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Endogenous Property Rights in a Hold up-Experiment

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ABSTRACT:

In a hold-up experiment designed to test theoretical predictions following from Hart (1995) and deMeza/Lockwood (1998) regarding investment behavior Sonnemans et al. (2001) (SOS) find only a partial confirmation of theory. According to SOS these deviations from standard theory can be explained by positive reciprocal behavior. In this paper, we replicate the experiment by SOS and add another group of treatments in which asset ownership is endogenized by auctioning off the assets. Our experiment shows that the results by SOS crucially depend on the ownership structure being exogenously assigned by the experimenter. We present experimental evidence that, by and large, corroborates the theoretical predictions made by Hart (1995).

Keywords: property rights, hold-up, experiment, endogenous ownership structure

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1. INTRODUCTION

Grossman/Hart (1986), Hart/Moore (1990) and Hart (1995) (in the following abbreviated by "GHM") develop a theory of the firm in which owning more assets increases the investor's incentives to make specific investments in his human capital. The reason for this is that owning more assets increases the marginal productivity of investment in the no-trade case. This, in turn, strengthens the investor's bargaining position at the renegotiation stage. The investor will accordingly invest more at the investment stage when he owns more assets than when he owns less. To turn the argument on its head, it means investment incentives do not change when owning more assets does not raise the investment's marginal productivity in the no-trade payoff.

In an experimental investigation of the theoretical predictions following from GHM and deMeza/Lockwood (1998) Sonnemans et al. (2001) (SOS) report on a finding that conflicts with this theoretical prediction. In their so-called TP-game (threat-point) they distinguish three different no-trade payoffs of the investor: low, intermediate and high where higher no-trade payoffs reflect ownership of more assets. Because no-trade payoffs are independent of investments the ownership structure theoretically does not affect investment incentives and accordingly investment levels.

In contrast to that theoretical prediction SOS find that, firstly, "average investment levels are below the socially efficient level [...] [but] always above the level predicted by subgame perfection" (2001, result 2, p. 805), secondly, that "average investment levels increase when the no-trade pay-off increases" (2001, result 1, p. 803) and finally, that "the impact of M1's no-trade pay-off on first and finally agreed offers is smaller than predicted" (2001, result 5, p. 810). Taking into account the bargaining behavior they point to positive reciprocal behavior as the driving force behind their result 2 (2001, p. 816): "Investments above the equilibrium level can be interpreted by the non-investor as fair behavior of the investor. This friendly behavior of the investor warrants a reward in the form of a larger return on the investment than predicted by subgame perfection. Investors anticipate this and therefore invest more than predicted. This

explains why [...] actual investment levels exceed predicted investment levels." Unfortunately, they do not discuss possible reasons for the increase of average investment levels when no-trade payoffs increase.

This paper reports on an experiment that adds an auction stage to the TP-game of SOS in which the ownership rights are auctioned off rather than assigned by the investigator. Thus, the distribution of ownership rights becomes an endogenous variable. The paper aims to test whether SOS's results regarding investment and bargaining behavior still hold with this experimental modification.

Hoffman/Spitzer (1982), Hoffman/Spitzer (1985) and Hoffman et al. (1994) have shown that endogenizing positions in ultimatum games by carrying out a knowledge competition among the participants led to results that are by far closer to subgame perfectness than games without endogenous positions. In a similar way, we hypothesize that making the distribution of ownership rights endogenous leads to a corroboration of theoretical equilibrium predictions based on subgame perfectness. Such a shift, however, should be taken very seriously because endogenous ownership structures are certainly closer to real world business where ownership rights have to be earned or bought.

There are several other experimental studies of the hold up problem. Hackett (1993) and Hackett (1994) study a hold up experiment with bilateral specific investments. Hackett finds a similar result of underinvestment with reference to socially optimal investment and overinvestment with reference to equilibrium predictions. Hackett shows that investing over equilibrium predictions is more pronounced if investments are observable than if they are not. Sloof et al. (2001b) is closely related to SOS. In contrast to SOS, no-trade payoffs are given to the non-investing party. Again, investments are above equilibrium predictions but below the social optimum. Sloof et al. (2000) and Sloof et al. (2001a) analyze the effect of different contract clauses (damage payments) in different hold up settings. Olcina et al. (2000) analyze bilateral specific investments as a coordination game. Koenigstein (2001) carries out a hold up experiment with bilateral specific investments and a take-it-or-leave-it bargaining game. Both papers do not find any underinvestment problem (with reference to the social optimum)! However,

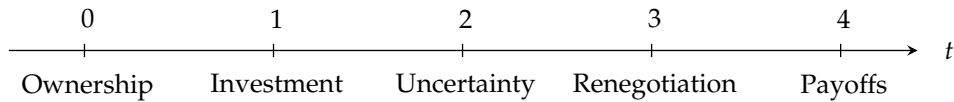
none of the above experimental studies focusses on our main object of analysis, i.e. the impact of endogenizing property rights.

The paper proceeds as follows. Section 2 presents the simplified version of Hart's (1995) model that we use for our experiment. Section 3 describes the experimental design and formulates the hypotheses. In section 4 we present and discuss our results. In Section 5 we draw some conclusions.

2. THEORY

We use a simplified version of Hart's (1995) model: There are two managers (M1 and M2) operating two assets (a_1 and a_2). With the aid of a_2 M2 produces one unit of an intermediate product that M1 processes to a final good using a_1 . The sequence of actions is as follows: In $t = 0$ M1 and M2 choose an ownership structure. In $t = 1$ M1 can make a specific investment which increases M1's revenue of selling the final good. Uncertainty about the precise type of the intermediate product M1 needs is resolved in $t = 2$. In $t = 3$ M1 and M2 bargain over the price of the intermediate product. In $t = 4$ trade occurs and the payoffs are realized (cf. Fig. 1).

FIGURE 1. Timeline



Like Hart (1995) we distinguish three basic ownership structures: Non-integration (type-0) where M1 owns a_1 and M2 owns a_2 , M1-integration (type-1) where M1 owns both a_1 and a_2 , and M2-integration (type-2) where M2 owns both assets. Formally the ownership structure is represented by A_j , $j = 1, 2$, where A_j denotes the set of assets j owns. For example, $A_1 = \{a_1, a_2\}$ and $A_2 = \emptyset$ denote the M1-integration.

M1's investment is denoted by i and represents both the level and the cost of investment. It is completely relation-specific, i.e. it has no value when trading with an outsider. When M1 and M2 agree to trade M1's revenue is $R(i)$ (with $R'(i) > 0$ and $R''(i) < 0$). M1's ex post payoff amounts to $R(i) - p$ where p denotes the

agreed upon price M1 pays for the intermediate product. M1's ex ante payoff is therefore $R(i) - p - i$. When M1 and M2 agree not to trade M1's revenue is $r(A_1)$ with $r(a1, a2) \geq r(a1) \geq r(\emptyset)$. M1's ex post payoff in this case is $r(A_1) - p^s$ where p^s is the spot market price for a general purpose intermediate product.

In case of trading with M1 M2's production costs equal C ; hence his ex post and ex ante payoff is $p - C$. When trade does not occur M2's production costs amount to $c(A_2)$ with $c(a1, a2) \leq c(a2) \leq c(\emptyset)$. His ex ante payoff then equals $p^s - c(A_2)$.

Hence, the ex post surplus equals $R(i) - C$ when trade occurs and $r(A_1) - c(A_2)$ when trade does not occur. The specificity of investment i is formalized by assuming that the ex post surplus in case of trade always exceeds the ex post surplus in case of no trade: $R(i) - C > r(A_1) - c(A_2)$, for all i , A_1 and A_2 .¹ Given these assumptions the first-best level of investment i^* satisfies $R'(i^*) = 1$. In general, this first-best level of investment is not achieved because M1 when choosing his investment level takes into account that she will lose part of the investment returns when bargaining with M2 over p in $t = 3$. Therefore her incentives to invest are not first-best.

