\overline{\boldsymbol{\xi}}\right)\right]\right\}^{-1} \end{pmatrix} \sqrt{N}\begin{pmatrix} \boldsymbol{E}_N\left[L^{E}_{\xi}\left(\boldsymbol{\xi}_0\right)\right] \\ \boldsymbol{E}_N\left[L^{WVS}_{\xi}\left(\boldsymbol{\xi}_0\right)\right] \end{pmatrix}\right\} + o_p(1) \tag{2.11}$$

where every element of $\overline{\boldsymbol{\xi}}$ lies in between corresponding elements of $\widehat{\boldsymbol{\xi}}^{E}$ and ξ_0. Under appropriate regularity conditions (Newey and McFadden, 1994), Both $\boldsymbol{E}\left[L^{E}_{\xi}\left(\boldsymbol{\xi}_0\right)\right]$ and $\boldsymbol{E}\left[L^{WVS}_{\xi}\left(\boldsymbol{\xi}_0\right)\right]$ are zero vectors and

$$\begin{aligned} \boldsymbol{E}_N\left[L^{E}_{\xi\xi'}\left(\overline{\boldsymbol{\xi}}\right)\right] \xrightarrow{U} \boldsymbol{E}\left[L^{E}_{\xi\xi'}\left(\boldsymbol{\xi}_0\right)\right] &\equiv \boldsymbol{H}_1 \\ \boldsymbol{E}_N\left[L^{WVS}_{\xi\xi'}\left(\overline{\boldsymbol{\xi}}\right)\right] \xrightarrow{U} \boldsymbol{E}\left[L^{WVS}_{\xi\xi'}\left(\boldsymbol{\xi}_0\right)\right] &\equiv \boldsymbol{H}_2 \end{aligned}$$

where $\xrightarrow{U}$ denotes uniform convergence. It follows that (2.11) converges in distribution to

$$\begin{aligned} &\mathcal{N}\left(\begin{pmatrix}\mathbf{0}\\ \mathbf{0}\end{pmatrix};\begin{pmatrix}\boldsymbol{H}_1^{-1} & \mathbf{0}\\ \mathbf{0} & \boldsymbol{H}_2^{-1}\end{pmatrix}\begin{pmatrix}\boldsymbol{J}^{E,E} & \boldsymbol{J}^{E,WVS}\\ \boldsymbol{J}^{WVS,E} & \boldsymbol{J}^{WVS,WVS}\end{pmatrix}\begin{pmatrix}\boldsymbol{H}_1^{-1} & \mathbf{0}\\ \mathbf{0} & \boldsymbol{H}_2^{-1}\end{pmatrix}\right) \\ = \ &\mathcal{N}\left(\begin{pmatrix}\mathbf{0}\\ \mathbf{0}\end{pmatrix};\begin{pmatrix}\boldsymbol{V}_{11} & \boldsymbol{V}_{12}\\ \boldsymbol{V}_{21} & \boldsymbol{V}_{22}\end{pmatrix}\right) \end{aligned}$$

where $J^{i,j} = \boldsymbol{E}\left[L^{i}_{\xi}\left(\boldsymbol{\xi}_0\right)L^{j}_{\xi}\left(\boldsymbol{\xi}_0\right)\right]$ for $i, j = E, WVS$. Finally, we get

$$\sqrt{N}\left(\widehat{\boldsymbol{\xi}}^{E}-\widehat{\boldsymbol{\xi}}^{WVS}\right) \xrightarrow{d} N\left(\mathbf{0};\boldsymbol{W}\right)$$

where $\boldsymbol{W} \equiv \boldsymbol{V}_{11}+\boldsymbol{V}_{22}-\boldsymbol{V}_{12}-\boldsymbol{V}_{21}$. Hence, equation (2.10) follows a chi square distribution with $K-2$ degrees of freedom. We compute the test statistic (2.10) by replacing $\boldsymbol{W}$ with a sample average evaluated at either $\widehat{\boldsymbol{\xi}}^{E}$ or $\widehat{\boldsymbol{\xi}}^{WVS}$, both equivalent under the null hypothesis.

2.A.2 Probit Model with Misclassification

Following Hausman, Abrevaya, and Scott-Morton (1998), the probability that a subject states that he trusts others is given by

$$\Pr\left(y_i^{WVS}=1|\boldsymbol{x}_i^{\boldsymbol{t}},y_i^{E}\right)=\alpha_{10}\left(y_i^{E}\right)+\left(1-\alpha_{10}\left(y_i^{E}\right)-\alpha_{01}\left(y_i^{E}\right)\right)\cdot\Pr\left(T_i^{*}>0|\boldsymbol{x}_i^{\boldsymbol{t}}\right) \tag{2.12}$$

where $\left(\alpha_{01}\left(y_i^{E}\right),\alpha_{10}\left(y_i^{E}\right)\right)$ and T_i^{*} are defined in section 2.3. It follows that in the absence of misreporting errors ($\alpha_{01}\left(y_i^{E}\right)=\alpha_{10}\left(y_i^{E}\right)=0$), equation (2.12) collapses to standard binary probit model (Maddala, 1983). The likelihood function of the binary choice model with misreporting is constructed using the choice probabilities (2.12).

2.B Instructions (Translation)

The first 3 screens of the experiment are the same for both senders and responders. Italic notes in the translation are comments by the authors.

- *First screen:*
  This experiment is a research project of researchers from Humboldt University Berlin and Catholic University of Brabant.[25]
  With this experiment you can make **real money** in terms of CentERpoints. You receive from the researchers **additional** CentERpoints (besides the usual telephone allowance).

- *Second screen*:
  During this experiment you will be matched with another member of the panel. You will not know who this person is, **both of you will stay anonymous**. Both of you receive 500 CentERpoints. Then the experiments starts.
  One of you has the possibility to send a share of this away. The amount of points sent will be doubled and given to the other person. The other person has then the opportunity to send a share of the own total amount back. The amount which is sent back will not be doubled.
  How many points you finally earn depends therefore on your decision and the decision of the person you are matched with. You will be randomly assigned to your role.

- *Third screen:*
  We now give you the chance to indicate whether you want to participate. If you decide not to participate, the experiment will end immediately. You will receive the usual telephone reimbursement. If you continue you will receive the 500 CentERpoints.
  Do you want to continue?
  ◯ Yes
  ◯ No

Subjects who choose to participate were then randomly assigned to their roles. Senders and receivers had to read decision screens tailored to their roles.

Senders

- *Fourth screen*:
  You have been matched with another member of the panel. Like you, this person received 500 CentERpoints. You can send a share of your 500 CentERpoints. The panel member with whom you are matched with receives the amount you sent multiplied by 2. Then, this person has the opportunity to send a share of the own total amount back (without knowing who you are). The amount which this person sends back to you will not be doubled.
  How many points do you want to give?
  (The sender could send one out of 11 possible amounts.)

◯ **0** the other person receives additionally nothing and has therefore 500 and you remain with 500 points.
...
◯ **500** the other person receives additionally 1000 and has therefore 1500 in total and you remain with 0 points.

- *Fifth screen:*
  (was depending on the decision taken at the fourth screen, here as example "200")
  You decided to send **200** points.
  The panel member you are matched with receives therefore 400 additional CentERpoints.
  He or she has therefore in total **900** CentERpoints.
  You remain with **300** CentERpoints.
  How many points do you think the other panel member with whom you are matched with will send to you?
  (Participants had to type in a number. In this example in the range of [0,900].)

- *Sixth screen:*
  This experiment is done with some panel members. Half of them interact in the same position as you. They can send a share of their 500 CentERpoints which is doubled and received by a person of the other position.
  How many points do you think those panel members have sent?
  (The sender could indicate one out of 11 possible amounts from 0 to 500).

Responders

- *Fourth screen:*
  You have been matched with another member of the panel. Like you, this person received 500 CentERpoints. This person is asked to send you a share from their own 500 CentERpoints. You will receive the amount of those points the other person has sent multiplied by 2.
  For example, if the other person sends 100 CentERpoints, you will receive 200 CentERpoints. Together with the 500 points you begin with, you will have in total 700 CentERpoints.
  From this amount you can return a share. The amount you send will not be doubled.

- *Fifth screen:*
  As we do not know now how many CentERpoints the other panel member with whom you are matched with has sent we present all possible amounts this person could send to you. The amount you actually receive is written in the next column. Please indicate in the last column what amount you would return **for each possible amount sent**.

  After the real decision of the other person is known the amount you indicated for this particular decision will be realized. The amount you will return will be

deducted from your total amount.
(The responder had to indicate for each of 11 possible amounts the sender could send what he would return. The table was designed as follows:)

If the other sends:	I receive:	In total with the 500 CentERpoints:	**In this case I return:**
0	0	500	_________
...	...	...	...
500	1000	1500	_________

- *Sixth screen:*
  How many points do you **expect** the panel member with whom you are matched with has sent to you?
  (The responder could indicate one out of 11 possible amounts from 0 to 500.)

After these screens the experiment was over. Nobody could go backwards and both senders and responders were asked the following post–experimental questions:

- *Seventh screen (Trust experience question):*
  The last two questions are about trust in general. This question is about your own trust experience.
  If you trust is your trust generally rewarded or exploited?
  Choose the number which is closest to your answer.
  always rewarded 1 2 3 4 5 6 7 always exploited.
  (Participants had to type in a number between 1 and 7).

- *Eight screen (WVS trust question):*
  Generally speaking would you say that most people can be trusted or that you cannot be too careful in dealing with people?
  1.) Most people can be trusted.
  2.) You have to be very careful.
  3.) I do not know.

	S		*R*		Not played		
	Mean	StD	Mean	StD	Mean	StD	
GENDER	0.46	0.5	0.46	0.5	0.43	0.5	Dummy, 1 if Man
AGE	42.66	15.55	43.98	15.67	53.79	16.55	Age
GR_INCP	2580.38	6133.47	1916.28	3289.66	2356.90	3584.27	Gross Personal Monthly Income (Guilders)
GR_INCH	4175.59	7526.39	4342.07	4313.76	4302.52	6593.33	Gross Household Monthly Income (Guilders)
HSIZE	2.27	1.12	2.53	1.33	2.17	1.23	Household Size
NCHILD	0.55	0.92	0.75	1.16	0.4	1.06	Number of Children in household
PARTNER	0.72	0.45	0.78	0.42	0.76	0.43	Partner is living in the household
LOWEDUC	0.043	0.204	0.076	0.266	0.143	0.354	Dummy, 1 if Low education level
SECONDEG	0.388	0.488	0.390	0.489	0.381	0.492	Dummy, 1 if Secondary degree
TRAINDEG	0.449	0.449	0.448	0.499	0.357	0.485	Dummy, 1 if Training degree
UNIVDEG	0.119	0.325	0.085	0.279	0.119	0.329	Dummy, 1 if University degree
WORK	0.591	0.493	0.511	0.501	0.429	0.501	Dummy, 1 if Labor Work
RETIRED	0.130	0.337	0.148	0.356	0.262	0.445	Dummy, 1 if Retired
HHEAD	0.637	0.482	0.609	0.488	0.667	0.477	Dummy, 1 if Head of household
CATHOLIC	0.297	0.458	0.349	0.478	0.309	0.468	Dummy, 1 if Catholic religion
PROTEST	0.210	0.408	0.265	0.442	0.191	0.397	Dummy, 1 if Protestant religion
OTHERS	0.493	0.501	0.386	0.488	0.500	0.506	Dummy, 1 if Atheist or Other religion
WVS	1.5	0.56	1.6	0.58	–	–	Dummy, 1 if Yes to WVS trust question
TRUSTEXP	3.16	1.18	3.45	1.31	–	–	1-7, 1 Always rewarded 7, Always disappointed
Nobs	276		223		42		

Table 2.1: Descriptive statistics

Specification	Experimental trust (1) Coef.	Std.	(2) Coef.	Std.	(3) Coef.	Std.	Stated trust (WVS) (4) Coef.	Std.	(5) Coef.	Std.
CONSTANT	−2.018**	0.736	−0.314	0.681	−1.828***	0.687	−3.069***	0.838	−3.787	1.424
GENDER	0.182	0.133	0.229*	0.134	0.217	0.160	−0.186	0.181	−0.335	0.244
AGE	0.067**	0.031	0.052*	0.029	0.060***	0.031	0.067*	0.037	0.071	0.051
AGE_SQ/1000	−0.897**	0.367	−0.662**	0.335	−0.803**	0.349	−0.909**	0.435	−1.026*	0.589
RETIRED	−0.193	0.374	0.271	0.293	−0.251	0.287	0.528	0.411	0.707	0.535
SECONDEG	0.908***	0.396	0.626*	0.308	0.845***	0.318	0.518	0.422	0.504	0.578
TRAINDEG	0.809**	0.404	0.589*	0.312	0.761**	0.332	0.394	0.436	0.446	0.602
UNIVDEG	0.729	0.457	0.716*	0.361	0.678	0.395	0.522	0.499	0.337	0.678
WORK	−0.219	0.193	−0.089	0.185	−0.189	0.208	−0.290	0.243	−0.451	0.373
LN(GR_INCP)	0.024	0.026	0.007	0.028	0.019	0.039	0.034	0.038	0.044	0.053
NUMCHILD	−0.118*	0.064	−0.043	0.066	−0.129	0.099	0.105	0.102	0.121	0.146
CATHOLIC	0.101	0.170	0.002	0.165	0.149	0.171	−0.476**	0.211	−0.567*	0.290
PROTEST	−0.016	0.164	−0.109	0.149	0.042	0.208	−0.524**	0.227	−0.699**	0.355
TRUSTEXP	0.045	0.059	0.090	0.065	−0.032	0.082	0.665**	0.085	0.905***	0.205
STHINK	0.003***	0.0001			0.003***	0.000				
SMEANS	0.423***	0.061			0.433***	0.044				
WVS					0.362**	0.169				
θ_0^{10}									−3.467	1.621
θ_1^{10}									0.387	0.233
θ_0^{01}									−1.311	1.881
θ_1^{01}									−1.119	1.583
Log-Likelihood	−406.42		−520.06		−403.58		−150.8		−148.11	

Table 2.2: Sender results. Specifications (1) to (3) refer to the ordered probit model, columns (4) and (5) to the binary probit model. The significance of parameters is based on robust standard errors and bootstrap empirical quantiles of the t-statistics (1000 repetitions), N=276.

	(1)		(2)		(3)		(4)	
	Coef.	*Std.*	*Coef.*	*Std.*	*Coef.*	*Std.*	*Coef.*	*Std.*
CONSTANT	−0.042	0.032	−0.071**	0.032	−0.076***	0.035	−0.056	0.035
γ_1	0.087***	0.004	0.0875***	0.004	0.087***	0.004	0.088***	0.004
γ_2	−0.005***	0.0003	−0.005***	0.0003	−0.0045***	0.0003	−0.005***	0.0003
GENDER	−0.018***	0.007	−0.019**	0.007	−0.017***	0.0068	−0.008	0.007
AGE	−0.002*	0.001	−0.001	0.001	−0.0014	0.0014	0.0003	0.001
AGESQ/1000	0.033***	0.017	0.019	0.016	0.032*	0.017	0.004	0.017
RETIRED	−0.004	0.015	−0.007	0.015	0.0052	0.014	0.017	0.015
SECONDEG	−0.053***	0.012	−0.060***	0.012	−0.054***	0.012	−0.052***	0.012
TRAINDEG	−0.065***	0.012	−0.067***	0.012	−0.067***	0.012	−0.062***	0.012
UNIVDEG	−0.053***	0.015	−0.057***	0.015	−0.058***	0.015	−0.058***	0.015
WORK	0.016*	0.009	0.011	0.009	0.019**	0.0089	0.013	0.009
LN(GR_INCP)	0.0006	0.0011	0.002	0.001	0.0001	0.0011	0.0002	0.001
CATHOLIC	−0.009	0.007	−0.009	0.007	−0.0062	0.0073	−0.002	0.007
PROTEST	0.006	0.008	0.004	0.008	0.0051	0.0076	−0.0005	0.008
WVS	0.045***	0.007			0.1035***	0.022	0.149***	0.021
TRUSTEXP	−0.003	0.003	0.0064**	0.0027	−0.017	0.009	−0.011	0.009
RTHINK	0.013***	0.001	0.013***	0.001	0.0255***	0.0044		
AGE×TRUSTEXP					0.0003	0.0002	0.0003	0.0002
AGE×WVS					−0.0014***	0.0005	−0.002***	0.0005
AGE×RTHINK					−0.0003***	0.0000		
σ^2	0.144***	0.002	0.021***	0.0006	0.0207***	0.0006	0.021***	0.0006
Log-Likelihood	936.52		916.75		946.47		903.53	

***: 1% Level. **: 5% Level. *: 10% Level

Table 2.3: Responder results – Tobit estimator. N = 2453.

	(1)		(2)		(3)		(4)	
	Coef.	*Std.*	*Coef.*	*Std.*	*Coef.*	*Std.*	*Coef.*	*Std.*
CONSTANT	−0.039	0.033	−0.069**	0.033	−0.042	0.036	−0.043	0.037
γ_1	0.079***	0.004	0.079***	0.005	0.079***	0.005	0.079***	0.005
γ_2	−0.004***	0.0004	−0.004***	0.0004	−0.004***	0.0004	−0.004***	0.0004
GENDER	−0.013**	0.007	−0.015**	0.007	−0.014**	0.007	−0.003	0.006
AGE	−0.001	0.001	0.0002	0.001	−0.0006	0.001	0.002	0.001
AGESQ/1000	0.017	0.017	0.004	0.016	0.009	0.018	−0.021	0.018
RETIRED	−0.002	0.017	−0.005	0.017	0.006	0.017	0.021	0.017
SECONDEG	−0.050***	0.016	−0.059***	0.016	−0.053***	0.015	−0.045**	0.016
TRAINDEG	−0.064***	0.016	−0.065***	0.016	−0.066***	0.015	−0.057**	0.016
UNIVDEG	−0.040***	0.018	−0.045***	0.018	−0.0469**	0.018	−0.043**	0.018
WORK	0.012	0.008	0.008	0.008	0.0128	0.008	0.008	0.008
LN(GR_INCP)	0.000	0.001	0.001	0.001	−0.0005	0.001	−0.0006	0.001
CATHOLIC	−0.014**	0.007	−0.012	0.007	−0.013	0.007	−0.008	0.007
PROTEST	0.010	0.007	0.009	0.007	0.012	0.008	0.004	0.008
WVS	0.042***	0.007			0.086***	0.020	0.129***	0.021
TRUSTEXP	−0.001	0.003	0.0082**	0.003	−0.025**	0.009	−0.017*	0.009
RTHINK	0.012***	0.001	0.012***	0.001	0.022***	0.0046		
AGE×TRUSTEXP					0.0006**	0.0002	0.0004**	0.0002
AGE×WVS					−0.0011**	0.0005	−0.002***	0.0005
AGE×RTHINK					−0.0002**	0.0000		
Hausman χ^2 test	3.106		2.762		15.656		19.234	

***: 1% Level. **: 5% Level. *: 10% Level

Table 2.4: Responder results – Symmetrically Trimmed Least Squares estimator. N = 2453.

2.D Figures

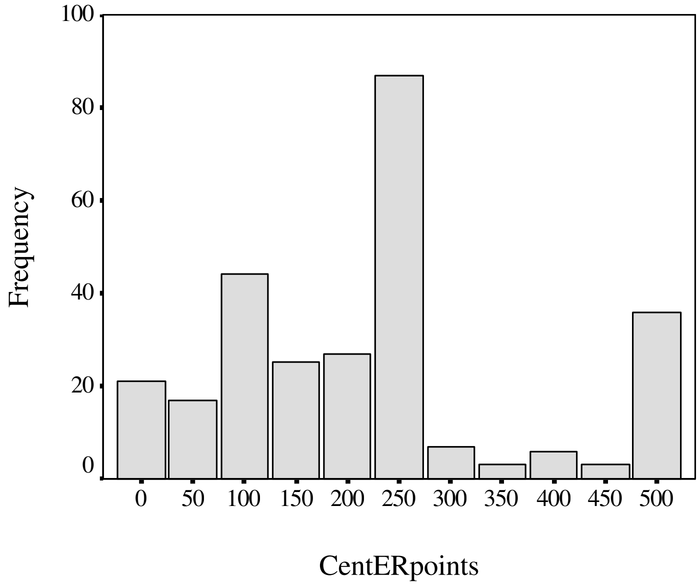

Figure 2.1: Distribution of experimental trust.

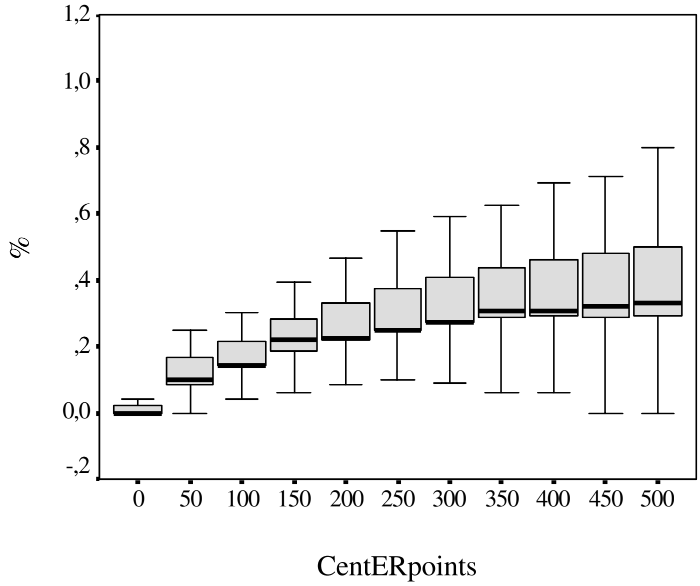

Figure 2.2: Return ratio of responders for each units received, strategy method.

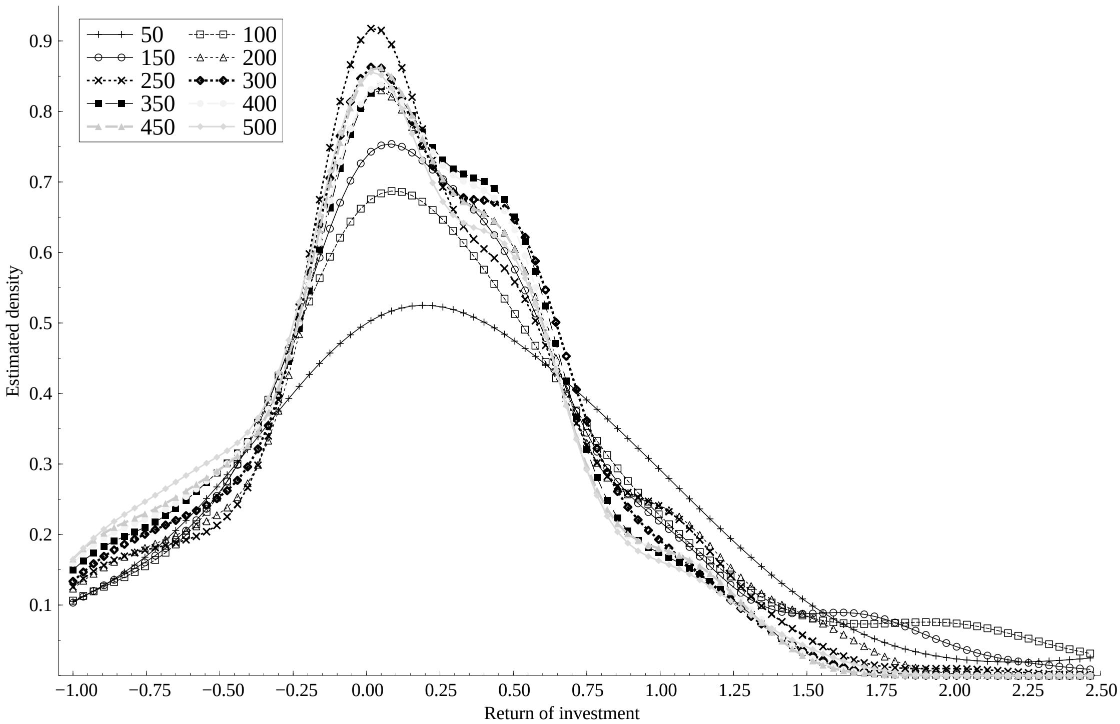

Figure 2.3: Estimated density of potential returns on investments in trust for each amount sent. Gaussian kernel density estimation. Rate of return computed as (amount returned - amount sent) / amount sent. The rate of return is infinity when the amount sent is zero and is not plotted here.

Notes

[1]For a description of the recruitment, sampling methods, and past usages of the CentERpanel see: www.centerdata.nl . Children below 16 years of age as well as immigrants are excluded from the panel. The latter for the reason being that their language proficiency in Dutch makes it difficult for them to answer the questions on a weekly basis.

[2]Computer screens of the original experiment (in Dutch) are available upon request. The translated text of all screens are enclosed in appendix 2.B.

[3]For ease of reading we keep the terms "sender" and "responder" for the different roles. In the experiment we omitted suggestive labels and referred to the person itself or to his opponent as "the matched panel member."

[4]There is weak evidence suggesting that a hot environment triggers stronger responses in two player games. Brandts and Charness (2000) find that the strategy method and the hot environment do not yield significant different responses in two simple sequential two player games.

[5]Results from repeated trust experiments can be found in Anderhub, Engelmann, and Güth (1999) and Willinger, Keser, Lohmann, and Usunier (2003).

[6]14 non–participants initially accepted to play but eventually backed out of the experiment after having observed the roles they were assigned to play. It is interesting to note that 11 out of those 14 panel members were assigned to the role of responders.

[7]We have experimented with a specification including cross–terms but none was found to be statistically significant.

[8]It has been argued that the causality may be in the opposite direction (Selten and Ockenfels, 1998, p. 526-529). We do not investigate these issues in this chapter.

[9]This definition of state dependance differs from that used in labor economics. Labor economists are generally interested in whether or not individuals in a state of unemployment are more likely to be unemployed in the future. In our experiment, we do not investigate whether having trusted in the past increases the likelihood of trusting in the experiment.

[10]In the probit model, $E\left(y|\boldsymbol{X}=\boldsymbol{x}\right)=\Phi\left(\boldsymbol{x}'\boldsymbol{\beta}\right)$ is approximately linear in $\boldsymbol{x}'\beta$ for individuals answering 1 to the WVS trust question with probability between 20% and 80% (e.g., Ruud, 2000). In general, the average probability of answering 1 to the WVS questions lies between 45% and 55% which implies that the linear model holds for most individuals in the sample.

[11]Tables of results are available upon request.

[12]Additionally removing age as a regressor further increased the correlation to 18.5%.

[13]In GLSS, pairs either knew each other before the experiment or got to know each other by jointly filling in a social connection survey. This survey includes among other questions to report the number of all personal acquaintances whom they have in common.

[14]$V\left(\varepsilon_i^E|\boldsymbol{x}_i^t\right)$ is also likely do differ across studies according to differences in the dimension of the choice space of players, the fictitious currency used in the experiment, the monetary endowments, the multiplier, whether the experiment is conducted in the lab, in the classroom, at home on paper or on a computer, and whether the strategy method is used or not.

[15]We use the test proposed by Andrews (2001) to deal with the fact that the probabilities of misreporting rest on the boundary of the parameter space under the null hypothesis. Computation of the test requires that the critical values of the log–likelihood ratio test be simulated. We report critical values based on 1000 simulations. See Andrews for further details.

[16]The minimal and maximal values are corrected to the presence of extreme outliers.

[17]These returns are computed as the (amount received - amount sent) / amount sent.

[18]We have estimated a less restrictive specification with dummy variables for each a category. Results were numerically identical to those presented above.

[19]RTHINK is coded from 0 to 10, where each unit is worth 50 CentERpoints.

[20]The log–likelihood ratio test value is 19.9 when comparing specifications 1 against 3, and 59.14 when comparing specifications 2 and 3. Both are significant at the 1% level.

[21]CP members are not paid to participate in the panel.

[22]Results are available upon request.

[23]This normalization requires that $\beta_k \neq 0$.

[24]The constant term parameter is generally not separately identified from the threshold parameters in both the binary and ordered probit models. Given their values are functions of ad hoc threshold assumptions, they are not used in computation of the test.

[25]Now: Tilburg University. The Catholic University of Brabant changed its name after the experiment.

Chapter 3

Risky Joint Ventures

3.1 Introduction

How do negotiations develop in the presence of large risks? We analyze such a bargaining game between a producer of a movie and an actor. Movie production is characterized by substantial risks: either the movie is a hit, in which case the producer's payoff is very large, or the movie is a flop. Then profits are small and often negative. In many cases, producers try to rehire core actors of top–grossing movies to produce a sequel. Producers seem to think that rehiring the main actors of the original is critical to the success of a sequel (in case of "When Harry met Sally," Meg Ryan and Billy Crystal, in case of "Rocky," Sylvester Stallone).[1] Clearly, the bargaining power of the actor is high when negotiating the contract for the sequel. Core actors of successful films know they are indispensable for the sequel, giving them effective monopoly power.

We present such situations by a two–stage bargaining game where "studios" have ultimatum power when casting the first film. Only if the original film has been successful, actors negotiate a second contract. However, at the second stage actors make a take it or leave it offer to the studio.

This setting has applications to situations outside the film industry where production leads to (1) a sequential resolution of uncertainty, (2) successive negotiations of contracts, and where (3) each round of negotiations carries the risk of terminating the relationship. The model structure therefore resembles risky partnerships and cooperations typical also for R&D joint ventures and venture capital.[2]

[0]We thank Tim Grebe for his help conducting the experiment. We are grateful to Charles Bellemare for providing his nonparametric OX–package and Jan Potters for helpful comments. The results in this chapter were first formulated in Güth, Kröger, and Maug (2003), "You May Have To Do It Again, Rocky! – An Experimental Analysis of Risky Joint Ventures –."

In the terminology of bargaining theory our model is a stochastic two–stage alternating offer game.[3] Stochastic uncertainty about the size of the pie has – to the best of our knowledge – so far been explored for one round (ultimatum games) and one–pie games only. Either only the proposer knows the size of the pie (see Mitzkewitz and Nagel, 1993) or only the responder (see Rapoport, Erev, and Zwick, 1995). However, the specific characteristics of repeated bargaining when uncertainty is large and early termination of a productive relationship is possible have not been studied in experiments before.

Standard game theory suggests that producers and actors both anticipate the potential of a sequel to the original film. Specifically, rational anticipation of a lucrative second contract should make actors inclined to accept offers *below* their outside opportunities at the first stage. So producers should make such offers and get them accepted.

We find that our experimental subjects deviate from such a strategy in important ways. Firstly, "actors" rarely accept offers below their first–stage opportunity costs. We hypothesize that this is the impact of the enormous risk subjects face: in our parametrization, based on field data, the probability of being able to bargain for a lucrative sequel–contract at the second stage is only 25%, so this potential reward is too risky to make subjects pay for this opportunity by foregoing a certain outside opportunity. Thereby "producers" either have to become the only risk taker or have no joint venture at all. To the best of our knowledge, ours is the first experimental study of such large risks in bargaining. Here and in the following we refer to our experimental roles as "actors" and "producers." However, the instructions to our experimental subjects contain no reference to the movie industry or to any other real–life setting this game may reflect.

Parameter calibration close to empirical data has hardly been used in experimental economics. Mostly parameter constellations for experiments are chosen to distinguish between competing theories. We are only aware of two studies by Grether and Plott (1984), and Hong and Plott (1982) which try to capture parameters of the field. We determine our parameters so as to match the moments of an empirical distribution.[4] This adds to the realism of the setting and also makes it easier to interpret our results. It allows us to study additional questions, e.g., to what extent our results could explain the fact that sequels typically have 20% higher production costs than the original film. We feel that results may not be completely independent of the parameters chosen in the experiment and our calibration makes us somewhat more confident about the relevance of our results.

In the following section 3.2 the model is introduced and solved. Section 3.3 is devoted to developing an alternative hypothesis based on equity theory. Section 3.4 explains the procedure we followed for calibrating the parameters of the model. Section 3.6 describes the details of the experimental design. Section 3.7 presents the major regularities of the experimentally observed behavior. There we compare the stylized facts of the case study with these regularities as well as with qualitative aspects of the theoretical solution. Section 3.8 concludes.

