

SFB Discussion Paper B-416

The Covered Response Ultimatum Game

by

KLAUS ABBINK, ABDOLKARIM SADRIEH & SHMUEL ZAMIR

February 1999

Abstract

We report an experiment on the covered response ultimatum game, in which the proposer is not informed about the responder's reaction to an unequal offer. In this game, no education of proposers is possible. A control experiment with informed proposers was also conducted. We observe high rejection rates with covered response. These are explained by responders' resistance to unfairness. But the rejection rates are lower than in the control group, due to the lacking possibility of educative punishment. Proposers in the open response treatment test responders' propensity to reject by making more unequal offers. We conclude that both resistance to unfairness and educative punishment are determinants of behaviour, but neither is sufficient on its own.

Keywords

Ultimatum bargaining, fairness, punishment, experimental economics

JEL Classification Codes

C78, C91, C92, D82

Acknowledgements

The authors thank the teams of the *RatioLab* at the Hebrew University of Jerusalem and the *Laboratorium für experimentelle Wirtschaftsforschung* at the University of Bonn for their aid in collecting the data, and Gary Bornstein, Bernd Irlenbusch, Bettina Kuon, Reinhard Selten, and seminar participants in Berlin, Haifa, Meißen, and Paris for helpful comments. Support by the *Deutsche Forschungsgemeinschaft* through the *Sonderforschungsbereich 303*, by the *German-Israeli Foundation ! GIF !*, by the European Union through the TMR programme ENDEAR, and by the *Land Nordrhein-Westfalen* is gratefully acknowledged.

Authors

Klaus Abbink	Abdolkarim Sadrieh	Shmuel Zamir
Laboratorium für experimentelle Wirtschaftsforschung Universität Bonn Adenauerallee 24-42, 53113 Bonn, Germany phone: 49-228-73-9192 fax: 49-228-73-9193		Dept. of Statistics The Hebrew University of Jerusalem Mt. Scopus, 91905 Jerusalem, Israel phone: 972-2-5883305 fax: 972-2-5825748
abbink@lab.econ1.uni-bonn.de	sadrieh@lab.econ1.uni-bonn.de	zamir@math.huji.ac.il

I. Introduction

In the well-known ultimatum game, one player (the proposer, male pronouns) offers a second player (the responder, female pronouns) a division of a fixed cake. The responder can either accept or reject. If she accepts, the proposed division is implemented, if she rejects, both receive nothing. The subgame perfect equilibrium is straightforward: the responder will not reject any positive offers, since even the smallest positive payoff is better than receiving zero. In anticipation of this, the proposer will not offer more than the smallest money unit. The responder will accept. Under common knowledge of rationality, the proposer will grab virtually the whole cake for himself.

Subjects' behaviour, however, is different. A huge experimental literature starting with GÜTH, SCHMITTBERGER, and SCHWARZE (1982) has focused on this simple model of bargaining, and in almost all studies dramatic deviations from the subgame perfect prediction are observed¹. Typically, responders do turn down small offers, and proposers offer substantial amounts of money, very often up to an equal division of the cake.

Why do responders reject when this just means leaving money on the table? Obviously, such behaviour is not rational, if subjects are concerned about own payoffs only and the game is only played once. Thus, a natural approach to explain rejections in the ultimatum game is to assume that for some reason responders prefer receiving nothing to receiving little. Such preference could be based on the fact that rejection creates an allocation in which the proposer also receives nothing. This approach is related to a fairness argument: Responders are willing to pay the price of receiving nothing because of some inherent *resistance to unfairness*, in the language of THALER (1988).

However, if we look at the experimental results, it is striking that, although responders give up money they could have taken, they are in the end much better off than in subgame perfect equilibrium play, at least if we consider the population of responders as a whole. So, is there a different kind of rationality at work - maybe some form of *population rationality*, as WINTER and ZAMIR (1996) suggest? Because responders reject small offers, proposers better not make them. Thus, responders in total earn more than in subgame perfect equilibrium. Could it be that it is not fairness, but some kind of monetary self-interest that drives them? In other words, the fundamental question we investigate in this paper is: Do responders reject because they want to earn more or do they earn more because they want to reject?

This question reflects a dispute that has entered the scene in the recent years. The hypothesis that it is a kind of self-interest that drives subjects' behaviour is supported by authors who apply adaptive learning dynamics to the ultimatum game (GALE, BINMORE, and SAMUELSON, 1995,

¹ Overviews over the literature on ultimatum experiments are given by GÜTH (1994), ROTH (1995), and CAMERER and THALER (1995).

ROTH and EREV, 1995²). They argue that, since rejections of small offers cause smaller losses for the responder than for the proposer, proposers learn faster not to make small offers than the responders learn to accept them. Simulations of such models of adaptive learning provide a rationale for experimental results. The surprising feature of these models is that responders are reluctant to accept small offers, even though they *only* respond to their own success, and have no preference for any kind of fairness.

ABBINK, BOLTON, SADRIEH, and TANG (1996) find that the reported data fit of these models does not allow general conclusions about second mover motivations and learning. To analyse how learning mechanisms and punishment motivation are connected, the authors use the *uncertain reward game* in which the proposer is either punished or rewarded by a rejection, but this is private information to the responder. Since both treatments of that study are identical with respect to the players' information on own payoffs³, reinforcement learning models predict the same results. The experiment, however, shows significantly more rejections when the responder knows that the proposer is punished by the rejection than when she knows that he is rewarded. Thus, adaptive learning alone cannot explain actual ultimatum game behaviour. Obviously, some concern about the proposer's payoff is involved in the responder's decision making. Simulations of an adaptive learning model using the uncertain reward game suggest that the learning mechanism mimics a punishment motivation of the responder agents. Therefore, the adaptive learning model predictions fit the data well in the punishment treatment, but overpredict rejection rates in the reward treatment.

BOLTON (1991), RABIN (1993), BOLTON and OCKENFELS (1997), and FEHR and SCHMIDT (1998) develop the second line of explanation. In these fairness utility models, subjects are concerned about their own payoff and the "fairness" of the outcome. Although fairness is modelled in different ways, the individuals' resistance to unfairness in all cases is a disutility of unfair outcomes. Standard game theoretical methods are then applied to games in which players have such modified utility (or motivation) functions. A remarkably good fit of their models' predictions to a wide range of different experimental data is found. The authors conclude that the observed behaviour is driven by fairness considerations of responders. The subjects in these models, however, are concerned about fairness only in a static manner.⁴ Dynamic considerations of decision making, that motivate punishment either as a response to history (reciprocity) or as an investment for future payoffs (educative punishment), are not modelled.

In this paper, we introduce the *covered response ultimatum game* which allows us to separate static from dynamic motives across treatments. The simple modification we apply to the

² For a refinement of the learning model see EREV and ROTH 1998.

³ The payoff structure for the proposer is identical except in one end node. But, since the payoff of this end node is not reported to him, he cannot distinguish between the two treatments. The responder's payoffs are not affected by the treatment variable.

⁴ GNEEZY and STOLER (1998) show that in a one-shot situation, social punishment behaviour can be observed next to personal revenge.

ultimatum game is that in one treatment (the *covered response treatment*) the responder's decision is not reported to the proposer immediately, while in a second treatment (the *open response treatment*) it is. Hence, rejections observed in the covered response treatment cannot be explained by educative punishment, whereas the fairness utility models cannot explain differences across the two treatments. The former is true because the proposer cannot be educated by rejections, since he does not observe the rejections until after the last decision has been made in the experiment. The latter holds because the payoff and the strategic structure in both treatments is identical, therefore fairness utility models would predict the same rejection rates in both treatments. The experimental results reported in this paper suggest that behaviour is motivated both by resistance to unfairness as well as by educational punishment, but neither explanation is sufficient on its own.