Following Hart (1995) M1 and M2 share the "Nash-cake", i.e. the ex post gains from trade $(R(i) - C - (r(A_1) - c(A_2)))$, equally. In $t = 4$ M1 and M2 receive payoffs according to the Nash-bargaining solution. This is each actor's no-trade payoff plus one half of the Nash-cake, $r_i(A_i) + \frac{1}{2}[(R(i) - C) - (r(A_1) - c(A_2))]$. In other words, both parties get their no-trade payoffs plus one half of the remaining surplus. M1's ex ante payoff therefore amounts to

$$\begin{aligned}\pi_1 &= r(A_1) - p^s + \frac{1}{2}[R(i) - C - (r(A_1) - c(A_2))] - i \\ &= -p^s + \frac{1}{2}[R(i) - C + r(A_1) + c(A_2)] - i\end{aligned}$$

Her payoff maximizing investment level i will thus satisfy

$$\frac{1}{2}R'(i) = 1.$$

Given the strict concavity of $R(i)$ it follows that investment levels are below the efficient investment level and are independent of the underlying integration form:

¹The basic GHM-model has a much broader scope: Both M1 and M2 invest, the no-trade payoffs depend on the investment-levels, and specificity also applies in a marginal sense (cf. Hart, 1995).

$i^* > i_1 = i_0 = i_2$ (subscripts denote integration forms).² Accordingly, as ownership does not have any effect on investment incentives the integration form does not matter from a social efficiency point of view.

3. EXPERIMENTAL DESIGN AND HYPOTHESES

Our design covers 2×3 treatments, corresponding to two methods of allocating the no-trade payoffs and three no-trade payoff levels. The so-called *basic treatment* replicates SOS's TP-game in which the no-trade payoffs are assigned by the investigator. In the *auction treatment* the no-trade payoffs are auctioned off. In each single session only one allocation treatment was considered. We ran two sessions per allocation treatment, so that we had four sessions in total. Overall 78 subjects participated in the experiment: two sessions with 20 subjects and two sessions with 18 subjects with 38 subjects per allocation treatment. The subject pool was mainly the student population of Clausthal University of Technology and some highschool students of the Robert-Koch-Gymnasium Clausthal. They earned on average 26.48€ (about 30.45\$) in about three hours.

In the following subsection we describe the basic setup of each experimental session. Subsequently we describe how the bargaining, investment and auction stages were framed and presented to the subjects. Finally, we present the hypotheses that result from our parameter choices.

3.1. Basic Setup of a Session. Each session comprised 18 periods. Each single period consisted of a single play of a two-stage game in the basic treatment or a three-stage game in the auction treatment (cf. Tab. 1).

TABLE 1. Setup of treatments

		Stage 1	Stage 2
basic treatment		investment	bargaining
auction treatment	auctions	investment	bargaining
		Stage 1	Stage 2
		Stage 1	Stage 3

²The strict equalities result from the fact that $r(A_1)$ does not depend on i .

The first stage of the auction treatment consisted of two successive english auctions in which one subject with the role of M1 and one subject with the role of M2 could bid for combinations of no-trade payoffs. The last two stages of both treatments are essentially the same. Moreover, they replicate the setup of SOS's TP-game: In the second-to-last stage the M1-subjects choose an investment level; in the last stage a M1 and a M2-subject bargain over the division of the surplus. In total, all subjects played 18 periods. One half of the subjects was assigned the role of M1, the other half was assigned the role of M2. Each participant kept this role for the whole session. These roles were communicated to the subjects after they had read and understood the complete instructions. In each single period, all M1s were grouped anonymously with a different M2. Using two different rotating schemes we ensured that the same subjects were not matched more than once during the first nine periods and during the last nine periods. The subjects were explicitly informed about this grouping procedure. This procedure guaranteed as few reputation effects as possible.³

The experiment was computerized.⁴ It was programmed and conducted with the software z-Tree (Fischbacher, 1999). First, subjects had to read the instructions, and subsequently they had to answer some questions correctly before the experiment started. For example, the subjects had to calculate the earnings of M1 and M2 in some hypothetical settings. Additionally, all subjects received a summary of the instructions on paper. Both, the instructions and the experiment were phrased as neutrally as possible. At the beginning of period one, all subjects were communicated their respective role for the whole session. After having played the 18 games the subjects completed a questionnaire asking for their gender, their previous education related to economics and their profession. Thereafter experimental points were exchanged for money and subjects were paid individually and discretely.

To make our experiment directly comparable to the experiment of SOS we chose the same functional forms and values: The linear form of the revenue function was

³In the first session a programming mistake had slipped in: In the last nine periods there were eleven out of eighty-one cases where two subjects were grouped that had been grouped before. However, this should not give cause for concern because subjects could not expect that this would happen and did not know that it happened.

⁴The experiments took place in the E_XECUT_E -lab (Experimental Economics Clausthal University of Technology). Subjects are separated by cubicles and do not know with whom they are grouped.

$R(i) = V + 10i$ with $V = 10,000$.⁵ The cost of investment was quadratic in $i \in \{0, 10, 20, \dots, 1,000\}$: $h(i) = \frac{i^2}{100}$. Furthermore, we chose M1's no-trade payoffs to be $r(\emptyset) = 1,800$, $r(a1) = 6,800$, and $r(a1, a2) = 7,800$, respectively. We normalized the price for the intermediate product on the spot market as well as M2's production cost both inside and outside the relationship to zero: $C = c_j = p^s = 0$, $j \in \{0, 1, 2\}$. Given these functions the first-best level of investment is $i^* = 500$. In subgame perfect equilibrium M1 chooses an investment level of $i_j = 250$ irrespective of the underlying ownership structure $j \in \{0, 1, 2\}$. As in SOS both the level of the no-trade payoffs and the base amount V do not affect investment incentives. Therefore, all ownership structures are equivalent from an efficiency point of view.

The exchange rate was 1 € for 7,500 experimental points. At the time the experiments were conducted 1 € was about 1.15 US-\$ so that 1 US-\$ corresponded to about 6,500 points.

In the basic treatment all values of the no-trade payoffs were considered within one session. In both sessions no-trade payoffs were ordered equally over the 18 periods. The subjects were told that each of the three values had a chance of $\frac{1}{3}$ of being chosen in each period, but they did not know the ordering itself. At the beginning of each period, they were informed about the value of r in that period. All pairs were confronted with the same value of r in each single period. The fixed sequence of r s used was equal to that in SOS.

In the auction treatment subjects had to bid for the no-trade payoffs that applied in that period in two successive English auctions. The status quo was $r = 1,800$ with r raising to $r = 6,800$ when both M1 and M2 won exactly one auction or to $r = 7,800$ when M1 won both auctions. Accordingly, the values of r could differ between groups within one period and were not evenly spread over the eighteen periods.

Finally we endowed all subjects with an initial stock of experimental points. In the basic treatment subjects with the role of M1 got an initial endowment of 10,000 points

⁵SOS chose the linear form to be $R(i) = V + v \cdot i$ with $V = 10,000 + r$ and $v = 100$. They added r to the base amount of 10,000 to make their TP-game comparable to their OO-game. Firstly, this was not necessary for us as we did not consider outside options. Secondly, and more important, $V = 10,000 + r$ would have destroyed any incentive for M2 to bid for ownership-rights: As r is defined as the no-trade payoff of M1 any variation in r would have cancelled out with regard to the equilibrium bargaining outcome. Finally, investment incentives are not affected by these differences.