3.2 The Model

We model a bargaining game between a single actor, denoted by A and a producer, denoted by P. The game starts with P making a wage offer W_1 to A. If A rejects the proposed wage, the game ends with A receiving his rather low outside option O_1^A and P the profit O_1^P which could be interpreted as the gain from producing the film with another (presumably less talented) actor. We explicitly permit $W_1 < O_1^A$ to allow the producer to offer A a lower wage than $A's$ outside option.

If A accepts the wage offer W_1, the movie is produced. Then chance determines the success s of the movie, where $s \in \{f, h\}$. The surplus or "pie" generated by the movie, to be divided between A and P, is denoted by C_1^s. With probability ω the movie is a "hit" (denoted by h) and generates a total surplus C_1^h, otherwise the movie is a "flop," denoted by f and generates only $C_1^f < C_1^h$, where $0 < \omega < 1$. The profit of the producer is always given by $\Pi_1^s = C_1^s - W_1$. Note that we do not allow for output-contingent contracts. However, we do not model effort–incentives, so the usual reasons for output–related pay do not apply.[5]

After a "flop" the game ends with A earning his wage W_1 and P the low profit Π_1^f of a "flop." After a "hit" the game proceeds to the second stage. Then A proposes a contract for the sequel project. The gain from producing the sequel is known to be C_2. The agent A proposes a wage W_2 that leaves P with profits $\Pi_2 = C_2 - W_2$. The reversal of bargaining power to the agent captures that in case of a "hit" the formerly unknown actor A is now a movie star and cannot easily be replaced. Accordingly, his outside option O_2^A is much larger than before, so $O_2^A > O_1^A$. However, here we do not investigate the raise of outside option as source for the increase of sequels' costs and keep the outside option constant for both stages in the experiment.

If P rejects A's contract offer, the game ends and A receives his outside option O_2^A in addition to his previous payoff W_1 whereas P does not produce the sequel and earns the outside option O_2^P in addition to his previous earnings Π_1^h. If P accepts, then both players collect their contractual earnings from both movies (see appendix 3.A for the stage form).

Altogether, the parameters are the probability ω for the "hit," the four outside option payoffs O_1^A, O_2^A, O_1^P and O_2^P, and the three pie sizes C_1^f, C_1^h, and C_2. In light of the qualitative facts reported in the case study we assume

$$C_1^f < 0 < C_2 < C_1^h \quad .$$

Risk Neutral Agents We first develop the game by assuming risk neutrality of procedures and actors. This solution serves as a benchmark and yields sharp, testable predictions. To render bargaining at all profitable we also stipulate

$$E(C_1^s) \quad > \quad O_1^A + O_2^P \quad , \tag{3.1}$$

$$C_2 \quad > \quad O_2^A + O_2^P \quad , \tag{3.2}$$

where $E(\cdot)$ denotes the expectation operator.

We solve this game by backward induction. At the second stage, A makes a take it or leave it–offer and offers P profits according to her outside option. Hence, the wage at the second stage is

$$W_2^* \quad = \quad C_2 - O_2^P \quad , \tag{3.3}$$

$$\Pi_2 \quad = \quad O_2^P \quad . \tag{3.4}$$

At the first stage, P makes a take it or leave it offer to A that makes A indifferent between accepting and rejecting the offer, so $W_1^* + \omega W_2^* = O_1^A$. Therefore,

$$W_1^* \quad = \quad O_1^A - \omega W_2^* \quad , \tag{3.5}$$

$$\Pi_1^s \quad = \quad C_1^s - O_1^A + \omega W_2^* \quad . \tag{3.6}$$

Equations (3.3), (3.4), (3.5), and (3.6) together with the assumption that offers (not) worse than the ones derived are (accepted) rejected represent the game–theoretic solution (GT) of the game for risk neutral agents.

Relaxing Risk Neutrality: Risk Averse Actors Now we partially relax the assumption of risk neutrality by assuming that agents are risk–averse. Producers are typically large studios owned by diversified investors. As the risk of movie success or failure is idiosyncratic, producers can reasonably be assumed to behave as if they were risk–neutral whereas the same is not true for actors. Moreover, this modelling strategy allows us to build in reservation wages that may vary across actors, and producers may not have full information about actors' reservation wages in bargaining. Hence, we introduce two assumptions:

- Actors are risk averse, while producers are risk–neutral.
- Producers are uncertain about actors' risk aversion.

We explore the implications of these assumptions for the game–theoretic solution in turn. Denote the agent's utility function by U and observe that there is no uncertainty at the second stage of the game, hence equations (3.3) and (3.5) still represent the solution to the second stage. Then we require:

$$U\left(O_1^A\right) \leq \omega U\left(W_1 + W_2^*\right) + (1-\omega)\, U\left(W_1\right) \tag{3.7}$$

for any acceptable W_1, where W_2^* is still given from (3.3). Then, define the lowest W_1 that is just acceptable to the agent by $\hat{W}_1$. Clearly, for any risk averse agent $\hat{W}_1$ exceeds (3.5). Also, it follows directly from (3.7) that any wage offer $W_1 \geq O_1^A$ will be accepted, even by an infinitely risk–averse agent. Hence, we have:

$$O_1^A - \omega W_2^* \leq \hat{W}_1 \leq O_1^A \quad . \tag{3.8}$$

In case the agent's utility function is common knowledge, we would now have $W_1^* = \hat{W}_1$ as before. However, we assume now that $\hat{W}_1$ is unknown to producers, who believe that actors' reservation wages are drawn from a continuous distribution $F\left(\hat{W}_1\right)$ with density $f\left(\hat{W}_1\right)$ and support given by (3.8). Hence, producer's expected payoff as a function of their wage offer is:

$$\begin{aligned} E\left(\Pi_P\left(W_1\right)\right) &= \left[E(C_1^s) - W_1 + E(O_2^P)\right] F(W_1) + (1-F(W_1))O_2^P \\ &= \left[E(C_1^s + O_2^P) - W_1\right] F(W_1) + (1-F(W_1))O_2^P \end{aligned} \tag{3.9}$$

where according to our model $E\left(C_1^s\right) = \omega \cdot C_1^h + (1-\omega) \cdot C_1^f$. Solving first order conditions $\frac{\partial E(\Pi_P(W_1))}{\partial W_1} = 0$ yields:[6]

$$W_1^* + \frac{F\left(W_1^*\right)}{f\left(W_1^*\right)} = E\left(C_1^s + O_2^P\right) - O_1^P \quad . \tag{3.10}$$

We develop a parametric example in appendix 3.C below, which allows us to obtain a closed–form solution for (10) and then convert this solution into quantifiable predictions.

3.3 Behavioral Hypotheses

We are sceptical whether the game–theoretic results, just derived, are in line with experimental behavior. In view of the former results of ultimatum (bargaining) experiments one may expect:[7]

Hypothesis 1: Claims will aim at equal splits which will be nearly always accepted.

Equity theory (Homans, 1961) predicts equal sharing but leaves open *what* is shared equally.[8] This can, for instance, be the expected pie of the given stage so that

$$W_1 = E\left(C_1^s\right)/2 \quad , \tag{3.11}$$

$$\Pi_2 = C_2/2 \quad . \tag{3.12}$$

Hypothesis 1 tries to predict just the modal type of behavior.

There might be some freedom to offer less than $E\left(C_1^s\right)/2$ at stage 1 as compensation on the second stage is possible. If lower first stage offers lead to lower second stage offers sharing the total expected surplus $E\left(C_1^s + C_2\right)/2$ equally is still possible. The actor will accept the lower offer and not be compensated with probability $(1-\omega)$. If the producer offers $E\left(C_1^s\right)/2 - \omega\Delta$ at the first stage in case of a hit the actor can offer $C_2/2 - \Delta$. To reach the equal split he should be compensated by Δ. We therefore allow

$$W_1 = E\left(C_1^s\right)/2 - \omega\Delta \quad , \tag{3.13}$$

$$\Pi_2 = C_2/2 - \Delta \quad , \; -\frac{C_2}{2} \leq \Delta \leq \frac{C_2}{2}. \tag{3.14}$$

Of course, some producers may deviate from equal sharing, e.g., by exploiting their ultimatum power. If so we expect that actors will reciprocate.

Hypothesis 2:

(i) Too meager offers (Π_2 and W_1 close to the game theoretic prediction from (4) and (5)) will be rejected.

(ii) If they are not rejected, then meager offers W_1 are followed by meager offers Π_2 such that Π_2 depends positively on W_1.

Hypotheses 1 and 2 essentially predict (positive and negative) reciprocity.

3.4 Calibrating Parameters

Our experimental design exactly matches the sequential game. We have determined most experimental parameters through calibration. This seems to be important for the case at hand since movie production is extremely risky. The easiest way to capture such risks involved is to rely on parameters that closely resemble those of the field study. Parameter constellations, far off those in the field study, may be interesting for their own sake but do not illuminate what happens in movie production. To avoid that hypotheses are just confirmed because they rely on prominent numbers (Albers, 1997) calibration will aim at non–prominent predictions whenever this can be reasonably justified.

The data for calibration are found in the case "Arundel Partners – The Sequel Project" (Luehrmann, 1992). The case assembles data for 99 movies produced by 6 major studios released in the United States in 1989. The data in this case study are taken from a database largely based on Variety Magazine, a trade magazine specializing on the movie industry. Based on Exhibit 7 of the case we calculate the net present value (NPV) of a first film as:[9]

$$NPV = \frac{PV\ of\ Net\ Inflows\ at\ year\ 1}{1.12} - PV\ of\ Negative\ Cost\ at\ year\ 0. \qquad (3.15)$$

Here, the present value of net inflows are gross box office proceeds in the US, plus international proceeds and revenues from video rentals net of distribution costs and expenses. These are discounted at an estimated cost of capital of 12%. Negative costs include all costs required to make the negative of the film of which prints can be made and rented to theaters. Negative costs include among others the salaries of actors and director, production management, special effects, lighting and music. Table 3.5 gives the total number of films per studio, the number of films that generated a positive NPV on the initial investment, and the total net present value over all 99 films for six major Hollywood studios.

Hence, the average value of a first film is \$736.6m/99=\$7.44m, and 42 films are profitable with the median film making a loss of \$2.26m. The standard deviation is \$34.16m, showing that movie–production is risky. Also, the risks and payoffs are distributed somewhat unevenly across studios with MCA being by far the most profitable and Sony being the least profitable, making losses on 26 of their 34 films in 1989. The most profitable film in the sample is Batman (Warner Brothers, NPV= \$224.33m), the greatest disaster was The Adventures of Baron Munchhausen (Sony, NPV= −\$45.54m).

The case study estimates the value of potential sequels. On average, costs of sequels are 120% of the costs of a first film, according to Hypothesis 4 largely due to the higher wages after a successful first film. Box office proceeds are on average 70% of the first film, and not every successful film in the sense of a large positive NPV leads to a potentially profitable sequel. Hence, on average sequels are less profitable than first (success) films. There are exceptions: Batman 2 was more successful than the original movie! The detailed calibration of experimental parameters with the help of such data is documented in appendix 3.B. Based on the calibration we choose the parameters listed in table 3.6.[10]

On the basis of table 3.6 we can now be more specific about the hypothesis we formulated above (see section 3.3 above). We distinguish between four theoretical approaches:

- the game–theoretic solution with risk–neutral players (3) - (6) (abbreviated by GT)
- and risk–averse actors (GT–risk),
- the equity–theoretic solution (11) - (12), (ET) and
- the equity–theoretic solution (13) - (14), based on the total pie (ET–total).

Using the calibration above, we obtain the predictions in table 3.7.

3.5 Model Predictions

Clearly, given our calibrated parameters game–theory and equity theory provide quite different forecasts (see table 3.7). The two predictions for W_1 would be either -4.5 or 4.75, respectively. This means that they are 11% apart from each other in the total action space $[-10, 68]$ and that game theory would predict the actor to accept negative wage offers.[11] The two remaining theories relax the assumptions by allowing for risk averse actors (GT–risk) and compensation for W_1-offers below the equal split of the expected first stage payoff in case of a success (ET–total). They would predict the W_1-offer to fall in the range of $[-4.5, 2]$ and $[0.625, 8.875]$, respectively, resulting in an overlapping range of $[0.625, 2]$. As there is no uncertainty at stage 2 about the joint profit of 33, actors will offer only the outside option of 7 to the producer resulting in a wage (W_2)

of 26 for both predictions by game theory. Equity theory would predict the offer of the actor to be 16.5, or depending on the deviation $\omega\Delta$ from the equal split offer at stage 1, a compensation of Δ with $\omega = 0.25$ (ET–total).

A more practical matter is, of course, to explain the higher costs of the sequel project compared to first films. The cost increase of 20% of the sequel project relative to the first film, as documented by the case study (Luehrmann, 1992), can be explained by the higher wage costs of actors on the second stage. Thus if Rocky does it again, it is much more expensive to hire him. And this should imply an increase of total production costs of about 20% as reported in the case study.

The last column of table 3.7 presents the estimated cost increase $\left(sci = \frac{W_2+PC_2}{W_1^h+PC_1}\right)$ of a sequel predicted by the different theories where PC_1, PC_2 denotes the production costs, excluding the actor's wages, for the original firm and for the sequel, respectively. It is based on the calibrated parameters for costs beside the actors' wage, $PC_1 = PC_2 = 48$ (see appendix 3.B), and on the assumption that both, actor and producer, follow the same heuristic. The GT predictions forecast the cost increase to be 48% for (expecting) risk averse actors and up to 70% for risk neutral players. Thereby the cost increase is driven by the fact that risk neutral actors would agree on a much lower wage at the first stage whereas they have the same wage as risk averse actors at the second stage. A cost increase for the ET–total prediction depends on the first stage offers and can therefore only specify a range for the increase. If the W_1- offer is at the lower range of the interval specified by equation (3.13) and (3.14), then the implied cost increase would be 67 %. Clearly, if the producer offers a higher Wage at the first stage (so, a smaller Δ in equation (3.13)), then the cost increase for the sequel would be lower. With a cost increase of 22% for sequels the prediction of equity theory, allowing for moderate sharing of (expected) profits on both stages, comes closest to the reported 20% in the field (Luehrman).

3.6 Experimental Design and Procedure

The computerized experiment was conducted at the laboratory of Humboldt University Berlin in November and December 2001. The computer program was developed using the software z–tree (Fischbacher, 1999). 72 Participants –mainly students of business administration, economics and information technology– were recruited via E–mail and telephone. We ran six sessions, each consisting of two matching groups. To allow for learning, participants played 18 rounds of the two–stage bargaining game. Participants first read the instructions and were then privately informed about their role.[12] Roles were neutrally framed as "participant *A*" and "participant *B*" for the role of the actor and producer, respectively. Participants remained either *A* or *B* throughout the whole experiment. One matching group consisted of three negotiation groups each with one *A* and one *B* player. After every round new pairs were formed randomly.[13] We continue to refer to participants as "actors" and "producers," although the experimental subjects were not aware of this interpretation.

Information feedback was as follows: After the first bargaining stage participants were told whether *A* had accepted *B*'s offer. If the offer was accepted, they were informed about the randomly selected pie size and their first stage earnings. After the second stage participants were told whether *B* had accepted *A*'s offer and what they have earned on the second stage. At the end of each interaction participants also were informed about their own cumulative payoffs.

A session lasted on average 140 minutes. The exchange rate was DM 2 for one experimental currency unit (ECU).[14] Participants were paid their average payoff of all 18 which was on average DM 21. More precisely, producers received on average DM 25 with a minimum payment of DM 1 and a maximum of DM 71. Actors were paid DM 17 on average with minimum payments of DM 8 and maximum of DM 26. Additionally, participants were paid an initial endowment of DM 10 and DM 5 for completely answering the post experimental questionnaire.

3.7 Results

3.7.1 First and Second Stage Offers

At the first stage which involved negotiations about the stochastic pie size of either -10 (flop) or 68 (hit), we observe in total 648 take it or leave it $W_1 - -$offers. Table 3.8 and figures 3.3 and 3.4 report means and standard deviations as well as histograms of the offers, acceptances and rejections on both stages. At stage 1 the producer offered on average 0.8 to the actor. In 435 cases actors accepted the offer with a mean of 4.5. Then chance decided for 143 producer–actor–pairs that a "hit" was realized and subjects continued at the second stage.

The amount actors offer to the producer at the second stage (Π_2 of $C_2 = 33$) is on average close to the producer's outside option O_2^p of 7 with median $\Pi_2 - -$offer of 8 and 47% of all second stage offers being either 7 or 8. $\Pi_2 - -$offers below the producer's outside option O_2^p are rare (2.1%). Second stage offers were mainly accepted (85%) the average offer being $\Pi_2 = 8.9$ leaving $W_2 = 24.1$ for the actor. From the remaining 213 producer–actor–pairs the actor did not accept the $W_1 - -$offer with mean -6.6 and the round finished immediately after stage one by both parties receiving their outside option.

3.7.2 Contrasting Predictions

Accepted offers at the first stage seem to corroborate the equity prediction of $W_1 = 4.75$ (table 3.7) whereas offers at the second stage are very close to the game theoretic solution of $\Pi_2 = 7$ leaving $W_2 = 26$ to the actor. In the following we investigate how far the theoretical predictions are from observed behavior. The measure used is the mean squared error (MSE) of all observations (x_t^i) and the prediction ($\boldsymbol{x}_t$) for $x = W, \Pi$ and $t = 1, 2$, i.e.,

$$\frac{1}{N}\sum_{i=1}^{N}\left(x_t^i - \boldsymbol{x}_t\right)^2 \quad .$$

Table 3.9 lists the resulting offers on the first and second stage as well as corresponding MSE for all predictors.[15] ET, GT, and GT–risk refer to the predictions of equity theory, game theory, and game theory assuming risk averse actors, respectively. ET–total refers to equal splitting of the total expected profit $E\left(C_1^s + C_2\right)$, such that more generous wage offers at the first stage (low Δ) are compensated by higher producer profits at the second stage. 18% of all observations fall into the range of the GT–risk prediction with a mean offer of 0.51. For each of the 76% of observations which fall into the ET–total range we derive the corresponding theoretical second stage offer conditional on the first offer and compare it to the actual response. For Π_2 equity theory is less ambiguous. It is remarkable, how strong game theory is confirmed. The observed behavior suggests that opportunistic rationality becomes stronger when the situation becomes simple (deterministic and non–dynamic). Even though ET–total seems to explain the data much better then pure equal split, behavior predicted by the game theoretic solution for risk averse actors with the lowest total MSE seems to be most supported by the data.

A more detailed analysis of the offer frequencies and acceptance rates reveals negative offers are almost never accepted (2%), and non–negative offers below the outside option are rarely accepted (26%). The acceptance rate for offers above the outside option is with 97% much larger. In figure 3.5 a non–parametric estimate of the acceptance probability is shown as a function of the first stage offers. The concave relationship in the range of $[-4.5, 2]$ might portend that the assumption of risk neutrality is too restrictive. Also on the matching group level it is a robust result that actors do (not) accept offers above (below) their outside option O_1^A. Actors seem to prefer to "dishwash" rather than gamble (for fame). We can therefore summarize our results so far:

Regularity 1

(i) Assuming risk averse agents is the best supported prediction according to the MSE criteria. This suggests that unequal splits at the first stage are accepted if the producer is willing to bear the risk of a flop alone.

(ii) Producers frequently offer negative wages W_1 which, however, are almost never accepted; $W_1 - -$offers below A's outside option 2 are rarely accepted.

(iii) Actors are willing to accept less than the equal split but not less than their outside option.

(iv) Equity concerns seems to be rarely indicated by second stage offers. According to equity theory actors (over)compensate at the second stage for first stage inequality by (too) low second stage offers. Almost all second stage offers confirm the game theoretic W_2^*–prediction. Offers close to O_2^P are rather often accepted.

Risk aversion seems to explain the data on average very well. However, offers outside the interval $[-4.5, 2]$ cannot be rationalized at all. This means that risk aversion can account for 18% of the first stage offers. Relaxing the assumption of risk neutral producers would not expand the range of offers which we can explain. A risk neutral producer would offer at maximum $W_1 = 2$ what even the most risk averse agent should accept. Depending on the risk aversion of the producer, the minimum offer an actor might accept is $W_1 = -4.5$ (see equation (3.5) and table 3.7) if he would be risk neutral. Therefore, producers who offer below -4.5 do not want to become engaged in a joint project, which might be explained by risk aversion. In fact, 50% of all producers never offer a wage below this threshold. 25% of all producers place one third of their offers below -4.5.

ET–total might be a more meaningful interpretation than risk aversion as it captures 76% of first stage offers and 83% of all second stage offers. However, before we investigate equal split as an alternative explanation on the individual level, we will briefly summarize results one obtains when estimating risk aversion on an individual level for actors and the reactions of producers given the uncertainty about the risk aversion of their bargaining partner.

3.7.3 Risk Aversion

Actors First we try to make inferences about actors' risk aversion from the offers rejected and accepted. Here we assume that actors behave rational over all 18 periods and infer individual risk preferences from their choices. However, for many subjects in our experiment the results are not informative.[16] We are left with only 15 out of 36 experimental subjects with usable results for estimating risk aversion. Estimating risk aversion[17] $\boldsymbol{\rho}\left(\hat{W}_1\right)$ by the highest rejected offer we obtain individual risk parameters in the range $[.69, 7.13]$. Assuming that the acceptance threshold lies in the middle of the interval of the highest rejected and the lowest accepted offer, we can estimate $\hat{W}_1$ by averaging the highest rejected and the lowest accepted offer. Then we obtain a larger range of risk parameters $[.21, 26.17]$.

Uncertainty about risk–aversion We model the uncertainty about actors' risk aversion by choosing a parametric family of probability functions $F\left(\hat{W}\right) = \left(\frac{\hat{W}-\underline{W}}{\overline{W}-\underline{W}}\right)^{\gamma+1}$ with $\underline{W} = -4.5$ and $\overline{W} = 2$ in (3.10) in section 3.2 above. We apply two ways to estimate the parameter γ. Our first approach is directly from the uses the arithmetic mean of all offers in the range $[-4.5, 2]$. Our second approach uses additionally the information of answers to those offers and applies maximum likelihood estimation. Details are explained in appendix 3.D. The parameter estimate for γ is 0.34 for approach 1 and 2.70 for approach 2. This result seems to indicate that producers might underestimate actors' risk aversion.

Nevertheless, those results should be interpreted cautiously as for the estimation of the risk aversion parameter only 44% of the subjects in the actor position could be analyzed. The decisions of all remaining subjects were not informative: they do not satisfy the requirement that the highest rejected offer is not higher than the lowest accepted offer. Also the estimation of the γ–parameter of the threshold density function cannot account for all data. It considers only offers in the interval $[-4.5, 2]$ which comprises 18% of first stage offers.

3.7.4 Equity Preferences

We will now explore how well equity theory can explain the data. From equation(3.13) we know that the deviation from the ET prediction at the first stage is $\omega\Delta = E\left(C_1^s\right)/2 - W_1$ which should be equal to the deviation at the second stage weighted by the probability to reach the second stage: $\frac{\omega\Delta}{\omega} = \Delta = C_2/2 - \Pi_2$. If behavior is guided by equity principles, then the ratio of stage–wise deviations from equity, $\omega\Delta$ and Δ, should be $\frac{\omega\Delta}{\Delta} = \omega$. Figure 3.6 plots the density of this ratio for all second stage offers and additionally in a separate graph the 118 cases satisfying the ET–total at the first stage. The mode of the ratio density is close to the commonly known probability $(\omega = 25\%)$ of reaching the second stage. The skewed density with more mass on lower values indicates that actors try to overcompensate the "losses" at the first stage in a self–serving way. However, this impact is not significant. Actors do not earn more than producers even if they are offered a first stage wage in the ET–total range. In only 3 out of 12 sessions average earnings of actors are higher than average earnings of producers.

Comparison of competing hypotheses purely on the basis of mean squared errors appears somewhat problematic. Such statistical approach evaluates a theory only by comparing how its prediction fits the data *on average*. Statistical criteria like mean squared errors neglect the fact that we have a significant number of observations that cannot be rationalized on the basis of game theoretic predictions at all.

Regularity 2 Allowing for risk aversion of actors only moderately improves the predictive success of the game theoretic solution. Equity considerations with the possibility of compensation for the actor seem to explain more of the observed behavior.

3.7.5 Reciprocity

It is interesting that despite the result from equity considerations actors seem not to react with their offer at the second stage to the producer's wage offer. Regressing Π_2 on W_1 indicates a constant second stage offer around 9 and no reaction towards the offer at first stage.[18] It could be that actors react in heterogeneous ways. We will now investigate how individual actors reciprocate in view of hypothesis 2(ii) how the offer Π_2 depends on the accepted offer W_1. Second stage offers conditional on first stage offers indicates three different types of behavior:

- no reaction regardless of the first stage offer,

- reciprocity reacting to to high (low) first stage offers by a increase (decrease) of second stage offers, and
- idiosyncratic reaction.

We partition 34 actors (for which Π_2–choices range from 2 to 7) into three subgroups:[19]

- 6 participants of a constant type (with no variation of Π_2) who all offer either O_2^P or the equal share (**Opportunistic/Fair Proposers**),
- 9 reciprocal participants (who respond in kind, i.e., react positively with Π_2 to W_1)[20] (**Linear Reciprocators**), and
- 19 participants, who neither relied on the same Π_2 nor reciprocated (in the above sense) (**Experimenters**, who try out different offers Π_2 in idiosyncratic ways).

Four of the first type actors behave rather opportunistically after a hit by essentially offering producers their outside option. Only 2 of these actors can be regarded as equity minded with constant mean Π_2–offers of 16 and 14. Reciprocators are actor types in line with hypothesis 2(ii).

The behavior of the 'experimenting' actors can partly be explained by directional learning. Directional learning (see, for instance, Selten and Buchta, 1998) predicts the direction of changing one's strategy by adapting it in the direction suggested by an ex post–analysis of past choices. For an actor reaching the second stage directional learning theory would predict that if his offer was rejected last time it will be increased next time. Similarly, in case of an accepted offer last time one should not increase the offer (or keep constant). 92% of all 'experimentator'–offers confirm directional learning (43% are constant offers mainly at 7.5 or 8, i.e., when the producer's outside option has been reached). Only 8% of the 'experimentator'–offers contradict directional learning.

Regularity 3 There is no support for general reciprocation by actors but we can distinguish different types of behavior amongst actor subjects: opportunistic/fair proposers, linear reciprocators, and adjusting in an experimental manner.

Considering different types of behavior by actors might already give some insights in sequels' costs increases. For opportunistic/fair actors the final picture of an increase in costs depends on the first stage offer. If it is low, the cost increase via an indirect wage claim is high, if it is high, the cost increase seems to be lower. This picture is amplified for reciprocators who compensate (or reciprocate) low first stage offers by high second stage claims and therefore higher costs for the producer. Only for the last group of subjects, experimenters, the cost increase of a sequel remains ambiguous.

3.7.6 Explaining Sequels Cost Increase

We will now investigate how much our experimental sequel would increase in costs. In section 3.5 we discuss the implications for the cost increase of a sequel by the different theories (see also table 3.7). Similarly to the field we consider for this analysis only movies which were produced and lead to a sequel, i.e., 143 observations where the first stage offer has been accepted and chance continued to the second stage. Sequels in our experiment lay with 37% above the ET prediction of 22% and very close to the ET–total prediction but below game theoretic prediction of 70% for risk neutral, or 48% for risk averse actors.[21]

If our calibrated scenario captures the crucial aspects of the field then in the experiment our actor subjects seemed to be slightly more greedy than real life actors. Nevertheless, as we model wage as the only source of a cost increase experimental subjects are much less greedy than game theory would predict. Of course, real life actors may not be able to anticipate the prospects of a movie as well as the participants in our experiment. In reality the process of negotiating will be also more complex. However, a more complex model would have more parameters that could not be identified. Of course, actors may also be offered incentive contracts (although there is no moral hazard problem).

Regularity 4 If wages account for a substantial part of a sequel's cost increase then the stylized facts seem to be rather close to equity predictions than to pure strategic considerations. According to our experimental data the cost increase of the sequel can be explained by more ambitious demands of the core actor as expected.

3.8 Discussion

Our experiment has been inspired by a field study (Luehrmann, 1992) to which we refer as the sequel project. A producer and an actor negotiate how to share the uncertain proceeds from a first movie and in case of a sequel the profits of the second movie in an alternating offer–way. Other related experiments[22] did not include such dramatic risks which seem crucial for the movie industry. In our view, these qualitative and quantitative differences to former experiments are too dramatic to expect similar results as in previous studies (Güth and Tietz, 1990, for instance, report much lower conflict rates in their review).

Another innovative aspect is that we rely on calibrated parameters. Our experiment which uses parameters calibrated from the field study, should imply more reliable insights and should avoid the missing parallelism of usual experiments. Actually, the data of the sequel project suggest such extreme parameters that we first were reluctant to use them. In hindsight we consider our results, however, as rather encouraging: Although "movie production" is risky, even in the laboratory there is "movie production" as some experimental subjects in the role of producers are willing to take on risks.

Moreover, according to our data producers either have to become the only risk taker or have no joint venture at all. Risk–aversion can partly account for actors' behavior. Often joint ventures fail since producers underestimate actors' acceptance threshold. Reciprocity ideas seem to explain other aspects of observed behavior, although some actors behave rather opportunistically. More generally, we could distinguish three types of actor behavior, namely, constant, reciprocal and experimenter the latter adjusting in a learning direction–mode. Altogether there seems to be some variety in what motivates behavior in such complex and risky joint ventures.

Our model and the experimental test "explain" an important aspect of the case study, namely the cost increase of sequels. In their risky joint venture producer and actor could share expected profits rather than only those resulting from a hit. Equity theory would predict a cost increase of a sequel by 22% or 35% depending on its specification for the model at hand. In the experiment actors do not just enjoy their "fame" after a hit, but also want to be compensated for their low payoff from the first film.

Appendix to Chapter 3

3.A The Stage Form of the Game Model

The extensive form of the game is therefore:

1. P offers a wage–contract to A that specifies a fixed wage W_1 for A and splits the uncertain gain from producing the original movie.

2. A can accept or reject. If A rejects, both parties receive their outside payoff and the game ends. If A accepts, the original movie is produced and the game continues.

3. Nature determines the success state s of the movie. Both parties receive a payoff dependent on the success of the movie according to their contract. If the movie is a flop, the game ends. If the movie is a hit, the game continues.

4. A offers P a contract that specifies a fixed wage for A and a fixed profit for P for producing a sequel to the original movie.

5. P can accept or reject this contract. If P rejects, both parties receive an additional payoff dependent on their outside opportunities and the game ends. If P accepts, the sequel is produced with gains from production C_2 that are split according to the contract and the game ends.

3.B Parameter Calibration

Calibrating Model Parameters. We estimate the profitability of sequels (in present value terms) estimating NPVs on the basis of projected revenues and costs. Note that the calculations are similar to those above, but for the first films we used actual data, whereas we use projected profitability for sequels based on the stylized facts reported above. Hence, this procedure reflects the expected and not the actual profitability of sequels. For example, it would never predict that a sequel is more profitable than its first film (like Batman 2). Also, while no studio would ever make a sequel with a negative NPV, sequels can turn out to make losses even after a successful first film. ("Look who is Talking 2" was a disaster.) We can then estimate the value of a sequel right, that is the economic value of the right of the movie studio to produce a sequel

Studio	Profitable Sequels	Value of sequel right	Sequel/First film
MCA Universal	9	\$6.69	30%
Paramount	3	\$2.68	32%
Sony	4	\$2.89	35%
20^{th} Century Fox	2	\$1.78	30%
Warner Brothers	3	\$7.33	42%
Disney	5	\$10.29	36%
Total/Average	26	\$4.96	34%

Table 3.1: Values of sequels

Parameter	Symbol	Value
Probability of hit	ω	0.25
Profit of hit	Π_1^h	66
Profit of flop	Π_1^f	-12
Exp. profit of sequel	Π_2	20

Table 3.2: Parameters

after observing the success of the first film. While only a small number of first film gives rise to profitable sequels, the movie studio does not have to produce sequels to flops. Table 3.1 gives the relevant data.