II. Hypotheses

As mentioned, our covered response ultimatum game provides a qualitative test of the underlying hypotheses of the static fairness utility models and the dynamic approaches. We now put the two main hypotheses we test in the present study more specifically. The first hypothesis, which we call the hypothesis of *resistance to unfairness*, specifies the motivational assumptions underlying the fairness utility models. In these models, dynamic considerations are irrelevant. Only the final outcome matters for the players. They have no preferences for the distribution of the feedback information.

H1: The hypothesis of resistance to unfairness

Responders are exclusively motivated by their own payoff and the final outcome. We will therefore observe no difference in rejection rates between the covered and the open response treatment.

The second hypothesis addresses an educational motive by responders in the ultimatum game. Responders punish proposers in order to teach them not to behave unfairly in future. They utilise the fact that proposers face a much greater damage after a rejection than responders. Proposers are therefore thought to be influenced towards more equal more generous offers by punishment. As a population, on average, the responders benefit from the punishments.

H2: The hypothesis of educative punishment

Responders are exclusively motivated by their own payoff and the payoff of their own population: they reject unfair offers in order to educate the population of proposers to offer more generously. We will therefore observe no rejections in the covered response treatment and high rejection rates in the open response treatment.

Note that according to this hypothesis responders educate for the sake of the responder population as a whole: The education need not affect a particular proposer for later occasions when the same proposer and responder meet again. It is known that educative motivations are relevant even if it is impossible to build up a reputation that pays back in future play, since a second encounter of the same pair of players is excluded. Arguments in this line have been used in video-taped group discussions in a one-shot experiment on a trust game conducted by JACOBSEN and SADRIEH (1996).

The covered response ultimatum game separates the two hypotheses in the sense that rejections in the covered response treatment can be explained by H1 but not by H2. Observing more rejections in the open than in the covered response treatment, however, can be explained by H2 but not by H1.

Both hypotheses do not rule out one another. It might be the case that both motivations are relevant determinants of behaviour. Thus, our design is subtractive: In the open response treatment, both motivations might be relevant. The covered response treatment removes the educative motive from the game. We can interpret the rejection rate in the covered response treatment as the basic rate of resistance to unfairness. The difference in rejection rates is then the part added by the motive of educative punishment.

III. Model and Procedure

To simplify data analysis, we use a reduced version of the ultimatum game, similar to what BOLTON and ZWICK (1995) introduced as the *cardinal ultimatum game*, or what GALE, BINMORE, and SAMUELSON (1995) refer to as the *ultimatum minigame*. In our game, the proposer is restricted to two alternatives: the equal split (5,5) or a division favouring himself (8,2). The responder cannot reject the equal split. She can, however, choose to reject the unequal offer, in which case both players receive nothing.

Compared to the standard ultimatum game, the reduced version used here allows simpler data analysis. All relevant data appears in one number for each role: subjects' behaviour is expressed in proposer equal offer rates and responder rejection rates only. Previous experimental studies (BOLTON and ZWICK 1995, ABBINK, BOLTON, SADRIEH, and TANG 1996) have shown that the reduced form ultimatum game captures the most relevant ultimatum game characteristics.

The game tree of the game we used in the experiments is depicted in figure 1. It is the same for the open and the covered response treatment. The difference between the treatments is that in the covered response condition the responder's choice was not reported to the proposer. Only after the complete session, proposers in the covered response treatment were informed about their earnings. From this information, they could derive how many times they were punished, but not when and by whom. During the sessions, however, no such attribution could be made.

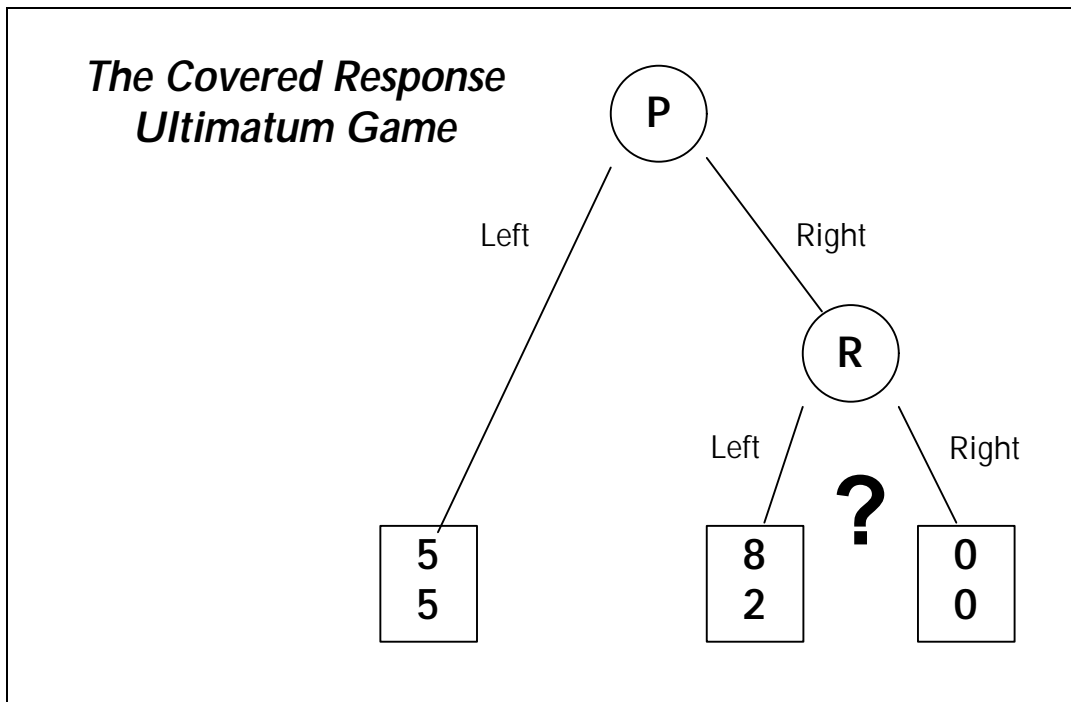


Figure 1

The experiments were computerised, with software developed using *RatImage* (ABBINK and SADRIEH 1995). The game was presented to the subjects in the game tree form, where the decisions were submitted by buttons at the branches of the tree. After a decision, the chosen branch was highlighted, the other ones lowlighted. In the open response treatment, the responder's choice and the resulting payoffs were immediately marked on the proposer's screen. In the covered response treatment, the proposer's screen only displayed a question mark between the two possible outcomes of that branch.

Each subject played in the same role, proposer or responder, during the entire session. The experiments were run using a revolving (or round-robin) matching scheme, such that each proposer met each responder only once. This was particularly important for the control condition with open responses. To ensure compatibility we used the same matching scheme in the covered response treatment, too. Eight rounds were played with eight proposers and eight responders.

Due to the revolving matching scheme, educative punishment cannot be applied to one's own benefit. A responder will never meet the same proposer again, thus she cannot punish a proposer in the hope of getting a better offer from him in later rounds. Only as a group can the responders hope to benefit from educative punishment. Thus, using the revolving matching scheme ensures that the motivation of educative punishment applied by populations cannot interfere with supergame effects.

Six sessions of each treatment were conducted, with a total of 192 subjects. The experiments were run in the *RatioLab* at the Hebrew University of Jerusalem, Israel, and in the *Laboratorium*

für experimentelle Wirtschaftsforschung at the University of Bonn, Germany. In a two-country experiment, the fact that the subjects in different countries are drawn from distinct subject pools must be taken into account. Differences in behaviour might arise from country-specific cultural environments, but also from a different composition of the subject pools with respect to educational background, gender, age, majors of study, and others. In fact, in the four country study by ROTH, PRASNIKAR, OKUNO-FUJIWARA, and ZAMIR (1991), especially the Israeli subjects' behaviour was slightly different from that in the other countries. In Jerusalem, lower offers and a higher tendency to accept low offers were observed in the ultimatum game. Although the study involves only few independent observations, we must be aware of the possibility of subject pool differences. We controlled for this by splitting the two treatments between the two locations evenly. Three sessions of each treatment were run in each laboratory. Thus, even if subject pool differences exist, they cannot be in conflict with treatment differences.