(1.54 US-\$) and subjects with the role of M2 got an initial endowment of 60,000 points (9.23 US-\$). This corresponds exactly to the initial endowments with SOS. In the auction treatment subjects with the role of M1 got an initial endowment of 20,000 points and subjects with the role of M2 got an initial endowment of 70,000 points. We chose different initial endowments for the two treatments mainly for one reason: In the auction treatment M1s could spend up to 10,000 points in the auctions. To avoid a bias in investment behavior because of an immediate debt at the investment stage we endowed M1s with additional 10,000 points. To preserve the relation between M1s' and M2s' endowments we added 10,000 points to M2s' initial endowments as well. All subjects received a show-up fee amounting to 10 € (about 11.5 US-\$).

3.2. Framing of the Bargaining, the Investment and the Auction Stage. In both treatments the bargaining stage corresponds exactly to the bargaining stage of SOS's TP-game: We used an alternating offer structure with exactly ten bargaining rounds. In each round one round-pie was negotiated between M1 and M2. The size of each round-pie was $\frac{1}{10}R(i)$. M1 and M2 alternated in making offers of the division of the ten round-pies with M2 starting in round 1. When the responder accepted an offer all remaining round-pies were automatically divided according to the accepted offer. If, for example, M2 accepted an offer in round 4, all seven round-pies from round 4 to 10 were divided according to that accepted offer. In case an offer was rejected the round-pie of that round got lost and the players received one tenth of their respective no-trade payoffs, i.e. $r(A_1)/10$ and zero. In case agreement was not reached at all – M2 rejected M1's offer in round 10 – the last round-pie also got lost and the bargaining ended with both players receiving only their no-trade payoffs.

The framing of the investment stage in the basic treatment corresponds exactly to that of SOS's TP-game: At the beginning of each period, M1 and M2 were informed about the size of the base round-pie and the value of the no-trade payoff. The size of the base round-pie equalled 1,000 experimental points. Subsequently, M1 decided on how much she wanted to add to the base round-pie. Thus, M1 could choose any investment level between 0, 10, 20, 30, ..., 1,000. Both M1 and M2 could read the investment costs incurred by M1 resulting from her investment decision from a table that had been

handed out to both M1 and M2 together with the summary of instructions before the start of the experiment. The size of the actual round pies was accordingly the sum of the base-round pie plus the amount M1 chose, i.e. $V/10 + i$. The game then continued to the second stage in which the two subjects bargained over the division of the ten actual round pies, as described above. The framing of the investment stage in the auction treatment differed only slightly from that in the basic treatment: M1 and M2 already knew the size of the no-trade payoffs from the auction stage. Accordingly, they were just informed about the size of the base round-pie.

Finally, the auction stage in the auction treatment consisted of two successive English auctions with a time limit after which the current highest offer was chosen. In these auctions the actual no-trade payoffs for that period were determined. When M1 won both auctions her no-trade payoff was 7,800 points and M2's no-trade payoff was 0, when M1 won one auction and M2 the other one M1's no-trade payoff was 6,800 and M2's 0, and finally, when M2 won both auctions M1's no-trade payoff was 1,800 and M2's 0. Thence, M2's interest in buying assets consisted solely in weakening M1's bargaining position. To summarize, three different combinations of no-trade payoffs could be realized (cf. Tab. 2).

TABLE 2. No-Trade Payoffs

Combination	X	Y	Z
M1's no-trade payoff	1,800	6,800	7,800
M2's no-trade payoff	0	0	0
Condition	M2 wins both auctions	M1 and M2 win one auction, respectively	M1 wins both auctions

The game in the auction treatment then continued to the second stage in which M1 decided on how much to add to the base round-pie.

3.3. Hypotheses. We present our hypotheses related to the basic treatment in view of the results of SOS. With regard to the auction treatment we draw parallels to the experiences of Hoffman/Spitzer (1982), Hoffman/Spitzer (1985) and Hoffman et al. (1994) who show that entitlements to a position in experiments may lead to behavior

that is closer to equilibrium. Therefore, we make predictions based on subgame perfect equilibrium. These hypotheses are summarized in Table 3. We formulate hypotheses both in terms of qualitative predictions and in terms of point predictions. In particular, we test the following hypotheses regarding investment and bargaining behavior:⁶

Investment behavior

- In the basic treatment average investment levels are below the socially efficient level but above the subgame perfect level.
- In the basic treatment average investment levels increase with increasing no-trade payoffs.
- In the auction treatment average investment levels are independent of the value of the no-trade payoff.
- In the auction treatment average investment levels equal 250 for all r .

Bargaining behavior

- In the basic treatment M1's share of the Nash-cake, i.e. his 'Nash-share', shrinks with increasing no-trade payoffs.
- In the basic treatment agreement is not immediate.
- In the auction treatment M1's share of the Nash-cake is independent of the value of r .
- In the auction treatment M1's share of the Nash-cake is 50%.
- In the auction treatment agreement is immediate.

4. RESULTS

In this section we work through the results of Sonnemans et al. (2001) and test whether our basic treatment replicates their results of the TP game and whether our auction treatment leads to significant differences to our basic treatment and to their results. We proceed as follows: The first four results present our findings on investment behavior, results 5 to 9 are related to bargaining behavior and results 10 and 11

⁶The bidding behavior will be the topic of another paper. In this paper the auctions are of interest only inasmuch they help making the ownership structure an endogenous part of the parties' decisions.

TABLE 3. Hypotheses

		Basic Treatment	Auction Treatment
Investment stage	Investment	$250 < i < 500$	$i = 250$
		i increases with r	i independent of r
Bargaining stage	Outcome	M1's share decreases with r	$\frac{1}{2}(1,000 + i + r/10)$
	Agreement	not immediate	immediate

Remark: The socially efficient investment level amounts to 500. The bargaining outcome is given as the share of M1, i.e. his round revenues according to the bargaining agreement.

are again related to investment behavior taking into account the results of bargaining behavior.

Result 1. *In the basic treatment mean investment levels increase with r , i.e. control over more assets induces higher investments.*

Result 1 corresponds perfectly to SOS's result 1 (2001, 803). Table 4 shows mean investment levels in the different treatments. Obviously, average investments are increasing in r . Pairs of superscripts (c, d, e, and f) show that differences in investments are significant whenever we compare the investment levels of different r -values with each other. Table 4 also shows that all significant differences perfectly coincide with those of Sonnemans et al. (2001, Table 2).

Result 2. *In the auction treatment average investments do not increase in r . This means the ownership structure does not influence investment behavior significantly.*

Result 2 stands in contrast to Result 1. Obviously, the introduction of endogenizing the ownership allocation by an auction changes the nature of investment behavior in a way such that the ownership structure (r) loses its impact on investments. Table 4 shows that there are, of course, some differences in average investments. However, these differences are not monotonically increasing in r . In contrast, during periods 1 through 9 investments at $r = 6,800$ are above those at $r = 7,800$ which is rather surprising. Furthermore, in the second half, there is just one single significant difference in investments at the 5% level, namely the difference between investments at $r = 1,800$