Hence, based on this model we would project that of 99 films, 26 would generate profitable sequels. Note that even Sony, which had a negative profit for its first films, would have expected positive profits for its sequels, since it would only make sequels of 4 of its 34 films. These data are volatile and can be driven by a small number of outliers. In the case of Sony, a large fraction of projected sequel profits comes from the successful “Look who is talking,” that generates about 80% of its projected sequel profits.[23] For our purposes, we now define a “hit” as a film that could give rise to a profitable sequel, hence our hit rate here would be 26/99 or 26.3%. Note that this hit rate probably overestimates the likelihood of a sequel being made, since it includes some movies where the script of the first movie would hardly give rise to a sequel (e.g., “Driving Miss Daisy”).

We reduce the empirical distribution of movies to a binary distribution as follows.

A film in our model is either a "hit" and produces a payoff of Π_1^h, or a "flop" with a payoff of Π_1^f, where $\Pi_1^h > \Pi_1^f$. A film is a hit with probability ω, hence the expected profitability of a film is:

$$\mu = \omega\Pi_1^h + (1-\omega)\,\Pi_1^f \quad . \tag{3.16}$$

The standard deviation of the binary distribution is:

$$\sigma = \left(\Pi_1^h - \Pi_1^f\right)\sqrt{\omega\,(1-\omega)} \quad . \tag{3.17}$$

The value of a sequel after a successful first film is denoted by Π_2, hence the value of the *sequel right* is $\omega\Pi_2$. We chose the parameters in table 3.2.

Parameter	Symbol	Value	Data	Error
Prob. of hit	ω	0.25	0.263	−4.8%
Expected profit	μ	\$7.50m	\$7.44	0.8%
Std. dev.	σ	\$33.77m	\$34.16	−1.1%
Exp. prof. of sequel	Π_2	\$20.00m	\$18.88	−5.6%
Sequel/first film	Π_2/Π_1^h	30%	34.1%	12.4%
Sequel right	$\omega\Pi_2$	\$5.00m	\$4.96	−0.8%

Table 3.3: Error statistics

Table 3.3 compares the actual values in the data, the calibrated values, and the errors between actual and calibrated values. The calibration captures the mean and standard deviation of the data very accurately. The profitability of the sequel and the value of a sequel right is also captured. The typical ratio of the expected profitability of a sequel to a successful first film is 30% for the model values, and 34.1% in the sample.

Calibrating Sequel Costs. With the calibrated parameters we adjust the values of the experiment the following way: If the company produces the movie it earns the revenue R and has to bear production costs, consisting of the actor's wages W and remaining production costs PC. The producer's profits Π_1 in the first stage for the "hit" (Π_1^h) and for the "flop" (Π_1^f) as well as profit for a sequel Π_2 can be written as:

$$\Pi_i^k = R_i^k - (W_i + PC_i)\,,\ \text{for } i=1, k\in\{f,h\}\,, \text{and } i=2 \text{ (without } k). \tag{3.18}$$

For calibrating R_2 we use the stylized facts as in the case study for the relation of the revenues of a successful film to a sequel, namely

$$R_2 \approx \frac{7}{10}R_1^h. \tag{3.19}$$

Parameter	Symbol	Value
Profit: hit	Π_1^h	66
Profit: flop	Π_1^f	−12
Profit: sequel	Π_2	20
Revenue: hit	R_1^h	116
Revenue: flop	R_1^f	38
Revenue: sequel	R_2	81
Additional costs hit/flop	PC_1	48
Additional costs sequel	PC_2	48
Wage costs hit/flop	W_1	2
Pie in case of a hit	C_1^h	68
Pie in case of a flop	C_1^f	−10
Pie in case of the sequel	C_2	33
Outside option actor both stages	O^A	2
Outside option producer both stages	O^P	7

Table 3.4: Experimental parameters

Furthermore, we assume that the additional production costs are the same in the film and its sequel, $PC_1 = PC_2$. With this system of equations and the calibrated values of $\Pi_1^h = 66$ (in case of a "hit"), of $\Pi_1^f = -12$ (in case of a "flop"), and $\Pi_2 = 20$ we chose the parameters according to the game with one modification as follows. Neither the field study nor our experimental data, give any evidence for W_1 below the outside option. That is why we prefer the calibration of W_1 as $W_1 = O_1^A = 2$ to $W_1 = O_1^A - \omega W_2^* = -4.5$, which also matches the case study in that the relation of total wage costs to cumulative costs (so called "negative costs" plus distribution expenses) is approximately one to five for a typical film, i.e., $\frac{1}{4}PC_1 > W_1$.

The actor and the producer negotiate about the remaining surplus, $C^j = \Pi_1^j + O_1^A = R_1^j - PC_1$, $j \in \{l, h\}$ before the movie is going to be produced. The two possible pie sizes are therefore $C_1^h = 68$ and $C_1^f = -10$ for the hit and the flop movie, respectively. In case of a successful first movie the actor and producer negotiate about the remaining share of the sequel's revenue which is $C_2 = R_2 - PC_2 = \frac{7}{10}R_1 - PC_2 = 33$, according to equation (3.19) and the assumption $PC_1 = PC_2$. In order to keep the whole game simple both players' outside options are kept constant at both stages, i.e., $O_1^A = O_2^A = 2$ and $O_1^P = O_2^P = 7$. Table 3.4 displays the calibrated parameters.[24]

3.C Parametric Example

Actors Assume actors have outside wealth W_0 and constant relative risk aversion (CRRA) with parameter ρ. Then

$$U(W_1) = \frac{(W_0 + W_1)^{1-\rho}}{1-\rho} \quad . \tag{3.20}$$

This expression can be used directly in (3.7) and solved for $\hat{W}_1$ (at least numerically) in terms of the parameters of the model.

Producers Define the lower and upper bound of the interval (3.8) by $\underline{W}$ and $\overline{W}$ respectively:

$$\underline{W} = O_1^A - \omega\left(C_2 - O_2^P\right) \quad , \tag{3.21}$$

$$\overline{W} = O_1^A \quad . \tag{3.22}$$

Then choose the following parametric family of distribution functions:

$$F(W_1) = \left(\frac{W_1 - \underline{W}}{\overline{W} - \underline{W}}\right)^{\gamma+1} \text{ with } \gamma \in [-1, \infty], \tag{3.23}$$

which have density

$$f(W_1) = \frac{(\gamma+1)(W_1 - \underline{W})^{\gamma}}{\left(\overline{W} - \underline{W}\right)^{\gamma+1}} \tag{3.24}$$

so that the second order condition becomes

$$\gamma(W_1 - \underline{W}) > 2(\gamma + 1) \quad . \tag{3.25}$$

Note that for $\gamma(W_1 - \underline{W}) > 2(\gamma+1)$ this family of distribution functions is sufficiently flexible for our example. For $\gamma = -1$ we obtain the uniform distribution, for $-1 < \gamma < 0$ we obtain distribution functions with the probability mass shifted to the left, and for $\gamma > 0$ we obtain distributions with the probability mass shifted to the right. Substituting these into the example above and solving (3.10) gives:

$$W_1^* = \min\left\{\overline{W}, \frac{\gamma+1}{\gamma+2}\left(E\left(C_1^s + O_2^P\right) - O_1^P\right) + \frac{1}{\gamma+2}\underline{W}\right\} \quad . \tag{3.26}$$

We have to guarantee that the solution lies in the interval (3.8), so the Min-operator makes sure that the expression does not exceed the upper bound $\overline{W}$. Hence, for interior solutions W_1^* is a weighted average of the minimum $\underline{W}$ (the reservation wage for a risk-neutral actor) and the producer's maximum willingness to pay, $E\left(C_1^s + O_2^P\right) - O_1^P$. Paying this amount would reduce the producer's expected payoff to his outside option. The solution is intuitive. Observe that

$$\frac{\partial W_1^*}{\partial \gamma} = \frac{E\left(C_1^s + O_2^P\right) - O_1^P - \underline{W}}{(\gamma+2)^2} > 2 \tag{3.27}$$

for all solutions. Hence, a distribution that assigns higher probabilities to higher reservation wages also leads to higher equilibrium wage offers. Note also that:

$$\lim_{\gamma \to \infty} W_1^* = \min\left\{\overline{W}, E\left(C_1^s + O_2^P\right) - O_1^P\right\} = \overline{W} \tag{3.28}$$

$$\lim_{\gamma \to -1} W_1^* = \underline{W} \tag{3.29}$$

Here, the first result follows from the definition of (3.21) and (3.5). Hence, if we choose γ small enough, then the probability distribution degenerates and all probability mass is put on the event where the actor is risk-neutral ($W_1^* = \underline{W}$ for all $\gamma + 1 < 0$). Hence, for $\gamma = -1$ we recover the original problem and the solution (3.5), (3.6). Conversely, for large γ, all actors are deemed to be infinitely risk averse and judge the payoffs from the maximin criterion, so $\hat{W}_1 = O_1^A$ ($W_1^* = \overline{W}$ for $\gamma + 1 > \frac{\overline{W} - \underline{W}}{E\left(C_1^s + O_2^P\right) - O_1^P - \overline{W}}$).

Equation (3.26) extends our game theoretic solution to risk averse actors. The importance of (3.26) lies in the fact that we can always find a probability distribution characterized by some parameter γ that would rationalize the behavior of producers as an outcome of this game, where producers are uncertain about the actor's reservation utility. Conversely, offers outside the interval (3.8) cannot be rationalized at all.

3.D Modelling Uncertainty about Risk-Aversion

We model the uncertainty about actors' risk aversion by choosing a parametric family of probability functions $F\left(\hat{W}\right) = \left(\frac{\hat{W} - \underline{W}}{\overline{W} - \underline{W}}\right)^{\gamma+1}$ in (3.10) in section 3.2 above. We apply two ways to estimate γ. Our first approach uses the arithmetic mean of all offers in the range $[-4.5, 2]$. In appendix 3.C we showed that (3.10) then becomes:

$$W_1^* = \min\left\{\overline{W},\ \frac{\gamma+1}{\gamma+2}\left(E\left(C_1^s + O_2^P\right) - O_1^P\right) + \frac{1}{\gamma+2}\underline{W}\right\} \quad . \tag{3.30}$$

We can calculate γ with the offers observed. For this, we insert the experimental parameters and the mean offer in equation (3.30):

$$E\left(C_1^s + O_2^P\right) - O_1^P = \frac{17}{4}$$
$$\underline{W} = O_1^A - \omega\left(C_2 - O_2^P\right) = -\frac{9}{2}$$

Then equation (3.30) reads:

$$W_1^* = \min\left\{2,\ \frac{\gamma+1}{\gamma+2}\left(\frac{17}{4}\right) + \frac{1}{\gamma+2}\left(-\frac{9}{2}\right)\right\} \quad . \tag{3.31}$$

with γ as the only unknown parameter. The mean (median) offer in the range $[-4.5, 2]$ is 0.52 (0.00) and yields $\gamma = 0.34$ (0.06) from direct substitution into (3.30).

Our second approach to estimate γ is maximum likelihood estimation. We assume that the first stage offer W_1 is accepted ($a = 1$) when the threshold parameter $\hat{W}$ is reached, i.e.,

$$a = \begin{cases} 1 & \text{if } W_1 \geq \hat{W}, \\ 0 & \text{if } W_1 < \hat{W}. \end{cases}$$

hence, the probability of accepting W_1 is

$$\Pr(a = 1) = \Pr\left(W_1 \geq \hat{W}\right) = F(W_1).$$

We assume that the unknown threshold parameter $\hat{W}$ follows the distribution $F\left(\hat{W}\right) = \left(\frac{\hat{W}-\underline{W}}{\overline{W}-\underline{W}}\right)^{\gamma+1}$, with $\underline{W} = -4.5$ and $\overline{W} = 2$. In figure 3.1 the log–likelihood function

$$l(\gamma|W_1) = \sum_{i=1}^{N}\left(a_i \cdot \log\left(\frac{W_{1i} - \underline{W}}{\overline{W} - \underline{W}}\right)^{\gamma+1} + (1 - a_i)\log\left(1 - \left(\frac{W_{1i} - \underline{W}}{\overline{W} - \underline{W}}\right)^{\gamma+1}\right)\right) \tag{3.32}$$

is drawn for the observed W_1, answers, and different values of γ between -1 and 25.[25] The log-likelihood function is maximized for $\gamma = 2.7$. Figure 3.2 plots the nonparametric estimation and the distribution function for both estimates of γ.

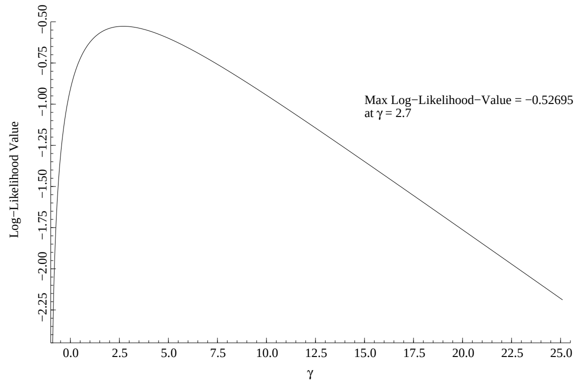

Figure 3.1: Log-likelihood function $l\left(\gamma|W_1\right)$ for different $\gamma-$values

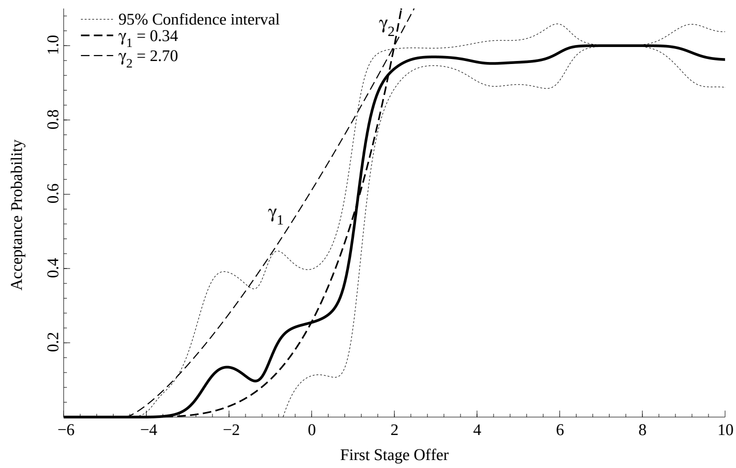

Figure 3.2: The figure compares two parametric approaches to a non-parametric approach to estimating the acceptance probability. γ_1 is a moment estimator based on the average offer; γ_2 is the maximum likelihood estimate. The acceptance probabilities $F(\hat{W}) = \left(\frac{\hat{W}-\underline{W}}{\overline{W}-\underline{W}}\right)^{\gamma_i+1}$ are compared to a nonparametric estimate.

3.E Instructions (Translation)

The experiment was conducted in German and the original experimental instructions were also in German. This is a shortened[26] *translated version of the instructions. Participants read the paper instructions before the computerized experiment started. In the beginning, subjects were informed that the instructions are the same for every participant, they receive an initial endowment of DM 10, that the payoff is according to the average earnings – wins and losses from all periods would be added, the exchange rate from ECU (Experimental Currency Unit) to DM: ECU 1 = DM 2, that communication was not allowed and questions would be answered privately and that all decisions will be treated anonymously. Then the main instructions started. Before the programm started participants were informed that they will interact in this way 18 periods and that their bargaining partner is randomly selected after each period.*

Two parties, two persons A and B negotiate in each period about how to share up to two amounts of money (all in ECU). Whether you act as A or B is determined randomly at the beginning of the experiment. You will keep your role for the whole experiment. The schedule of the decision making is as follows:

First B offers an amount v_1 , with $-10 \leq v_1 \leq 68$, to A of a later randomly determined amount G_1. Then A decides whether he accepts or rejects offer v_1 of B.

$\Rightarrow$ In case of rejection you receive:
as A : 2 and
as B : 7.
The interaction is finished.

$\Rightarrow$ In case of acceptance you receive:
as A : v_1
as B : $G_1 - v_1$

If A accepted the offer v_1 the amount G_1 which is to be shared is determined randomly. Thereby with a probability of 75% the amount has the value of -10 and with probability 25% the value of 68. Please note, that $G_1 = -10$ causes a loss for player B.

If $G_1 = -10$ the interaction is finished.

Otherwise (after $G_1 = 68$) the interaction proceeds and A offers B a share v_2 , with $-10 \leq v_2 \leq 33$, about an additional amount G_2 of 33. Participant B decides whether he accepts or rejects the offer v_2 of A.

$\Rightarrow$ In case of rejection you receive additionally to the previous profit:
as A : 2 and
as B : 7.
The interaction is finished.

$\Rightarrow$ In case of acceptance you receive additionally to the previous profit:
as A : $G_2 - v_2$ $(= 33 - v_2)$
as B : v_2
The interaction is finished.

At the end you will be informed again about the decisions of your interaction partner and your corresponding payoffs. Please note, that losses are possible.

3.F Tables

Studio	Number of films	Positive NPV Films	Total NPV
MCA Universal	14	11	\$263.7
Paramount	10	5	\$25.7
Sony	34	8	−\$55.4
20^{th} Century Fox	11	5	\$23.2
Warner Brothers	19	7	\$233.1
Disney	11	6	\$246.2
Total	99	42	\$736.6

Table 3.5: Profitability of first films

Parameter	Symbol	Value
Probability of hit	ω	0.25
Pie in case of a hit	C_1^h	68
Pie in case of a flop	C_1^f	−10
Pie in case of the sequel	C_2	33
Outside option actor	$O_1^A = O_2^A$	2
Outside option producer	$O_1^P = O_2^P$	7

Table 3.6: Experimental parameters

Prediction	Equation	Acronym	Model Predictions		
			W_1	W_2	sequel cost increase *sci*
Game Theory (GT)	(3.3) - (3.6)	GT	-4.50	26.0	0.70
GT risk averse actors	(3.26) - (3.28)	GT–risk	$[-4.5, 2.00]$	26.0	$[0.48, 0.70]$
Equity Theory (ET)	(3.11) - (3.12)	ET	4.75	16.5	0.22
ET–total	(3.13) - (3.14)	ET–total	$4.75 - \omega\Delta$	$16.5 + \Delta$	$[-0.16, 0.67]$
			$\Delta \in [-16.5, 16.5]$		

Table 3.7: Predictions of game and equity theory

	Stage 1 offer (W_1)			Stage 2 offer (Π_2)		
	Nobs	Mean	Std.dev	Nobs	Mean	Std.dev
All	648	0.8	6.8	143	8.9	2.9
Accepted	435	4.5	4.1	121	9.3	2.3
Not accepted	213	-6.6	4.8	22	6.6	4.4

Table 3.8: Offers: number of observations, mean and standard deviation

Prediction for W_1 (N = 519)	$\boldsymbol{W_1}$	MSE
GT	-4.50	85
GT–risk (N = 92)	$[-4.5, 2.00]$	19
ET	4.75	22
ET–total (N = 393)	$\omega\Delta = 1.26$	8

Prediction for Π_2 (N = 143)	$\boldsymbol{\Pi_2}$	MSE
GT = GT–risk	7.00	12
ET	16.50	66
ET–total (N = 118)	$16.50 - \Delta$	49

Table 3.9: Predictions according to the calibrated parameters and MSE of the actual data

3.G Figures

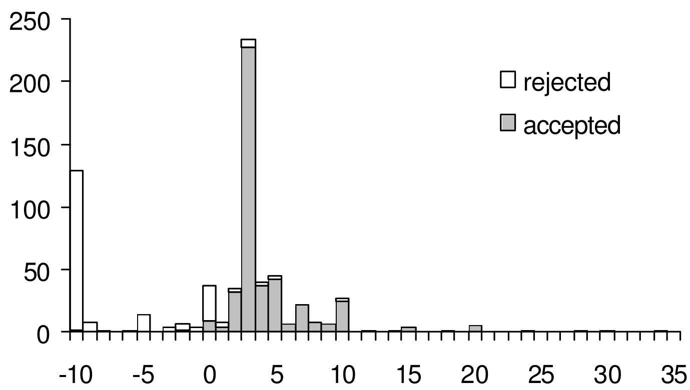

Figure 3.3: Frequencies and acceptance/rejection of stage 1 offers (N=648)

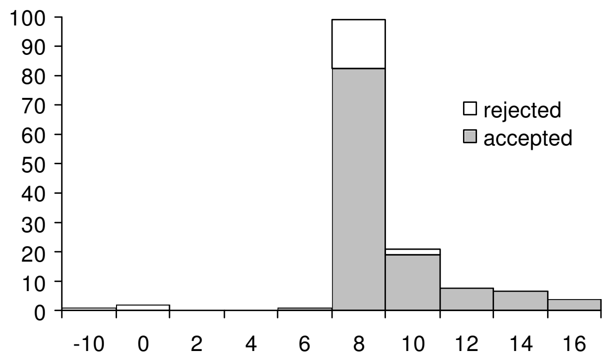

Figure 3.4: Frequencies and acceptance/rejection of stage 2 offers (N=143)

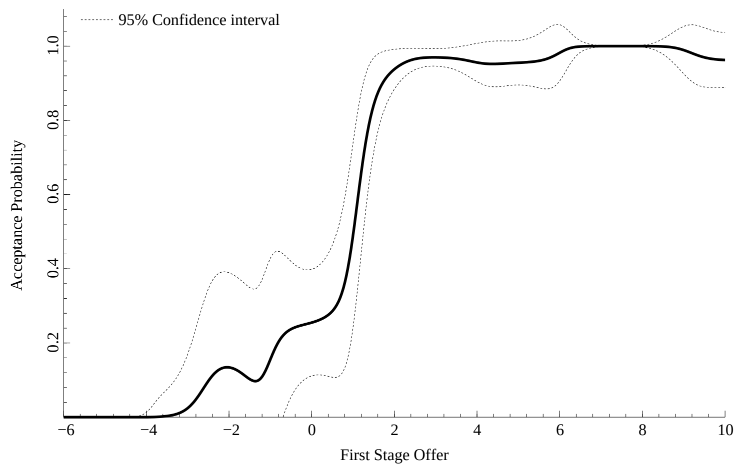

Figure 3.5: Acceptance Probability of first stage offers

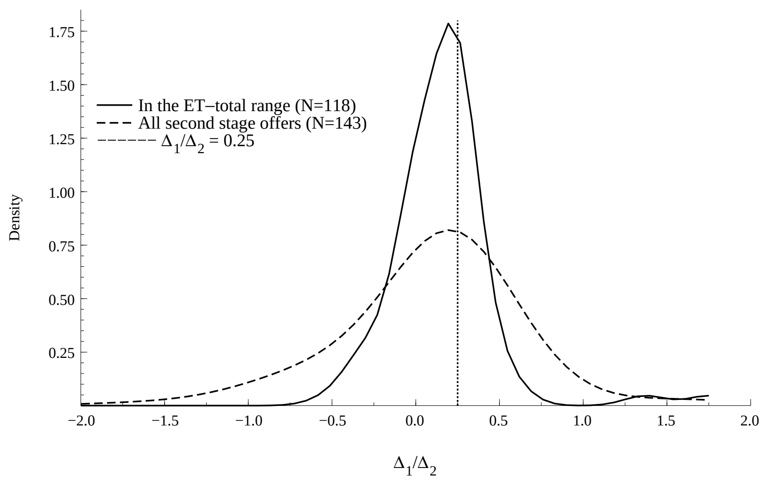

Figure 3.6: Ratio of first and second stage deviations from equity theory, N=118

Notes

[1] A notable exception are the James Bond–movies that led to a remarkable number of sequels, albeit with different actors.

[2] Venture capital firms finance their portfolio firms in stages. At each stage, the venture capitalist either negotiates another round of financing or refuses further financing and terminates the relationship. See Gompers (1995).

[3] See the original experiments of Binmore, Shaked, and Sutton (1985) without stochastic uncertainty and at most one contract.

[4] Our calibrations and the data for our study are based on a case study (Luehrmann, 1992) that contains data on 99 movies in the 1989–season and some additional data on the profitability of sequels, based on 60 sequels produced between 1970 and 1990. Luehrmann bases his data on Variety Magazine and some other industry sources.

[5] See Holström (1979) and Grossman and Hart (1983) for the traditional argument for output-contingent contracts. See Güth and Maug (2002) for an example of a principal-agent model with effort–incentives where pay is fixed.

[6] The second order condition for payoff maximization is $f'\left(W_1^*\right) F\left(W_1^*\right) > 2\left(f\left(W_1^*\right)\right)^2$.

[7] See Güth (1995) and Roth (1995) for surveys.

[8] See Güth (1988) for an attempt to add specificity to this concept.

[9] The discount rate of 12 % is suggested by the case writer.

[10] The full calibration results for the parameters are listed in table 3.4 in appendix 3.B.

[11] A negative first stage wage might be a reasonable result as unknown actors might become engaged in rather costly actions to get the chance of their life and become a movie star.

[12] See appendix 3.E for translated instructions.

[13] Rematching was restricted to matching groups. Participants were not informed about the restriction of rematching within matching groups what should have further discouraged repeated–game effects.

[14] DM 1 $\approx$ EUR 0.51.

[15] Producers had no chance to decline from negotiation. The only opportunity to drop out of the negotiation was to offer a first stage wage at the lower boundary of the offer space ($W_1 = -10$). Those offers which comprise 20% of the first stage offers would bias the evaluation of different predictions, we exclude them from the MSE analysis.

[16] In total we excluded 21 subjects from the analysis for one of the following reasons: (1.) subjects rejected offers of $W_1 = 2$ and higher, which is inconsistent with any interpretation based on risk–aversion, (2.) the highest offer rejected was smaller than the lower bound $\underline{W} = -4.5$, (3.) the lowest accepted offer was higher than the highest offer rejected.

[17] We estimate risk aversion by stipulating that $W_0 = 20$ (approximately equal to average experimental earnings) and solve equation(3.20) in appendix 3.C for $\hat{W}_1$.

[18] Regressing Π_2 on W_1 $\left(\Pi_2^i = \alpha_0 + \alpha_1 \cdot W_1^i + \varepsilon^i\text{, with } \alpha_1 > 0\right)$ gives $\hat{\alpha}_0 = 9.3\ (0.4), \hat{\alpha}_1 = -0.09\ (0.07)$ for the estimates with standard errors in parenthesis and $R^2 = 0.01$. Whereas strict ET–total would have predicted the parameter estimates to be $\alpha_0 = 0.625\ , \alpha_1 = 0.25$.

[19] There is a total of 36 actors. Two participants reacted only once at the second stage or received and offered the same amount in both stages. Therefore, we could not classify them.

[20] A linear regression $\left(\Pi_2^i = \alpha_0 + \alpha_1 \cdot W_1^i + \varepsilon^i\text{, with } \alpha_1 > 0\right)$ for those participants results in $\alpha_0 = 6.9\ (0.2), \alpha_1 = .41\ (0.04)$ for the estimates with standard errors in parenthesis and $R^2 = 0.80$.

[21] The mean of all offers supporting ET–total is $W_1 = 3.49$, so $\omega\Delta = 1.26$. The theoretically corresponding mean offer at the second stage would be $\Pi_2 = 21.5$ if subjects' behavior follows the ET–total prediction. Given first and corresponding second stage offer, a sequel would therefore result in a cost increase of 35%. It is not surprising that the observed data is close to this prediction. Equity preferences have been shown to bee supported by the data for the relation of first and second stage offers. Together with the fact that the cost increase for this prediction is based on the average of the observed first stage offers makes this point clear.

[22] See Roth (1995) for a survey of simpler experiments.

[23]Two sequels to this film were made, but their economic success was far lower than expected on the basis of the first film.

[24]One could assume that the outside option O_2^A of a movie star is much larger than before becoming famous, so $O_2^A > O_1^A$. Manipulating the second stage outside option of the actor would allow to draw conclusions whether an increase in the sequel's costs might be explained by a raise in the movie star's real outside option. However in light of the results of the experiment a treatment with increased second stage outside options became obsolete.

[25]The likelihood function is $L\left(\gamma\,|W_{11}...W_{1N}\right) = \prod_{i=1}^{i=N} F\left(W_{1i}\right)^{a_i}\left(1 - F\left(W_{1i}\right)\right)^{1-a_i}$. Substituting for $F(\hat{W})$ and taking logs gives (3.32).

[26]The complete German instructions are available at request.

Chapter 4

Durable–Goods Monopoly with Privately Known Impatience

4.1 Introduction

Eversince Plato (1941)[1] people seem to be aware that they may suffer from rational anticipation of own future behavior.[2] A very prominent intra–personal decision conflict is one faced by a durable–goods monopolist (Coase, 1972). In a market with a durable good, a monopolistic seller could easily collect the monopoly profit by excluding any future price cut. Buyers will, however, anticipate that future prices are opportunistically chosen by the monopolist; in particular, that the good will be sold cheaper in later periods. For this reason, the monopolist loses market power.[3] Coase conjectured that this can even lead to competitive and thus efficient market results.

Much of the literature on durable–goods monopoly has focused on the question under which conditions the Coase conjecture proves to hold and under which conditions it does not hold. For example, Stokey (1981) and Gul, Sonnenschein, and Wilson (1986) show that there is an equilibrium in which the price is (arbitrarily) close to marginal cost if the number of successive sales periods is infinitely high. Others have shown that product durability does not necessarily reduce the monopolist's market power (Ausubel and Deneckere, 1989; Bagnoli, Salant, and Swierzbinski, 1989). Güth and Ritzberger (1998) show that a durable–goods monopolist may even increase profits above monopoly level when the model allows for a difference between the discount factor of the monopolist and that of the potential buyers. Under this assumption, Güth

[0]We thank Tim Grebe for his help by running the experiment. We gratefully acknowledge the constructive comments by Pio Baake, Margrethe Aanesen, and Jan Potters. The results in this chapter were first formulated in Güth, Kröger, and Norman (forthcoming), "Durable–Goods Monopoly with Privately Known Impatience," Economic Inquiry.

and Ritzberger show that even over a finite number of periods the monopolist may significantly increase market power, provided the buyer has a lower discount factor. This is the so–called Pacman Conjecture (Bagnoli, Salant, and Swierzbinski). If the seller has a lower discount factor, he loses profits compared to a one–period monopolist.

Insights from durable–goods markets also help to understand other markets and their dynamics. First of all, quite many products can exhibit characteristics of durable goods as they yield a flow of services to the owner over a significant long time. Several products are traded on second hand markets so that durability or planned obsolescence are of great importance. Empirical studies have focused on optimal durability and the presence of second hand markets (Swan, 1985). Another interesting aspect is that the concept of durable–goods captures behavioral aspects of time preferences (see Hausman, 1979, who investigates the relation between discount factors of consumers and purchase of goods). Nevertheless, not many empirical studies seem to exist which deal with durable–goods monopolies (Suslow, 1986, who investigates Alcoa's aluminium pricing seems to be an exception). One reason being that it seems to be rather difficult to distinguish the relevant differences in assumptions in the field data. Hence, empirical evidence relies mainly on experimental studies (Güth, Ockenfels, and Ritzberger, 1995, Reynolds, 2000, Cason and Sharma, 2001) to actually investigate the predictive power of the theory.

Models taking time preferences of agents into account usually assume that players have identical discount factors. However, there is ample evidence that discount factors may be highly idiosyncratic in social environments.[4] In this chapter, we follow Güth and Ritzberger in their less restrictive approach and allow for heterogeneous discount factors. In addition, we assume that discount factors are privately known. Commonly known impatience of players seems unlikely—at least, it requires further justification. How eager sellers and buyers are to obtain monetary rewards over time is presumably difficult to observe for others. So the assumption of privately known discount factors seems less restrictive. More specifically, we assume that discount factors can be either high or low, for both the monopolist and the buyer. Which state is realized is private information. For this scenario, we analyze a two–period game with one seller and one buyer whose valuation is also private knowledge, and derive the solution play in closed form. Therefore, this chapter additionally contributes to the theoretical and experimental literature on bargaining with asymmetric information.[5]

Experimental studies have investigated durable–goods monopolies with two, and more periods. Results of those studies mentioned above support the theoretical prediction of intra–personal price competition in durable–goods monopolies. There is strong evidence that monopolists indeed lose monopoly power when selling a durable good. Where most experimental studies focus on dynamics of markets with the same discount factor for all subjects Güth, Ockenfels, and Ritzberger investigate the case of heterogeneous discount factors for buyers and the monopolist.