All subjects were volunteers and were only given monetary incentives. The sessions started with an introductory talk of about 10 minutes. The written instructions that were handed out to the subjects are reproduced in the appendix. To ensure that the instructions were equivalent in both countries, although the sessions were conducted in the local language of each country, the instructions were first written in English. They were then translated into each local language by a member of the local laboratory team. In the next step, another team member translated the translation back into English, without seeing the original English text. This procedure was repeated until the back-translation and the original text had converged. Convergence, however, was reached rather quickly, since a very neutral and technical phrasing was used. The same method was applied to the screen output, which was also displayed in the local language of each country.

The social composition of the two subject pools was similar, since both laboratories are located in social science buildings. Both subject pools mainly consisted of students of social sciences. The exchange rates of points to cash were adjusted in the two countries in a way that total earnings were comparable, in terms of teaching assistants' average hourly wage rates at each location. The exchange rate of DM 0.50 (roughly US-\$ 0.33 at that time) per point was used in Bonn, and NIS 0.75 (about US-\$ 0.25) was used in Jerusalem. Since the experiments were short (about 30-45 minutes in total), the resulting payoffs were on average well above the typical student's per hour wage.

III. Results

First, we focus on the responder behaviour. We begin with the analysis of aggregate rejection rates over the sessions.

III.1 Responder Rejection Rates

Table 1 shows the average overall rejection rates (rejected unequal offers to total unequal offers) in the six sessions of each treatment, ordered from the lowest to the highest. The Jerusalem sessions are marked with a cross, the Bonn sessions with an asterisk.

Table 1. Average rates of rejected unequal offers in each session (in percent)

covered	6.5 ⁺	17.6*	22.7 ⁺	26.1*	31.0*	31.3 ⁺	Ø=22.5
open	10.0 ⁺	20.0*	40.0*	43.5*	50.0 ⁺	80.0 ⁺	Ø=40.6

The rejection rates show that, even when the response is not reported to the proposer, almost one quarter of all unequal offers are turned down. This is clear evidence for responders' resistance to unfairness that is entirely independent of all considerations of monetary self-interest. Responders obviously cannot punish proposers in the covered response treatment - not even as a group - in order to receive higher payoffs later. Thus, the relatively high average rejection rate (22.5%) is evidently motivated by negative emotions towards unfair actions or distributions. Responders are willing to pay a price solely to soothe their anger concerning the proposer's greed. The disutility of unfair outcomes in fairness utility can be interpreted as a formalisation of such an emotional component.

However, fairness utility models do not explain a different aspect of our data. There is a significant difference in the rejection rates of the two treatments. The average rejection rates of the open response condition are about 75% higher than those of the covered treatment. The difference is significant with a *p*-value of 0.051, according to the Mann-Whitney U-test applied to the average rates of rejection in sessions. The extent to which more rejections are observed in the open response treatment must be attributed to the visibility of the rejection. The difference caused by visibility could be based on the fact that visibility turns rejection into an act of reciprocal punishment. Since visibility allows that the reciprocation can uniquely be ascribed to the reciprocating subject, the connection between punisher, the reason for punishment, and the addressee of the punishment becomes unambiguous. This - in a second step of reasoning - allows for an immediate perception of the educational goal of punishment by the proposers.

In this sense, visibility enables a responder to *educate* a proposer in an attempt to educate the group of proposers as a whole. Obviously, proposers, who assume to be faced with a group of responders that strictly reject any unfair proposals, will tend to switch to more equal split offers. A similar type of behaviour is reported by WINTER and ZAMIR (1996). An attempt to educate the group of proposers in fact is well in line with the hypothesis of population rationality put forward in that work.

III.2 Does Behaviour Change Over Time?

Figure 2 shows the aggregate rejection rates over the eight rounds of the experiment. It is consistent with the hypothesis of educative punishment that the differences between the rejection rates across treatments diminish towards the end of the session: in the last two rounds, when educative punishment makes little or no sense, almost no difference between the treatments can be observed. The fact that average rejection rates get closer to one another is mainly due to the tendency of the rejection rates in the open response treatment to fall over time. From the first to the second half of a session (four rounds each), the aggregate rejection rate falls from 39.8% to 29.1% in the open response treatment. The decrease is significant at $p = 0.015$, according to the Wilcoxon matched pairs signed rank test, applied to the difference of rejection rates between the first and the second half of the experiment in the six independent sessions. In contrast, the rejection rates in the covered response treatment fall only slightly (from 24.2% to 21.4%), and the decrease is not significant. The corresponding numbers for the single sessions are shown in the following table 2.

Table 2. Rejection rates in the first and the second half of the experiment

Session	Open treatment			Covered treatment		
	Rounds 1-4	Rounds 5-8	Δ	Rounds 1-4	Rounds 5-8	Δ
1*	0.44	0.43	-0.01	0.38	0.23	-0.15
2*	0.18	0.21	+0.03	0.33	0.29	-0.04
3*	0.44	0.40	-0.04	0.19	0.17	-0.02
4 ⁺	0.52	0.43	-0.09	0.20	0.25	+0.05
5 ⁺	0.82	0.75	-0.07	0.29	0.33	+0.04
6 ⁺	0.18	0.07	-0.11	0.13	0.00	-0.13
Average	0.43	0.38	-0.05	0.25	0.21	-0.04
Aggregate	0.40	0.29	-0.11	0.25	0.21	-0.04

*= Bonn session ⁺= Jerusalem session

This result, together with the result that average rejection rates over the complete sessions in the covered response treatment are significantly lower than those in the open response treatment, leads to the following conjecture: Responders' rejection rates in ultimatum games consist to a certain extent of rejections due to resistance to unfairness, and to some degree of rejections motivated by educative punishment. It seems that the fairly stable rate of rejections in all rounds of the covered response treatment reflects some kind of basic rate of rejections due to resistance to unfairness in our subject pool. The higher rates of rejection in the early rounds of the open response treatment appear to incorporate some amount of rejections motivated by educative

punishment in addition to the basic rate of rejections due to resistance to unfairness. Towards the end of the sessions under the open response condition, the added rate of rejections motivated by educative punishment decreases, leading to the observed decrease in the difference between the total rejection rates of the two treatments.

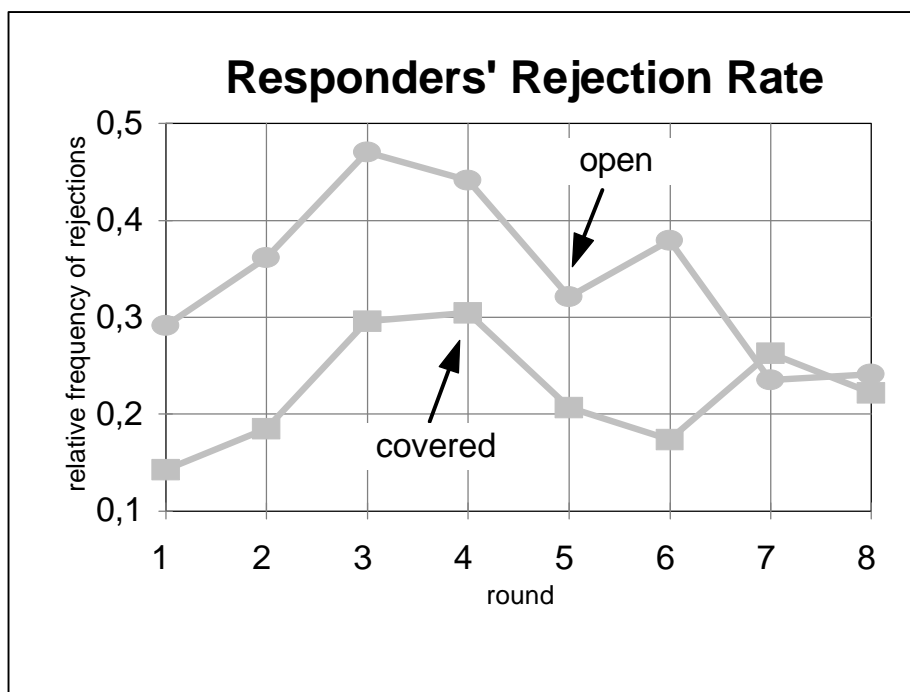


Figure 2

III.3 Proposer Equal Offer Rates

In figure 3, the round by round aggregate equal offer rates are depicted. Table 3 shows the session averages equal offers rates, ordered from the smallest to the largest. Again, Jerusalem sessions are marked with a cross, and Bonn sessions with an asterisk.