TABLE 4. Comparison of mean investment levels

<i>Mean Investment Levels</i>				
Periods		Sonnemans et al.	Basic Treatment	Auction Treatment
All	1,800	^{cd} 309 _{ab}	^{cd} 238 _a *	^{c(d)} 251 _b *
	6,800	^{ce} 392 _b	^{ce} 370 _T	^c 300 _{bT}
	7,800	^{de} 435 _b	^{de} 417 _T	^(d) 262 _{bT} *
First half (1-9)	1,800	[324] _b	^{cd} 288*	^{cd} 248 _b *
	6,800	[377] _b	^{ce} 390 _T	^c 304 _{bT}
	7,800	[470] _b	^{de} 461 _T	^d 215 _{bT} *
Second half (10-18)	1,800	^{cd} 294 _a	^{cd} 188 _{aT}	^c 255 _T *
	6,800	^c 407 _{ab}	^c 350 _{a(T)}	^c 294 _{b(T)}
	7,800	^d 400 _b	^d 374 _T	294 _{bT} *
Final three (16-18)	1,800	^{cd} 303 _a	^{cd} 184 _a	280*
	6,800	^c 401 _{(a)b}	^c 332 _(a)	282 _b *
	7,800	^d 415 _b	^d 353	292 _b *

The subgame perfect investment level is 250 in all treatments. Mean investment levels in square brackets (first half of mean investment levels from SOS) have been deduced from the mean investment levels of all periods and the second half. Left pairs of superscripts indicate significant differences (5%) within the same column and time horizon. Right pairs of subscripts indicate significant differences within the same row across treatments. Asterisks indicate that we *cannot* reject 250 being the mean of our corresponding subsamples at a 5%-level of significance using t-tests. Super- or subscripts in brackets are only significant at the level of 10%. Left superscripts have been derived by Wilcoxon sign-rank tests for matched pairs. Right subscripts for the basic treatment and the auction treatment have been derived by Mann-Whitney U-tests. The "a" and "b" subscripts show the results of t-tests whether mean investments in Sonnemans et al. (2001) can be rejected as the mean of our corresponding investment subsample.

and $r = 6,800$. This significance, however, disappears during periods 16 through 18. This is perfectly in line with the predictions of standard subgame perfect Nash Equilibrium. We explain different behavior in earlier and later periods with learning processes which seem to converge towards the equilibrium.

Result 3. *In the basic treatment average investment levels are below the socially optimal level of $i^* = 500$. If $r = 1,800$ then investments are near to or below the equilibrium level and if $r \neq 1,800$ investments are above the equilibrium level.*

Table 4 shows this quite clearly. Analyzed for all periods, at $r = 1,800$ investments are close to the equilibrium value ($i^{NE} = 250$) with the difference not being significant, i.e. we cannot reject 250 being the mean of our corresponding subsamples. In the second half of the periods investments are significantly *below* 250 which clearly contradicts SOS's result. In cases of $r \neq 1,800$, however, investments are significantly above the equilibrium level. This confirms SOS's results. The deviation in case of $r = 1,800$ can, however, partly be explained by a considerable difference in investment levels between SOS's and our experiment. Table 4 shows that virtually all investments in our basic treatment are below those of SOS. Some of them are significant, others are not. We conclude from this that subjects in our experiment were more reluctant to invest than the dutch students. It is, however, not clear to us why this is so much more pronounced at $r = 1,800$. Despite these differences we think that our basic treatment replicates the investment behavior by SOS quite well.

Result 4. *In the auction treatment average investments are*

- *well under the socially optimal level,*
- *closer to subgame perfect equilibrium predictions than in the basic treatment, and*
- *most times only insignificantly different from subgame perfect investments.*

Analyzing the cases of $r = 6,800$ and $r = 7,800$ the overall impression from Table 4 is that investments in the auction treatment are lower than those in the basic treatment. The difference is significant for all periods as well as for periods 10 through 18. In case of $r = 1,800$ the average investment level in the auction treatment is significantly higher than in the basic treatment only in the second half of the experiment. Thus, our first major insight is that investment behavior in the auction treatment differs substantially from investment behavior in the basic treatment. With endogenous ownership structures investments move towards the equilibrium prediction.

It is somewhat even more remarkable how close average investments in the auction treatment are to the subgame perfect equilibrium. In cases of $r = 1,800$ and $r = 7,800$ average investments are *not* significantly different from 250, i.e. we cannot reject 250 being the mean of our corresponding subsamples. This is also true for the last three periods and $r = 6,800$. Taking into account all periods (and the complete second half

of the periods), however, in case of $r = 6,800$ average investments are significantly above the equilibrium prediction. We now turn to the bargaining behavior.

Result 5. *In the basic treatment finally agreed offers, i.e. the amount the investor M1 gets each round in the bargaining game, are in between the equilibrium values of the Outside Option bargaining game (DMO-solution) and the equilibrium of our bargaining game (NB-solution). Finally agreed offers are closer to the equilibrium of our bargaining game.*

SOS compare two treatments of the hold up-experiment which differ with respect to their bargaining game. In one treatment they implement an outside option bargaining game. The equilibrium of this game is characterized by an equal split of the whole ex post surplus if the outside option of the investor is smaller than 50% of the whole surplus. Otherwise the investor only gets his outside option value ("deal me out"). In our bargaining game we implemented SOS's TP-game which has the Nash bargaining solution as a unique equilibrium. In our basic treatment finally agreed offers are on average 37.45 points below the Nash bargaining solution and 194.97 points above the equilibrium of the Outside Option bargaining game. Accordingly, result 5 is completely in line with the results of SOS.

Result 6. *In our auction treatment the average value of finally agreed offers (that the investor (M1) receives) is above both the average DMO-solution and the average NB-solution. Furthermore, finally agreed offers are close to the NB-solution.*

As before, we calculated the difference between the theoretical solutions and finally agreed offers. It shows that finally agreed offers are on average 12.57 points above the NB-solution and 240.46 points above the DMO-solutions. Both deviations from the equilibrium values are significant. The average deviation of 12.57, however, is rather small in absolute terms. This means that bargaining in the auction treatment is different from bargaining in the auction treatment. Our overall conclusion is that with regard to the distribution of surplus the subgame perfect equilibrium is quite a good – though not perfectly precise – predictor of bargaining behavior in our auction treatment.

TABLE 5. Accepted distribution of period payoffs

<i>Mean Investor's Share</i>				
	<i>Basic Treatment</i>		<i>Auction Treatment</i>	
<i>r</i>	Surplus-share	Nash-share	Surplus-share	Nash-share
1,800	^{ab} 60.22	^{ab} 53.26	^{ab} 58.93	51.80*
6,800	^{ac} 68.69 _s	^a 35.97 _n	^{ac} 77.17 _s	50.80 _n *
7,800	^{bc} 73.32 _s	^b 38.42 _n	^{bc} 81.96 _s	49.36 _n *
All	67.36	42.55	74.22	50.66*

Surplus-share and Nash-share in percent. Left pairs of superscripts indicate significant differences (1%) within the same column. Right pairs of subscripts indicate significant differences (1%) within the same row across treatments. Asterisks indicate that we cannot reject 50 being the mean of our corresponding subsamples at a 5%-level of significance using t-tests.

Table 5 gives further information about the agreed distributions of profits. The *surplus-share* describes the share (percentage) that the investor gets from total surplus (gross of investment costs). The "Nash-cake" consists of total surplus minus no-trade payoffs, i.e. the additional amount of money that the negotiators can realize if they reach an agreement. *Nash-share* then describes the percentage of the "Nash-cake" that the investor gets in each round after agreement. Subgame perfection predicts that the Nash-share should always be 50%. Table 5 shows that the surplus-share is always above 50%, i.e. the DMO-solution if the outside option is not binding. It is also evident that the investor's average share is smaller if $r = 1,800$ and larger otherwise.

All average Nash-shares in the basic treatment are significantly different from 50, the equilibrium prediction. In the auction treatment, however, all average Nash-shares are *not* significantly different from 50%, i.e. we cannot reject 50 being the average of our corresponding Nash-share subsamples. Most times within the basic treatment surplus- and Nash-shares vary with the ownership structure (r). Again, this effect completely vanishes within the auction treatment with respect to the Nash-shares.