However, in experimental studies also a large number of observations have been made which indicate that subjects' behavior is inconsistent with the predictions. Reynolds observed that initial prices were higher in multi–period experiments than in single–period monopoly experiments. In all experiments, there is more demand withholding than theory predicts. For example, Cason and Sharma observed more trading periods than predicted due to higher demand withholding. Finally, durable–goods experiments seem to require a number of repetitions due to their complexity. In Güth, Ockenfels, and Ritzberger, there was no opportunity for learning. Prices failed to conform to comparative statics predictions and were often higher than the theoretical benchmark. With experienced subjects, observed prices were closer to the prediction, but participants still had serious difficulties to understand the crucial aspects of such dynamic markets. Further experimental investigation is needed to solve the ambiguity of previous results but also to provide a wider basis on which conclusions about the predictive power of the theory can be drawn.

In addition to our theoretical contribution, we therefore provide experimental evidence. Experimental data may reveal to what extent subjects' behavior conforms to (rational expectations) theory, but it may also show that bounded rationality limits the predictive power of standard theory in durable–goods games. Theory has a number of interesting testable implications in our market. Will sellers with a low discount factor charge lower prices than a patient seller, as predicted? Similarly, will buyers with a high discount factor refuse to purchase in period one more often compared to impatient buyers? Considering bounded rationality, two kinds of behavior may be important. First, buyer subjects may withhold demand, that is, they may reject profitable purchases because of fairness reasons. Such behavior may soften the monopolist's pricing behavior and may generally limit the predictive power of standard theory in durable–goods games. Second, certain behavior might help to provide the missing commitment device about future prices. It seems possible that seller subjects might feel committed by mere intentions about their future behavior—even when there is no formal commitment device. This again could limit the predictive power of the theory. The conflict of a durable–goods monopolist between avoiding the effects of intra–personal price competition and reacting opportunistically, and how this enters the price expectations of the buyer is an important behavioral issue with further theoretical implications and suitable to be investigated with an economic experiment.

In view of these previous experiments and their results, it seems important to limit attention to the simple case of markets with two periods. We also have provided ample opportunities for learning by letting participants play the same market repeatedly in our computerized experiment. This allows us to incorporate a further complexity, namely that relative impatience is private information.

In section 4.2, we describe the model and in section 4.3 derive the game–theoretic solution play for two–period markets. Section 4.4 explains the design of the experiment whose results are described and statistically analyzed in section 4.5. We summarize in section 4.6.

4.2 The Basic Model

The monopolistic seller has an indivisible commodity which he evaluates by 0, whereas the only buyer evaluates the commodity by $v \in [0, 1]$. The value v is, however, the buyer's private information. The distribution of v is uniform over the unit interval $[0, 1]$, and this is commonly known.

We consider two successive sales periods. The discount factor $\zeta \in (0, 1)$ represents the seller's weight for future (period $t = 2$) versus present (period $t = 1$) profit. Similarly, δ reflects the buyer's impatience where $\delta \in (0, 1)$.[6] We denote by p_1 the price in period $t = 1$ and by p_2 the price in period $t = 2$.

The decision process is as follows:

Period $t = 1$:

- The seller chooses his sales price $p_1 \in [0, 1]$ for this period.
- Knowing p_1 and her value v, the buyer decides whether or not to buy. If she does, this ends the interaction; otherwise period $t = 2$ follows.

Period $t = 2$:

- The seller chooses his sales price $p_2 \in [0, 1]$ for this period.

- Knowing p_2 and her value v, the buyer decides whether or not to buy. This ends the interaction.

The profit of the seller is p_1 if there is trade in period $t = 1$, it is ζp_2 if trade occurs in period $t = 2$, and it is 0 if there is no trade. For the buyer, the payoff is $v - p_1$ for trade in period $t = 1$ it is $\delta (v - p_2)$ for trade in period $t = 2$, and 0 in the case of no trade.

If both discount factors are commonly known, and if the seller is risk neutral, the solution prices p_1^* and p_2^* depend on the discount factor ζ of the seller and δ of the buyer as follows:[7]

$$p_1^* = \frac{(2-\delta)^2}{2\left[4 - 2\delta - \zeta\right]}, \; p_2^* = \frac{2-\delta}{2\left[4 - 2\delta - \zeta\right]}. \tag{4.1}$$

Note that, with just one trading period, the monopoly price[8] would be $p^* = \frac{1}{2}$, implying a profit of $\frac{1}{4}$. The polar cases of relative impatience correspond to

- $\zeta \searrow 0$ and $\delta \nearrow 1$ with $\lim p_1^* = \frac{1}{4} = \lim p_2^*$: as only buyers with $v \geq \frac{1}{2}$ buy in period $t = 1$, the seller earns only half of what he would earn as a usual monopolist, namely $\frac{1}{4}(\frac{1}{2}) = \frac{1}{8}$ in period $t = 1$ (revenues in period $t = 2$ are neglected since $\zeta \searrow 0$).

- $\zeta \nearrow 1$ and $\delta \searrow 0$ with $\lim p_1^* = \frac{2}{3}$ and $\lim p_2^* = \frac{1}{3}$: the (extremely patient) seller engages in price discrimination over time by collecting $p_1^* = \frac{2}{3}$ whenever v is in the interval $1 \geq v \geq \frac{2}{3}$ and $p_2^* = \frac{1}{3}$ when $\frac{2}{3} > v \geq \frac{1}{3}$. This yields an expected profit of $\left(\frac{2}{3} + \frac{1}{3}\right) \cdot \frac{1}{3} = \frac{1}{3}$, more than the static monopoly profit.

We assume that discount factors are private knowledge. In addition to information about their discount factors, players observe the following: In period $t = 1$, the buyer is informed about his valuation and the seller's price offer. If there is trade in period $t = 1$, the seller learns that there is trade. If there is no trade in period $t = 1$, the buyer additionally observes the price p_2, and the seller learns whether or not she sold the commodity in period $t = 2$. In order to simplify the analysis, we assume that the discount factors of buyers and sellers can adopt only two values, low or high. That is, we assume

$$0 < \underline{\delta} < \overline{\delta} < 1 \text{ and } 0 < \underline{\zeta} < \overline{\zeta} < 1 \tag{4.2}$$

where the probability for $\overline{\delta}$ is $w \in (0,1)$ and that for $\overline{\zeta}$ is $\omega \in (0,1)$. To allow for a clear–cut benchmark solution,[9] we assume that all the parameters $\underline{\delta}$, $\overline{\delta}$, $\underline{\zeta}$, $\overline{\zeta}$, w, and ω are commonly known.

4.3 The Solution Play

Our first point is obvious but useful to note. Whenever $p_2 \geq p_1$ the buyer would not buy in period $t = 2$ as $\delta < 1$. We therefore obtain

Proposition 1: The solution play of the two–period game involves a price decrease, that is, $p_1 > p_2$.

Given the buyer's discount factor $\delta \in \{\underline{\delta}, \overline{\delta}\}$, when will she buy the commodity? Consider the decision to buy in period $t = 1$ or $t = 2$. If a type $v \in [0,1]$ has not bought in period $t = 1$ at price p_1, she will buy in period $t = 2$ at price p_2 whenever $v \geq p_2$. Assume now a type $v \geq p_2$ who anticipates the actual solution prices p_1 and p_2. Since buying in period $t = 1$ yields $v - p_1$, whereas delaying it yields $\delta\,(v - p_2)$, type v prefers to buy early if

$$v - p_1 \geq \delta\,(v - p_2) \text{ or } v \geq \frac{p_1 - \delta p_2}{1 - \delta} \quad . \tag{4.3}$$

This establishes

Proposition 2: If the solution play involves prices p_1 and p_2,

(i) sale occurs in period $t = 1$ if

$$v \geq \left\{ \begin{array}{ll} \underline{v} = \frac{p_1 - \underline{\delta} p_2}{1 - \underline{\delta}} & \text{for } \delta = \underline{\delta} \\ \overline{v} = \frac{p_1 - \overline{\delta} p_2}{1 - \overline{\delta}} & \text{for } \delta = \overline{\delta} \end{array} \right\} \tag{4.4}$$

and in period $t = 2$ if

$$\begin{aligned} \underline{v} > v \geq p_2 \text{ for } \delta = \underline{\delta} \\ \overline{v} > v \geq p_2 \text{ for } \delta = \overline{\delta} \quad , \end{aligned} \tag{4.5}$$

whereas

(ii) $v < p_2$ implies no sales at all.

Note that Proposition 1 implies that the two thresholds $\underline{v}$ and $\overline{v}$ in Proposition 2 satisfy $\underline{v} < \overline{v}$.

Next, we discard the possibility that the seller serves only the $\overline{\delta}$–buyer types in period $t = 2$. Assume, by contrast, that this is true. Then the $\underline{\delta}$–buyer would only switch between buying at price p_1 in period $t = 1$ and not buying at all, implying that only $\underline{\delta}$–buyers with $v \geq p_1$ buy in period $t = 1$. But, since $p_1 > p_2$, $\underline{\delta}$–buyer types v with $p_1 > v \geq p_2$ would like to buy in period $t = 2$, contradicting the assumption that only $\overline{\delta}$–buyer types are served in period $t = 2$. Thus we have proved

Proposition 3: Trade in period $t = 2$ involves both buyer types $\delta \in \{\underline{\delta}, \overline{\delta}\}$ with positive probability, i.e., $\underline{v} > p_2$.

We can now proceed to derive the full solution play of the game. We start by solving the last period. Note that, in period $t = 2$, the seller knows that the $\underline{\delta}$ $(\overline{\delta})$–buyer has no value $v \geq \underline{v}(\overline{v})$. Thus, his posterior probability of trade in period $t = 2$ at price p_2 is

$$D(p_2) = \frac{(1-w)(\underline{v} - p_2) + w(\overline{v} - p_2)}{(1-w)\underline{v} + w\overline{v}} \quad , \tag{4.6}$$

where, in view of Proposition 3, both terms of the numerator on the right–hand side above are positive. Maximization of $p_2 D(p_2)$ yields

$$p_2 = p_2(\underline{v}, \overline{v}) = \frac{(1-w)\underline{v} + w\overline{v}}{2} \quad . \tag{4.7}$$

Substituting p_2 in (4.4), the equations for $\underline{v}$ and $\overline{v}$, yields a system of two equations with two unknowns

$$\underline{v} = \frac{2p_1 - \underline{\delta} w \overline{v}}{(2 - \underline{\delta}(1+w))}, \; \overline{v} = \frac{2p_1 - \overline{\delta}(1-w)\underline{v}}{(2 - \overline{\delta}(2-w))} \quad . \tag{4.8}$$

This system can readily be solved as

$$\underline{v} = p_1 \frac{\left(2 - \underline{\delta} w - \overline{\delta}(2-w)\right)}{\left(2 - \underline{\delta}(1+w) - \overline{\delta}(2-w) + \underline{\delta}\overline{\delta}\right)} \quad , \tag{4.9}$$

$$\overline{v} = p_1 \frac{\left(2 - \underline{\delta}(1+w) - \overline{\delta}(1-w)\right)}{\left(2 - \underline{\delta}(1+w) - \overline{\delta}(2-w) + \underline{\delta}\overline{\delta}\right)} \quad . \tag{4.10}$$

Since the optimal price $p_2 = ((1-w)\underline{v} + w\overline{v})/2$ depends on $\underline{v}$ and $\overline{v}$, it can be expressed as a function of p_1 only:

$$p_2(p_1) = p_1 \frac{\left(1 - \underline{\delta} w - \overline{\delta}(1-w)\right)}{\left(2 - \underline{\delta}(1+w) - \overline{\delta}(2-w) + \underline{\delta}\overline{\delta}\right)} \quad . \tag{4.11}$$

We will use $\gamma = (1 - \underline{\delta} w - \overline{\delta}(1-w))$ and $\varepsilon = (1 - \underline{\delta} - \overline{\delta} + \underline{\delta}\overline{\delta})$ to simplify the notation. Note $\underline{v} = p_1(\gamma + 1 - \overline{\delta}) / (\gamma + \varepsilon)$, $\overline{v} = p_1(\gamma + 1 - \underline{\delta}) / (\gamma + \varepsilon)$, $p_2(p_1) = p_1 \gamma / (\gamma + \varepsilon)$.

With the help of these derivations, the expected profit from trade over the two sales periods can be defined as a function of p_1, the price of period $t = 1$, namely

$$\begin{aligned} & p_1 \left[(1-w)\left(1-\underline{v}\left(p_1\right)\right) + w\left(1-\overline{v}\left(p_1\right)\right) \right] + \\ & \zeta p_2\left(p_1\right) \left[(1-w)\left(\underline{v}\left(p_1\right) - p_2\left(p_1\right)\right) + w\left(\overline{v}\left(p_1\right) - p_2\left(p_1\right)\right) \right] \quad , \end{aligned} \tag{4.12}$$

where, $\zeta \in \{\underline{\zeta}, \overline{\zeta}\}$. Maximizing this function with respect to p_1 yields

$$p_1\left(\zeta\right) = \frac{\left(\gamma+\varepsilon\right)^2}{2\gamma\left(2\left(\gamma+\varepsilon\right)-\zeta\gamma\right)} \tag{4.13}$$

and thus

$$\underline{v}\left(\zeta\right) = \frac{\left(\gamma+\varepsilon\right)\left(\gamma+1-\overline{\delta}\right)}{2\gamma\left(2\left(\gamma+\varepsilon\right)-\zeta\gamma\right)} \quad , \tag{4.14}$$

$$\overline{v}\left(\zeta\right) = \frac{\left(\gamma+\varepsilon\right)\left(\gamma+1-\underline{\delta}\right)}{2\gamma\left(2\left(\gamma+\varepsilon\right)-\zeta\gamma\right)} \quad , \tag{4.15}$$

$$p_2\left(\zeta\right) = \frac{\gamma+\varepsilon}{2(2\left(\gamma+\varepsilon\right)-\zeta\gamma)} \quad . \tag{4.16}$$

Hence, we have derived the solution[10] play described by

Proposition 4: For $\zeta \in \{\underline{\zeta}, \overline{\zeta}\}$, the solution play of the two–period game is as follows:

- In period $t = 1$, the price is $p_1(\zeta)$ which induces all buyers with $v \geq \overline{v}(\zeta)$ and $\delta = \overline{\delta}$ and those with $v \geq \underline{v}(\zeta)$ and $\delta = \underline{\delta}$ to buy.

- In period $t = 2$, all buyers with $\overline{v}(\zeta) > v \geq p_2(\zeta)$ and $\delta = \overline{\delta}$ and those $\underline{v}(\zeta) > v \geq p_2(\zeta)$ and $\delta = \underline{\delta}$ buy, whereas

- all remaining buyer types abstain from trading.

According to $p_1(\zeta)$, the seller with time preference $\zeta \in \{\underline{\zeta}, \overline{\zeta}\}$ reveals his impatience by his first–period price p_1.[11] Therefore, the buyer can rationally anticipate $p_2(\zeta)$ after observing p_1. The seller in turn only learns after the first sales period whether or not the buyer has bought in this period. Thus, his demand expectations for the second sales period are as expressed by $D(p_2)$.

4.4 Experimental Design

Our experimental design exactly matches the above setup of the durable–goods monopoly with privately known impatience. We employ the parameters $\underline{\delta} = \underline{\zeta} = 0.3$, $\overline{\delta} = \overline{\zeta} = 0.7$, $w = \omega = 0.5$. These parameters imply the values in table 4.2. If the buyers' valuations are drawn from the unit interval as assumed in the theory section, the two columns on the left apply. In the experiment, we took buyers' valuations from the interval $[50, 150]$. Therefore, the absolute price prediction is according to the two right columns of table 4.2. For the sake of plausibility of the frame, we introduced a "production on demand"–cost of 50. Sellers could choose prices from the interval $[0, 200]$.

We ran six sessions, each consisting of two matching groups, giving us twelve independent observations. Each round was conducted exactly as follows:[12] One group consisted of three sellers and three buyers. Within the groups, sellers and buyers were randomly rematched after each round.[13] Subjects learned their role, seller or buyer, only after they had read the instructions (see appendix 4.B), and they did not switch roles during the experiment. In order to allow for learning, we decided to run the experiment over 40 rounds.[14]

Sellers learned their discount factor, then they had to choose their price. Knowing their discount factor and value, buyers had to decide whether or not to buy at the period–one price p_1. If they decided not to, period two would commence and so forth. At the end of each round, subjects were informed about their private earnings in the previous round as well as their cumulative earnings up to this round. They did not receive any information about the other persons discount rate or payoffs.[15]

The computerized experiments were conducted at Humboldt University, Berlin, in December 2001 and January 2002, using the software z–tree (Fischbacher, 1999). The 72 participants were mainly business and economics students who were recruited via email and telephone. Payments were 16 Euros on average, including a show–up fee of 2.5 Euros. Sessions lasted roughly 90 minutes.

4.5 Results

Let us first check whether buying and pricing behavior is consistent with a few qualitative theoretical implications. It seems worth emphasizing that consistency even with very basic principles cannot be taken for granted in a complex durable–goods setting. For example, Güth, Ockenfels, and Ritzberger (1995) report a surprising amount of inconsistency[16] in a durable–goods experiment. Similarly, Reynolds (2000) emphasizes the necessity of experience with the trading environment. Therefore, we find it useful check consistency first.

Consider the buyers. Basic understanding of the situation implies that buyers would never purchase at a price above their valuation. It seems impossible that some argument based on repeated games or bounded rationality could plausibly support such loss–inducing purchases. Out of $1,440$ possible sales, we observed $1,037$ actual purchases. In all but six purchases, buyers had valuations above the prices. That is, there are virtually no such loss–making purchases, and we can conclude that basic buyer behavior was consistent in this sense.[17]

Buyers knew that profits from sales made in period $t = 2$ are discounted. Thus, $\overline{\delta}$–buyers should reject a profitable purchase in period $t = 1$ more often than a $\underline{\delta}$–buyer. Given any path of (expected) seller prices $\{p_1, p_2\}$, the impatient buyer has to purchase early more often, as her second–period opportunities are less attractive. Even if we take repeated–game effects like demand withholding into account, it seems implausible for the more impatient buyer to reject more often because it is more costly for her to reject. Confirming this, the data show that, in period $t = 1$, the $\overline{\delta}$–buyers reject profitable offers (i.e., offers with $p_1 \leq v$) with 41% significantly more often than $\underline{\delta}$–buyers with 16%. Because of possible dependence of observations within the groups of six subjects, we count group averages including all periods as one observation. Unless mentioned, all tests reported in this chapter are therefore based on matching group averages. See appendix 4.A for summary statistics of all matching groups. Relative acceptance rates are lower with $\overline{\delta}$ for all groups, the according non–parametric test is highly significant (one–sided Wilcoxon, $p = 0.0002$). We conclude that buyers do understand the basic impact of discounting.

Now consider the sellers. Did they understand the implication of discounting? If so, sellers with a high discount factor should charge a higher price in both periods than sellers with a low discount factor. As shown above (see table 4.2), this is the prediction. Even if subjects do not behave according to the solution play, it should be apparent to them that a high discount factor makes it relatively more attractive to charge a high price in period $t = 1$ as there is still another profitable opportunity to come. As both types of sellers should (and indeed did) reduce their price in $t = 2$, a higher period

$t = 1$ price for high discount factor types also implies higher period $t = 2$ prices. By contrast, the impatient seller has to make his sales early and, therefore, charges also a lower period $t = 2$ price. The data show that average prices of $\overline{\zeta}$ sellers were higher ($t = 1 : 91$ and $t = 2 : 81$) than those of $\underline{\zeta}$ sellers prices ($t = 1 : 84$ and $t = 2 : 78$) in all groups and in both periods. Accordingly, the test is highly significant (one–sided Wilcoxon, $p = 0.006$). It appears that sellers understood the impact of their discount factor.

Proposition 1 states that sellers should charge lower prices in period $t = 2$ compared to period $t = 1$. The intuition is that a discounting buyer has no incentive to buy at a higher price in period $t = 2$. If sellers want to exploit the opportunity to sell in period $t = 2$, they should lower the price. However, the prediction of a price decrease over the two periods is not the only plausible behavior. Boundedly rational sellers may refuse to charge a lower period $t = 2$ price in an attempt to solve the commitment problem.

In 750 cases, there is no trade in period $t = 1$, and therefore a period $t = 2$ price is observed. In the vast majority of these cases, sellers indeed charged a lower price in period $t = 2$. In total, only 33 out of 750 period $t = 2$ prices were strictly higher than p_1, and this figure gets even smaller over time. Over the last 10 rounds, only 3 out of 155 period $t = 2$ prices were strictly higher than p_1. In many instances (13 out of 33 and 3 out of 3 cases, respectively), we observe the maximum price of 200 in period $t = 2$, and all but one of these 13 observations were caused by a single seller.[18] In these cases, the higher price does not appear to be a mistake but a signal. In addition, there are another 33 observations (7 over the last 10 rounds) in which the price was constant over the two periods. The vast majority of these cases can be attributed to only a few sellers.[19] We never observed a seller who regularly behaved as a one–period monopolist in the sense of $p_1 = p_2 = 100$. To summarize, we find only few violations of Proposition 1. A few subjects occasionally charged $p_2 = p_1$ or $p_2 = 200 > p_1$. This may be interpreted as attempts to solve the durable–goods monopolist's commitment problem. The remaining number of inconsistencies is small and scattered over time and subjects.

Result 1: Subjects' behavior is consistent with several qualitative predictions. Buyers virtually never make unprofitable purchases. Almost all sellers systematically lowered prices in period $t = 2$. Patient buyers reject profitable purchases in period $t = 1$ more often. Patient sellers charge higher prices in both periods.

Let us now compare the data to the exact predictions of p_1, p_2, $\underline{v}$ and $\overline{v}$. Consider buyer behavior first. Buyers withhold demand whenever an offer $v > p$ is rejected. The prediction is that any price offer smaller than v (in period $t = 2$) or smaller than $\overline{v}$ or $\underline{v}$ (in period $t = 1$) should be accepted independently of the history of the game. There can be rational and boundedly rational (or irrational) demand withholding. In period $t = 1$, when $v > p_1$ but $v < \overline{v}$ or $v < \underline{v}$ respectively, a rejection is rational. In period $t = 2$, there is no rational demand withholding. While demand withholding as part of boundedly rational strategy has been frequently observed (Ruffle, 2000, investigates strategic buyer behavior in oligopoly experiments, and Engle–Warnick and Ruffle, 2002, in a monopoly experiment), in this experiment, demand withholding in order to establish a reputation for aggressive buyer behavior is particularly difficult. First, there is the random rematching, and the design does not allow to identify buyers. Moreover, sellers do not know whether their offer was rejected because of demand withholding or because it was not profitable. By contrast, in many posted–offer experiments, buyers' valuations are known, and demand withholding can much better serve as a signal.

Buyer behavior in period $t = 2$ is simple to analyze as there are no future effects to consider. Buyers' period $t = 2$ behavior is also independent of δ. Any $p_2 \leq v$ should be accepted by all buyers. Table 4.3 reports the numbers of observed price offers, their acceptance conditional on the relation of price offer and threshold $\overline{v}$ and $\underline{v}$ for both negotiation periods. In the data, we find that 68 out of 413 offers (16.5%) with $p_2 \leq v$ were rejected (see column 6 ("$p_2 \leq v$")). These rejected offers typically left only a small profit margin for the buyers. This margin was $(v - p_2)/v = 0.0693$ on average for the rejections. Two thirds of all rejections involved a margin of less than 8%. Regarding accepted offers, buyers often were willing to accept even low margins and, in four cases, buyers accepted a period $t = 2$ price at which they just broke even. Two thirds of all accepted prices gave them a less than 26% profit margin. Buyers never rejected margins of more than 25%. Figure 4.1 illustrates the acceptance and rejection averages of $(v - p_2)/v$ for the twelve groups (provided $v - p_2 \geq 0$). Overall groups, offers which left on average at least 13% of the buyers' valuation were accepted. As the acceptance and rejection average margins are not overlapping, there seems to exist a quite robust acceptance threshold margin interval of [11%, 13%] below and above which offers are rejected and accepted, respectively. Recall that buyers knew the production cost of the seller (50). Therefore, besides the impact of the discount factor, they were able to identify the seller's profit and compare it to their own. Take buyers' reaction to the median period $t = 2$ price, $p_2 = 75$, as an example. Buyers with $v < 100$ knew that the seller would get a larger profit from the sale, but they rejected only in 9 out of 38 cases (taking only buyers with $v \geq p_2 = 75$ into account). Thus, it seems that aversion against disadvantageous inequality played only a little role here.[20] Nevertheless, there is demand withholding in period $t = 2$.

We turn to buyer behavior in period $t = 1$. The prediction is that, after observing the equilibrium price $p_1(\zeta)$, buyers with $v > \underline{v} > p_1$ or $v > \overline{v} > p_1$ should accept. (Henceforth, we will refer to "$\overline{\underline{v}}$" whenever we want make a statement about "$\underline{v}$ or $\overline{v}$".) For out-of-equilibrium prices $\widehat{p}_1$, buyers with $v > \underline{v}(\widehat{p}_1) > \widehat{p}_1$ and $v > \overline{v}(\widehat{p}_1) > \widehat{p}_1$ should accept (see equation (4.4)). This relation holds if buyers interpret out-of-equilibrium prices as decision errors and believe sellers behave rationally in period $t = 2$ and will set the equilibrium price $p_2(\widehat{p}_1)$ as in equation (4.11). Acceptance numbers for the observed prices p_1^o and according to $\overline{\underline{v}}(p_1^o)$ computed thresholds taking out-of-equilibrium prices into account are listed in the first three columns of table 4.3. First, consider buyers with $\overline{\underline{v}} > v \geq p_1^o$ which are predicted to reject (these are cases of rational demand withholding). Out of 206 cases (see column 2 $\left("p_1^o \leq v < \overline{\underline{v}}"\right)$), buyers rejected in 174 cases (84.5%). That is, to a large extent, buyers' behavior was in accordance with the theory. There are, however, some inconsistencies, namely the 32 accepted offers yielding a profit margin of $(v - p_1)/v = 0.159$. These buyers did not realize that a lower period $t = 2$ price should have given them a higher discounted margin. Second, did buyers with $v \geq \overline{\underline{v}}$ accept? If $(v - \overline{\underline{v}})/v$ is positive 90% of all offers were accepted. If $(v - \overline{\underline{v}})/v > 0.1$ even 96% of all offers were accepted. The average rejected margin was $(v - p_1)/v = 0.115$. Note that this margin is larger than the one in period $t = 2$, so there is more demand withholding in period 1. These are cases of irrational (or boundedly rational) demand withholding.

Result 2: Buyers' behavior is to a large extent consistent with the prediction. Buyers usually accepted profitable offers in period $t = 2$ while, in period $t = 1$, they accepted only if the offer gave them a more than positive profit margin. Both in period $t = 1$ and $t = 2$, there is some irrational (or boundedly rational) demand withholding, that is, buyers sometimes reject margins higher than those predicted.

Now consider seller behavior. We report deviations from the (conditional) predictions rather than absolute values because the optimal prices p_2 depend on the realization of p_1, and p_1 is often different from the predictions $p_1\left(\underline{\zeta}\right) = 90$ and $p_1\left(\overline{\zeta}\right) = 97$. Accordingly, we refer to $p_2\left(p_1^o\right)$ rather than $p_2\left(\underline{\zeta}\right)$ and $p_2\left(\overline{\zeta}\right)$, and we define $\Delta p_1\left(\zeta\right) = p_1^o - p_1\left(\zeta\right)$, $\Delta p_2 = p_2^e - p_2(p_1^o)$. Note that Δp_2 does only depend on p_1 but not on the realization of ζ. We find that $\Delta p_1\left(\underline{\zeta}\right) = -4.73$, $\Delta p_1\left(\overline{\zeta}\right) = -6.46$, and $\Delta p_2 = +0.89$.[21] We find that both $\Delta p_1\left(\zeta\right)$ are significantly different from zero (two–sided Wilcoxon, $p = 0.006$) while Δp_2 is not. In absolute terms, the average prices charged are $p_1^o\left(\underline{\zeta}\right) = 84.27$ and $p_1^o\left(\overline{\zeta}\right) = 90.54$.

Given that buyers charged prices in period $t = 1$ partly far away from the prediction, it is more difficult to analyze period $t = 2$ pricing behavior. If we interpret the $p_1 \notin \{90, 97\}$ as decision errors, and if we assume that both buyers and sellers behave fully rationally in the continuation game, then the appropriate period $t = 2$ price is $p_2(p_1^o)$ as in equation (4.11). As mentioned, we report the difference between actual prices in $t = 2$ and this prediction: $\Delta p_2 = p_2^e - p_2(p_1^o)$. Now, $\Delta p_2 = 0.89$ is surprisingly small what we interpret as support of the rationality hypothesis when the situation is simple (in $t = 2$, sellers do not have to anticipate own future choices any longer). Regarding group averages, figure 4.2 shows that all except one group have a rather small Δp_2 while the Δp_1 observations are more dispersed and clearly negative. We do not distinguish between ζ–values as the picture is roughly the same. The fact that Δp_2 average is slightly positive does not mean that pricing behavior in period $t = 2$ changes qualitatively from that in period $t = 1$. Sellers start with a lower price, reducing it by the proportion predicted. Hence, whatever accounts for the lower prices in period $t = 1$, this behavior carries over to period $t = 2$. This picture holds also for the individual sellers, but there is more variability.

Result 3: Sellers charge prices lower than predicted, both in period $t = 1$ and period $t = 2$. The reduction of period $t = 2$ prices is consistent with (conditional) rationality.

To conclude the analysis of seller behavior, we discuss the only significant deviation from the prediction, the lower period $t = 1$ prices. This is a robust finding in that it is very similar for both discount rates $\underline{\zeta}$ and $\overline{\zeta}$. One explanation for p_1^o prices below the theoretical prediction might lie in the specific design of the experiment. Instead of a continuous demand function, we have assumed a single buyer whose value is private information. The density of the value plays the role of the continuous demand function. Theoretically, this does not matter much for the outcome, but it may matter behaviorally as in such bilateral encounters fairness concerns may become stronger, and this could account for low first–period prices (which would imply more balanced distributions of surplus from trade). Alternatively, risk considerations (an attitude of sellers to ensure trade) may explain the result. We did not control for fairness concerns nor for risk aversion of sellers. Because the buyer's valuation is private knowledge, sellers only know the expected buyer profit. Though it is possible for buyers to make inter–personal profit comparisons for trades in period $t = 1$, it is quite difficult to do so for sellers and, regarding profits made in the second period, there is uncertainty about the discount factor. Therefore, compared to pure bargaining experiments, it seems less likely that fairness matters, suggesting that the lower period $t = 1$ prices rather reflect the risk attitude of sellers.

We finally analyze the impact of the constellation of the discount factors. It is a central feature of our model that the discount factor of the seller, as compared to the buyer's, determines whether the seller suffers from intra–personal competition or gains by price discrimination. In this sense, a higher discount factor implies higher "power," affecting both acceptance rates and profits. Above, we already reported the impact of discount factors, separately for buyers and sellers. Here, we compare acceptance rates and profits for all (ζ, δ) seller–buyer combinations.

