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by

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Equitable Allocations in Experimental Bargaining Games: Inequality Aversion versus Efficiency*

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Abstract In this paper, we report on a series of free-form bargaining experiments in which two players have to distribute four indivisible goods among themselves. In one treatment the monetary payoffs associated with each bundle of goods are common knowledge; in a second treatment only the ordinal ranking of the bundles is given. We find that in both cases, the following qualitative rule yields a good explanation of individual behavior: First determine the most equal distribution, then find a Pareto improvement *provided* that this does not create "too much" inequality. In the ordinal treatment, individuals apparently use the ranks in the respective preference orderings over bundles as a substitute for the unknown monetary value. Interestingly, we find much less Pareto-damaging behavior due to inequality aversion in the ordinal treatment.

1. Introduction

Recent research in explaining observed behavior of individuals in laboratory experiments has focused on the question of how to model the agents' *distributional preferences*, see, e.g., Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Charness and Rabin (2001) (henceforth F&S, B&O and C&R, respectively). The common assumption in these models is that agents are not only motivated by their own material payoff but by the entire ditribution of monetary rewards. Specifically, F&S and B&O suggest parametric forms of the utility function incorporating different notions of *inequality aversion* according to which utility decreases with the differences in individual payoffs. By contrast, C&R propose a model of *social-welfare* preferences according to which agents are concerned with maximizing a combination of the aggregate payoff for the group and the payoff of the worst-off individual. By assigning significance to differences and sums of monetary rewards, both approaches use individual utility information in a *cardinal* and *interpersonally comparable* way. While this can be justified, e.g. by assuming quasi-linearity of the underlying preferences, it also shows that the applicability of the existing models is restricted to situations in which individual monetary rewards are known to all agents and in which preferences over allocations can be adequately described in terms of the distribution of monetary rewards.

The purpose of the present paper is to demonstrate that the basic intuitions behind the distributional preference approach can be fruitfully applied in more general situations. To this end, we conducted a series of free-form bargaining experiments in which two players had to jointly determine an allocation of four indivisible goods. In one treatment both agents were informed about the specific monetary value associated with the bundles of goods for each player (the same bundle usually had different monetary value for the two players). In the other treatment, each player was only informed about her own and the opponent's ordinal ranking of the bundles, i.e. only the ordinal ranking of the later payments associated with each bundle was given. Despite the lack of numerical payoff information in the latter treatment, we find that individuals rely on interpersonal comparisons also in this case. Indeed, we find strong evidence that agents use the rank of a bundle in the respective preference ordering as a substitute for its unknown monetary value. Taking these ranks as the basis for interpersonal comparisons, the motives behind the formation of distributional preferences, such as inequality aversion or social concerns in general, are relevant also in the treatment with ordinal information. In fact, the comparison between the two treatments reveals that individual behavior can be accounted for by a simple unifying *qualitative* theory of distributional preferences. Specifically, the outcomes that we observed in our bargaining experiments suggest that a significant proportion of agents behave according to the following rule:

> Conditional Pareto Improvement from Equal Split (CPIES):
> First, determine the most equal distribution of rewards. If this allocation is Pareto optimal, then choose it. Otherwise, if there is the possibility to make everyone better off, implement such a Pareto improvement *provided* that this does not create "too much" inequality.

If the monetary rewards are known, the "most equal" distributions are of course the ones with minimal difference of the numerical payoffs for the two agents.[1] If, on the other hand, only the ordinal rankings of the bundles of goods are given, then the "most equal" distributions are those with minimal difference of the ranks in the respective preference orderings. Similarly, "too much inequality" is to be understood in terms of differences in monetary payoffs and ranks, respectively. Of course, how much precisely "too much" is, depends on individual preferences and varies from subject to subject.

The above rule combines elements of the inequality aversion approach of F&S and B&O on the one hand, and the social preference approach of C&R on the other. With the former it shares the important role played by interpersonal equality, with the latter the demand for Pareto optimality (in the payoff space).[2] Interpersonal inequality plays a twofold role here. First, the absence of inequality determines an initial *reference* point for the bargaining problem. Secondly, it serves as a *constraint* in the process of achieving a

[1] In our setting with two players, there are at most two such distributions in the feasible set. With more than two players, the meaning of "most equal" distribution can be made precise using the theory of inequality measurement; for instance, by applying the (partial) criterion of Lorenz dominance.
[2] Pareto optimality is defined in *payoff space* as opposed to utility space, since the relevant notion of optimality here is based on the distribution of material payoffs (respectively, ranks), not on the subjective distributional preferences.