Table 3 Average rates of equal offers in each session (in percent)

covered	31.3 ⁺	46.9*	50.0 ⁺	51.6 ⁺	54.7*	64.1*	Ø=49.8
open	21.9*	21.9*	21.9 ⁺	28.1*	43.8 ⁺	76.6 ⁺	Ø=35.7

In the covered response treatment, we observe equal offers in half of the cases. This rate is about 40% higher than in the open response treatment. Aggregated over all eight rounds, the difference across treatments is significant at $p = 0.026$ (one-tailed), according to the Mann-Whitney U-test, applied to average equal offer rates in the sessions.

Already in the very first round, we observe a higher equal offer rate in the covered response treatment. 70.8% of the proposers choose an equal offer in the first round, whereas 50.0% do so in the open response treatment. According to Fisher's exact test, this difference is significant at

$p = 0.019$ (one-tailed). Note that the individuals' equal offers are independent in the very first round of the experiment.

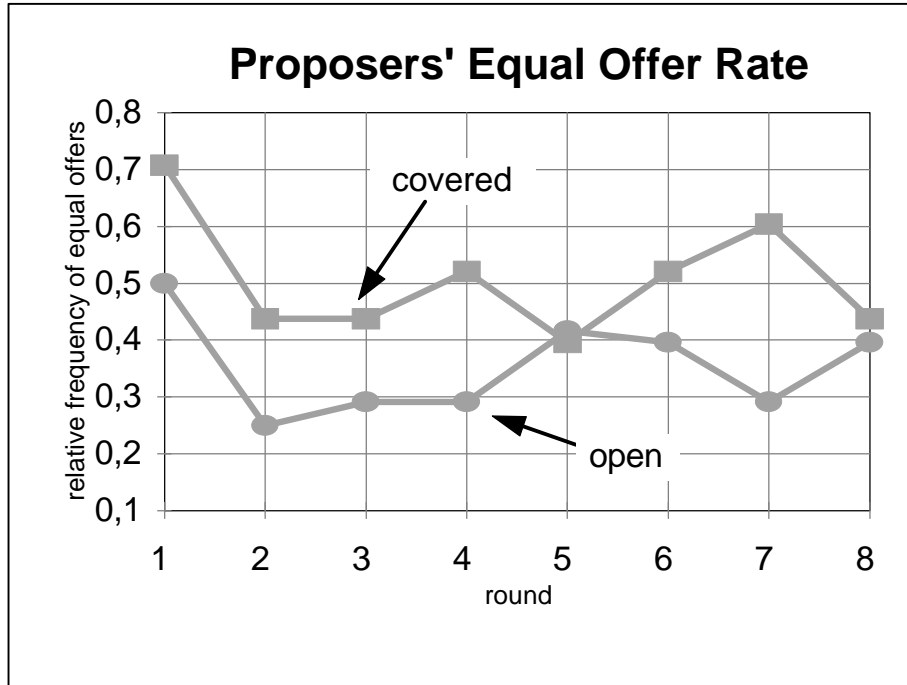


Figure 3

It is interesting that the equal offer rates in the covered response treatment are significantly higher than in the open treatment, although the rejection rates of unequal offers are lower. It seems plausible that the high equal offer rates in the covered response treatment are due to a type of risk avoidance of the proposers. If the proposer makes an unequal offer, his payoff can vary between 0 and 8, depending on the responder's choice. In contrast, he is guaranteed a payoff of 5 when making an equal offer. Thus, if the proposer believes that there is a positive probability for some responders to reject the unequal offer, the payoff on the right branch is uncertain, with an unknown probability.

To explain the discrepancy in proposers' first round behaviour across treatments, the following behavioural hypothesis seems suitable: Subjects proposing in the open response treatment tend to test responders' propensity to reject by selecting the unequal offer branch. Learning from success or failure, however, is completely impossible for proposers in the covered response treatment. It seems that the lack of feedback increases the tendency to avoid risk and leads to the significantly higher equal offer rates in the entire session in the covered response treatment.

III.4 Is Educative Punishment Effective?

Given that responders in the open response treatment use punishment as a means of education, the question arises whether their attempt is successful, in the sense that in the long run, proposers tend to shy away from the unequal offer because of the expectation of a punishment. If this is the case, then we should observe an increase of equal offer rates in those proposer populations in which rejection rates are highest. For each session, we look at rejection rates in the first half of the experiment and the change of equal offer rates from the first to the second half. We compute the correlation of these two measures over the sessions. If educative punishment is effective, then a tendency for high first half rejection rates to be followed by high increases in equal offer rates should be observed.

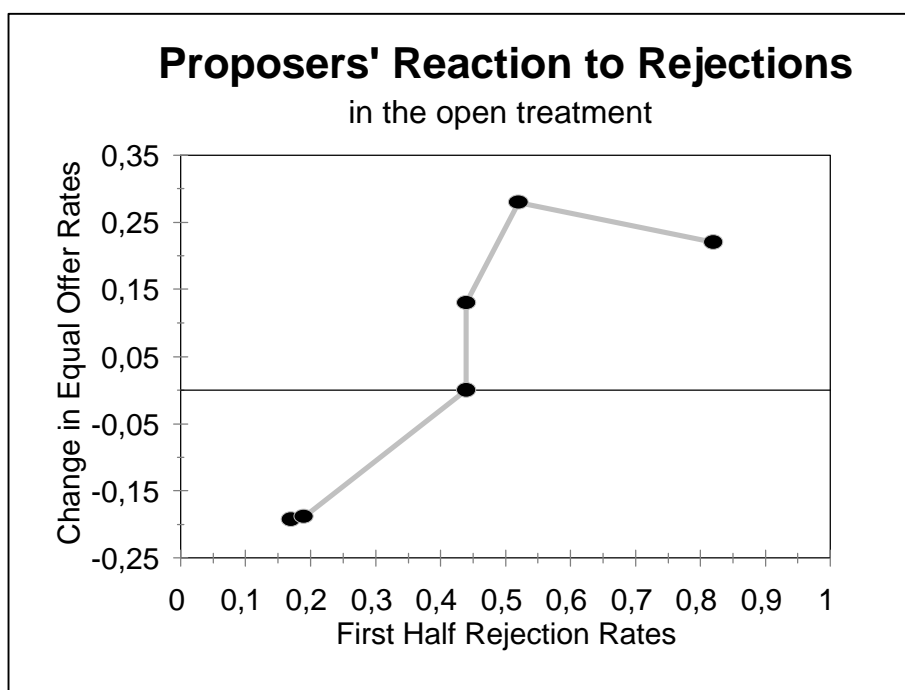


Figure 4

Figure 4 shows that in fact, there is a strong correlation in the predicted direction. The greater the responders' reluctance to accept unfair offers, the greater is the rise in the frequency of equal offers with time. The Spearman rank correlation coefficient of $r_s = 0.93$ is significantly different from zero at $\forall = 0.05$ (one-tailed). Thus, high frequencies of rejections correlate to higher increases in equal offer rates.