We start with the percentage of accepted offers. Let $a_t(\zeta, \delta)$ denote the rate of acceptance for some (ζ, δ) seller–buyer combination in period t (see appendix 4.A for the data of the matching groups). Theory predicts that sellers with a high discount factor charge higher prices both in period $t = 1$ and period $t = 2$, and that buyers with a high discount factor reject profitable purchases in period $t = 1$ more often. This immediately implies that, in period $t = 1$, $a_1(\overline{\zeta}, \overline{\delta})$ should have the smallest and $a_1(\underline{\zeta}, \underline{\delta})$ the highest acceptance rate, while $a_1(\underline{\zeta}, \overline{\delta})$ and $a_1(\overline{\zeta}, \underline{\delta})$ should be intermediate. Deducing acceptances rates from table 4.2, the prediction is $a_1(\underline{\zeta},\overline{\delta}) < a_1(\overline{\zeta},\underline{\delta})$. This turns out to hold in our data (see table 4.4). The acceptance rates for the four combinations are $a_1(\overline{\zeta},\overline{\delta}) \overset{(.011)}{<} a_1(\underline{\zeta},\overline{\delta}) \overset{(.021)}{<} a_1(\overline{\zeta},\underline{\delta}) \overset{(.001)}{<} a_1(\underline{\zeta},\underline{\delta})$ with corresponding significance level of the one–sided Wilcoxon tests above the inequality signs. Intuitively, the acceptance rates in period $t = 2$ must exhibit the opposite inequality signs: if there are fewer acceptances in period $t = 1$, more buyers are left to accept in period $t = 2$. In accordance with this intuition, one can deduce $a_2(\overline{\zeta},\overline{\delta}) > a_2(\underline{\zeta},\overline{\delta}) > a_2(\overline{\zeta},\underline{\delta}) > a_2(\underline{\zeta},\underline{\delta})$ from table 4.2. We find that $a_2(\underline{\zeta},\overline{\delta}) \overset{(.006)}{>} a_2(\overline{\zeta},\underline{\delta})$ and $a_2(\underline{\zeta},\overline{\delta}) \overset{(.032)}{>} a_2(\underline{\zeta},\underline{\delta})$ as predicted and significantly so (one–sided Wilcoxon test), but neither $a_2(\overline{\zeta},\overline{\delta}) > a_2(\underline{\zeta},\overline{\delta})$ (as predicted) nor $a_2(\underline{\zeta},\underline{\delta}) > a_2(\overline{\zeta},\underline{\delta})$ (not predicted) were significant.

Now consider profits (see appendix 4.A for the group data again). Predictions are simple. Given the discount factor of the other player, a high own discount factor implies a higher profit. Given the own discount factor, a high discount factor of the other player implies a lower profit. It turns out that this holds in the experimental data for all possible (ζ, δ) combinations (see table 4.5). That is, though high and low discount factor types can actually realize the same profit in period $t = 1$, high discount factor types make larger profits because of the trade shifted to period $t = 2$. Let $u_S(\zeta, \delta)$ and $u_B(\zeta, \delta)$ indicate the average profits made in a (ζ, δ) seller–buyer encounter. The average $u_S(\zeta, \delta)$ was roughly 19, and the average $u_B(\zeta, \delta)$ was about 21. The following inequalities are significant (with the corresponding significance level of the one–sided Wilcoxon tests above the inequality signs). We find that $u_S(\overline{\zeta},\underline{\delta}) \overset{(.002)}{>} u_S(\overline{\zeta},\overline{\delta}) \overset{(.076)}{>} u_S\left(\underline{\zeta},\overline{\delta}\right)$, and $u_S\left(\underline{\zeta},\underline{\delta}\right) \overset{(.0005)}{>} u_S\left(\underline{\zeta},\overline{\delta}\right)$ for the seller, and $u_B\left(\underline{\zeta},\overline{\delta}\right) \overset{(.008)}{>} u_B(\overline{\zeta},\overline{\delta})$, and $u_B\left(\underline{\zeta},\underline{\delta}\right) \overset{(.005)}{>} u_B(\overline{\zeta},\underline{\delta})$ for the buyer. Further, we find $u_S\left(\underline{\zeta},\underline{\delta}\right) \overset{(.021)}{>} u_S(\overline{\zeta},\overline{\delta})$ and $u_B\left(\underline{\zeta},\underline{\delta}\right) \overset{(.055)}{>} u_B(\overline{\zeta},\overline{\delta})$ because of the high rejection rates which a $(\overline{\zeta},\overline{\delta})$ combination implies.

Result 4: We find, in line with the theoretical predictions, that high discount factors of either the seller or the buyer reduce the probability of a successful trade in period $t = 1$. Nevertheless, higher average earnings can be realized if the opponent has a low discount factor. Whereas the own discount factor has no influence profits.

4.6 Discussion

The literature substantiating the intuition of Coase's (1972) durable–goods monopolist has inspired much theory but only few experiments. In this chapter, we have extended both lines of research. We solve, for the first time, the simplest case where discount factors are private information. Second, by conducting a laboratory experiment, we provide a test of the theory.

The experimental results of former studies on durable–goods monopolies and bilateral bargaining with informed proposers are less supportive for theory. Participants in our experiment behaved according to the qualitative predictions of the model. There are few unprofitable purchases, and there are generally lower prices in the first period as predicted. Furthermore, participants reacted adequately to changes in discount factors (within–subject comparisons) and, as buyers, maintained higher acceptance thresholds in the first than in the second period. Ceteris paribus, a higher discount factor of at least one player shifts more trade to the second period. Whenever the situation becomes rather simple, as for instance in the second period, "conditional" rationality can account for most of the decision data. Additionally, our results suggest that the own discount factor has rather insignificant impact on earnings. Whereas negotiating with an impatient opponent, yields significant higher income.

Our results might indicate that short term problems and learning shifts behavior closer to theoretical prediction of the durable–goods monopoly model. In our study the time horizon was 2 periods whereas in Rapoport, Erev, and Zwick (1995), and Cason and Sharma (2001) subjects interacted "infinitely" long with each other. Short term problems might be better capable by boundedly rational subjects than long term bargaining situations. Another reason for our supportive results for theory might be, that subjects in our experiment had more learning opportunities. Participants interacted in 40 durable–goods markets which allowed them to gain more experience.[22] However, those conclusions should be interpreted cautiously as our experimental design differed in more than one variable from studies mentioned above.

It has already been indicated that we view durable–goods monopolies as very intriguing. They challenge the conventional wisdom that several competitors are needed to induce competitive outcomes; they are also philosophically challenging by claiming intra–personal decision conflict. After all, it is due to rational anticipation of own future behavior that the durable–goods monopolist may earn so much less than a usual monopolist. It seems remarkable that such insights seem to have been well understood by the participants.

Appendix to Chapter 4

4.A Descriptive Statistics

The summary statistics for all presented variables and tests are reported in table 4.1 on matching group level. All variables meet the notation in the chapter p_1 and p_2 are first and second period prices, $u_S(\zeta, \delta)$ and $u_B(\zeta, \delta)$ denote seller's and buyer's earnings, and $\Delta p_1\,(\zeta)$ and $\Delta p_2\,(p_1)$ stand for the differences of prices to the theoretical prediction. We introduce the new notation $a_1(\zeta, \underline{\delta})$ and $a_1(\zeta, \overline{\delta})$ distinguishing acceptance rates of buyers with low and high discount factors. For the acceptance rate of buyers we consider only offers below buyer's valuation, i.e., no rejection due to losses. All data with all individual decisions from the experiment, i.e., all 1,440 negotiations, are available at http://www.wiwi.hu-berlin.de/˜skroeger/dgm/ .

matching group	p_1	p_2	$a_1(\zeta, \underline{\delta})$	$a_1(\zeta, \overline{\delta})$	$u_S(\zeta, \delta)$	$u_B(\zeta, \delta)$	$\Delta p_1\,(\zeta)$	$\Delta p_2\,(p_1)$
1	83.87	75.46	0.85	0.62	20.22	26.29	−9.52	−1.91
2	84.54	77.13	0.89	0.66	19.12	22.57	−8.84	−0.29
3	90.73	83.15	0.88	0.70	20.94	17.77	−2.65	1.87
4	100.73	85.49	0.87	0.46	19.19	14.71	7.34	−3.08
5	87.53	81.27	0.81	0.48	17.89	20.97	−5.86	1.43
6	83.71	74.98	0.81	0.69	17.06	21.23	−9.68	−0.93
7	91.08	80.12	0.79	0.70	22.89	20.13	−2.31	−1.12
8	87.91	97.87	0.93	0.56	18.98	21.70	−5.48	18.72
9	88.56	77.35	0.81	0.50	18.70	19.85	−4.83	−0.86
10	83.58	76.67	0.84	0.52	19.81	24.20	−9.81	0.84
11	85.12	73.32	0.82	0.48	16.74	22.59	−8.27	−4.21
12	80.27	73.16	0.74	0.69	15.41	22.78	−13.12	0.26
all	87.30	79.79	0.84	0.59	18.91	21.23	−6.08	0.79

Table 4.1: Summary statistics: Mean values for each matching group

4.B Instructions (Translation)

The experiment was conducted in German, and the original experimental instructions were also in German. This is a shortened[23] translated version of the instructions. Participants read the paper instructions before the computerized experiment started. In the beginning of the instructions, subjects were informed that the instructions are the same for every participant, that they receive an initial endowment of DM 5, that wins and losses from all periods would be added, that the exchange rate from ECU (Experimental Currency Unit) to DM: 30 ECU = DM 1, that communication was not allowed and questions would be answered privately, and that all decisions will be treated anonymously. Then the main instructions started.

Two parties, a seller S and a buyer B, negotiate in each period about the sale of a product. The buyer's product value v is $50 \leq v \leq 150$ (all in ECU). The valuation is the payoff a buyer receives if he purchases the product. In each period, there will be a new v drawn from this interval, with all values being equally likely. The seller has production costs of 50 if he sells the good.

Whether you act as S or B is determined randomly at the beginning of the experiment. You will keep your role for the whole experiment. You will interact in total over 40 periods. Your bargaining partner will every time be randomly determined at the beginning of each period.

Trade takes place according to the following rules:

1. S decides about the price p_1 with $0 \leq p_1 \leq 200$ within a first sales opportunity.

2. B decides whether to buy and pay p_1 or not.

   (a) If B purchases the product, S receives $p_1 - 50$. B receives v and pays p_1, i.e., his profit is $v - p_1$.
   The period is over.

   (b) If B does not purchase, there will be a second sales opportunity. In this case, S decides about a second price p_2 with $0 \leq p_2 \leq 200$. B decides whether to buy and pay p_2 or not to buy at all.

      i. If B purchases the product, S receives a discounted profit $\zeta\,(p_2 - 50)$. B receives v and pays p_2, i.e., his discounted profit is $\delta\,(v - p_2)$.
      (The discount rates ζ and δ of the seller and the buyer, respectively, specify with which factor the profit from the second sales opportunity is multiplied.)

      ii. If B does not purchase (i.e., does not buy at all), both parties receive zero profits.

   The period is over.

[*At this point, the decision process is also graphically illustrated.*]

There are only two values possible for both discount rates ζ and δ, namely 0.3 and 0.7. Possible (ζ, δ) constellations are therefore $(0.3, 0.3)$, $(0.3, 0.7)$, $(0.7, 0.3)$, and $(0.7, 0.7)$. The likelihood for both discount rates' values is the same, and the values are randomly determined at the beginning of each period independently for seller and buyer. All four constellations have the same probability. Only S knows which of the two values ζ has been selected. Correspondingly, only B knows his realized δ value.

At the beginning of each period, you are, according to your role, informed about:

- As seller S: Your discount rate ζ.
- As buyer B: Your discount rate δ.
  and your valuation for the product v.

At the end of each period, you will be informed about your profit in each period and your total payoffs.

Thank you for participating!

4.C Tables

	$v \in [0,1]$		$v \in [50,150]$	
	$\overline{\zeta}$	$\underline{\zeta}$	$\overline{\zeta}$	$\underline{\zeta}$
$p_1(\zeta)$	0.47	0.40	97	90
$p_2(\zeta)$	0.33	0.28	83	78
$\underline{v}(\zeta)$	0.53	0.45	103	95
$\overline{v}(\zeta)$	0.79	0.67	129	117

Table 4.2: Experimental parameters

	Period $t = 1$				Period $t = 2$		
Offers	$v < p_1^o$	$p_1^o \leq v < \underline{v}$	$\underline{v} \leq v$	All	$v < p_2^e$	$p_2^e \leq v$	All
All	511	206	723	1440	337	413	750
Rejected	507	174	69	750	335	68	403
Accepted	4	32	654	690	2	345	347

Table 4.3: Acceptance numbers for different value classification concerning observed p_1^e and p_2^e

$t = 1$		Buyer	
	Discount rate	$\underline{\delta} = 0.3$	$\overline{\delta} = 0.7$
Seller	$\underline{\zeta} = .3$	60.4% (11.0)	41.7% (9.6)
	$\overline{\zeta} = .7$	49.8% (8.7)	31.9% (10.6)
$t = 2$		Buyer	
	Discount rate	$\underline{\delta} = 0.3$	$\overline{\delta} = 0.7$
Seller	$\underline{\zeta} = .3$	43.6% (17.4)	54.7% (12.2)
	$\overline{\zeta} = .7$	36.1% (11.4)	54.6% (17.6)

Table 4.4: Share of accepted first- and second-period offers separately for all four (ζ, δ)-constellations. Standard deviation based on independent observations (matching groups) are reported in brackets

All		Buyer			
	Discount rate	$\underline{\delta} = .3$		$\overline{\delta} = .7$	
Seller	$\underline{\zeta} = .3$	$u_S : 20(3)$	$u_B : 22(5)$	$u_S : 15(3)$	$u_B : 24(5)$
	$\overline{\zeta} = .7$	$u_S : 22(3)$	$u_B : 19(4)$	$u_S : 17(4)$	$u_B : 20(4)$
In case of sale		Buyer			
	Discount rate	$\underline{\delta} = .3$		$\overline{\delta} = .7$	
Seller	$\underline{\zeta} = .3$	$u_S : 26(3)$	$u_B : 29(5)$	$u_S : 20(4)$	$u_B : 33(5)$
	$\overline{\zeta} = .7$	$u_S : 32(4)$	$u_B : 28(5)$	$u_S : 25(3)$	$u_B : 29(5)$

Table 4.5: Profits for all observations separately for each role and all parameter constellations. Standard deviations based on independent observations (matching groups) are reported in brackets

4.D Figures

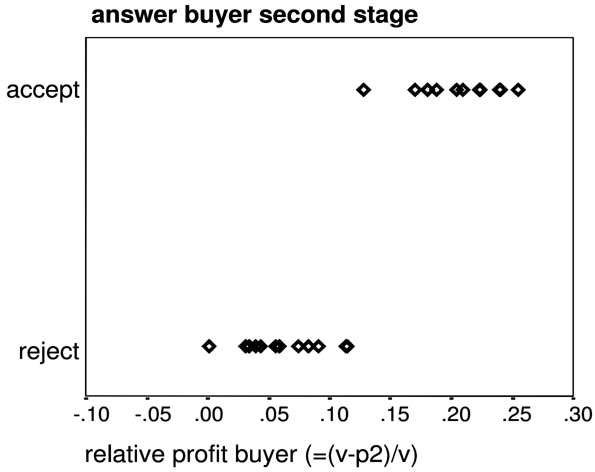

Figure 4.1: Average group acceptance thresholds in $t = 2$ (provided $v \geq p_2$)

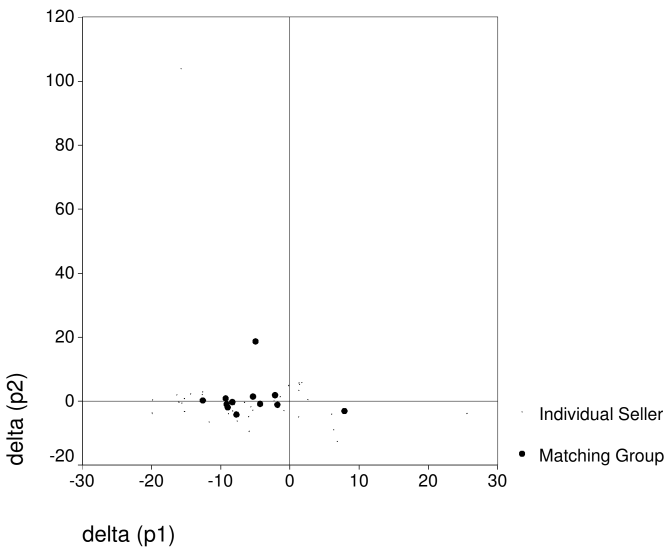

Figure 4.2: Δp_1 and Δp_2 averages for individual sellers and matching groups

Notes

[1]See Frank (1996) for a modern analysis.

[2]For Homer's Ulysses, who binds himself to the ship's mast, there is a way out of the dilemma. But usually such escape does not exist.

[3]A similar intra–personal decision conflict arises in vertically related markets. An upstream monopoly selling to multiple downstream firms may significantly lose its market power (for experimental evidence, see Martin, Normann, and Snyder, 2001).

[4]There is the substantial "myopia" or "short–terminism" literature. Take over threats, career concerns and risk considerations can induce managers not to maximize the discounted value of the firm but to choose projects with a high return early. Such factors are likely to differ across managers. Thus, managers ultimately operate with different discount factors. See, e.g., Stein (1989), or Palley (1997) for more references.

[5]The study by Rapoport, Erev, and Zwick (1995) is closely related to the present one where the responder (buyer) is informed about the pie size and the proposer (seller) only knows its range. Bilateral bargaining with a reversed asymmetric information structure has been experimentally investigated by Mitzkewitz and Nagel, 1993, and Rapoport and Sundali, 1996.

[6]Only the assumption $\delta < 1$ is actually necessary for deriving a well–defined solution play. The boundary case $\delta = 1$ can only be analyzed via $\delta \nearrow 1$ (see Güth and Ritzberger, 1998). Note that $\delta = 1$ renders buying in period $t = 1$ or $t = 2$ as homogeneous trades in view of the buyer. The fact that $\delta = 1$ cannot be solved directly provides an example that price competition for homogeneous products should be solved as the limiting case of such competition for heterogeneous products when heterogeneity vanishes.

[7]The general case of finitely many sales periods can be solved via backward induction, and the infinite horizon via approximation by letting the number of sales periods approach ∞ (see Güth and Ritzberger, 1998).

[8]Resulting from maximizing $p\,(1-p)$ where p is the unique sales price and $1-p$ the probability by which the seller expects his price p to be accepted due to $1-p = \int_p^1 dv$.

[9]Except for highly special games, e.g., when all players have unique not dominated strategies, game–theoretic analysis requires commonly known rules of the game.

[10] A pooling equilibrium, based on the ex ante expected impatience parameter $\widetilde{\zeta} = (1-w)\,\underline{\zeta} + w\overline{\zeta}$, would not satisfy sequential rationality since both seller types would like to deviate from the common price $p_1(\widetilde{\zeta})$ as shown by our derivation.

[11] For the more patient seller it does not pay to mimic the price $p_1\left(\underline{\zeta}\right)$ since the additional revenue in period $t = 1$ is overcompensated by the $\overline{\zeta}$–weighted revenue loss in period $t = 2$. For $\zeta = \underline{\zeta}$ the opposite is true. Note, that the second stage price p_2 is a best response to the residual demand. It does not depend on the impatience parameter of the monopolist. For all monopolists with different discount rates the relative reduction from p_1 to p_2 will be the same and is given by the distribution of impatience amongst buyers (see equation 4.11).

[12]See appendix 4.B for the translated instructions.

[13]Participants were not informed that they were randomly matched in a group of six only which should have further discouraged repeated–game effects.

[14]In the durable–goods experiment by Reynolds (2000), subjects interacted in 12 durable–goods markets.

[15]As "production on demand"–costs were commonly known buyers could deduce their seller's payoff when the sale took place during the first period.

[16] Demand withholding, i.e., waiting to buy or not buying at all, exceeded by far the level predicted by theory. In the last period uninstructed sellers set the theoretically predicted one shot monopoly price against the remaining set of consumers only in half of all cases.

[17] In two cases, buyers accepted a higher price than their valuation in period $t = 2$. The average loss, -2.5, was quite small suggesting the possibility that a preference for efficiency might explain these loss–making decisions; in particular, as they occurred in later rounds (16, 38). By contrast, three of the four

cases in which buyers accepted a price higher than their valuation in first period occurred early (rounds 1, 1, and 7). Here, the average loss was -27. Rather than efficiency–seeking behavior, these cases can be seen as mistakes.

[18] This seller followed a pricing policy of $p_1 = 75$ and $p_2 = 200$ in many rounds. With an expected value of v of 100, this splits the expected surplus of 50 evenly in period $t = 1$. If this price is not accepted, this seller refused to transact at all by offering a price above the buyer's value ($p_2 = 200 > 150 \geq v$). As a referee pointed out, this seller might have tried to build up a reputation despite the random matching scheme (which, apparently, he or she misunderstood).

[19] Four sellers followed this pricing policy four or more times, explaining 27 out of 33 observations.

[20] This suggests that inequity aversion (Bolton, 1991) loses influence in situations where at least some individual payments are private information or difficult to guess.

[21] The reported numbers are group averages. Individual averages have the same means for Δp_1. As the number of trades which continue in period $t = 2$ differs within groups, for individual observations the mean also slightly differs: $\Delta p_2 = +0.79$.

[22] In Güth, Ockenfels, and Ritzberger (1995) subjects interacted in either 1 or 2 and in Reynolds (2000) in 12 sequential markets.

[23] The complete German instructions are available upon request.

Chapter 5

Procurement Experiments with Unknown Costs of Quality

5.1 Introduction

In most countries all major public investments are decided by organizing procurement auctions. To have something specific in mind imagine a municipality which wants to build a new concert hall. The major reason for organizing a procurement auction is that only the potential sellers, here the potential construction firms, know how costly it is to build such a concert hall.[1]

What one, however, often observes in public but also in private procurement is that, after granting the deal, the buyer and the seller start bargaining about the additional costs of quality improvements, changes in the design etc.[2] If, however, the buyer does not know how costly such changes are, he now is confronting a monopolistic seller with unknown cost.

There is a simple solution to such problems which we want to illustrate for our experimental scenario. It assumes that a round concert hall (variant 2) provides better acoustics than a square one (variant 1). The buyer (the municipality) does not know the costs of the variants, especially not how more costly variant 2 is. It is, therefore, difficult to decide between variant 1 and 2.

Our recommendation is to organize a vector auction: Every bidder $i = 1, ..., n$ can submit a vector bid $b^i = \left(b^i_1, b^i_2\right)$ where b^i_j is bidder i's bid for variant $j = 1, 2$. Since the auction rules determine for every bid vector $b = \left(b^1, ..., b^n\right)$ the price vector (p_1, p_2)

[0]We gratefully acknowledge the helpful comments by Jan Potters. The results in this chapter were first formulated in Güth, Ivanova–Stenzel and Kröger (2002), "Procurement Experiments with Unknown Costs of Quality," Discussion Paper –Economics Series–, No. 173, Humboldt Universität zu Berlin, Berlin, Germany.

with p_j for $j = 1, 2$ being the price for variant j, after the vector auction the buyer knows the prices of both variants and can thus reasonably decide whether to order variant 1 or 2 or none at all. The advantages of this proposal seem to be obvious, but it is – to the best of our knowledge – rarely used (Kröger, 2000, who reviews the legal rules of public procurement in the Federal Republic of Germany does not find any rules concerning (such uncertainties about) cost differences between variants). Actually we are only aware of examples in private procurement, usually organized by (more buyer oriented) architects.

We do not claim that quality aspects, i.e., the possibility of alternative variants, has been totally neglected. Actually the legal rules or the accepted practise has been to ask the buyer for a most detailed specifications of all quality aspects and to rule out offers not meeting them (see, e.g., Gandenberger, 1961). Our basic argument is that asking the buyer for a most detailed specification is unreasonable when the buyer does not know the cost differences of alternative specifications of essential aspects. The homogeneity requirement (all acceptable offers should meet the specification of the buyer) can thus be weakened to the variants rather than being applied to the product in general.

To compare our proposal to situations which resemble more closely the public procurement practise we also study the case when the buyer first organizes a variant 1–auction and then bargains[3] with the chosen contractor whether or not to order variant 2 instead of variant 1. Why would the buyer apply such a procedure instead of organizing a vector procurement auction? One argument would be that the technology of the winning bidder is superior in general and therefore lower prices can be expected for all quality variants. Such reasoning neglects the fact that the seller might exploit his bargaining power even when he has the best technology with lowest costs. Another answer might be that specifying more than one variant on all details by the buyer implies higher costs as it is much more work.

In the following section 5.2 we describe the two experimentally explored procurement procedures (vector and half auction). We then discuss the experimental design in section 5.3 and report its results in section 5.4. Our conclusions in section 5.5 discuss the actual practise in the light of the main findings.

5.2 Vector and Half Procurement Auctions

We now describe the two procurement procedures, which we want to study experimentally.

Let n (≥ 2) be the number of (a priori) symmetric bidders $i = 1, ..., n$. For $i = 1, ..., n$ we denote by

$$Y = \left\{ \left(c^i, d^i\right) \in [0,1]^2 : c^i < d^i \right\} \tag{5.1}$$

the set of bidder i's possible cost characteristics $\left(c^i, d^i\right)$: c^i is i's cost for variant 1 and d^i for variant 2, i.e., it is never cheaper to deliver variant 2 instead of variant 1. Further we assume that only bidder i knows his own cost characteristic (private–value case). The buyer and all his co–bidders $j \neq i$ expect $\left(c^i, d^i\right)$ to be chosen according to some positive density

$$\varphi\left(c^i, d^i\right) \text{ over } Y, \tag{5.2}$$

which is commonly known. All bidders j expect that each individual characteristic $\left(c^i, d^i\right)$ is independently and $\varphi(\cdot)$ randomly determined (iid–assumption).

Let v denote the buyer's utility of variant 1 and by w with $0 \leq v < w$ his utility for variant 2. The density $\Psi(v, w)$ guiding the selection of the pair (v, w) is assumed to be commonly known whereas the resulting vector (v, w) remains (the buyer's) private information. Both sequential decision processes assume that all former decisions (of personal players) are commonly known. After the initial (fictitious, see Harsanyi, 1967/68) chance move determining independently v, w as well as c^i, d^i for $i = 1, ..., n$ procurement is organized by either the vector auction or the half auction whose rules are now described:

(i) The **vector auction**:

1. The buyer states upper bounds $\overline{p}_1$ and $\overline{p}_2$ with $\overline{p}_1 \leq \overline{p}_2$of maximal prices for variant 1 and 2, respectively.
2. All bidders $i = 1, ..., n$ submit their vector bids $b^i = \left(b^i_1, b^i_2\right)$. The prices p_j for $j = 1, 2$ are determined by the lowest bids b^i_j.
3. The buyer selects the variant j with $\overline{p}_j \geq p_j$ $(j = 1$ or $2)$,yielding the higher profit or none at all.

(ii) The **half auction**:

1. The buyer chooses $\overline{p}_1$, the upper price limit for the cheaper variant 1.

2. All bidders $i = 1, ..., n$ submit their bids b_1^i. The bidder i with the lowest bid b_1^i becomes the contractor with $p_1 = b_1^i$ if $\overline{p}_1 \geq p_1$.
3. The buyer chooses p_2 $(\geq p_1)$, i.e., his price offer for variant 2.
4. The contractor delivers variant 2 at price p_2 if variant 2 yields a higher profit.[4]

A theoretical analysis of the two allocation mechanisms is extremely difficult since they combine the difficulties of signaling games, requiring updated beliefs, and of unusually complex auction games. In the appendix we therefore derive only the (equilibrium) incentives. The first–order conditions would imply to set the partial derivatives of these payoff functions equal to zero what, in case of the vector auction, would lead to a system of interrelated differential equations. Due to the missing subgame structure even a numerical solution of such auction mechanisms is extremely difficult. Furthermore, game theoretic benchmark solutions are not needed for the qualitative arguments supporting our main hypothesis that the vector auction is more desirable.

The models assume a Bayesian setup, based on commonly known and consistent private beliefs as specified by the densities $\Psi(\cdot)$ and $\varphi(\cdot)$ (see Harsanyi, 1968). Since in the experiment the privately known variables v and w as well as c^i and d^i for $i = 1, ..., n$ are integers (as all decision variables), an analytic solution is even more difficult to derive.

Our **main hypothesis** (other hypotheses will be discussed when actually testing them) is that the buyer will be better off using the vector instead of the half auction. Since there are as many buyer types as there are vectors (v, w) with $v < w$, the main hypothesis may not hold generally but rather only within a certain (important) region of (v, w)–pairs. If $(w - v)$, for instance, is small, i.e., when the buyer does not care so much which variant she buys, the competition via bids b_1^i to become the contractor in case of the half auction may be fiercer. Here bidders would still hope to deliver variant 2 at a high price p_2 what could result in a cheaper price of variant 1. For low values $(w - v)$ the buyer thus might prefer the half auction.

We capture the actual practise by a best–case modelling. Since the main disadvantage of the actual practise is its exclusion of competition when wanting to substitute the contracted variant, the best case for the actual practise is when competition is minimal: when there are only two bidders the lack of competition for providing variant 2 in case of the half auction is least serious. More generally, the half auction prevents $n - 1$

bidders from competing for variant 2. Clearly, for larger numbers $n-1$ the detrimental effect of the half (as compared to the vector) auction will be more serious.

When bargaining whether to consider another than the contracted variant the best case for the actual practise is to apply the most favorable bargaining rules for the buyer. This is done by assigning ultimatum power to the buyer in the half auction. The buyer makes a take it or leave it–price offer p_2 when confronting her contractor with the price for variant 2.[5] In actual life often the initiative but always the willingness to consider alternative variants rests on the buyer who will therefore often also determine the rules of bargaining. Therefore, our design is a worst–case scenario for testing the main hypothesis.

Other designs would have been more favorable for our main hypothesis. One should, however, start with a worst–case scenario. If this already confirms the main hypothesis, more favorable conditions should only strengthen the effect. Given the fact that we have a worst–case scenario we apply the rather weak requirement ($p < .1$) when testing for statistically significant effects.

5.3 Experimental Design

The instructions (see appendix 5.B) describe the decision process in each of the 30 rounds of playing the vector, respectively the half auction with three (in every round) randomly chosen partners. In each session participants first were partitioned into two groups, the group of buyers and the twice as large group of bidders. A subject remained a buyer or bidder throughout the experiment. In the vector auction the software automatically selects the variant yielding the larger positive profit. Similarly, for the half auction the contractor automatically delivers the variant yielding the larger profit for him. In case of equal profits always the higher quality, variant 2, is selected.

Information feedback was kept minimal. In addition to their private information (the random variables v and w for the buyer, the cost levels c^i and d^i for bidders $i = 1, 2$) participants learned whether and if so which variant has been delivered at which price, their profit in the present round and as well as their accumulated profit. Further information could have created additional path dependencies in which we were not particularly interested (for an experimental study which systematically varies feedback information see Huck, Oechssler, and Normann, 1999). In actual procurement procedures one often observes that bidders are informed about other bids as well. A follow–up study could provide richer feedback information.

The software of the computerized experiment was developed with the help of z–Tree (Fischbacher, 1999). To avoid negative surplus values, we have imposed (generically) disjoint parameter regions by

$$50 \leq c^i < d^i \leq 100 \leq v < w \leq 150 \quad \text{for } i = 1, 2. \tag{5.3}$$

The privately known values, the reselling values (v, w) of the buyer and the costs (c^i, d^i) of the bidders were independently drawn with all possible constellations being equally likely. All values and decision variables had to be integers.

Upon arriving at the computer laboratory (of Humboldt University Berlin) participants were randomly seated at visually isolated terminals where they found the typed instructions which they were asked to read carefully. This took on average 20 minutes. We performed 10 sessions with 2 matching groups each consisting of 6 (yielding two simultaneous plays) participants each; 10 matching groups for both institutions,[6] i.e., on one session 12 participants took place. On average a session needed 100 minutes. The uniform show up–fee was DM 10 per participant. As shown in Table 5.1 participants earned on average as buyers DM 29.17 and as bidders DM 14.96 (excluding their show up–fee.).

5.4 Results

Will a buyer fare better with the vector rather than with the half auction in spite of the worst–case scenario with just two bidders (where excluding competition for providing variant 2 is least serious) and ultimatum power of the buyer (when bargaining with the contractor whether variant 1 or 2 should be delivered)? Pooling all data (10 matching groups $\times$ 2 plays $\times$ 30 rounds = 600 procurement auctions in the vector as well as in the half auction institution) confirms our basic intuition.

5.4.1 Prices

In the half auction, the buyer cannot rely on competition when negotiating for the second variant. Ultimatum power may allow to exploit her contractor but not to switch to the other supplier if this supplier has lower costs for variant 2. Furthermore, the buyer does not know how much more costly variant 2 is for her contractor, which makes exploiting ultimatum power very risky. This explains why the mean price of variant 2 over all sessions of the half auction (86.23) exceeds the mean price of variant 2 in the vector auction (83.36). This is illustrated by figure 5.1 and can be (weakly) statistically corroborated (Mann–Whitney U test, henceforth: MWU, one–tailed, $p =$ .095).[7]

The mean price of variant 1 in Figure 5.1 is higher for the vector auction (69.52) than for the half auction (67.30) in 7 out of 10 sessions (MWU, one–tailed, $p = .083$). In the half auction bidders may bid more aggressively for variant 1 since they hope to supply variant 2 at an even higher profit. This is also confirmed by the higher average bidders' earnings in case of the vector auction when variant 1 is provided (MWU, one–tailed, $p = .072$).