A comparison of the basic and the auction treatment shows that surplus- and Nash-shares differ significantly at $r = 6,800$ and $r = 7,800$ but not at $r = 1,800$. Asymmetric (theoretical) bargaining power thus is much more effective within the auction treatment. It seems as if fairness or reciprocity considerations play a larger role within

TABLE 6. Regression results explaining the finally agreed offers $M1$ receives in the basic treatment

V	i	r	$period$	n	Adj. R^2
0.4796	0.6478	0.2649	6.3329	329	0.76
(0.017)#	(0.028)#	(0.022)#	(1.058)#		

Standard errors in parentheses. Marks “#” indicate a significance at the 1%-level.

the basic treatment but only a minor role within the auction treatment where higher bargaining power due to higher no-trade payoffs is exploited much better.

Result 7. *In the basic treatment*

- (1) *finally agreed offers give the investor a larger return on investment than theory predicts and*
- (2) *the impact of the ownership structure (r) on finally agreed offers is smaller than theory predicts.*

Subgame perfectness predicts that finally agreed offers (FAO) increase with r and with investments (i). In each case the investors profit should increase by 0.5 times the increase in investment and r , respectively. To test this prediction we estimated the equation $FAO = \beta_0 V + \beta_1 i + \beta_2 r + \beta_3 period + \epsilon$ with $V = 1,000$. The results of this estimation are given in Table 6.

The estimated coefficient of i is larger than 0.5, the coefficient of r is smaller than 0.5. All coefficients are significantly different from zero. Both differences to 0.5 are significant at the 1%-level, too. This means that investment returns are higher than theory predicts and that the effects of asset allocation are weaker. These results are perfectly in line with SOS.⁷ Sonnemans et al. (2001, p. 812) explain this divergence from theory with reciprocity. Reciprocity means that players want to punish other players who have acted in an unfriendly way and they want to reward players who acted cooperatively or generously. Translated to hold up-experiments this means that investments are interpreted as a friendly action that induces a reward by $M2$. Furthermore, they

⁷SOS also analyze the amounts $M1$ would get from $M2$'s first offers. We do not discuss the details of this question. In our experiment, however, results are similar both to SOS's results and to the analysis of finally agreed offers.

TABLE 7. Regression results explaining the finally agreed offers in the auction treatment

V	i	r	$period$	n	Adj. R^2
0.4538	0.6442	0.4646	4.100	329	0.67
(0.023)#	(0.033)#	(0.030)#	(1.293)#		

Standard errors in parentheses. Marks “#” indicate a significance at the 1%-level.

explain that in many bargaining experiments deviations towards an equal split are common. This explains why the effect of r is smaller than theoretically predicted.

Result 8. *In the auction treatment*

- (1) *finally agreed offers give the investor a larger return on investment than predicted and*
- (2) *the impact of M1’s no-trade payoff (r) on finally agreed offers is close to theoretical predictions.*

For a sensible comparison of our treatments, we estimated the same regression equation with respect to finally agreed offers in the auction treatment. Results are given in Table 7.

Again, all estimated coefficients are significant at the 1%-level. As above, the investment coefficient is significantly above 0.5 and of nearly the same magnitude as the corresponding value in the basic treatment. Consequently, investments still give larger returns than theory predicts and reciprocity may be one plausible explanation.

This is, however, different for the r -coefficient. Here the value is clearly higher than in the basic treatment and a t-test shows that the new coefficient is not significantly different from 0.5 any more. Above, we mentioned that in bargaining experiments there is often a deviation from theory towards a more equal distribution of payoffs which could explain the low r -coefficient within the basic treatment. This, however, seems to be rather irrelevant in the auction treatment. There does not seem to be a significant deviation of bargaining behavior from theory. The aspect of equality of payoffs seems to have lost its impact on bargaining behavior after subjects have “earned” their bargaining position by winning an auction.

TABLE 8. Average Lengths of the Bargaining Processes

	SOS		Basic Treatment		Auction Treatment	
	periods	periods	periods	periods	periods	periods
r	1-9	10-18	1-9	10-18	1-9	10-18
1,800	2.68 _a	^m 2.06 _a	2.89	2.78	3.45	ⁿ 3.43
6,800	2.40	^m 2.57	2.96	3.09	3.26	^(p) 3.68
7,800	2.45	2.22	3.27	3.04	3.58	^{n(p)} 2.90

Left pairs of superscripts indicate significant differences (5%) within the same column. Right pairs of subscripts indicate significant differences within the same row across treatments. Super- or subscripts in brackets are only significant at the 10%-level.

Result 9. *In both treatments bargaining is not efficient because agreement is not reached immediately. Furthermore, the average length of bargaining is equal for different ownership structures (r).*

Table 8 gives an overview of the average lengths of bargaining games. It shows that participants in the SOS-experiment on average reached agreements earlier than participants in our basic treatment. They also have shorter bargaining processes than participants in our auction treatment. Ownership structures hardly play any role at all. There is just one case where r makes a difference: In periods 10 - 18 in the auction treatment the average bargaining length in case of $r = 7,800$ is significantly (5% and 10%, respectively) shorter than in case of $r = 1,800$ and $r = 6,800$, respectively. Learning effects do not seem to play an important role as there is no significant difference between the first and the second half of the experiments.

The fact that in SOS and in both of our treatments agreement is reached only after 2-3 rounds is inconsistent with subgame perfection that predicts immediate agreement. This is reinforced by the fact that there are 26 cases in total (13 cases in each treatment) in which agreement is not reached at all. Inefficiencies in alternating offers bargaining experiments are not at all a rare exception.⁸ Consequently, bargaining behavior in our experiment is in line with experimental bargaining literature. Summing up, although subgame perfectness provides a workable prediction of the distribution of final agreements it fails to explain bargaining inefficiencies. Consequently, we submit that a

⁸See e.g. Camerer (2003, pp. 161-67), Sonnemans et al. (2001, p. 814).

workable theory of incomplete contracts must take into account all kinds of bargaining costs to get a deeper understanding of the reasons of real behavior.

Result 10. *In the basic treatment investment behavior differs substantially from 'optimal' investments for each value of r . However, if the effect of r is neglected, average investments are close to 'optimal' investments.*

Like Sonnemans et al. (2001) we estimate the investor's profit function. Given the estimated profit function it is possible to derive 'optimal' investments and compare these values to actual average investments. In contrast to SOS we do not use the investor's fictive profits that he would have earned if he had taken M2's first offer. We think that the use of such fictive values neglects strategic bargaining behavior by the investor and bargaining inefficiencies.⁹ Our approach uses the investor's *realized* profits. The estimated equation is $\pi = \beta_0 + \beta_1 i + \beta_2 i^2 + \epsilon$. The results of our estimation for the basic treatment are given in Table 9.

TABLE 9. Regression results estimating the investor's profit function in the basic treatment

r	Constant	i	i^2	n	Adj. R^2	\hat{i}^*	mean i	deviation (# of std. err.)
1,800	4741.36 (279)	6.609 (1.94)	-0.011 (0.003)	114	0.12	312 (33.14)	238	3
6,800	7485.91 (349)	1.906 (1.76)#	-0.0068 (0.002)	114	0.36	141 (91.21)	370	3
7,800	8310.43 (262)	1.852 (1.24)#	-0.008 (0.001)	114	0.65	121 (61.10)	417	5
All	5762.68 (219)	8.045 (1.20)	-0.012 (0.001)	342	0.19	339 (17.52)	342	1

Standard errors in parentheses. If a standard error is marked with "#" then the coefficient is not significantly different from zero at the 5%-level. All other coefficients are significant at the 5%-level.

⁹Nevertheless, our estimates using realized profits lead to the same qualitative results we get using fictive profits for both treatments (see Appendix A).