We now examine whether the positive correlation between rejection rates and the change in equal offer rates can be detected on individual level. To see this, we check each round in which a proposer switches from a preceding round's unequal to a current round's equal offer. We count how often the equal offer was made after the last round's unequal offer was rejected, and

compare this to the frequency of equal offers following an accepted unequal offer. If educative punishment were successful, then we should observe systematically more switches after punishments than after accepted unequal offers.

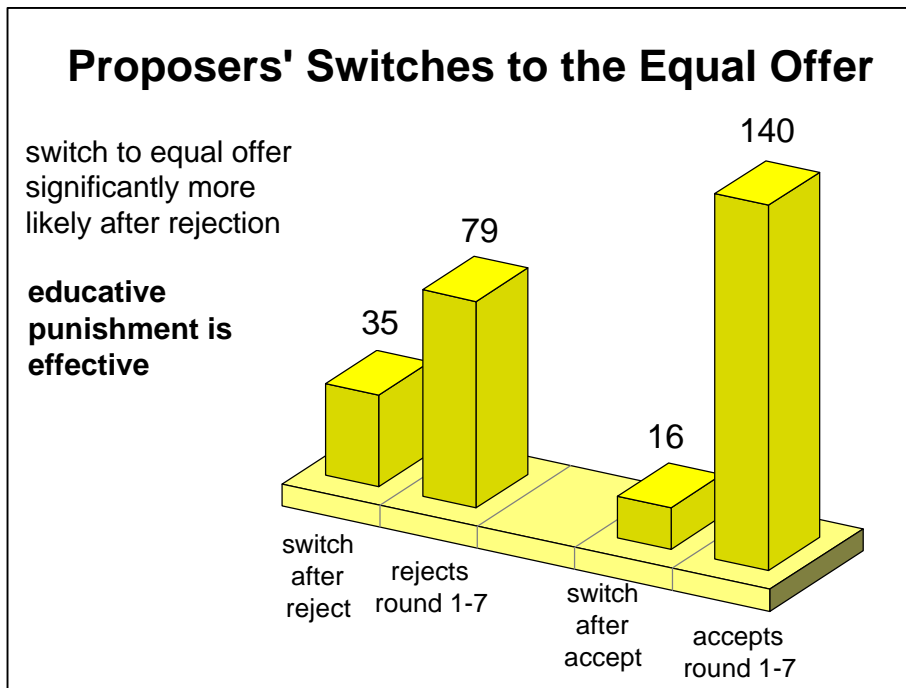


Figure 5

Figure 5 shows that educative punishment is indeed effective on the individual level. Obviously, observing a rejection, i.e. being punished, influences the proposers' behaviour. Proposers' propensities to choose the equal offer is rather low if their unequal offer in the last round was accepted (relative frequency 11.4%). But, if punished, their tendencies to switch is almost four times higher (relative frequency 44.3%). This is consistent with the hypothesis that proposers in the open response treatment attempt to test the probability of rejection and switch to the equal offer if that probability is perceived as high.

The observed pattern of switches points in the same direction in all six sessions of the open response treatment. Table 4 shows the frequencies of switches after punishments and after accepted unequal offers. The last column shows the difference between the relative frequencies of switches. In all six sessions this measure has a positive value. Thus, a switch occurs relatively more often after punishments than after accepted unequal offers. The Wilcoxon matched-pair sign test rejects the null hypothesis that a switch is equally likely after punishment as after an accepted unequal offers with at $p = 0.016$ (one-tailed).

Table 4. Frequency of switches from the unequal of the equal offer

Session	Switch after Rejection (1)	Rejections Rounds 1-7 (2)	Switch after Accepted offer (3)	Accepted offers rounds 1-7 (4)	Difference in rel. frequency (1)/(2)-(3)/(4)
1	7	17	3	24	+0.29
2	2	10	2	33	+0.14
3	5	18	3	26	+0.16
4	8	17	4	17	+0.24
5	11	12	1	3	+0.58
6	2	5	3	37	+0.32
Σ	35	79	16	140	+0.33

After having switched to an equal offer, proposers often switch back. Of all equal offers, 46% are followed by an unequal offer. Thus, though proposers do react to punishment, they also show a strong tendency to switch back to the unequal offer. This implies that high rejection rates are necessary for a sustained educative effect. Interestingly, the frequency of switching from the equal to the unequal offer is almost the same in both treatments: under the covered response condition we observe a relative frequency of 47%.

The results show that proposers in fact tend to react to punishment in a manner that is favourable to the responders. If responders anticipate such behaviour, it can be reasonable for them as a group to reject unequal offers. Of course, when applying punishment as a means of education, responders are in a social dilemma situation. Due to the revolving matching scheme of the experiment, no responder can profit from her own punishment. She can only benefit from other responders' social reasoning, and others will benefit from her punishment. The revolving matching scheme may reduce educational motives in behaviour, but it does not seem to eliminate them.

III.5 Is Educative Punishment Profitable?

We have found evidence that educative punishment is effective in the sense that proposers are influenced towards making more equal offers, after having observed rejections. However, since rejections are costly for a responder, the effectiveness of educative punishment does not immediately imply that such behaviour is profitable for responder populations. In this section, we analyse how rejection rates and responder profits are distributed over the populations.

Figure 6 shows the relationship between the rejection rates in a session and the average responder payoff (in per cent of the cake) in that session. The numbers are computed on the

basis of all eight rounds of the session. If educative punishment is profitable, then we should observe higher responder payoffs in the sessions with high rejection rates.

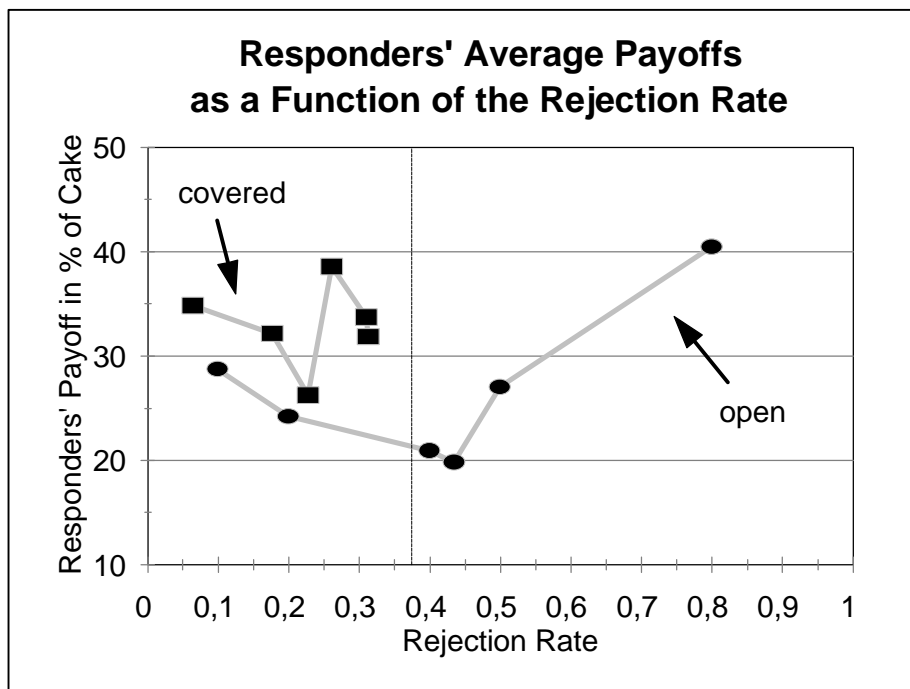


Figure 6

The correlation between responder payoff and rejection rates seems to be U-shaped rather than monotonously rising or falling. It seems that the more extreme patterns of behaviour are more profitable than those in between. Educative punishment seems to be profitable only if the rejection rates are sufficiently high to induce a strong effect. Where the rejection rates are intermediate, the responders pay the price for punishment, but the frequency of unequal offers is not sufficiently decreased to compensate the costs of punishment. To illustrate this point, we have included the vertical line into the figure. At a rejection rate of 0.375, expected payoff maximising proposers are exactly indifferent between the equal and the unequal offer. Thus, rejection rates close to that line are least profitable. The very low and especially the very high rejection rates are preferable for payoff maximising responder populations. However, it should be noticed that the data basis is too small to provide clear statistical evidence for this effect.