Observation 1: If variant 2 is ordered, the buyer pays significantly less in case of the vector auction whereas the half auction induces significantly lower prices for variant 1.

5.4.2 Profits

Table 5.2 reports profit numbers for buyer and bidders. There is no difference in profits for the buyer regardless the auction type used and what variant bought (MWU, one–tailed, $p = .34$ and $p = .29$ for variant 1 and 2, respectively). Bidders' profits in case of variant 1 in the half auction are even significantly lower than in the vector auction ($p = .072$).

The analysis so far is a rather global one. As already discussed in section 5.2 the main hypothesis may hold or be strongly confirmed only for certain regions (e.g., with large $(w - v)$ values) of (v, w)–pairs or, as already suggested by the global effects, for variant 2 only. We distinguish 4 regions of (v, w)–pairs (see Table 5.3), namely of small and large values v, respectively value differences $(w - v)$ where we separate "small" and "large" by median splits of the altogether 600 randomly selected pairs (v, w). The median v–value is 116 and for the $(w - v)$–level 16. The average low (high) v–value is 107 (128) whereas the average low (high) $(w - v)$–level is 8 (28). According to Table 5.3 our intuition is confirmed: For large $(w - v)$–values the buyer is considerably better off with the vector than the half auction whereas for small value differences this is reversed. Comparing the two (vector and half auction) distributions of buyer's profits separately for small and large $(w - v)$–values (MWU, one-tailed, $p = .112$ for small, $p = .009$ for large $(w - v)$–values) corroborates the effects statistically. This justifies our

Observation 2: As claimed by our main hypothesis the buyer is better off when using the vector rather than the half auction, especially for certain (important) regions (with large $(w-v)$–values) of (v,w)–pairs.

Behaviorally there exists no conflict of interest regarding the auction types since also the bidders fare (partly significantly) better with the vector rather than the half auction. This result indicates efficiency gains in the vector compared to the half auction.

5.4.3 Efficiency

To illustrate the realized surplus per trade, i.e., the sum of the bidders' and the buyer's profit, for both auction types, figure 5.2 compares the realized surplus with the maximum surplus of all 600 plays. Clearly the realized surplus of the vector auction is closer to the optimum than that of the half auction. Actually, differences between maximal and actual surplus are significantly smaller for the vector than for the half auction (MWU, one–tailed, $p = .000$).

Efficiency requires that the maximal surplus

$$E = \max\{v - \min\{c^1, c^2\}, w - \min\{d^1, d^2\}\}$$

is realized. Thus inefficiencies can result from

(i) delivering the less efficient variant,

(ii) selecting the more costly bidder, and

(iii) not delivering at all.

In case of the vector (half) auction 409 (332) of the 600 plays were efficient. One classification of all inefficient plays is given in table 5.4. Of the altogether 191 (268) inefficient plays in the vector (half) auction 102 (136) occurred during the first 15 periods (the three reasons (i), (ii), and (iii) for early and late plays are also listed in table 5.4). Thus the likelihood of inefficient deals does not decrease with experience.

An inefficiency may of course, be minor. Denote therefore by

$$E(k) = \begin{cases} v - c^{j(k)} & \text{if variant 1 is provided} \\ w - d^{j(k)} & \text{if variant 2 is provided} \\ 0 & \text{in case of no delivery} \end{cases}$$

the surplus of the actual play k where $j(k)$ is the contractor of the k–th play. Table 5.5 lists the average efficiency rate $E(k)/E$ of all inefficient plays and of all inefficient deals (excluding reason (iii)), separately for early and late plays and for both auction types. All experience effects are minor and partly contradictory. Therefore, we conclude:

Observation 3: Efficiency is usually quite large (above 2/3 of the maximal surplus) but does not increase with experience. Especially, the efficiency of both institutions (vector-, half auction) does not differ much. The causes of inefficiencies, however, partly differ between institution (for the half auction delivering the wrong variant is, for instance, much more important.)

We can illustrate the different efficiency of both auction types by the rate of efficiency per trade, namely the ratio of the realized and the maximum surplus. There are fewer inefficient sales in the vector auction (150) than in the half auction (224). Furthermore, these deals are more profitable in case of the vector auction. The graph of the efficiency rate for all sales is shown in Figure 5.3 for both mechanisms.

Variant 2 is ordered less frequently in the half auction (32.4% of all sales) than in the vector auction (52.2% of all sales). The total number of realized sales of the second variant in the half auction (180) is significantly smaller than in the vector auction (292) (MWU, one–tailed, $p = .000$). Similarly there are significantly more sales of variant 1 in the half auction (376) than in the vector auction (267) (MWU, one–tailed, $p = .000$). There is no significant difference between auction types with respect to the "no transaction"–frequencies (see table 5.6 which counts sales of variant 1, respectively 2 and the "no transaction"–frequencies in both auction types for all 10 sessions).

5.4.4 Bidders and Buyers

In the following we will investigate the behavior of sellers and buyers, their bids and upper price limits. Since for both variants the price is the lowest bid, any bidding strategy $b^i(c^i, d^i)$ with $b_1^i(c^i, d^i) < c^i$ or $b_2^i(c^i, d^i) < d^i$ is weakly dominated as far as the vector auction is concerned. Aiming at a positive profit requires $b_1^i(c^i, d^i) > c^i$ or $b_2^i(c^i, d^i) > d^i$. The scatterplots of all bids (see figures 5.4 and 5.5) reveal that the overbidding incentives were nearly always understood. Every bidder was aware of the upper price limits $\overline{p}_1$ and $\overline{p}_2$ and that exceeding bids would be disregarded. Hence, a bidder making an invalid bid in range $[\overline{p}_k + \varepsilon; 150]$, with $\varepsilon > 0$ and $k = 1, 2$, is effectively refusing to participate in the auction. In the following invalid bids (b_1^i or b_2^i)

are excluded from the analysis. In the half auction the number of generally valid bids (bid b_1^i is valid) are 983 (out of 1200 bids). Out of the 956 generally valid bids (at least bid b_1^i or b_2^i is valid) in the vector auction 846 and 721 were placed for variant 1 and 2, respectively.

Table 5.7 lists the total number of bids below cost and thereby realized profits. In the half auction this happens 54 times[8] and can be justified by the chance to deliver variant 2 instead of variant 1. Placing bids below cost in the vector auction is clearly non–optimal. Only 0.1% (1 of 846) and 0.6% (4 of 721) of the bids, for variant 1 and 2, respectively, in the vector auction are below cost. From table 5.7 one can see that the 18 realized transactions in the half auction yielded on average a minor positive payoff (1.11 ECU). This corroborates the hypothesis of different incentives and different strategies in the two auction types when bidding for variant 1.

By imposing upper price limits lower than the maximal cost of 100 the buyer can restrict the bidder's attempts of exploitation by strategic overbidding (see Riley and Samuelson, 1981). How upper price limits depend on the value parameters v, respectively w of the buyer is illustrated by the scatterplots in figures 5.6 and 5.7. Buyers seem to behave similarly in the vector and the half auction. We could not observe any other differences in the buyer's behavior regarding the upper price limit $\overline{p}_1$. However, different buyers use different strategies in placing their upper price limits. Some buyers (25% , both auction types) do not actively restrict the bidders' amount of overbidding (they chose price limits near 100 ECU). Other buyers take the risk that no transaction takes place by stating lower upper price limits.

5.5 Concluding Remarks

We compare procurement via two procedures, the vector and the half auction. In the vector auction each bidder places two bids, one for variant 1 and one for variant 2. In the half auction bidders first compete for delivering the cheaper variant 1. Then the contractor is confronted by the buyer with a price offer for the higher quality variant 2. In actual procurement the contractor himself often makes a take it or leave it–offer for quality improvements. We designed the half auction as a worst–case scenario for testing our main hypothesis that the vector auction should be preferred (at least by the buyer).

We find that for large quality differences the vector auction performs better than the half auction. Whereas there seems to be no impact from the auction format on the buyers' profits if only small quality improvements can be achieved by establishing variant 2 instead of variant 1. Compared to the vector auction the half auction mechanism is less efficient due to its too few sales of the higher quality variant 2. Thus more competition, as in the vector auction, leads to higher efficiency, more product variety and cheaper prices for the higher quality variant. Which also explains why the need for the vector auction is less strong if the differences between both variants are rather small.

We, therefore, propose that a vector auction should be applied when quality differences of variants are large and there is uncertainty about different costs of two or more variants in procurement. For small quality improvements the half auction is justifiable when preparing a vector auction is more costly. This might be a reasonable concern as two instead of one variant have to be precisely specified.

In actual procurement buyers often become aware of another version of the product only during or even after the auction. In such a case it would not be possible to perform a vector auction. Therefore, it seems to be crucial to prepare the procurement more carefully and consider several variants before calling for bids.

Appendix to Chapter 5

5.A Incentives of Vector and Half Auctions

We describe the incentives implied by a symmetric and monotonic equilibrium

$$b^i\left(c^i,d^i\right)=\left(f\left(c^i,d^i\right),g\left(c^i,d^i\right)\right) \tag{5.4}$$

for all $\left(c^i,d^i\right)\in Y$ and $i=1,...,n$ of a **vector auction**. Denote for $b_1^i\leq\overline{p}_1$ and $b_2^i\leq\overline{p}_2$ by

$$P_1\left(b^i\right)=Prob\left\{\begin{array}{l} b_1^i\leq f\left(c^j,d^j\right)\text{ for } j\neq i\text{ and}\\ v-b_1^i>w-\min\left\{b_2^i,g\left(c^j,d^j\right)\text{ for } j\neq i\right\}\end{array}\right\} \tag{5.5}$$

the probability that i with bid b^i delivers variant 1 and by

$$P_2\left(b^i\right)=Prob\left\{\begin{array}{l} b_2^i\leq g\left(c^j,d^j\right)\text{ for } j\neq i\text{ and}\\ w-b_2^i\geq v-\min\left\{b_1^i,f\left(c^j,d^j\right)\text{ for } j\neq i\right\}\end{array}\right\} \tag{5.6}$$

the probability that i delivers variant 2. With the help of this notation bidder i's payoff expectation if he bids $b^i=\left(b_1^i,b_2^i\right)$ and all other bidders $j\neq i$ behave according to $(f(\cdot),g(\cdot))$ can be written as

$$E\left(b^i\mid c^i,d^i\right)=\left(b_1^i-c^i\right)P_1\left(b^i\right)+\left(b_2^i-d^i\right)P_2\left(b^i\right). \tag{5.7}$$

Whereas the profits $b_1^i-c^i$ and $b_2^i-d^i$ increase with the respective bid the winning probabilities $P_1\left(b^i\right)$ and $P_2\left(b^i\right)$, respectively, will typically decrease. As in usual auctions bidders must thus balance profits and winning probabilities.

For the **half auction** let i be the bidder who has won the auction for variant 1, i.e., whose bid $b_1^i=p_1$ for variant 1 has been the lowest. The price offer p_2 for variant 2 will be accepted by bidder i if

$$p_1-c^i\leq p_2-d^i, \tag{5.8}$$

otherwise variant 1 will be delivered. Denote by

$$P\left(p_2|p_1\right)=Prob\left\{p_1-\left(p_2-d^i\right)\leq c^i\right\}, \tag{5.9}$$

i.e., the buyer's posterior (after learning about p_1) probability for (5.8). The buyer chooses $p_2^*=p_2^*\left(p_1\right)$ maximizing

$$(v-p_1)\left(1-P\left(p_2|p_1\right)\right)+(w-p_2)\,P\left(p_2|p_1\right). \tag{5.10}$$

On stage (ii.2) a bidder $j=1,...,n$ of type $\left(c^j,d^j\right)$ bidding b_1^j wins, i.e., becomes the contractor i, if $b_1^j\leq h\left(c^k,d^k\right)$ for all $k\neq j$ where $b_1^k=h\left(c^k,d^k\right)$ denotes the symmetric and monotonic equilibrium bid function for variant 1. If he wins, he delivers variant 2 with probability $P\left(p_2^*|b_1^j\right)$. The payoff expectation resulting from b_1^j is

$$E\left(b_1^j \mid c^j, d^j\right) = \int\limits_{b_1^j \leq h\left(c^k, d^k\right), \quad k \neq j} [P\left(p_2^*|b_1^j\right)\left(p_2^*\left(b_1^j\right) - d^j\right) + \left(1 - P\left(p_2^*|b_1^j\right)\right)\left(b_1^j - c^j\right)]\varphi\left(c^k, d^k\right) d\left(c^k, d^k\right). \tag{5.11}$$

Thus bidding lowest for variant 1 offers the additional chance to provide variant 2 whenever this is profitable.

The symmetric and monotonic equilibrium bid functions will, of course, depend on $\overline{p}_1$ (and $\overline{p}_2$ in case of the vector auction). The derivation of such symmetric equilibria is extremely difficult and might at present only be possible by applying numerical methods. But even this would be extremely difficult.

5.B Instructions (Translation)

Instructions (Vector tender)[9]

Please read the instructions carefully. If you have any question please raise your hand. We will try to answer your question privately. All participants have received identical instructions.

During the experiment you will participate in several tenders. The experiment consists of 30 periods. In every period three persons (two bidders and one buyer) negotiate about trading a commodity of which two different variants are possible.

In the beginning of the experiment you are randomly assigned to a role (buyer or bidder). You keep this role for the entire experiment.

The composition of a group (two bidders, one buyer) will usually change between periods since buyers and two bidders are randomly matched in each period. Every single period (tender) proceeds as follows:

The buyer is informed about his individual reselling values v and w for variant 1 and 2 of the commodity, respectively. The values of v and w are randomly determined in the range of 100 and 150. The inequality $v < w$ always holds. Knowing these individual reselling values the buyer places upper price limits for variants 1 and 2, i.e. what he is at most willing to pay. The buyer is later on not allowed to accept offers above these limit prices.

The bidders have production costs c (for variant 1) and d (for variant 2) when they deliver the commodity. The values c and d are randomly determined in the range 50 and 100. The inequality $c < d$ always holds. The bidders learn the buyer's upper price limits for variant 1 and 2. Knowing their own production costs and the upper price limits the bidders ($i = 1, 2$) place their bids (price offers) for both variants ($j = 1, 2$) in the range from 0 to 150.

bids (b_j^i)	bidder 1	bidder 2
variant 1	b_1^1	b_1^2
variant 2	b_2^1	b_2^2

Outcome of the tender:

The lowest bid for a variant, below or equal to the buyer's price limit, determines its price.

1. If for both variants there exists no such a bid, no sale takes place. In this case everybody's (the buyer's and the bidders') payoff is zero.

2. If there exist such bids, the variant yielding the higher profit for the buyer is chosen. The corresponding bidder (whose bid for this variant is lowest) is the winner of the tender. If both variants guarantee the same profit for the buyer variant 2, the higher quality, is selected. If the lowest bid is chosen by both bidders, the contractor will be chosen randomly. The price is determined by the lowest bid.
The contractor delivers the chosen variant and has to pay the corresponding individual production costs c and d for variant 1 and 2, respectively, and collects

the price (his bid for the chosen variant). The buyer has to pay the price and receives the corresponding individual reselling value v and w for variant 1 and 2, respectively. The other bidder does not deliver and thus makes 0–profit in this period.
You learn:
· whether you are the contractor;
· which variant was delivered;
· the price;
· your profit in this period;
· your total profit until this period.
For the chosen variant and depending on your role you are informed again about:
· as a buyer: reselling value and the upper price limit;
· as a bidder: production cost, bid (price offer).

You will not get to know the privately known reselling values, resp. production costs of other participants.

The privately known reselling values of the buyer and costs of the bidders are all independent. Every constellation (v, w) with $100 \leq v < w \leq 150$ and (c, d) with $50 \leq c < d \leq 100$ is equally likely. Thus the costs c and d of the two bidders will usually be different. Each participant learns only his own parameters (v and w as a buyer, c and d as a bidder) and not the ones of the other participants.

Please, notice that losses are possible!
For the buyer this can happen when the limit prices exceed his reselling values.
For the bidders losses can occur when the bids are below the costs. Thus every participant can avoid any risk of losses by deciding accordingly: The buyer by avoiding limit prices above the reselling values; the bidders by not bidding below costs.

You will type your decisions into the computer. You will learn nothing about the identity of the other participants during the experiment since decisions are anonymous. All reselling values, costs and profits are in ECU (Experimental Currency Unit). Your valid exchange rate from ECU to DM is given on the screen. You receive an initial endowment of 10.00 DM to cover possible losses. This will be added to your profits (or losses) in the 30 rounds. There will be a test period at the beginning. The results of this test period will not matter for your total profit.

[*Only different parts of the*] Instructions (Half tender)

Every period will proceed as follows:

First there is a tender for variant 1:

The buyer is informed about his individual reselling values v and w for variant 1 and 2 of the commodity, respectively. The values of v and w are randomly determined in the range of 100 and 150. The inequality $v < w$ always holds. Knowing these individual reselling values the buyer places an upper price limit for variant 1, i.e. what he is at most willing to pay for variant 1. The buyer is later on not allowed to take any price offer above this value.

The bidders have production costs c (for variant 1) and d (for variant 2) when they deliver the commodity. The values c and d are randomly determined in the range 50 and 100. The inequality $c < d$ always holds. The bidders learn the upper price limit for variant 1. Knowing their own production costs and the buyer's upper price limit the bidders place their bids (price offers) in the range from 0 to 150 for the first variant.

Outcome of the tender:

The lowest bid below or equal to the buyer's upper price limit determines the price.

1. If there exists not such a bid, no sale takes place. In this case everybody's (the buyer's and the bidders') profit is zero. **The period is over**.

2. If there exists such a bid, the corresponding bidder is the winner of the tender. If the lowest bid is chosen by both bidders the contractor will be chosen randomly. The price is determined by the lowest bid. The other bidder does not deliver, does not produce and thus makes 0–profit in this period.
You will be informed:
· whether you have been chosen to deliver variant 1;
· the price;
Depending on your role you are again informed about:
· as a buyer: reselling value and the upper price limit;
· as a bidder: production cost, bid (price offer)
You will not get to know the privately known reselling values or production costs for variant 1 of other participants.
For the bidder, who has not won the tender, the period is over.

The period continues as follows: The buyer offers the contractor a price for variant 2. The offer will be accepted if it yields at least the same profit as variant 1 for the contractor. Otherwise variant 1 is sold at its agreed upon price.
The winner of the tender delivers the agreed upon variant and has to pay his corresponding production costs c for variant 1 or d for variant 2 and collects the price (bid for variant 1 , resp. the buyer's price offer for variant 2). The buyer has to pay the price and receives the corresponding individual reselling value v for variant 1 or w for variant 2.

You will be informed:
· which variant was finally delivered;
· the price of the delivered variant;
· your profit in this period;
· you total profit until this period.

Depending on your role you are informed again for the chosen variant about:
· as a buyer: reselling value and the upper price limit;
· as a bidder: production cost, bid (price offer).

You will not get to know the privately known reselling values or production costs of the other participants.

5.C Tables

Auction type		Buyer	Bidder	Both
	Min	21.84	3.80	
Vector	Max	33.12	27.80	
	Mean	29.70	14.56	19.61
	Min	20.74	2.60	
Half	Max	31.82	31.60	
	Mean	28.65	15.35	19.78
Both	Mean	29.17	14.96	19.69

Table 5.1: Experimental payoff statistics in DM (show–up fee excluded)

	Buyer Auction type		Bidder Auction type	
Variant	Vector	Half	Vector	Half
1	52.33	51.55	11.04	8.96
2	53.86	51.47	9.82	8.92
Both	53.13	51.52	10.41	8.95

Table 5.2: Buyer's and bidders' profit

		Vector auction Distance $(w-v)$				Half auction Distance $(w-v)$			
		small		large		small		large	
v	Variant	Profit	N	Profit	N	Profit	N	Profit	N
	no sale	0	(8)	0	(22)	0	(15)	0	(14)
small	1	39.07	(55)	42.12	(42)	41.73	(64)	40.32	(109)
	2	39.26	(35)	53.39	(138)	41.05	(19)	48.81	(79)
	no sale	0	(6)	0	(5)	0	(10)	0	(5)
large	1	59.81	(154)	52.81	(16)	62.83	(162)	52.12	(41)
	2	55.28	(47)	60.94	(72)	54.46	(35)	57.92	(47)
	no sale	0	(14)	0	(27)	0	(25)	0	(19)
all	1	54.35	(209)	45.07	(58)	56.85	(226)	43.55	(150)
	2	48.44	(82)	55.98	(210)	49.74	(54)	52.21	(126)
all	both variants	52.69	(291)	53.62	(268)	55.48	(280)	47.5	(276)
average profit			53.13	(559)			51.52	(556)	

Table 5.3: Buyer's average profit for small and large values v and value differences $(w-v)$ (N - number of observations)

Auction type	Reason	Periods 1 – 15	16 – 30	all
	(i)	22	21	43
vector	(ii)	35	24	59
	(i+ii)	27	21	48
	(iii)	18	23	41
	(i)	54	49	103
half	(ii)	24	21	45
	(i+ii)	40	36	76
	(iii)	18	26	44

Table 5.4: Classification of all inefficient plays

Inefficiency	all			without (iii)		
Periods	1 – 15	16 – 30	all	1 – 15	16 – 30	all
Vector auction	0.697	0.672	0.685	0.856	0.890	0.872
Half auction	0.700	0.656	0.678	0.807	0.816	0.811

Table 5.5: Average efficiency rate of inefficient plays

	Variant 1		Variant 2		No Transaction	
Session	Vector auction	Half auction	Vector auction	Half auction	Vector auction	Half auction
1	25	40	31	18	4	2
2	33	42	25	16	2	2
3	29	34	26	19	5	7
4	26	45	29	12	5	3
5	22	38	28	16	10	6
6	29	40	26	19	5	1
7	27	38	29	19	4	3
8	26	28	33	24	1	8
9	25	34	34	17	1	9
10	25	37	31	20	4	3
all	267	376	292	180	41	44

Table 5.6: Number of transactions

		Number of bids		Profit		
	Variant	Total	Realized	Min	Max	Mean
Half auction	1	54	18	−28	27	1.11
Vector auction	1	1	0	–	–	–
	2	4	2	−20	−4	−12

Table 5.7: Statistics of bids placed below cost and realized corresponding profits

5.D Figures

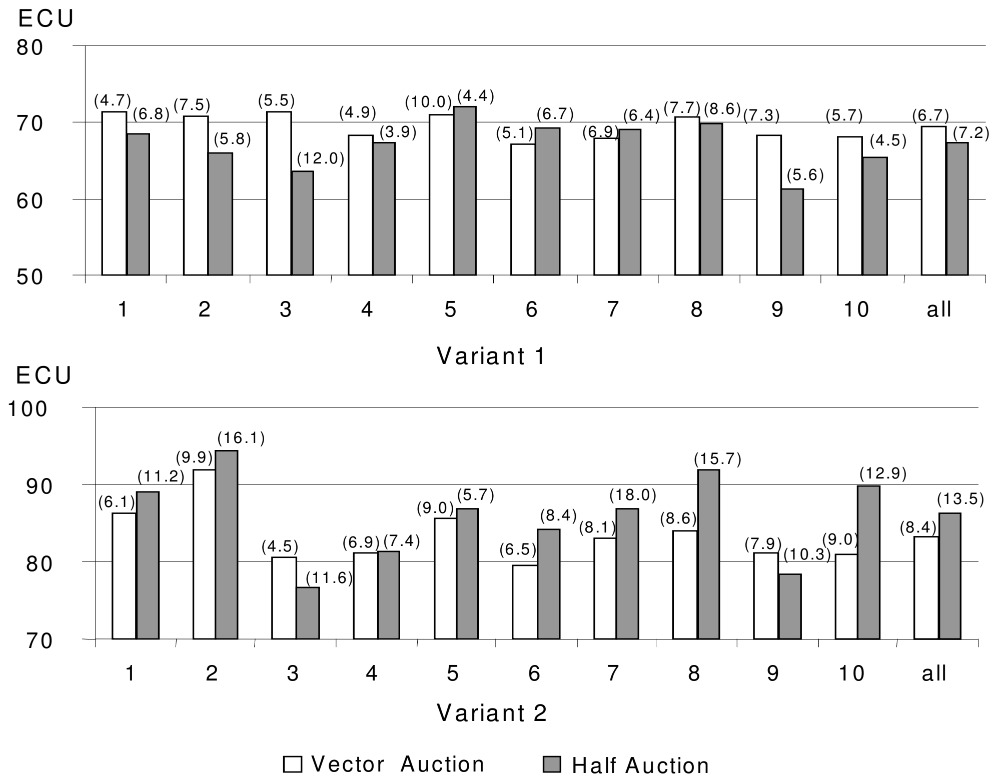

Figure 5.1: Mean prices (std. deviation in brackets, numbers on the abscises refer to the different sessions)

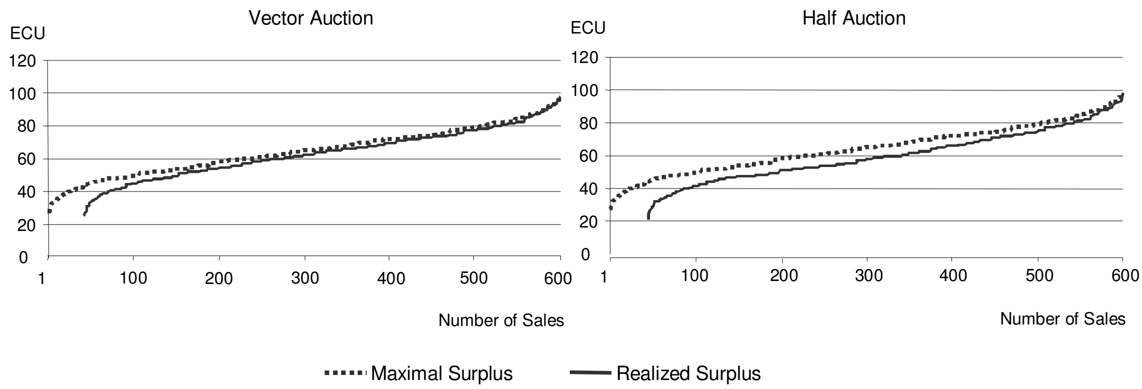

Figure 5.2: Cumulative distributions of realized and maximum surplus

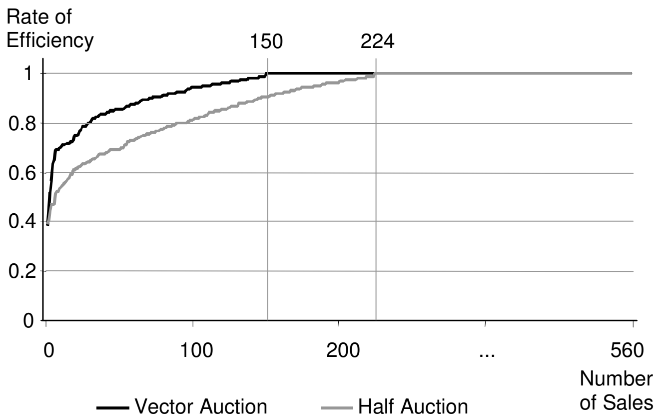

Figure 5.3: Efficiency rates

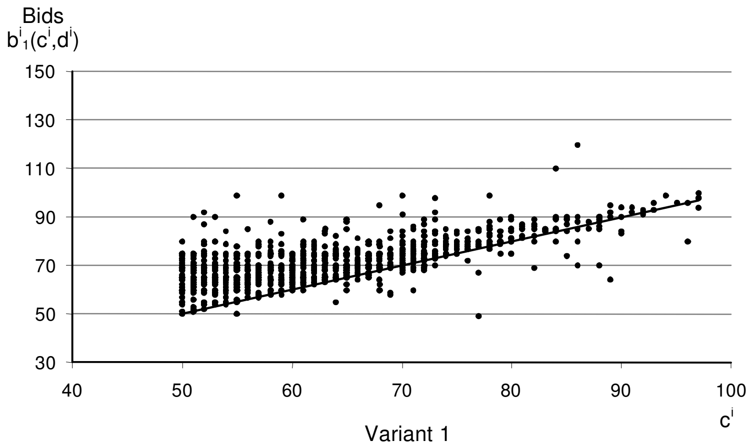

Figure 5.4: Scatter plots illustrating systematic tendencies of overbidding: half auction

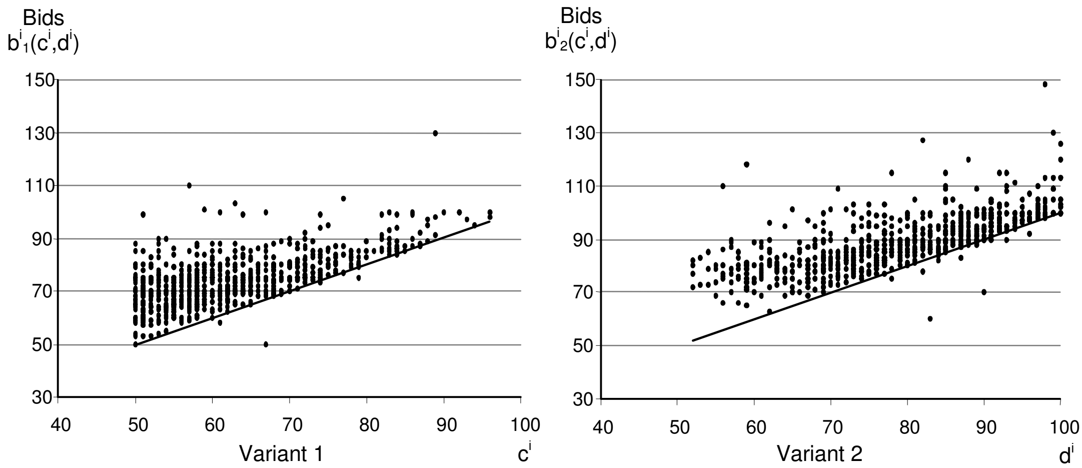

Figure 5.5: Scatter plots illustrating systematic tendencies of overbidding: vector auction

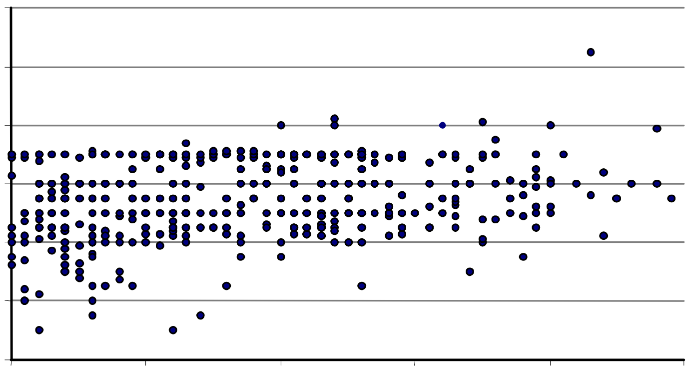

Figure 5.6: Scatter plots of upper price limits $\overline{p}_1$in the half auction

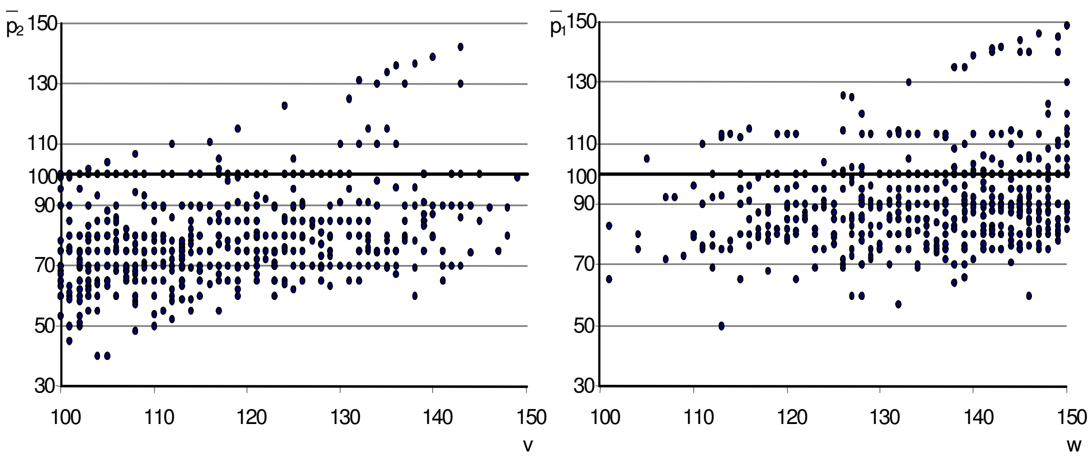

Figure 5.7: Scatter plots of upper price limits $\overline{p}_1$ and $\overline{p}_2$ in the vector auction

Notes

[1]In their theoretical study McAfee and McMillan (1986) also allow that even the potential construction firms do not know all their costs but only its basic component (the experimental investigation by Cox, Chech, Conn, and Isaac (1996) is based on their study).