Irrespective of the value of r the difference between \hat{i}^* and i is rather large and ranges from within 3 up to within 5 standard errors of estimated \hat{i}^* . This indicates that investment behavior is not optimally adapted to the different ownership structures. This result is inconsistent with the findings of SOS who find only limited deviations from 'optimal' investments, i.e. deviations within 1 ($r = 1,800$), 2 ($r = 7,800$), and 3 ($r = 6,800$) standard errors.

Surprisingly, however, 'optimal' investments are close to average investments if we base our optimization on an estimation that ignores the impact of r . The deviation between our estimated \hat{i}^* and mean i is within 1 standard error of \hat{i}^* . Our interpretation of this finding is that subjects in our experiment *do* have learned to adapt their investment level in general but do *not* have learned to adapt the investments to different ownership structures!

Result 11. *Investment behavior in the auction treatment is close to 'optimal' investment behavior.*

As before, we estimate investor's profits¹⁰ as a function of i and i^2 , calculate M1's optimal investments and analyze the deviation of 'optimal' investment from the mean of real investment behavior. In Table 10 we give the results of regression estimates for all periods and for periods 10-18.

In seven out of eight estimations mean investments are within one standard deviation of the estimated optimal investment \hat{i}^* . The only remaining case, $r = 7,800$ in periods 10-18, lies within two standard deviations. However, the 1-standard-deviation-zone is missed only slightly. Consequently, investment behavior is much closer to optimal investment behavior than in SOS and in our basic treatment. However, we should mention that the regressions for $r = 1,800$ are dissatisfactory and it is only the large standard deviation of estimated optimal investments that keeps the distance within the zone.

¹⁰We used profits gross of auction costs as the dependent variable because at the time of investment auction costs are already sunk and irrelevant for investment decisions. However, we also estimated net profits and it showed that in this case the corresponding 'optimal' investments were even closer to average investments. Accordingly, the results presented here are rather conservative.

TABLE 10. Regression results estimating the investor's profit in the auction treatment

r	C	i	i ²	n	Adj. R ²	\hat{i}^*	mean i	deviation (# of std. err.)
<i>All Periods</i>								
1,800	4497.36 (349)	1.965 (2.15)#	-0.0023 (0.003)#	79	-0.01	430 (253)	251	1
6,800	7542.71 (232)	4.782 (1.36)	-0.0085 (0.0018)	177	0.14	280 (32.89)	300	1
7,800	8295.75 (213)	6.766 (1.29)	-0.0139 (0.0016)	86	0.56	244 (23.19)	262	1
All	6898.98 (226)	5.380 (1.346)	-0.0092 (0.0018)	342	0.08	293 (30.57)	279	1
<i>Periods 10-18</i>								
1,800	4555.63 (504)	3.653 (3.977)#	-0.0073 (0.007)#	36	-0.03	251 (93.31)	255	1
6,800	7946.67 (330)	2.738 (1.888)#	-0.0061 (0.0025)	84	0.10	224 (79.13)	294	1
7,800	8541.32 (279)	4.193 (1.656)	-0.0089 (0.0021)	51	0.38	237 (45.57)	294	2
All	7342.44 (328)	3.043 (1.948)	-0.0059 (0.0026)	171	0.03	258 (73.76)	286	1

Standard errors in parentheses. If a standard error is marked with “#” then the coefficient is not significantly different from zero at the 5%-level. All other coefficients are significant at the 5%-level.

Note that optimal investments are remarkably close to equilibrium investments (250) in the final 9 periods. For these cases we find the largest deviation to be 26 investment points which is very little compared to the basic treatment. Furthermore, equilibrium investments of 250 are within one standard deviation of the estimated optimal investments in all but one cases.

Investment behavior in our auction treatment thus is strikingly different from investment behavior in both our basic treatment and the corresponding treatment in SOS. SOS and the basic treatment report higher investment levels for exogenous property rights. Note that ownership rights give bargaining power to the investor and that the investors invest more if this bargaining advantage is not “earned”. But why are investors more “generous” when they get ownership rights as a gift?

One possible explanation for this is that fairness considerations play a role. If there exists something like inequality aversion (see e.g. Fehr/Schmidt (1999) and Bolton/Ockenfels (2000)) then investors may invest more than subgame perfectness predicts in order to get a more equal distribution of profits. However, if investors have to buy ownership rights by winning an auction they do regard their bargaining power as something they have earned and have spent money on. Consequently, taking into account ownership costs, there is not as much comparative advantage of owning the assets which leads to a more selfish investment behavior of inequality averse subjects.¹¹

5. CONCLUSION

In the preceding section we compared experimental data of our basic treatment with that of SOS's TP-game. Comparing these two experiments in which ownership structures are exogenous we get the following results:

- In both treatments investments increase with r .
- In our basic treatment investments are generally lower than in SOS.
- In both treatments investments are in between the socially optimal level and the equilibrium level for $r = 6,800$ and $r = 7,800$. For $r = 1,800$ this is also true in SOS, but not in our basic treatment.
- In both treatments finally agreed offers are in between the equilibrium values of the outside option-bargaining game and the subgame perfect equilibrium of our bargaining game, i.e. the Nash Bargaining solution. In both treatments finally agreed offers are closer to the Nash Bargaining solution.
- In both treatments finally agreed offers (higher no-trade payoffs) give the investor a larger (smaller) return on investment than theory predicts.
- In both treatments agreement is not reached immediately and the average length of the bargaining process is independent of the ownership structure. However, in our basic treatment on average bargaining took more time than in SOS.

¹¹We think that arguments that solely rest on reciprocity do not work as well here. First, (positive) reciprocity should still be possible even if subjects have to buy assets. Second, among others Charness/Rabin (2002) have shown that positive reciprocity is less widespread than negative reciprocity which means that arguments based on positive reciprocity should be taken with some care.

- Investment behavior in our basic treatment differs substantially from estimated 'optimal' investment. This is true for all values of r and for fictive and actual profits. This is inconsistent with the findings of SOS. However, if 'optimal' investment is estimated neglecting the impact of r then average investments are close to 'optimal' investments.

In our basic treatment most results of SOS can be replicated. We suspect that the remaining differences between SOS and our basic treatment can at least partially be explained by their different definition of the base amount V . In SOS $V = 10,000 + r$, so that the costs of delaying agreement are higher encouraging earlier agreements. This, in turn, increases investment incentives which seem to be particularly low in our basic treatment with r -values of 6,800 and 7,800. Summarizing, we believe that our results, by and large, confirm those of SOS.

The central question of this paper, however, is whether introducing endogenous ownership structures influences subjects' behavior. The main deviations of behavior in the auction treatment from behavior in the basic treatment are:

- Mean investment do not increase in r anymore.
- Average investments are much closer to equilibrium predictions based on subgame perfectness. In most cases we cannot reject the hypothesis that equilibrium investments are the mean of laboratory investments.
- Finally agreed distributions of payoffs are very close to the equilibrium prediction. In neither case can we reject the hypothesis that the equilibrium distribution of the Nash-share (50%) is the mean of our subsamples of Nash-shares.
- The impact of r on finally agreed offers is close to theoretical predictions.
- Investment decisions are close to 'optimal' investments.

The most important parallels between our two treatments are:

- Finally agreed offers give the investor a larger return on investments than predicted by theory.
- In contrast to theory, bargaining is inefficient, i.e. agreement is usually not reached immediately.
- The average length of the bargaining process is, by and large, independent of r .