III.6 Does Educative Punishment Increase Efficiency?

Efficiency in our context can simply be understood as the total payoff gained by responders and proposers in a session together. Obviously every rejection reduces the total payoff in a round. The efficiency loss, in points, is exactly the absolute overall number of rejections in a session, multiplied by the cake size of 10. On the other hand, if the proposers are successfully educated

by high early rejection rates to increase the number of equal offers, the total number of rejections will drop, since equal offers are not rejected. Hence, if the effect of educative punishment is strong enough, then high rejection rates in the beginning of a session need not necessarily lead to less efficiency than in a session with a constant moderate rate of unequal offers and rejections.

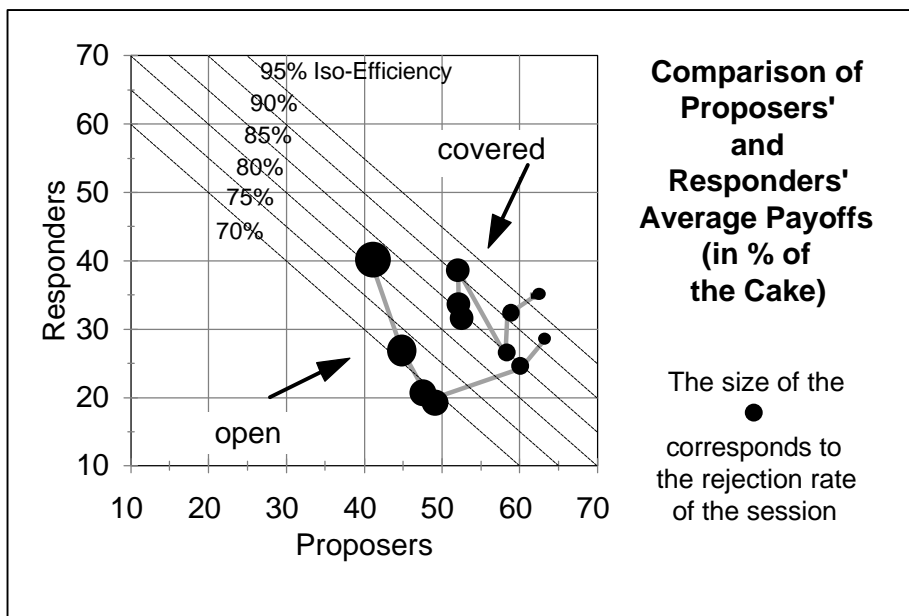


Figure 7

In figure 7, we depicted the average proposer payoffs on the x-axis and the average responder payoffs on the y-axis. The dots mark the average responder and proposer payoffs in the sessions, where the size of the dots correspond to the average rejection rate in that session. The diagonal lines are iso-efficiency lines. Different points on the same iso-efficiency line represent allocations with the same efficiency, but a different distribution between proposers and responders. It can be seen that not only the average responder payoff, but also efficiency is lowest with intermediate rejection rates. The highest efficiency is achieved in the session with the lowest rejection rate, where evidently most of the efficiency gain is extracted by the proposers. Thus, high rejection rates have a similar effect on efficiency as on responder payoffs. If rejection rates are intermediate, then the effect of educative punishment is too small to outweigh the efficiency losses caused by the rejections. However, we must once again point out that the data basis is small and no conclusive inference can be drawn.

Finally, in figure 7 it can immediately be noticed that efficiency in the covered treatment is greater than in the open treatment. This is due to the lower rejection rates coinciding with higher equal offer rates. The difference is significant at $p = 0.017$ (one-tailed) according to the Mann-Whitney U-test applied to the average per round total payoff in the single independent sessions.

IV. Summary and Conclusions

We conducted an experiment using a simple ultimatum game. In the covered response treatment, the responder's choice was reported to the proposer, while it was not in the open treatment. Rejection rates in the covered response treatment - with an average of 23.3% - were considerable. Since rejections cannot have been motivated by educative punishment in the covered response treatment, we conclude that the high rejection rates in these sessions are due to the responders' inherent resistance to unfairness. This is in line with the fairness utility models as suggested by BOLTON (1991), RABIN (1993), BOLTON and OCKENFELS (1997), and FEHR and SCHMIDT (1998).

We, however, also observed a significantly higher rate of rejections in the open response treatment than in the covered response treatment. This difference cannot be explained by the fairness utility models mentioned. Hence, we conclude that rejections in the open response treatment are not only motivated by responders' resistance to unfairness, but also by educative punishment. Educative punishment in this setting, in which each responder met each proposer only once, means that the group of responders, as a whole, attempts to influence the behaviour of the group of proposers, as a whole, in direction of equal offers. Educative punishment motivation, however, cannot account for the high rejection rates in the covered response treatment, since educating proposers who do not see the rejection is impossible.

Given the results of the experiment, we conjecture that responders' rejection in ultimatum bargaining is in part motivated by a resistance to unfairness, and in part by educative punishment. We find support for this conjecture in the dynamics of rejection behaviour. The average per round rejection rates in the open response treatment decrease towards the end of the experiment. In the last rounds, in which educative punishment makes little or no sense, these rates approach the average rates of rejection of the covered response treatment, which are have no clear time trend. This suggests that there is a basic rate of rejections in ultimatum game behaviour, due to the responders' resistance to unfairness. This basic rate corresponds to the rate of rejections in the covered response treatment and in the last rounds of the open response treatment. The excess of the rejection rates over the basic rate in the early rounds of the open response treatment can seemingly be attributed to educative punishment.

We hypothesise that proposers in early rounds of the open treatment "test" responders' reactions to unequal offers. This would explain the significantly higher number of unequal first round offers in the open compared to the covered response treatment. Additionally, there is strong evidence that educative punishment is effective. The higher the rejection rates in the first half of the experiment, the more do proposers change their behaviour towards equal offers. Furthermore, proposers switch to equal offers significantly more often after rejected than after accepted unequal offers of the previous round. There is no clear evidence that educative punishment is profitable for responder groups, but it seems that the more extreme patterns of

behaviour (either very low or very high rejections) are more profitable than the intermediate ones.

All in all, the experiment reported suggests that both the educative punishment approach and the fairness utility approach explain behaviour in ultimatum bargaining to some extent. Neither approach, however, seems completely satisfactory on its own. Future research should focus on a conciliatory synthesis which takes both aspects of behaviour into account.

References

- ABBINK, Klaus, Gary E BOLTON, Abdolkarim SADRIEH, and Fang-Fang TANG (1996): Adaptive Learning versus Punishment in Ultimatum Bargaining. SFB Discussion Paper B-381, University of Bonn.
- ABBINK, Klaus, and Abdolkarim SADRIEH (1995): RatImage - Research Assistance Toolbox for Computer-Aided Human Behavior Experiments. SFB Discussion Paper B-325, University of Bonn.
- BOLTON, Gary E (1991): A Comparative Model of Bargaining: Theory and Evidence. *American Economic Review*, 81:5, 1096-1135.
- BOLTON, Gary E, and Axel OCKENFELS (1997): ERC - A Theory of Equity, Reciprocity and Competition. Working paper, PennState University and University of Magdeburg.
- BOLTON, Gary E, and Rami ZWICK (1995): Anonymity versus Punishment in Ultimatum Bargaining. *Games and Economic Behavior*, 10, 95-121.
- CAMERER, Colin, and Richard H. THALER (1995): Anomalies - Ultimatums, Dictators and Manners. *Journal of Economic Perspectives*. 9:2, 209-219.
- EREV, Ido, and Alvin E. ROTH (1998): Predicting How People Play Games: Reinforcement Learning in Experimental Games with Unique, Mixed Strategy Equilibria. *American Economic Review*, 88:4, 848-882.
- FEHR, Ernst, and Klaus M. SCHMIDT (1998): A Theory of Fairness, Competition, and Cooperation. How to Account for Fair and Unfair Outcomes. Working Paper, University of Zürich and University of Munich.
- GALE, John, Kenneth G BINMORE, and Larry SAMUELSON (1995): Learning to be Imperfect: The Ultimatum Game. *Games and Economic Behavior*, 8, 56-90.
- GNEEZY, Uri, and Avraham STOLER (1998): Why Punish? Norms and Revenge in an Experimental Game. Working paper, University of Haifa and University of Tel Aviv.