[2]Theoretical and experimental studies of procurement procedures like Cox, Isaac, Chech, and Conn (1996), Holt (1980), McAfee and McMillan (1986) exclude such uncertainty about which variant one wants to buy. Their basic problem is that the cost of the uniquely defined product is more (for the bidders) or less (for the buyer) certain, an aspect which our study neglects. Other studies investigate multidimensional auctions. A bidder submits bids with characteristics of the product and the price. The buyer evaluates bids using a weighting function (scoring rule) for the different features of the bid (McAfee and McMillan, 1987, Che, 1993). In our study bidders compete about prices for different quality levels which are predetermined by the buyer. Bidders only differ in their cost structure.

[3]Since we want to prove the superiority of the vector auction we assume the most favorable negotiation rules for the buyer in the half auction, i.e., the worst case for our hypothesis.

[4]Thus we do not only assign ultimatum power to the buyer but also do not burden him with the fear that the offer p_2 will be rejected because of social concerns of the contractor. We want to prove our main hypothesis in a worst–case scenario.

[5]Although there is quite a tradition of ultimatum experiments with incomplete information (see Roth, 1995, for a survey), none of the studies corresponds to the situation on the second stage of the half auction. Here neither the buyer (the proposer in the terminology of ultimatum bargaining) nor his contractor (the responder) knows the size of the "pie" which may be even negative, namely when the positive value difference $(w - v)$ is small but the cost difference $d^j - c^j$ of his contractor j large. It is therefore questionable whether the usual regularities of ultimatum experiments apply here. The embedding of ultimatum bargaining may, furthermore, matter (ultimatum games as subgames of larger games usually yield different experimental results, see Güth and Tietz, 1990).

[6]One may suspect that this could induce repeated game–effects. We, however, think that in a situation where at least one party is left empty–handed, such effects are less likely. This rematching within small groups of 6 participants is justifiable.

[7]Since we have chosen only for one institution randomly the individual characteristics and then applied the same sequence of individual characteristics for the other institution (in order to eliminate stochastic effects) not only one session of one institution corresponds to one of the other institution but also their ordering. This explains why in figure 5.1 a session of the vector auction can be compared with the same session of the half auction.

[8]Actually there are 57 cases but 3 of them are disregarded since the subjects who submitted them mentioned in the post–experimental questionnaire that these bids were done by mistake.

[9]This is a shortened translation of the instructions. The original (German) instructions are available at the authors upon request.

Chapter 6

Pricing of Information Goods

6.1 Introduction

To enjoy "Alice in Wonderland" by Lewis Carroll one can either buy a book in a bookstore, make paper copies, or scan the book into a data file. In the latter case, the data file is stored in a computer and can be transmitted as electronic version, either on CD or via the internet from which it finally can be read on a computer screen or printed again. Alice in Wonderland is an example of an information good. This class of goods includes amongst others, literature, music, and software. Information goods may be defined as any good that can be redistributed in its original format or in a digitalized way at zero marginal cost (Shapiro and Varian, 1999).

It is well established that information goods are an important factor for a society's progress. Policy interventions aimed at stimulating their creation are justified by the concern for their underprovision. Is such reasoning, which is generally a concern for public goods, convincing? It will be if information goods also exhibit non–rivalry and non–excludability features which characterize public goods. The nature of information goods guarantees their non–rivalry as consumption does not reduce their availability. Establishing that they are non–excludable is less straightforward as unlimited access is narrowed by distribution restrictions and reproduction costs which have lead Novos and Waldmann (1984) to describe information goods as being "partially" non–excludable. However, cheap copying opportunities and publicly available networks like the internet reduce reproduction costs which enhances the non–excludability fea-

[0]The author is grateful to Dominikus Gerst for programming and help conducting the experiment. Also, the author is indebted to Charles Bellemare and Jan Potters for constructive comments and suggestions. The results in this chapter were first formulated in Kröger (2003), "How to Sell Information Goods."

ture of information goods.[1] Hence, it is likely that underprovision is also present in such markets.

The cost structure characterized by high investment costs required to produce an information good prototype and negligible copying costs for reproduction is one of the explanation for the underprovision of information goods. As a result, innovators bear the risk of low returns on investments because others might reproduce their creation and undercut the price. By granting the right to control reproduction and redistribution, intellectual property protection shields them against the risk of being copied for a limited amount of time. These rights grant the innovator a monopoly position. If welfare is the measure with which artificial exclusion is justified, welfare losses due to monopoly pricing, such as underutilization, have to be taken into account. Monopoly pricing excludes consumers who cannot afford current prices but would be willing to pay prices at the competitive level. Copy protection artificially reintroduces excludability in information product markets and channels the counterbalancing forces of welfare increase due to reduction of underproduction of information goods and welfare decrease due to monopoly pricing. The literature investigating economic issues of intellectual property rights discusses the trade off between incentivizing producers and restricting the (re)use of information goods by patents, copyright laws, trade marks, and trade secrets (see Besen and Raskind, 1991, and Gordon, 2003).[2]

Economists who have considered this type of markets typically assume that consumers are price–takers and compare on this basis profits of institutions with and without intellectual property protection. The welfare optimal incentive compatible legal system in the presence of investment costs is derived from this comparison (see Louvry, 1979). What one, however, observes in reality is that consumers reject profitable offers, they withhold their demand. The assumption that consumers behave as passive price takers has been questioned already by Galbraith (1952). Lately, consequences of demand withholding have been investigated theoretically (see for instance Snyder, 1998, and Inderst and Wey, 2002), but also analyzed experimentally in oligopoly markets (Ruffle, 2000) and monopoly markets (Engle–Warnick and Ruffle, 2002). Demand withholding is costly as gains of potential trades are lost. Consumers forgo the difference of valuation and price and sellers cannot realize the difference of price and costs.

Fairness considerations of consumers can account as one reason for demand withholding and might originate in the conflict of *how* to share the gain between seller and buyers. Fairness as a constraint on profit seeking has been investigated by Kahneman, Knetsch, and Thaler (1986). The authors conclude that the role of reputation effects is widely recognized as an explanation of 'fair' pricing, but also the willingness to punish unfairness and intrinsic motivation to be fair can contribute to fair behavior in the marketplace. Fairness considerations might receive even higher attention for the purchase of products under consideration as pricing of information goods is solely oriented on consumers' valuation. The cost structure of information goods does not allow cost oriented pricing but rather implies that prices are set according to the valuation of consumers.

In light of the presence of demand withholding, comparing markets with and without protection of information goods under the assumption of price–taking consumers might be questionable. Demand withholding in a market with protection and fixed prices might lead to an increase in welfare losses. On the other hand demand withholding could also decrease welfare losses as sellers might lower their prices and therefore make the product affordable to consumers who would otherwise be excluded by monopoly prices. The impacts of such consumer behavior in a market without protection are also not apparent. Demand withholding is less costly for an individual consumer than in a market with protection and might therefore increase. The reasons for lower individual costs are twofold: First, prices are predicted to be higher in a market without protection. This leads to lower gains from trade and thereby lower costs in case of rejecting the price for the consumer than in a market with protection. Second, in case of lower prices, there might also be a possibility that other consumers will purchase the good as result of which one could copy the product instead of buying directly from the producer. The second aspect introduces a strategic component as consumers might even gain from demand withholding. If, on the other hand, fairness considerations are important buyers might also accept to transfer part of their surplus in case of lower prices to the seller rather than only thinking of their own consumer rent, despite the strategic component of demand withholding. Therefore, it is less clear whether an environment without intellectual property protection would ensure less coverage of investment costs. In this sense price setting and welfare implications in information good markets seem to be not well understood. To draw conclusions about welfare implications of different environments one should analyze the outcome of in the different institutions given human behavior.

In this chapter, we study the impact of excludability on the outcome of information good markets: the behavior of a monopolist and consumers, achieved welfare and surplus shares. We investigate two different legal settings, resulting in markets with a monopoly or a public good structure. A market where only buyers can use the product is compared to a market where everybody is able to benefit from the good when it is bought at least once. The proposed model is tested experimentally. We do not explicitly investigate the production side of the market and take investment costs as sunk. Nevertheless, observed profits allow indirectly conclusions on returns on investment.

The results can be summarized as follows. Theory would predict that prices are higher in the market without exclusion, which is corroborated by the behavior of some sellers. Furthermore, we observe sellers who do not change their price setting behavior across the two institutions, and some who even adjust in a opposite way, i.e., set lower prices in the market without exclusion. Interestingly, behavior of the latter seems to be a best reply given the observed demand behavior. Buyers react to the fact that demand withholding in a market with non–exclusion is less costly by increasing it. As a result, prices that would maximize a seller's profit, are lower in the market without exclusion than in the market with exclusion. Finally, we find that welfare is about the same in the two markets.

The chapter is organized as follows. Section 6.2 presents the model. Section 6.3 describes the design and procedure of the experiment. Results are presented in section 6.4 and in section 6.5 closes with a discussion.

6.2 The Model

In real world situations, producers take investment costs, expectations about the demand curve, and expected profits under the existing legal system into account when deciding whether or not to invest into the production of an information good. In this chapter, we consider producers who have already undertaken their investment and focus on the interactions in markets with different institutional settings. For simplicity, we assume the demand curve as given and the production costs as sunk at the moment of selling.[3]

We will distinguish between two institutional settings, the *Pro* and the *Free* market environment. In the *Pro* market, usage of the information good is protected and exclusively restricted to buyers; reproduction is legally impossible. In the *Free* market, usage of the information good is not limited to buyers. Consumers can copy the information good at a low cost. Approaching the problem from a dynamic perspective, all initial buyers become sellers in the following period, a process which, when repeated, would ultimately drive down the price of the good towards its competitive price level (the costs of reproduction), which we normalize to zero.[4] In the static model at hand, this feature of dynamic markets is incorporated by assuming that once the product is purchased by at least one consumer, all remaining consumers who have not bought receive the good for a price of zero.

To formalize the model we consider a monopoly facing a linear downward sloping demand curve with complete information (i.e., all agents know the rules of the market and are informed about the distribution of all consumers' valuation for the product). The market consists of a seller S producing a single good, and three consumers L, M, H whose valuations of the information good satisfy the following inequalities $0 < v_L < v_M < v_H$. The seller posts a take-it-or-leave-it price offer to consumers who must decide simultaneously whether or not to buy the good. Below, we derive equilibrium strategies for consumers and sellers for the parameter values[5] $v_L = 10, v_M = 20, v_H = 30$.

Let δ_i denote a binary indicator taking a value of 1 if consumer i pays the price announced and 0 otherwise. Furthermore, let φ denote a binary indicator taking the value of 1 when the good is sold to at least one consumer and 0 otherwise (i.e., $\varphi = 1$ if $\sum_i \delta_i > 0$ and $\varphi = 0$ if $\sum_i \delta_i = 0$ for $i = L, M, H$). The payoff of the seller can be expressed as $\Pi = p \cdot \sum_i \delta_i$ while that of the consumer i, with $i = L, M, H$, is given by

$$\begin{aligned} &Pro: \quad u_i^{Pro} = \delta_i [v_i - p]. \\ &Free: \quad u_i^{Free} = \varphi \cdot [v_i - \delta_i \cdot p]. \end{aligned}$$

Proposition 6.1. *In case of a Pro market the following strategy profile is a unique subgame perfect Nash equilibrium*

Consumer: $\delta_i^*(p) = \begin{cases} 1 & \text{if } p \leq v_i \\ 0 & \text{if } p > v_i \end{cases}$ *for* $i = L, M, H$

Seller: set a price of $p^* = 20$

Proof. Because consumer i purchases the product only if the price lies below his valuation, the seller's decision problem becomes

$$\max_{p \geq 0} p \cdot x(p), \text{ where } x = \sum_{i=L,M,H} \delta_i \text{ and } \delta_i = 1 \text{ if and only if } v_i \geq p, \text{ for} i = L, M, H.$$

Under our choice of parameters, the seller maximizes his profits by setting $p^* = 20$. At this price, both H and M purchase the product and receive respective payoffs of 10 and

0 while L does not purchase the good and settles for a payoff of 0. The seller's profit is $\Pi^{Pro} = 40$. Because no consumer has an incentive to deviate from his strategy, the Nash equilibrium solution of the game in the *Pro* market is
$\left(p^* = 20, \delta_H^*(\cdot) = 1, \delta_M^*(\cdot) = 1, \delta_L^*(\cdot) = 0\right)$. □

Proposition 6.2. *In case of a Free market the following strategy profile is a unique subgame perfect Nash equilibrium*

Consumer: $\delta_H^*(p) = \begin{cases} 1 & \text{if } p \leq v_H \\ 0 & \text{otherwise} \end{cases}$, $\delta_j^*(p) \equiv 0$ *for* $j = L, M$

Seller: set a prize of $p^* = 30$*.*

Proof. If the seller sets the price at the level which would be optimal in the *Pro* market ($p = 20$), both H or M have an incentive of not buying given the other still purchases the product. This is so despite that $v_i \geq p$. Regardless whether H or M buys, the seller earns only 20. Lowering the price does not improve the profit of the seller as one of the consumers pays and the others benefit for free. As he can only sell the product to one consumer, the seller maximizes his profit by raising the price to the highest valuation v_H, which provides him with an payoff of $\Pi^{Free} = 30$. At this price, H is the only buyer, and M and L enjoy the benefits of the good without paying for it. The payoffs of consumers are respectively $0, 20$, and 10. Because no consumer has an incentive to deviate from his strategy, the Nash equilibrium solution of the game in the *Pro* market is $\left(p^* = 30, \delta_H^*(\cdot) = 1, \delta_M^*(\cdot) = 0, \delta_L^*(\cdot) = 0\right)$. □

Even for prices off the equilibrium (for all $p < 30$) consumer H should always purchase the product under the assumption that the consumer buys who can afford the price most easily.

Our model captures the general features of information good markets. The assumption of a downward sloping demand reflects heterogeneous valuation for the product, which might be sensible in light of various tastes within a population. Non–excludability of information goods is implemented in the *Free* market as consumers who did not pay for the product can benefit from it. In case of the *Pro* market artificial exclusion, the main feature of a market with intellectual property protection, is enforced: only buyers can use the product. In our model price discrimination is not possible as only one posted price is offered to all buyers.[6] For instance, sellers of music CDs or books can hardly price discriminate as products are offered for a take-it-or-leave-it price at the same time to all customers. In the model at hand we do not specifically investigate the production decision. Nevertheless, it can easily be incorporated into the model. Such extension would clarify which investment projects could be undertaken, given the profit expectation.[7]

In our model prices of $p^*_{Pro} = 20$ in the *Pro* market and $p^*_{Free} = 30$ in the *Free* market lead to the following welfare implications and profit shares. Welfare, defined as the sum of all market parties' profits, is socially optimal in the *Free* market as there is no exclusion possible.[8] In the *Pro* market welfare losses are caused by underutilization, i.e., prices exclude consumers who would be willing to pay at the competitive level and even above, but cannot afford current prices. If welfare is the policy variable, the *Free* market would be the preferred institution as it is welfare optimizing. Finally, profit shares of the seller and the average consumer are with 50% and 17% in the *Free* market more balanced than in the in the *Pro* market with 80% and 7% which might be important the distribution of the social surplus is of interest.

These results rely on the assumption that the high value consumer always purchases the product in the *Free* market. The decision of H to accept or reject the price offer, is decisive for the benefits of the seller and remaining consumers. H is therefore in a similar position as responders in two–persons ultimatum games. Further, the resulting division of surplus is unequal not only between the seller and H but also within consumers to the disadvantage of H. H might decide not to buy at high prices because of fairness considerations. The costs to reject the price offer are low for H as he does not receive anything in case of a successful trade. As a result, markets would either close without a purchase and lost gains from trade for the seller, M, and L, or the seller could reduce his price offer such that also H benefits in an adequate way and would accept the offer.

In the *Pro* market prices are predicted to be lower and affordable by H and M. The behavior of consumers in this market only affects the seller's and the own payoff but has no influence on other consumers' profits. Also here, consumers might refuse to buy, leaving the seller with a lower profit. For instance, M does not gain by purchasing for the predicted price, so that he has no costs from demand withholding. M might refuse to buy leaving the seller with a lower profit. The seller could either raise the price up to the acceptance threshold of H, only selling to the high value buyer, or lower the price to the acceptance threshold of M still selling to both or maybe selling also to L.

Even with the possibility of side payments amongst buyers H's and M's fairness considerations and countervailing behavior might comply with the inequality towards the seller which would still be high.[9] The first 2 columns of table 6.1 summarize the predictions corresponding to the experimental parameters.

6.3 Experiment: Design and Procedure

In order to investigate the behavior in the markets described above we conducted a computerized experiment with two treatments, the *Free* and the *Pro* market, at the laboratory of the Humboldt University Berlin. For the implementation the software z–Tree by Fischbacher (1999) was used. A session was conducted the following way. Before the experiment seller and buyer roles were assigned to each computer. In order to keep roles private information role assignments to computers were not openly announced and changed after each session. Upon arriving participants were seated on visually separated terminals randomly and received instructions (see appendix 6.B for a translated version). In one session participants interacted repeatedly over several periods remaining in their roles. The particular buyer valuations (H, M, and L) was allocated within the buyers randomly each period. To exclude repeated game effects participants are rematched after each period. In a market one seller interacted with three buyers.

To obtain both, sellers' prices and buyers' willingness to pay, we apply a version of a Nash demand game (Nash, 1950). This implies that sellers and buyers decided simultaneously about the price, p, and their willingness to pay, wtp, respectively. The product is sold to buyers whose wtp is equal or higher than the posted price.[10] In order to receive a complete strategy of every subject in the role of a buyer for all valuation v, $v \in \{H, M, L\}$, we apply the strategy method (Selten, 1967), i.e., buyer i is asked to state conditionally for each possible valuation a corresponding $wtp^i = \left(wtp^i_H, wtp^i_M, wtp^i_L\right)$.[11] After all buyers and the seller have made their decisions each buyer in the market is randomly assigned to one valuation. Additionally, in the *Free* market buyers who did not purchase the product also benefited from it if at least another buyer in the market bought. Payoffs depended on the market form corresponding to the above described model.

After each period buyer i was informed about the posted price, his valuation and reminded of the corresponding wtp^i_v, whether he bought the product, and the profit earned. A seller learned the number of sold products. He was reminded of the price he had posted and informed about his profit. The market structure, distribution of reselling values, payoff functions, the exchange rate, and the matching procedure were common knowledge.

We were also interested how individual subjects interact in different institutions and whether an institutional change has an impact on behavior, we employ a within subject design where every participant interacted successively for 10 periods in both treatments. After the first 10 periods participants received instructions with information of the change in institutions concerning the second treatment (see appendix 6.B). To control for treatment order effects for half of the sessions the sequence of treatments was reversed.[12] Table 6.2 summarizes the design of the experiment. Sessions and their corresponding treatment order are presented, where "I" and "II" indicate whether subjects interacted in first or second half of the experiment in a specific treatment and subscripts "P" and "F" indicate the treatment, either *Pro* or *Free* market.

Altogether 120 persons, mainly students of economics, interacted for 20 periods. We conducted 10 sessions consisting of 3 parallel markets with a total of 12 participants each. The experiment took on average 70 minutes including answering the post–experimental questionnaire. All monetary units were expressed in ECU (Experimental Currency Units). The exchange rate was ECU 25 = Euro 1. Participants were paid immediately after the experiment. They earned on average Euro 10.5, more precisely Euro 8.7 as buyer and Euro 15.9 as seller. In total we observe 600 markets consisting of 150 markets per treatment and order.

6.4 Results

In the following section, we investigate the pricing behavior of sellers and discuss it in light of the theoretical predictions. Further, we will examine how well prices respond to the actual demand behavior and continue by analyzing the behavior of buyers. Finally, we compare welfare realizations and surplus shares. This includes the inquiry of demand behavior for all prices which will enable us to conclude on the surplus division within the market for given prices. Together with the actual pricing behavior of sellers we can explore the impact of excludability on welfare and surplus distribution.

The last four columns of table 6.1 present the mean value and the standard deviation of the outcome in both markets observed in the experiments: welfare, posted prices, willingness to pay for different valuations, realized profits and surplus shares of sellers and buyers for the different markets.

6.4.1 Posted Prices

Table 6.1 contrasts the theoretical prices with the actual mean prices observed in the experiment. Average posted prices are 13.2 in the *Pro* market and 12.8 in the *Free* market: far below the prediction of 30 and 20, respectively. Furthermore, there seems to be almost no difference in average posted prices between both markets. However, as such conclusions are only based on aggregated prices, we might miss part of the information in the price setting behavior, especially in case of bimodal densities like those at hand.

Figure 6.1 reports nonparametric kernel–based density estimates of posted prices for both treatments and orders.[13] Indeed, density estimates of both treatments suggest that the institutional environment has an impact on price setting. The first area in figure 6.1 shows both treatments on top of each other. The picture indicates that price density estimates are similar for low prices around the first mode of 9 but different for prices around the second mode of 15. In the range from 12 to 30 the posted price density in the *Free* market is lower for low prices and higher for high prices compared to the *Pro* market which portends the theoretical prediction of higher prices for at least part of the posted prices. To test whether both density estimates are significantly different from each other we use the nonparametric approach proposed by Li (1996).

In the following we will briefly introduce our methodology, which we will use later on to detect differences between profit and welfare densities. In our analysis we compare densities of prices in the two treatments (I_F and I_P) and the different orders (I_F and II_F, as well as I_P and II_P) with the Li test.[14] The test is based on the distance measure $D = \int (f(x) - g(x))^2 dx$ between the two densities ($f(x)$ and $g(x)$). Note that we do not make any assumptions about the form of the distribution function in separate treatments and test the null hypothesis, $H_0 : f(x) = g(x)$ for all x, against $f(x) \neq g(x)$ and that the test does not require independence of observations. The first area in figure 6.1 visualizes the approach of the test for differences in prices between treatments which were both conducted in the first 10 periods $(I_F - I_P)$, i.e., the treatment effect. The second and third area indicate the differences of the same treatment between sessions of different sequence $(I_F - II_F)$, and $(I_P - II_P)$, i.e., the treatment order effects. Treatment order effects allow to observe whether there is pathdependance of behavior in the two institution. In absence of treatment order effects, we can pool data of the same treatment, and explore the treatment effect with increased number of observations. The Li test statistic follows a standard normal distribution, i.e., test values above 1.96 reject the null hypothesis that both observed distributions are drawn from the same population at a significance level of 5%, and are presented in table 6.3. The test values correspond to the following comparisons $t_1 : (I_F - I_P)$, $t_2\ (I_F - II_F)$, and $t_3\ (I_P - II_P)$. From the Li test (see table 6.3) we conclude that sellers do react to the two legal systems under consideration and post different prices (t_1 : 5.47).[15]

Result 1 Observed mean prices are very similar but price setting differs across treatments.

To understand the price setting process better, we will now explore individual behavior of sellers in two subsequent markets. Each of the 30 sellers experiences both markets sequentially for 10 periods. From the price adjustment of sellers in the different institutions we will try to infer on their pricing strategies. The price adjustment of individual subjects reveals that the theoretical predicted direction of change, higher prices in the *Free* market, is observed for one–third of all sellers.[16] Median prices of these sellers are $p_{Free} = 15$ and $p_{Pro} = 9$ which is about half of the theoretical predicted prices. 13% of the sellers adjust opposite to the predicted direction, i.e., they set lower prices in the *Free* market than in the *Pro* market (median prices $p_{Free} = 9$ and $p_{Pro} = 15$). The remaining half of subjects keep their offer distribution with median prices of $p_{Free} = p_{Pro} = 12$ constant for both treatments.[17]

Result 2 Sellers adjust rather differently to the two institutions. One–third of the sellers behave according to the comparative statics of the theoretical prediction. They ask for higher prices in the *Free* market. However, given the observed demand, prices which maximize expected profits should be opposite to the theoretical prediction, namely higher in the *Pro* market, which is realized by 13% of sellers. The remaining half of sellers does not change their behavior.

The financial consequences of such price setting for both treatments are presented in figure 6.2. Realized profits of both markets are shown as scatterplots (triangles = *Pro* and circles = *Free* market), and dashed (*Pro*) and solid (*Free*) lines denote expected profits. They are projected on their corresponding prices. Realized profits increase up to a price of 9 above which in both markets the variance increases. In the *Free* market realized profits then start to decline, whereas in the *Pro* market realized profits increase up to a price of 15, after which they decrease. From the posted price densities (see first area in figure 6.1) we know that prices above 9 in the *Free* and above 15 in the *Pro* market are less frequent which reduces our observations for those prices and can explain the increase in the variance for prices above 9 and 15. We can compensate for this shortcoming with the information we have about the acceptance threshold of buyers. By an analysis of buyers' willingness to pay we derive how many buyers of each valuation would have bought on average at a given price. The expected profit of a seller is computed as the product of the posted price and the expected number of buyers given this price, taking into account that only one buyer of each valuation is in the market. Expected profits are shown in figure 6.2 by the solid and dashed line for *Free* and *Pro* market, respectively. They are close to actual realized prices especially for ranges where more prices are observed.

In the *Pro* market profit maximizing prices lay around 15 with an expected number of buyers of 1.56 and corresponding earnings of 23, whereas in the *Free* market profit maximizing prices are around 9 with an expected number of buyers of 1.83 and corresponding earnings of 16.50. This result contradicts not only the values of the theoretical price prediction but also its comparative statics. Given the experimentally observed demand, a price of 15 (9) would maximize expected profits in the *Pro* (*Free*) treatment. As we have seen above, 13% of sellers seem to follow this pattern.

Continuing an established price policy is applied by 57% of all sellers which might be a rather simple solution to the question of price adjustment in the subsequent markets. In this sense, posted prices around 12 minimize by one price the distance between profit maximizing prices of both institutions. Further discussion on the influence of the institution on the seller's profit is delayed to section 6.4.3 below.

Result 3 Prices are much lower than predicted by the model. This can be explained by the fact that buyers withhold demand and sellers benefit less than predicted from high prices.

Posted prices result from the interaction of sellers and buyers. Buyers withhold demand in both markets for high prices which indicates that sellers could not have successfully implemented theoretical prices. To understand why this is the case, we turn now to buyer behavior.

6.4.2 Demand

Buyers' maximal willingness to pay is much below their valuation. The demand curves estimated from buyers' willingness to pay of the first area in figure 6.3, where the *Free* market is shown to lay below the *Pro* market demand curve. The remaining areas in figure 6.3 plot demand curves for both orders. Table 6.1 reports the maximal willingness to pay of H, M, and L (22.0, 15.5, 7.9) indicating that consumers want to keep at least $33\%, 20\%$, and 20%, respectively, from their valuation in the *Pro* market.[18] Demand withholding in the *Pro* market is costly for consumers. In the *Free* market consumers state that they are willing to pay 17.4, 11.0, and 5.5, as H, M, and L, respectively, which compared to the theoretical prediction is less for H and more for the remaining consumers.

From the aggregated data we understand that consumers would hardly support theoretically predicted prices, i.e., they mainly reject very high offers. This result is corroborated by an analysis of the individual data which reveals that only 6% of the subjects actually would be willing to pay up to their valuation for the good in both markets.[19]

Result 4 The willingness to pay is much lower than predicted for all buyers in the *Pro* market and for high valuation buyers in the *Free* market. Only 6% of all buyers support by their willingness to pay the theoretical prediction. Relative to the theoretical prediction in the *Free* market, high valuation consumers are willing to pay less but lower valuation consumers are willing to pay more.

The observed demand withholding for H in the *Free* market cannot be explained by theory according to which the high value consumer should be willing to pay any price below his valuation. Demand withholding leads to lower prices in the *Free* market such that also other consumers than only H could afford the product. Even though also non buyers benefit from the product in the *Free* market, as long it is bought by someone else, one might expect that if fairness considerations play an important role, in case of lower prices also more than one consumer might buy to transfer part of their surplus to the seller. We actually observe, that M and L are willing to pay in the *Free* market 11.0 and 5.5, respectively.

The willingness of M and L to purchase the product in the *Free* market offers the opportunity to H to receive the product for free instead of buying it himself. This strategic component of demand withholding in the market without protection might explain the on average lower willingness to pay of 17.4 of the high value consumer in the *Free* market compared to the *Pro* market. Demand withholding of H becomes free riding at the expense of "poorer" consumers and the seller if prices are very low. From information of the individual willingness to pay we observe that 8% of the consumer subjects seem to free–ride generally as they are not willing to pay even prices of 5 in the *Free* market regardless of their valuation.[20]

Result 5 Demand withholding by the high valuation consumer is higher in the *Free* than in the *Pro* market which might account for the fact that demand withholding is less costly in the *Free* market (since there is a positive probability that one of the others might buy the product). In fact, we observe less demand withholding of the remaining buyers than predicted indicating a general willingness to support low prices. On the other hand, some subjects seem not be willing to buy at all for any price in the *Free* market.

We will now explore in greater detail the consequences which the behavior of consumers and sellers have on the welfare in the different institutional settings.

6.4.3 Profits, Welfare and Surplus Division

The realized welfare of both markets lays below the theoretical prediction (see table 6.1). The test statistics (t_1 : 51.49) indicate that welfare realizations of both markets are significantly different, despite similar averages of 47.2 in the *Free* market and 43.6 in the *Pro* market. This implies that the rejection of the null hypothesis of identical distributions is driven by variances (*Free:* 24.6, *Pro:* 15.6). The difference in variances can be explained by fact that the *Free* market leads to either 100% or 0% realization of the total possible welfare, whereas the *Pro* market allows for intermediate efficiency rates, depending on whether one, two, or three consumers bought the product. Welfare losses are 27% in the *Pro* market and lay 10% above the theoretical prediction whereas in the *Free* market they are with 20% far above the theoretical prediction of 0% in the *Free* market.[21]

Result 6 Both institutions display rather high welfare losses which does not support the theoretical implication of no welfare losses in the *Free* market.

Let us now investigate how the surplus is distributed between consumers and sellers. The data indicates that only consumers gain from the *Free* market whereas it is only for the seller beneficial to protect the product.[22] Profits of the seller are 13.4 in the *Free* market and 21 in the *Pro* market, which is almost two–thirds lower than predicted for the *Free* and half of the profit prediction in the *Pro* market (see table 6.1). The density estimates of seller's and consumer's profit for both markets (first left and right areas in figure 6.4) corroborate this picture. The third area of the seller's profit densities reports treatment order effects. The seller earns less in the *Free* market if subjects had experienced a market with exclusion before (t_1 : 5.07). The earnings of the seller in the *Pro* markets (second area on the left) and of the consumer for both markets (area two and three on the right) do not depend on the order of treatments.

Result 7 The *Pro* market is more beneficial for the seller. Consumers gain more in the *Free* market.

Divisions of surplus between sellers and consumers are on average 48% and 17% (28% and 24%) in the *Pro* (*Free*) market (see table 6.1).[23] Compared to the theoretical prediction surplus shares in both markets are more balanced. In the *Free* market average consumer and seller shares resemble almost an equal split between all market participants. Additionally, also surplus shares within consumers are more balanced in both markets than predicted. Contrary to the theoretical predictions the high value consumer obtains the highest income amongst consumers even in the *Free* market. Nevertheless, compared to the *Pro* market high valuation consumers earn less in the *Free* market whereas the income of M and L in the *Free* market increases weakly corroborating some comparative statics of the theoretical model.