We think these results are evidence for the hypothesis that it makes a *huge* difference whether the ownership structure in hold up-experiments is assigned exogenously or determined endogenously. This is quite important because asset ownership in real life business is almost always endogenous, too. In general, endogenizing asset ownership moves behavior further towards standard theory. Sonnemans et al. (2001) conclude that their results "support the theory in a relative sense". We believe, our evidence shows that making asset ownership endogenous supports theory even more. In this sense, our data show that Property Rights Theory has indeed a solid empirical basis.

Furthermore, we submit that there is good reason to endogenize endowments and positions in economic experiments much more often than is common by now. Hoffman/Spitzer (1982), Hoffman/Spitzer (1985) and Hoffman et al. (1994) showed that entitlements to positions have significant effects on behavior. If this assumption of endogeny is both the rule in business life and relevant for behavior we should not neglect it in experimental designs.

However, important caveats remain. In particular, the assumption of efficient bargaining is clearly violated in SOS's as well as in our experiment. We have to admit, though, that the type of alternating offers bargaining games we and SOS used overemphasizes the aspect of bargaining costs because every single rejection of an offer leads to immediate substantial losses. Real bargaining situations in business typically involve comparatively smaller costs of delay than in our experiments.

The Nash Bargaining solution seems to describe the final distribution of payoffs rather well. Having this in mind it is even more surprising that regressions on finally agreed offers show that investors get a larger return on their investments than predicted. We do not yet have an explanation for this.

There are numerous open questions for future research. First of all, we believe that there is a need to clarify the role of the bargaining game. In particular, we wonder whether *unstructured* bargaining processes with lower costs of delay may lead to different magnitudes of investment. Second, most experiments we know of use only one-sided investment. There is some evidence that underinvestment decreases in situations of bilateral investments. Third, we should test experimental designs in which

asset ownership alters marginal investment incentives. Finally, all this should be realized with endogenous asset ownership, of course.

APPENDIX A. REGRESSION RESULTS USING SOS'S FICTIVE PROFIT'S SUGGESTION

The following tables show the results estimating the investor's fictive profit funktion in both treatments assuming that the investor accepted the first offer the non-investor made. As can be easily seen the results are qualitatively in accordance with the results estimating the investor's real profits (gross of auction costs).

TABLE 11. Regression results estimating the investor's fictive profit function in the basic treatment

r	Constant	i	i ²	n	Adj. R ²	\hat{i}^*	deviation	
							mean i	(# of std. err.)
1,800	4698.54 (261)	9.335 (1.82)	-0.015 (0.002)	114	0.26	306 (21.77)	238	4
6,800	6585.14 (338)	2.180 (1.704)#	-0.0042 (0.002)	114	0.08	260 (95.24)	370	2
7,800	7115.76 (322)	5.172 (1.520)	-0.010 (0.002)	114	0.49	254 (37.66)	417	5
All	5439.78 (196)	8.309 (1.07)	-0.012 (0.001)	342	0.21	359 (15.28)	342	2

Standard errors in parentheses. If a standard error is marked with "#" then the coefficient is not significantly different from zero at the 5%-level. All other coefficients are significant at the 5%-level. The correlation between the non-investor's first offer and the agreed on offer is 85.8%.

TABLE 12. Regression results estimating the investor's fictive profit function (gross of auction costs) in the auction treatment

r	Constant	i	i ²	n	Adj. R ²	\hat{i}^*	deviation	
							mean i	(# of std. err.)
1,800	4197.40 (345)	1.000 (2.13)#	-0.0002 (0.003)#	79	-0.01	3106 (51964.82)	251	1
6,800	6644.44 (284)	6.794 (1.66)	-0.0117 (0.0022)	177	0.17	290 (28.49)	300	1
7,800	7360.45 (352)	4.575 (2.13)	-0.0073 (0.0027)	86	0.06	314 (61.16)	262	1
All	6201.99 (236)	5.041 (1.406)	-0.0078 (0.0019)	342	0.05	323 (35.72)	279	2

Standard errors in parentheses. If a standard error is marked with "#" then the coefficient is not significantly different from zero at the 5%-level. All other coefficients are significant at the 5%-level. The correlation between the non-investor's first offer and the agreed on offer is 77.4%.

APPENDIX B. SUMMARY OF INSTRUCTIONS – BASIC TREATMENT

At the beginning of the experiment you are informed about your role you keep during the whole experiment. You are assigned either the **role of a participant A** or the **role of a participant B**. You will keep this role for the whole experiment. Furthermore you receive at the beginning of the experiment your initial endowment of experimental points. Participants with the role of A receive 10,000 points, participants with the role of B receive 60,000 points.

The experiment comprises **18 periods**. Each single period consists of **two stages**. Whereas **Stage I** is not divided **Stage II** is divided in **10 rounds**. At the beginning of each period the participants are grouped. Each single group consists of one participant with the role of A and one participant with the role of B. The grouping was determined before the start of the experiment. It was done such that it is impossible that you are grouped with the same participant in two successive periods. Moreover, you will be grouped with the same participant at most twice during the whole experiment. When you will be grouped with the same participant is not predictable. During one period you will stay together with that same participant. You are not informed about who this other participant is.

Each single period of this experiment is structured in the following way. At the beginning of each period both participants of a group are informed about the individual **base amounts** that apply in that period. Only one of the following combinations X, Y, and Z are possible:

Combination	X	Y	Z
Base amount of participant A	1,800	6,800	7,800
Base amount of participant B	0	0	0

Which combination is in effect in a particular period is determined by the main computer drawing a random number at the beginning of each period. The random number is drawn such that each of the three combinations has equal probability of $\frac{1}{3}$ of being chosen. On average combination X will be in effect in 6 out of 18 periods, combination Y in 6 out of 18 periods, and combination Z in 6 out of 18 periods. However, this applies only on average. The probability that a special combination will be in effect in the following period is independent of the respective combinations in previous periods and will again be $\frac{1}{3}$.

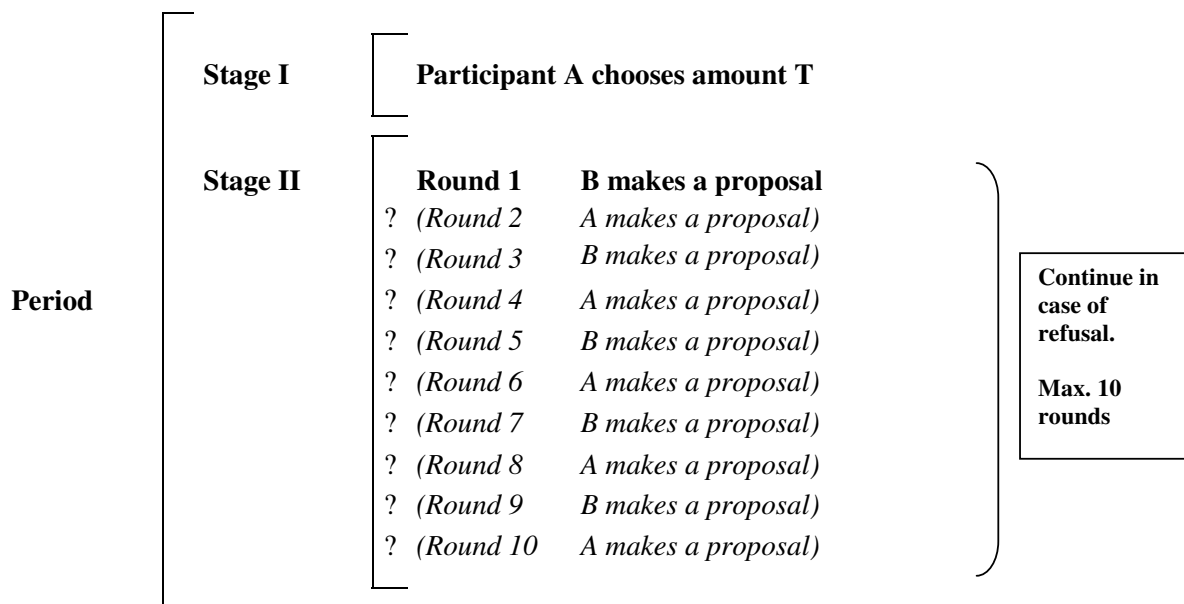
When both participants are informed about the combination of the current period the period actually starts. In the first stage of the period only participant A makes a decision. In the second stage of the period both participants of a group make decisions. In detail, the stages of each single period have the following structure:

Stage I: Participant A chooses an **amount T**. T has to be a multiple of 10 and has to lie in between 0 and 1,000. The sum of amount T and 1,000 points make the **round pie**. When A has chosen B is informed about the amount T. By choosing T only participant A has to bear costs amounting to $(T/10)^2$. In the table that is handed out to you you find for every possible amount T the exact costs participant A has to bear. Please note that the chosen amount T is in effect in every single round of stage III. When participant A chooses amount T every single round-pie of stage III increases by T. The costs for amount T, however, incur only once in stage I.