- GÜTH, Werner (1995): On Ultimatum Bargaining Experiments - A Personal Review. *Journal of Economic Behavior and Organization*, 27, 329-344.
- GÜTH, Werner, R. SCHMITTBERGER, and B. SCHWARZE (1982): An Experimental Analysis of Ultimatum Bargaining. *Journal of Economic Behavior and Organization*, 3, 367-388.
- JACOBSEN, Eva, and Abdolkarim SADRIEH (1996): Experimental Proof for the Motivational Importance of Reciprocity. SFB Discussion Paper B-386, University of Bonn.
- RABIN, Matthew (1993): Incorporating Fairness into Game Theory and Economics. *American Economic Review*, 83, 1281-1302.
- ROTH, Alvin E. (1995): Bargaining Experiments. In John Kagel and Alvin E. Roth (eds): *Handbook of Experimental Economics*. Princeton University Press.
- ROTH, Alvin E., Vesna PRASNIKAR, Masahiro OKUNO-FUJIWARA, and Shmuel ZAMIR (1991): Bargaining and Market Behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo. *American Economic Review*, 81, 1068-1095.
- ROTH, Alvin E., and Ido EREV (1995): Learning in Extensive Form Games: Experimental Data and Simple Dynamic Models in Intermediate Term. *Games and Economic Behavior*, 8, 164-212.
- THALER, Richard H. (1988): The Ultimatum Game. *Journal of Economic Perspectives*, 2, 195-206.
- WINTER, Eyal, and Shmuel ZAMIR (1996): Experimenting Ultimatum Bargaining with Virtual Players. Working Paper, Hebrew University of Jerusalem.

Appendix A: The Written Instructions

Player Types:

There are two types in the experiment: player 1 and player 2.
After the introduction, each participant draws one of 16 cards.
The drawn card defines the terminal number of the participant.
The terminal number determines the participant's type for the whole experiment.

Structure:

The experiment consists of eight rounds.
In each round 8 pairs of participants are formed:
 each pair with one player 1 and one player 2.
In every round, every player 1 meets a different player 2, and vice versa.
Thus, no participant meets the same participant a second time.
The participants are not allowed to speak with each other during the experiment.

Decisions:

Each round begins with player 1 choosing one of two alternatives: Left or Right.
Player 2 is informed about the choice of player 1.
If player 1 chooses Left, the round ends.
If player 1 chooses Right, player 2 chooses one of two alternatives: Left or Right;
 player 1 is not informed about the choice of player 2;
 then the round ends.

Profits in Points:

Player 1	Player 2	Player 1 receives	Player 2 receives
chooses Left	has no choice	5	5
chooses Right	chooses Left	8	2
chooses Right	chooses Right	0	0

The Question Mark:

If player 1 chooses Right, he will only see a question mark on the screen: since he will not be informed about the choice of player 2, he will neither know his own profit, nor the profit of player 2.
After the last round of the experiment all participants will be informed of their total profits, but the players 1 will not be informed of the value of each received ? separately.

Exchange Rate:

Each point earned in the experiment is equivalent to 50 pfennigs / 75 Agorot.

Appendix B: The Data

Session 1: Covered, Bonn

1	2L 16	11R 15R	13R 3L	8L 1	14R 9L	17L 6	12R 5L	18L 7
2	2R 15L	11L 3	13R 1R	8R 9R	14R 6L	17L 5	12R 7L	18L 16
3	2L 3	11R 1R	13R 9R	8L 6	14L 5	17R 7L	12R 16L	18L 15
4	2L 1	11L 9	13R 6L	8L 5	14R 7L	17L 16	12R 15R	18L 3
5	2L 9	11R 6L	13R 5L	8R 7L	14L 16	17L 15	12L 3	18L 1
6	2R 6L	11L 5	13R 7L	8R 16L	14R 15R	17R 3L	12L 1	18L 9
7	2L 5	11L 7	13L 16	8L 15	14L 3	17L 1	12L 9	18L 6
8	2R 7L	11R 16L	13L 15	8R 3L	14R 1R	17R 9R	12L 6	18L 5

Session 2: Covered, Bonn

1	2L 16	11L 15	13L 3	8L 1	14L 9	17L 6	12L 5	18L 7
2	2L 15	11L 3	13R 1L	8L 9	14L 6	17L 5	12L 7	18L 16
3	2L 3	11R 1R	13R 9L	8R 6L	14L 5	17R 7L	12R 16L	18R 15L
4	2R 1R	11L 9	13L 6	8R 5R	14L 7	17L 16	12L 15	18L 3
5	2L 9	11R 6L	13R 5R	8R 7R	14R 16L	17L 15	12R 3R	18L 1
6	2L 6	11L 5	13L 7	8L 16	14R 15L	17L 3	12L 1	18R 9L
7	2L 5	11L 7	13R 16L	8R 15L	14R 3R	17R 1L	12L 9	18L 6
8	2L 7	11R 16L	13R 15L	8L 3	14R 1L	17L 9	12L 6	18L 5

Session 3: Covered, Bonn

1	2R 16L	11L 15	13L 3	8L 1	14R 9L	17L 6	12L 5	18L 7
2	2R 15L	11R 3L	13R 1L	8L 9	14R 6L	17L 5	12L 7	18R 16L
3	2R 3L	11R 1L	13L 9	8R 6R	14R 5R	17L 7	12R 16R	18R 15L
4	2R 1L	11R 9L	13L 6	8L 5	14R 7L	17L 16	12L 15	18L 3
5	2R 9L	11R 6L	13R 5L	8R 7L	14R 16L	17L 15	12L 3	18R 1L
6	2L 6	11R 5R	13R 7L	8L 16	14L 15	17L 3	12L 1	18L 9
7	2R 5R	11R 7L	13L 16	8L 15	14L 3	17R 1L	12L 9	18L 6
8	2R 7L	11R 16L	13R 15L	8R 3L	14R 1L	17L 9	12R 6R	18R 5L

Session 4: Covered, Jerusalem

1	15R 4L	11L 2	13L 3	9R 1L	14R 8L	16L 6	12R 5L	10L 7
2	15L 2	11R 3L	13L 1	9R 8R	14R 6L	16R 5L	12L 7	10R 4L
3	15R 3L	11L 1	13R 8L	9R 6L	14R 5R	16R 7L	12L 4	10L 2
4	15L 1	11R 8L	13R 6L	9R 5R	14R 7R	16R 4L	12R 2L	10L 3
5	15R 8R	11L 6	13R 5L	9R 7L	14R 4L	16L 2	12R 3L	10R 1R
6	15R 6L	11R 5L	13R 7L	9R 4L	14R 2L	16R 3L	12R 1R	10L 8
7	15L 5	11L 7	13R 4L	9R 2L	14R 3L	16R 1R	12R 8R	10L 6
8	15L 7	11R 4L	13R 2L	9R 3L	14R 1R	16R 8L	12R 6L	10L 5