Result 8 The *Free* market fosters more equitable outcomes than the *Pro* market. Contrary to the theoretical prediction, the high value holder receives the highest income within consumers not only in the *Pro* but also in the *Free* market.

On the basis of consumer's willingness to pay, figure 6.5 plots the expected surplus share of the seller and the average consumer as well as the welfare loss for all prices and both institutions. These graphs show that surplus shares in the *Free* market of seller and consumer are quite close, for prices below 7.5 they are higher for the consumer and above 7.5 they are slightly higher for the seller. Whereas in the *Pro* market surplus shares for prices above a price of 5 favor the seller much more.

Welfare losses due to lost gains from trade and underutilization can be measured in relative terms as ratio of the difference between the maximum possible surplus and the surplus actually reached. Welfare losses in the *Free* market are for almost all prices below than in the *Pro* market. Interestingly, for prices below 5 welfare losses arise only in the *Free* market. For prices above 5 welfare losses increase faster and are higher in the *Pro* market.

Result 9 Compared to a market without exclusion, an increase in prices in the *Pro* market leads to higher inequality between consumers and the seller but also to an increase in demand withholding resulting in higher welfare losses.

6.5 Discussion

A central problem has been the conflict between society's goals of achieving efficient use of information goods once produced and providing ideal motivation for production of information goods (Hirshleifer and Riley, 1979). In order to incentivize the production of information goods society grants intellectual property rights to inventors which allow control of reproduction and redistribution. These monopolies artificially created would be efficient if perfectly price discriminating fees could be charged. In practise, however, owners of those rights post one price and thereby artificially reintroduce exclusion in information good markets accompanied by welfare losses due to underutilization. The economic literature has investigated the impacts of intellectual property protection in light of the trade off between underproduction and underutilization assuming price taking buyers. Empirical evidence seems to suggest that people might be influenced by other preferences than usually assumed profit maximization and price taking giving rise to outcomes different from the predicted.

In this chapter we have investigated the interaction in markets for information goods under the two legal regimes, with and without exclusive usage (copyright) of the product by buyers. We use a standard market model wherein a non–excludable good is offered by a monopolistic seller and compare it to a market where the good is excludable. In order to appreciate the behavioral consequences of exclusion in a market after production has taken place, we conduct a laboratory experiment which captures the main characteristic features of information good markets with and without protection. We observe interesting patterns of behavior, which if present in real world markets should be considered when deciding for one or the other legal system. To our knowledge, this is the first attempt to implement experimentally a public good structure in a market to investigate information goods.

We observe that prices in experimental markets are much lower than predicted which can be explained by consumers' demand withholding. Despite lower prices, welfare losses are still present. Demand withholding accounts for a substantial share of these welfare losses in the market with exclusion. In the environment without exclusion, welfare losses can be explained by demand withholding of high valuation consumers alone. In this market demand withholding has not only an impact on the seller but has also external effects on other consumers. The outcome of the market with non–exclusion depends strongly on high value consumers who should, according to theory, be the only buyer. We observe that high value consumers exhibit higher demand withholding than in a market with exclusion which indicates that they also want to gain from the situation when the seller is less powerful and try to free ride on other consumers who might buy. Also consumers with lower valuation are willing to buy in the market without exclusion.

Sellers who want to maximize their profits in the market without exclusion should therefore decrease their prices instead of following the theoretical prediction to increase them compared to a market with exclusion. Lower prices in the market without exclusion lead to a decrease in demand withholding and therefore welfare losses and result additionally in lower inequality than in a market with exclusion. Given the demand behavior observed in the experiment, the welfare in a market consisting only of expected profit maximizing sellers would be higher in a non–exclusive environment than in the exclusive environment.

As our experiment indicates, seller behavior is quite heterogeneous. Only some sellers act in this best reply pattern and set lower prices in the market without exclusion. We also observe sellers who do not change their price setting behavior across the markets as well as opposite adjustment, more in line with theory which predicts higher prices in the non–exclusion market. The heterogenous behavior of sellers results in average prices leading to similar high welfare losses in both markets.

In light of these results, no market seems to be more preferable from a welfare perspective, as both exhibit similar welfare losses. Therefore, further policy goals should be considered in order to decide for one institution or the other: copy protection of the information good might be less desirable if reallocation of income and equality are important, on the other hand if higher profits can be achieved by sellers in a market with protection than such market structure might facilitate production of information goods with high investment costs. Additionally, it is important to mention that precedent market institutions seems to influence behavior. We have some indication for treatment order effects concerning the behavior of sellers and their earnings.

Our analysis of information good markets under different legal systems has two implications for their production. First, in view of buyer countervailing power theoretically predicted profits could not be realized. This might directly lead to overestimation of the investment budget. Second, overestimation would be higher in case of no intellectual property protection where profits comprise only one–third of the theoretical prediction. In the protected environment sellers in our experiment could achieve half of the predicted profits.

To understand the impact of buyer and seller interaction in the real world of information good markets more investigation would be required. There are two crucial features whose combination might be interesting for further investigation. First, consideration of the production decision would be more realistic, in particular when invested costs are private information to the producer and cannot been observed by consumers. This raises the questions what impact the institution has on production for which investments have to be undertaken which are only privately known to the seller. And whether consumers are willing to withhold demand less in presence of investment costs and no information about their volume?

Another related issue is the pricing of information goods. In the real world price discrimination by the seller is hardly possible which leads to inefficient usage. It could be that posted pricing is not the optimal pricing scheme for information goods. Even payment alternatives, such as donations, which allow for a kind of "self price discrimination" of buyers may be considered. An advantage is that all buyers could access the product which would terminate welfare losses due to underutilization. The drawback is that non–exclusion might lead to free riding, so that some consumers might not pay even when they use the product. Even though free riding behavior was present in the current study, most participants were willing to pay even as low valuation consumers in the market without exclusion which might indicate that self price discrimination might successfully be implemented. Given the fact that under non–exclusion more consumers can access the product, contributions might even outweigh losses from free riding and, finally, lead to more preferable outcomes for both market sides. Further analysis might therefore be promising.

Appendix to Chapter 6

6.A Estimation and Tests

6.A.1 Density Estimation

Each reported density function, $f(x)$, estimate is based on the standard normal kernel function and optimal bandwidth:

$$\widehat{f}(x) = \frac{1}{nh}\sum_{i=1}^{n} K\left(\frac{x_i - x}{h}\right)$$

where K is a bounded, nonnegative (kernel) function with $\int K(\psi)\, d\psi = 1$. The bandwidth h is a function of the sample size n and $h \to 0$ as $n \to \infty$ and $nh \to \infty$ as $n \to \infty$. The selection of the bandwidth is crucial. A very large bandwidth oversmoothes the data and leads to biased estimates. A very small bandwidth results in a noisy density estimate with high variance. The chosen kernel is a Gaussian Kernel estimator and the bandwidth for the estimates at hand are derived using the optimal bandwidth for a standard normal density $h = 1.06\sigma n^{-1/5}$ (see Pagan and Ullah, 1999, for a detailed discussion).

6.A.2 Comparison of Unknown Densities

Let $f(x)$ and $g(x)$ be two continuous probability density functions. We compare those two unknown densities and test $H_0 : f(x) = g(x)$ against $H_1 : f(x) \neq g(x)$ following Li (1996). The test statistic, based on the space between the estimated density functions $\widehat{I}(f,g) = \int_x \left(\widehat{f}(x) - \widehat{g}(x)\right)^2 dx$, is

$$T = \frac{n\sqrt{h}\widehat{I}}{\widetilde{\sigma}} \sim \mathcal{N}(0,1)$$

where $\widehat{I} = \frac{1}{n^2 h}\sum_{i=1}^{n}\sum_{j=1}^{n}\left[K\left(\frac{x_i-x_j}{h}\right) + K\left(\frac{y_i-y_j}{h}\right) - 2K\left(\frac{y_i-x_j}{h}\right)\right]$ and

$\widehat{\sigma}^2 = \frac{2}{n^2 h}\sum_{i=1}^{n}\sum_{j=1}^{n}\left[K\left(\frac{x_i-x_j}{h}\right) + K\left(\frac{y_i-y_j}{h}\right) - 2K\left(\frac{y_i-x_j}{h}\right)\right]\int K^2(\psi)\, d\psi$ or

$\widetilde{\sigma}^2 = \widehat{\sigma}^2 - \frac{4h}{n}\left(2\sum_{i=1}^{n} f_n(x_i) - \sum_{i=1}^{n} f_n^2(x_i)\right)$. Li proves that the variance estimate $\widetilde{\sigma}^2$ converges faster to the true σ^2 than $\widehat{\sigma}^2$ for small sample sizes. The tests described above are asymptotically valid to compare either independent as well as dependent samples (see Li).

6.B Instructions (Translation)

This is a translated version of the original German instructions which are available at the author upon request. The basic instructions are for the *Pro* treatment. Italic notes in parenthesis mark differences to the *Free* treatment and in brackets mark notes

General information

Please read the instructions carefully. Please notify us in case you do not understand. We will come and answer your questions privately. Please do not communicate with your neighbors, in which case we would have to exclude you from the experiment and could not reimburse you.

The instructions are identical for all participants. You will make your decisions at the computer. All decisions will be anonymous, i.e., you will not get to know the identity of others and your identity will not be revealed to others.

You can earn money during this experiment. The amount will depend on your decisions. Please note, that losses are possible and will be accounted with your profits. With appropriate behavior you can avoid losses.

During the experiment your income will be counted in ECU (Experimental Currency Unit). The exchange rate ECU to Euro is: 25 ECU = 1 Euro. In the following all payment details will be given in ECU. At the end of the experiment your profit will be converted into Euro and paid immediately.

Instructions

Four agents, a seller (S) and three consumers (L, M, and H), interact on a market. Consumers L, M, and H, can buy a product from S. Whether you will interact as seller or one of the consumers, will be randomly determined at the beginning of the experiment. You will keep this role during the entire experiment. The constellation of market participants will be randomly determined every round, i.e., in a subsequent period you will not interact with the same persons as the current period.

Every consumer values the product differently (L=10, M=20, H=30). The value corresponds to the payoff a consumer gets if he receives the product. Whether a consumer L,M, or H is, will be reassigned randomly before each round.

One round proceeds as follows:

Consumers announce for each possible valuation (10, 20, and 30) how much they would maximal be willing to pay for the product. Simultaneously the seller posts a price for the product.

Following the consumers will be randomly assigned to one of the types L, M, or H. The decision for the assigned role will then be compared to the price posted by the seller. A consumer buys the product if his maximal willingness to pay **equal or higher** then the posted price. He pays the product to the seller.

Your profits will be determined according to your role as follows:

Consumer:

Buyer: If you have bought the product you receive your valuation and pay the price to

the seller. Your profit is your valuation minus the price.
Non–Buyer: If you have not bought the product you do not have to pay.
Your profit is zero. *(In case of a market transaction, i.e., if at least one of the other consumers bought, you will also receive the product. Your profit is your valuation in case of a transaction and zero in case nobody bought the product.)*

Seller: Your profit is the product of sales and posted price. In case nobody bought your profit is zero.

At the end of each round you receive the following information:

Consumer:

○ which buyer type was randomly assigned this round,
○ whether you bought the product,
○ as a reminder your willingness to pay for this type,
○ the price and
○ your profit in this round.

Seller:

○ the price,
○ how many consumers bought the product,
○ your profit in this round.

[*Subjects received the following additional instructions after the 10^{th} round.*]

You have participated in the market for ten rounds. There will be 10 additional rounds with the same proceeding and calculation of profits for buyers and sellers and the following difference for non buyers:

In case of a market transaction, i.e., if at least one of the other consumers bought, also a non buyer will receive the product. The profit is the valuation in case of a transaction and zero in case nobody bought the product. *(Non buyers do not receive the product at all. They do not have to pay. The profit is zero. Only buyers receive the product.)*

6.C Tables

			Prediction		Data			
			Free	*Pro*	*Free*		*Pro*	
WTP		H	30	30	17.4	(9.0)	22.0	(7.1)
	buyer	M	0	20	11.0	(6.8)	15.5	(4.5)
		L	0	10	5.5	(4.1)	7.9	(2.0)
(Posted) Prices			30	20	13.2	(5.3)	12.8	(4.3)
Profits								
	seller		30	40	13.4	(9.1)	21.0	(7.8)
	average buyer		10	3	11.3	(6.2)	7.5	(3.6)
		H	0	10	15.5	(11.4)	16.0	(6.6)
	buyer	M	20	0	11.4	(8.7)	6.2	(5.0)
		L	10	0	7.0	(4.8)	0.4	(0.9)
Total surplus (welfare)			60	50	47.2	(24.6)	43.6	(15.6)
Share of surplus (in %)								
	seller		50	80	28		48	
	average buyer		17	7	24		17	
		H	0	20	33		37	
	buyer	M	33	0	24		14	
		L	17	0	15		1	

Table 6.1: Predictions and data: willingness to pay (WTP), prices, profits, welfare, and surplus shares, mean and standard deviations in parentheses. The standard deviations for the buyer refer to group averages.

Session	Treatment order	Periods 1 – 10	11 – 20
$1-5$	*Free-Pro*	I_F	II_P
$6-10$	*Pro-Free*	I_P	II_F

Table 6.2: Experimental design

		t_1 : $(I_F - I_P)$	t_2 : $(I_F - II_F)$	t_3 : $(I_P - II_P)$
Posted prices		5.47	5.59	1.96
Welfare		51.49	-0.29	-0.49
Profits	Seller	13.67	5.07	1.20
	Buyer	67.28	1.79	-0.31

Table 6.3: Test statistics of the Li test

6.D Figures

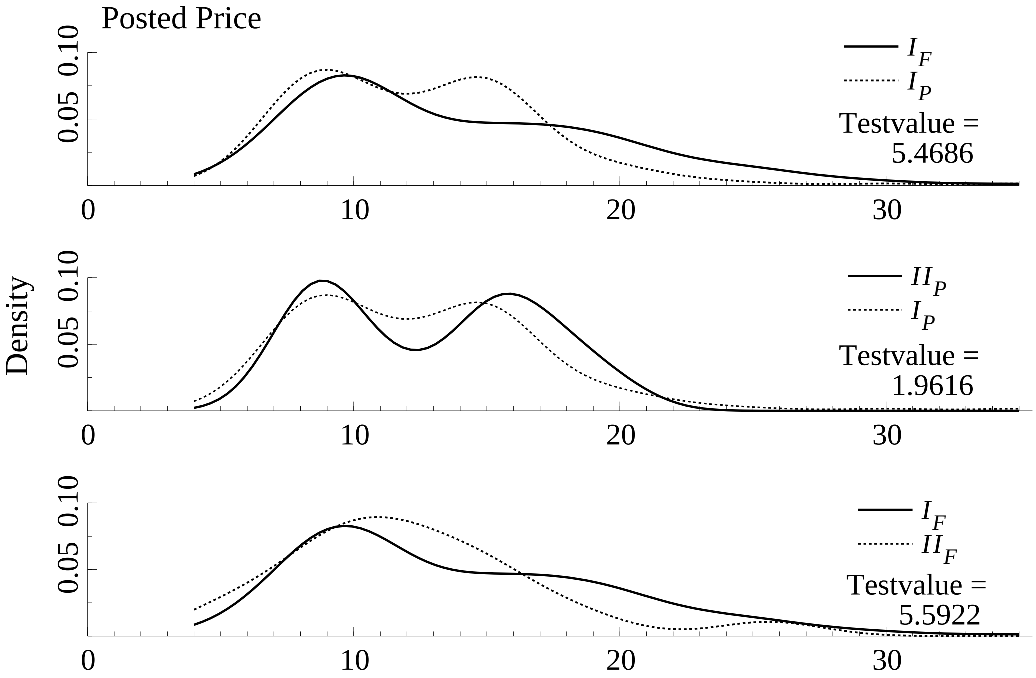

Figure 6.1: Test statistics: posted prices

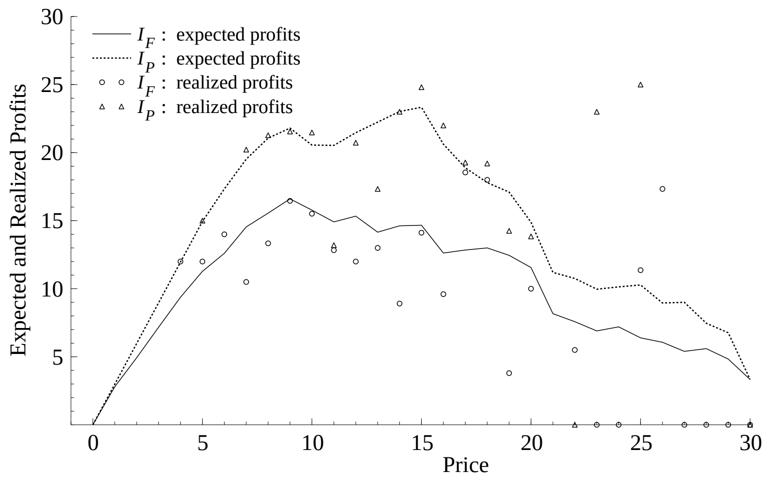

Figure 6.2: Expected profits of sellers given stated demand for *Pro* and *Free* market

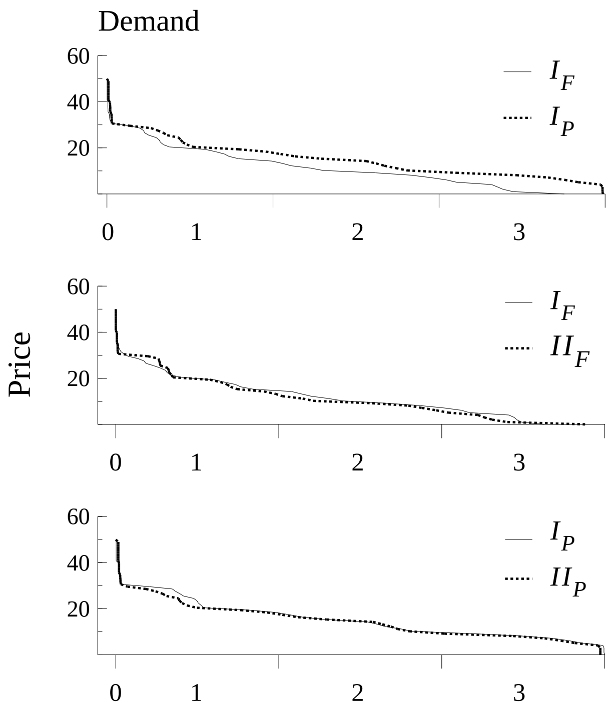

Figure 6.3: Estimated demand of buyers' stated willingness to pay

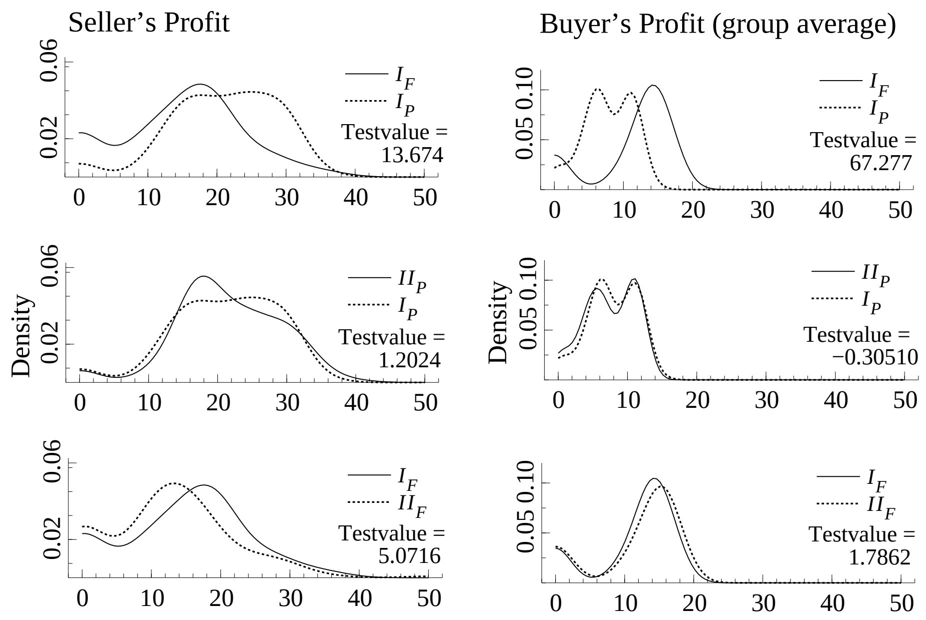

Figure 6.4: Test statistics: profit densities of different treatments and treatment orders for seller and (average) buyer

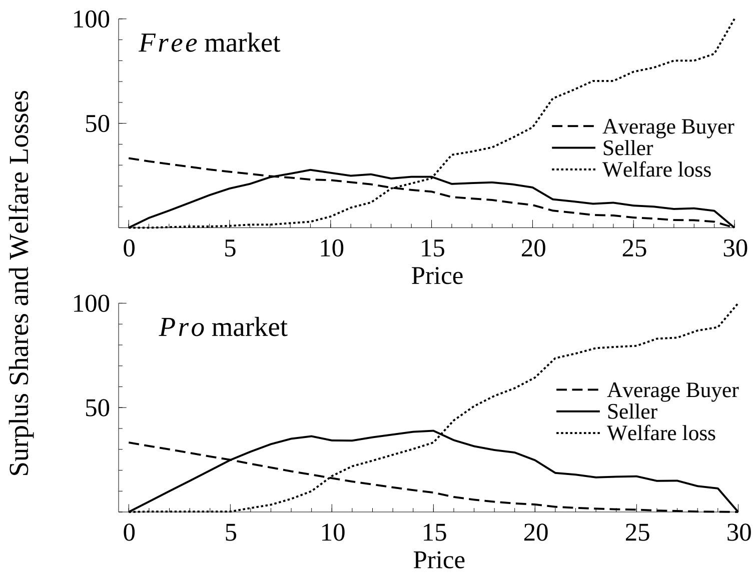

Figure 6.5: Expected surplus shares and welfare losses for all prices on basis of the observed demand

Notes

[1]For instance, email, websites, or more commonly peer-to-peer (P2P) file sharing systems are used to exchange information goods via the internet.

[2]Disputes have raged over the length of patents (Scotchmer, 1991) and copyrights (Varian, 2001). The "price" feature of intellectual property rights has been criticized to lead to socially wasteful overinvestment (Dasgupta and Stiglitz, 1980, Gilbert and Newbery, 1982). Varian (2001) discusses consequences of software patents which might lead to an increase in costs rather than in gains for society.

[3]Theoretically, incorporation of production costs does not change price setting, whereas the decision whether to produce will of course depend on those costs.

[4]These assumptions are made for simplicity without loosing the basic economics of such markets. Landes and Posner (1989) model basic copyright protection assuming that authors and copiers produce quality–adjusted copies which are perfect substitutes. Novos and Waldman (1984) explicitly model the secondary market for copies and analyze the effects of copy protection in a world with heterogeneous reproduction costs for buyers.

[5]The experiment which will be described later realizes exactly this parametrization. Our results are nevertheless very general. We receive the standard monopoly pricing in case of the protected market and price discrimination of the high value holder when the seller can sell the product to one consumer only, in the unprotected market.

[6]In case of personalized information goods price discrimination is more easily possible. Other ways of price discrimination can be established by taking time preferences of buyers into account. Hard cover books are sold way before the paperback version of a book to a higher price, price discriminating impatient buyers. Similar price patterns can be observed for music CDs which are sold for much higher prices before they are on sale later. We will not investigate these features of information goods here.

[7]Information goods with investment costs below 30 would be undertaken in both markets. For investment costs above 30 and below 40 the good would only be produced in a *Pro* market environment.

[8]The optimal effects of the *Free* market on welfare hold only if the good is sold at least once. This will be warranted as the seller is interested to sell his product and therefore sets a price equal or slightly below the highest valuation of the consumers. Correspondingly, the price–taking consumer will always buy when the price is equal or below his valuation.

[9]One way to think about side payments is an environment with repeated interaction where the valuation position of buyers changes. Such that all buyers earn on average the same.

[10]In a Nash demand game two players bargain about the division of a pie. Both players decide simultaneously about their claims, p and b, on a pie, v, to share. If the sum of the two demands does not exceed the pie $(p + b \leq v)$ both receive what they demanded, otherwise $(p + b > v)$ they receive nothing.
In the situation at hand the pie is the buyer's valuation, v, and claims are the price p asked by the seller and b is determined indirectly by the buyer stating $wtp(= v - b)$. The payoff rule here is that the seller receives his claim and the buyer the remainder $(v - p)$.

[11]Brandts and Charness (2000) have shown the strategy method to lead to similar responses as methods where agents directly act on a choice made by their interaction partners, so called "hot" decisions (see also Seale, 2000, and Sonnemans, 2000).

[12]In a within subject design decisions might be influenced by behavior of earlier treatments. In the current study we control for such so called "treatment order effects" by comparing behavior of corresponding treatments with reversed treatment order.

[13]For a description of the estimation method see appendix 6.A.1.

[14]See appendix 6.A.2 for a detailed description of the test applied.

[15]In case of the *Free* market also the sequence of interaction has an impact on price setting ($t_2 : 5.59$). Average posted prices are $I_F : 14.1(5.7)$; $II_F : 12.2(4.8)$ with standard deviation in parentheses.

[16]For this analysis we apply the Li test for each individual seller and test the null hypothesis that both densities are the same. In case of rejection we compare median prices in both treatments to conclude on the direction of adjustment. Most distributions are unimodal which would justify such an approach.

[17]These subjects include 3 sellers whose median price is the same in both institutions event though the Li test rejects due to different dispersion.

[18]This share is computed as $\frac{v-wtp_v}{v}$, $v = H, M, L$.

[19]Each buyer subject interacted for all valuation types in both treatments subsequently for 10 periods, which allows us to investigate the individual level of support for the theoretical prediction. Buyers supporting the strategic prediction, have a median willingness to pay (wtp_H, wtp_M, wtp_L) of $(30, 20, 10)$ in the *Pro* market and of $(30, \cdot, \cdot)$ in the *Free* market.

[20]Those subjects stated a median willingness to pay (wtp_H, wtp_M, wtp_L) of $(0, 0, 0)$ in the *Free* market for prices greater or equal to 5. A price of 5 would on average result in equal split of the surplus between the seller and average consumer if all consumers buy.

[21]According to the theoretical model welfare losses account for 10/60 (= 17%) of the social surplus in the *Pro* market.

[22]We refer to the findings for the average consumer buyer.

[23]We report numbers from pooled data for the *Pro* market as there are no treatment order effects. For the *Free* market we observe treatment order effects and report numbers corresponding to the first sequence (I_F). Nevertheless, surplus shares do not change that much (II_F : sellers: 27% and consumers: 24%).

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Samenvatting

Dit proefschrift bestaat uit een vijftal artikelen die bestuderen hoe instituties en individuele preferenties tesamen menselijk gedrag beïnvloeden en daardoor economische uitkomsten zoals prijzen en efficiëntie bepalen. Terwijl institutions bestaan uit waarneembare regels, zijn individuele preferenties veelal niet direct waarneembaar. Zowel institutions als preferenties hebben vele dimensies. Deze dissertatie richt zich op vier dimensies van preferenties die belangrijk zijn voor economische transacties: rechtvaardigheidsverwegingen, de neiging iemand te vertrouwen, risico houding en tijdsvoorkeur. De invloed van deze preferenties wordt bestudeerd in verschillende onderhandelings- en marktsituaties.

Theoretische analyse helpt de strategische structuur van een situatie te begrijpen. Door aannames te maken over preferenties en de rationaliteit van actoren, kunnen theoretische resultaten over het gedrag van actoren en de uitkomsten van interactie worden afgeleid. De valideit van de gemaakte aannames is uiteindelijk echter een empirische vraag. Economische experimenten verschaffen een methode waarmee menselijk gedrag en de uitkomsten van interactie empirisch kunnen worden onderzocht. Theorie en experimenten zijn daarbij complementair. Hun combinatie verschaft de onderzoeker een belangrijk instrument om de interacties in markten, bilaterale onderhandelingen en andere vormen van interactie te analyseren en begrijpen.

Hoofdstuk 2 combineert een economisch experiment met enquêtegegevens om de determinanten van vertrouwen en betrouwbaarheid in de Nederlandse samenleving te onderzoeken. We stellen de gevolgtrekkingen die kunnen worden gedaan over de neiging tot vertrouwen met behulp van verklaarde en gebleken voorkeuren tegenover elkaar. We vinden dat personen van middelbare leeftijd en individuen met een hogere opleiding relatief meer vertrouwen maar relatief minder betrouwbaar zijn. Het effect van leeftijd en religie op vertrouwen blijkt zeer af te hangen van de vraag of de ge-

[0]The author is deeply indebted to Bas van Groezen and Jan Potters for their engagement in making this summary understandable to a Dutch speaking audience.

bruikte vertrouwensmaatstaven volgen uit experimenten of enquêtes.

Hoofdstuk 3 onderzoekt op theoretische en experimentele wijze opeenvolgende onderhandelingen in risicovolle samenwerkingsverbanden met additieve boden en afwisselende aanbiedingen ingeval van acceptatie. Deze situatie is gebaseerd op de productie van een film die aanleiding kan geven tot een vervolgproductie, waarby acteurs en producenten met elkaar onderhandelen. Om de situatie zoveel mogelijk in overeenstemming te brengen met de werkelijkheid, parametriseren we het experiment op basis van empirische data van de filmindustrie. We vergelijken de voorspellingen van alternatieve theoretische benaderingen gebaseerd op verschillende aannames betreffende de voorkeuren van de onderhandelingspartijen. De speltheoretische voorspelling (uitgaande van risico-averse actoren) lijkt de geaggregeerde data het best te verklaren. Nadere bestudering van de individuele risicoparameters brengt echer inconsistenties met de theoretische aannames aan het licht, wat de voorspellende waarde van risico-aversie twijfelachtig maakt. Rechtvaardigheidstheorie (eerlijk delen) blijkt een betere verklaring te geven voor het waargenomen gedrag, ook op individueel niveau. De onderhandelingspartijen lijken risico te willen delen omdat zij daarvoor bij later succes worden gecompenseerd.

In hoofdstuk 4 modelleren we een duurzame monopoliemarkt waarin de martpartijen elkaars tijdsvoorkeur niet kennen. Het betreffende model is analoog aan een twee-perioden bilateraal onderhandelingsspel met private informatie over de kosten van uitstel en asymmetrische informatie over het bedrag waarover wordt onderhandeld. We leiden de theoretische uitkomst af en vergelijken die met het experimenteel geobserveerde gedrag. Onze resultaten tonen aan dat proef personen toekomstige winsten goed anticiperen en reageren op korte-termijn problemen in overeenstemming met de theoretische voorspelling.

Hoofdstuk 5 onderzoekt met behulp van experimenten de efficiëntie en winstgevendheid van twee verschillende vormen van aanbesteding voor het geval dat de kwaliteit van producten belangrijk is. We bestuderen één methode met meer mededinging aan de productiekant (vector veiling) met een andere methode, die de feitelijke praktijk van aanbesteding weergeeft, namelijk, het organiseren van een veiling voor de goedkope variant en vervolgens onderhandelen met de contractant over de additionele kosten van de hogere kwaliteitsvariant. Onze belangrijkste hypothese, dat kopers beter af zijn wanneer de vector veiling wordt gebruikt in plaats van de standard methode, wordt bevestigd.

Hoofdstuk 6 bestudeert in welke mate de markt voor informatiegoederen wordt beïnvloed door het wettelijke kader. De vraag is hoe producten die makkelijk na te maken en te verdelen zijn, worden verkocht op een monopolistische markt met en zonder bescherming tegen namaak. De implicaties van een theoretisch model worden vergeleken met de uitkomsten van een economisch experiment. We observeren dat kopers soms niet bereid zijn het goed aan de schaffen zelfs voor een prijs die theoretisch gezien wel voordelig zou moeten zijn. Rechtvaardigheidsoverwegingen lijken hiervoor verantwoordelijk. Het gevolg is dat prijzen vaak lager zijn dan voorspeld. Deze "vraagonthouding" hangt af van het wettelijke kader en heeft welvaartsimplicaties die niet overeenkomen met de theoretische voorspelling.

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