Stage II: In stage II A and B have 10 bargaining rounds to agree on a division of the round-pies. Participant B always makes the **first proposal**, i.e. in round 1, how to divide the round-pies. Participant A can **accept** that offer or **refuse** it. When A accepts the offer the bargaining and the period end. In case of refusal the right to make an offer changes sides for the following round.

Thus, in round 2 participant A makes a proposal on the division of the round-pies. Again, participant B can accept or refuse that offer. In case of acceptance the bargaining and the period end. In case B refuses the right to make an offer again changes sides for the next round. This scheme is repeated until an offer is accepted or the end of round 10 has come. When a proposal on division of the round-pies is accepted in some round the current and all remaining round-pies are divided according to the accepted offer. When a proposal is refused both participants only receive their individual base amounts.

STRUCTURE OF THE EXPERIMENT



APPENDIX C. SUMMARY OF INSTRUCTIONS – AUCTION TREATMENT

At the beginning of the experiment you are informed about your role you keep during the whole experiment. You are assigned either the **role of a participant A** or the **role of a participant B**. You will keep this role for the whole experiment. Furthermore you receive at the beginning of the experiment your initial endowment of experimental points. Participants with the role of A receive 20,000 points, participants with the role of B receive 70,000 points.

The experiment comprises **18 periods**. Each single period consists of **three stages**. **Stage I** is divided in **two auctions**. **Stage II** is not divided, and **stage III** is divided in **10 rounds**. At the beginning of each period the participants are grouped. Each single group consists of one participant with the role of A and one participant with the role of B. The grouping was determined before the start of the experiment. It was done such that it is impossible that you are grouped with the same participant in two successive periods. Moreover, you will be grouped with the same participant at most twice during the whole experiment. When you will be grouped with the same participant is not predictable. During one period you will stay together with that same participant. You are not informed about who this other participant is.

Each single period of this experiment is structured in the following way:

Stage I: In **stage I** both you and the participant with whom you are grouped participate in two successive auctions. The outcomes of these auctions determine the individual base amounts that are used in stage III of the period. Only one of the following combinations X, Y, and Z are possible:

Combination	X	Y	Z
Base amount participant A	1,800	6,800	7,800
Base amount participant B	0	0	0
Condition	B wins both auctions	A and B win one auction, respectively	A wins both auctions

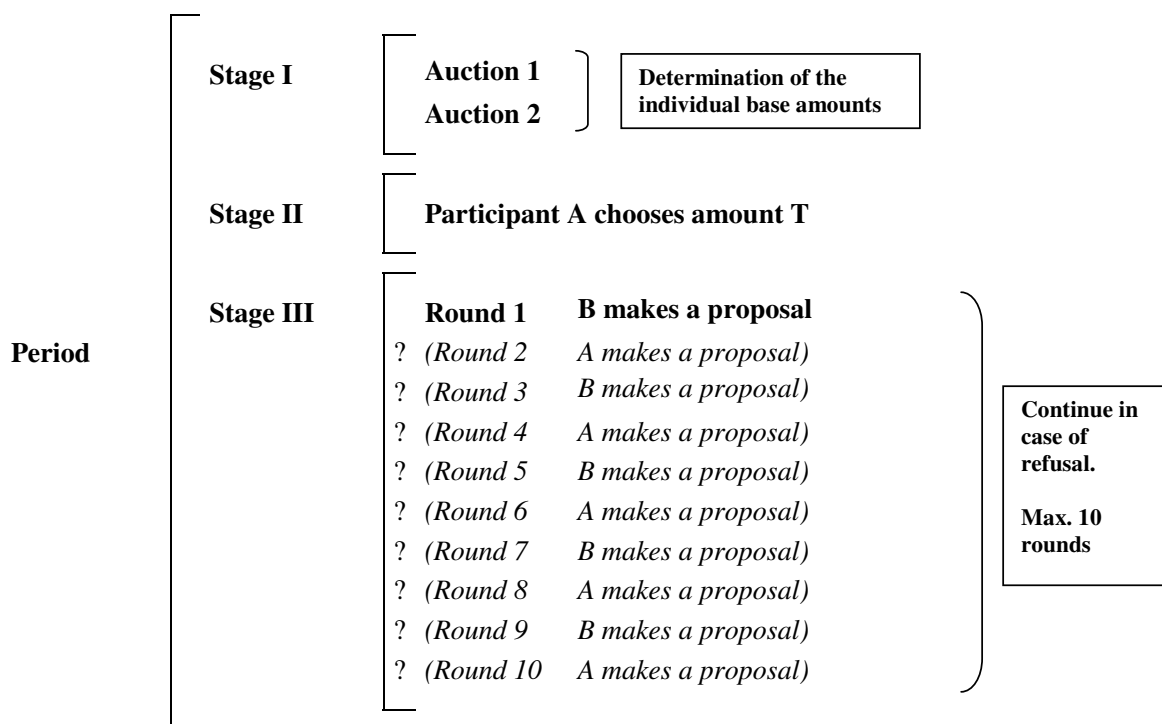
Combination X of the individual base amounts is in effect when participant B of a group wins both auctions. Combination Y is in effect when participant A of a group wins one auction and loses the other one. Combination Z is in effect when participant A of a group wins both auctions.

Stage II: Participant A chooses an **amount T**. T has to be a multiple of 10 and has to lie in between 0 and 1,000. The sum of amount T and 1,000 points make the **round pie**. When A has chosen B is informed about the amount T. By choosing T only participant A has to bear costs amounting to $(T/10)^2$. In the table that is handed out to you you find for every possible amount T the exact costs participant A has to bear. Please note that the chosen amount T is in effect in every single round of stage III. When participant A chooses amount T every single round-pie of stage III increases by T. The costs for amount T, however, incur only once in stage I.

Stage III: In stage III A and B have 10 bargaining rounds to agree on a division of the round-pies. Participant B always makes the **first proposal**, i.e. in round 1, how to divide the round-pies. Participant A can **accept** that offer or **refuse** it. When A accepts the offer the bargaining and the period end. In case of refusal the right to make an offer changes sides for the following round. Thus, in round 2 participant A makes a proposal on the division of the round-pies. Again,

participant B can accept or refuse that offer. In case of acceptance the bargaining and the period end. In case B refuses the right to make an offer again changes sides for the next round. This scheme is repeated until an offer is accepted or the end of round 10 has come. When a proposal on division of the round-pies is accepted in some round the current and all remaining round-pies are divided according to the accepted offer. When a proposal is refused both participants only receive their individual base amounts that were determined in the auctions of stage I.

STRUCTURE OF THE EXPERIMENT



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