Session 5: Covered, Jerusalem

1	15L 4	11L 2	13R 3L	9R 1L	14L 8	16L 6	12L 5	10L 7
2	15R 2R	11R 3R	13R 1L	9R 8L	14R 6L	16L 5	12R 7L	10L 4
3	15L 3	11L 1	13R 8R	9L 6	14R 5L	16R 7L	12L 4	10L 2
4	15R 1L	11R 8L	13R 6L	9R 5L	14R 7R	16L 4	12R 2R	10L 3
5	15R 8L	11R 6L	13R 5L	9R 7L	14L 4	16R 2R	12L 3	10L 1
6	15L 6	11L 5	13R 7R	9L 4	14R 2L	16L 3	12R 1L	10L 8
7	15R 5L	11R 7L	13R 4R	9L 2	14R 3L	16L 1	12L 8	10L 6
8	15R 7R	11R 4R	13R 2L	9L 3	14L 1	16L 8	12L 6	10L 5

Session 6: Covered, Jerusalem

1	15L 4	11L 2	13R 3L	9L 1	14L 8	16R 6R	12R 5L	10L 7
2	15R 2L	11R 3L	13R 1L	9L 8	14R 6L	16L 5	12R 7L	10L 4
3	15L 3	11L 1	13R 8L	9L 6	14R 5L	16R 7R	12R 4L	10L 2
4	15R 1L	11R 8L	13R 6L	9L 5	14L 7	16L 4	12R 2L	10L 3
5	15L 8	11R 6L	13R 5L	9L 7	14L 4	16R 2L	12R 3L	10L 1
6	15R 6L	11L 5	13R 7L	9L 4	14L 2	16R 3L	12R 1L	10L 8
7	15L 5	11L 7	13R 4L	9L 2	14R 3L	16R 1L	12R 8L	10L 6
8	15L 7	11L 4	13R 2L	9L 3	14R 1L	16L 8	12R 6L	10L 5

Session 7: Open, Bonn

1	2L 16	11R 15L	13L 3	8L 1	14R 9L	17R 6L	12R 5R	18R 7L
2	2R 15R	11R 3R	13R 1R	8R 9L	14R 6L	17R 5R	12R 7L	18L 16
3	2L 3	11R 1L	13L 9	8R 6L	14R 5R	17R 7L	12R 16R	18R 15R
4	2R 1L	11R 9L	13R 6L	8R 5R	14R 7L	17R 16R	12R 15R	18L 3
5	2L 9	11R 6L	13R 5R	8R 7L	14R 16R	17L 15	12R 3R	18R 1L
6	2R 6L	11R 5L	13L 7	8R 16R	14R 15L	17R 3R	12L 1	18R 9L
7	2L 5	11R 7L	13L 16	8R 15L	14R 3R	17L 1	12L 9	18R 6L
8	2R 7L	11R 16R	13L 15	8R 3R	14R 1L	17L 9	12L 6	18R 5R

Session 8: Open, Bonn

1	2R 16L	11R 15L	13R 3L	8L 1	14L 9	17L 6	12R 5L	18L 7
2	2R 15L	11R 3R	13R 1L	8L 9	14R 6L	17R 5L	12R 7L	18L 16
3	2R 3L	11R 1L	13R 9L	8R 6L	14R 5R	17L 7	12R 16R	18L 15
4	2R 1L	11R 9L	13R 6L	8R 5L	14L 7	17R 16L	12R 15R	18L 3
5	2R 9L	11R 6R	13R 5R	8R 7L	14R 16L	17L 15	12R 3L	18L 1
6	2R 6R	11R 5L	13R 7L	8R 16R	14R 15L	17R 3R	12R 1L	18R 9L
7	2R 5L	11R 7L	13R 16R	8R 15L	14R 3L	17L 1	12R 9L	18R 6L
8	2R 7L	11R 16L	13R 15L	8R 3L	14R 1L	17L 9	12R 6L	18R 5L

Session 9: Open, Bonn

1	2R 16L	11R 15L	13L 3	8R 1L	14L 9	17R 6R	12R 5L	18R 7R
2	2R 15L	11L 3	13R 1L	8R 9L	14R 6R	17R 5L	12R 7R	18R 16L
3	2R 3R	11R 1L	13R 9L	8R 6R	14L 5	17R 7R	12R 16R	18L 15
4	2R 1L	11R 9R	13R 6R	8R 5L	14R 7R	17R 16L	12L 15	18L 3
5	2R 9L	11L 6	13R 5L	8R 7R	14L 16	17R 15L	12R 3L	18L 1
6	2R 6R	11R 5L	13R 7R	8R 16R	14L 15	17R 3R	12R 1L	18L 9
7	2R 5L	11R 7R	13R 16L	8R 15L	14R 3L	17R 1L	12R 9L	18R 6R
8	2R 7R	11L 16	13L 15	8R 3L	14R 1L	17R 9L	12R 6R	18R 5L

Session 10: Open, Jerusalem

1	15R 4L	11L 2	13L 3	9R 1L	14L 8	16R 6R	12R 5L	10L 7
2	15R 2R	11R 3L	13R 1L	9R 8R	14L 6	16R 5L	12L 7	10R 4R
3	15R 3L	11R 1R	13R 8L	9R 6L	14R 5R	16R 7L	12R 4R	10L 2
4	15R 1L	11R 8R	13R 6R	9L 5	14R 7R	16R 4R	12R 2R	10L 3
5	15R 8R	11L 6	13L 5	9L 7	14L 4	16L 2	12L 3	10R 1L
6	15R 6L	11L 5	13R 7L	9R 4L	14L 2	16R 3R	12L 1	10L 8
7	15R 5R	11L 7	13R 4R	9R 2R	14L 3	16L 1	12R 8L	10R 6L
8	15R 7L	11L 4	13L 2	9R 3L	14L 1	16L 8	12L 6	10R 5R

Session 11: Open, Jerusalem

1	15L 4	11L 2	13R 3R	9R 1R	14L 8	16L 6	12L 5	10L 7
2	15R 2L	11R 3R	13L 1	9L 8	14L 6	16R 5R	12R 7R	10L 4
3	15R 3R	11L 1	13L 8	9R 6R	14L 5	16L 7	12L 4	10L 2
4	15R 1R	11L 8	13R 6L	9L 5	14R 7R	16L 4	12L 2	10L 3
5	15L 8	11L 6	13R 5R	9R 7R	14L 4	16L 2	12L 3	10L 1
6	15L 6	11L 5	13L 7	9L 4	14L 2	16L 3	12L 1	10L 8
7	15R 5R	11L 7	13R 4L	9L 2	14L 3	16L 1	12L 8	10L 6
8	15L 7	11L 4	13L 2	9L 3	14L 1	16L 8	12L 6	10L 5

Session 12: Open, Jerusalem

1	15L 4	11L 2	13R 3L	9R 1R	14L 8	16R 6L	12L 5	10L 7
2	15R 2L	11R 3L	13R 1L	9L 8	14R 6L	16R 5L	12L 7	10R 4L
3	15R 3L	11R 1R	13R 8L	9L 6	14R 5L	16R 7R	12R 4L	10R 2L
4	15R 1L	11L 8	13R 6R	9R 5L	14L 7	16R 4L	12R 2L	10R 3L
5	15R 8L	11R 6L	13R 5L	9R 7L	14L 4	16R 2L	12R 3L	10R 1L
6	15L 6	11R 5L	13R 7L	9L 4	14L 2	16R 3L	12R 1R	10R 8L
7	15R 5L	11R 7L	13R 4L	9R 2L	14R 3L	16R 1L	12R 8L	10R 6L
8	15R 7L	11R 4L	13R 2L	9R 3L	14R 1R	16R 8L	12R 6L	10R 5L

Legend:

Each line represents one round of the session. Each column stands for one match.

“15 R 4 L” read: terminal 15, proposer, chose Right, terminal 4, responder, chose Left.