Pareto optimal outcome. In contrast to the prediction of C&R's model, we systematically find Pareto-damaging behavior in the treatment with known monetary rewards.[3] Interestingly, however, such behavior is only very rarely observed in the ordinal treatment. Our conjecture is that this is due to the uncertainty about the differences in final payments associated with differences in ordinal ranks. Indeed, it seems that rank inequality becomes acceptable because it does not *necessarily* correspond to unequal monetary payoffs. One conclusion from our study is thus that, by making inequality precisely quantifiable, monetary payoff information hinders the realization of Pareto improvements.

While the CPIES rule contains inequality aversion as a prominent element, it goes beyond purely static models of distributional preferences by introducing a *procedural* aspect: In a first step the bargaining partners have to determine a "disagreement point" which then serves as the reference distribution for the later bargaining process. Although this first step is sometimes left implicit, we do find explicit comparisons with the perfectly equal (or almost equal) distribution in the communication protocols of the experiments.[4]

Our experimental design differs from the literature in several respects. First, while most of the studies on distributional preferences have focused on variants of either dictator or ultimatum games, we consider free-form bargaining here. Secondly, we frame our decision problem as one of distributing indivisible goods. By consequence, the feasible payoff distributions are explicitly derived from an underlying economic allocation. More importantly, our design allows us to only induce ordinal preference information and to compare the corresponding results with those obtained under full (cardinal) payoff information.

The remainder of the paper is organized as follows. Section 2 describes the experimental design, and Section 3 the division problems that we tested. Section 4 summarizes the results. In particular, we test the CPIES rule against other rules and criteria proposed in the literature. Section 5 concludes.

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[3] We are always referring to the basic version of C&R's model that does not incorporate negative reciprocity.

[4] For a detailed analysis of the protocols and an elaboration of the procedural aspects of our bargaining experiments, see Herreiner and Puppe (2004b).

2. Experimental Design

Each of the four free-form bargaining experiments (EXP I – IV) consisted of six sessions with different subjects. Eight subjects participated in each session, so that the total number of subjects was 192 (=8*6*4). Each session consisted of five independent rounds (R1 – R5). The results of earlier rounds had no impact on the payoff structure of later rounds.[5] Subjects were re-matched in pairs after each round; no subjects met twice.[6] The experiments took place in June and July 2001 (EXP I, II and III) and November 2002 (EXP IV) at the University of Bonn. Almost all subjects were students at the University of Bonn, most of them in economics or law.

In each round of each experiment, the subject pairs had to bargain over the distribution of four indivisible goods, denoted by A,B,C and D, among themselves. Bargaining partners had to reach an agreement within[7] 10 minutes, otherwise neither received anything. Partners could communicate via computer by sending proposals at any time. A proposal consisted of a specification of a distribution of the four goods between the two players. Each good could be given to only one player, and all four goods had to be distributed (no free disposal). In addition to sending proposals, bargaining partners had the possibility to support their proposals by verbal messages that they sent via a "chat" device on the screen (see the instructions in Appendix 3 for screen examples). When two partners had made consistent proposals (i.e. if both proposed the same allocation of goods), they were asked for confirmation. If both confirmed, the goods were allocated accordingly and the round was over for them. If two partners could not reach an agreement within 10 minutes neither player received a good and the round was over.

Each bundle of goods corresponded to a value in experimental currency ("Taler"). The Taler values of the bundles ranged from 0 (for the empty bundle) to 100 (for all four goods combined). The conversion rate was 24 Taler for € 1.[8] Importantly, the same

[5] While the rounds were strictly independent in terms of design, we can of course not exclude that the *behavior* or experience in earlier rounds had an impact on how subjects behaved in later rounds. We did not find any apparent influence on the allocations chosen in later rounds. Therefore, we treated the results in each round as an independent observation (see also C&R, p.827 for a similar approach).
[6] For the way in which subjects were re-matched, see Appendix 1.
[7] Once all pairs had agreed on an allocation, the round was over for everybody and the next round started immediately.
[8] In the experiments before January 1, 2002, the conversion rate was 12 Taler for 1 DM.

bundle typically had a different value for different players. Moreover, the value functions were not additive, i.e. the value of a combination of goods was in general different from the sum of the values of its components. Thus, the valuations allowed for complementarities between goods. For instance, in Table 1 below, good B is worth 7 Taler and good C is worth 3 Taler for player 2; however, the bundle BC that combines them is worth 75 Taler. Also, the bundle BC is more valuable than the bundle AC although good A taken in isolation is more valuable (8 Taler) than good B taken in isolation.

There were two different treatments. In treatment *CARD* each subject was informed about the precise Taler value of each bundle for either player (as in Table 1 below). In treatment *ORD* the subjects were only informed about the ordinal ranking of the bundles of goods for either player and about the fact that the empty bundle earned 0 whereas all four goods combined yielded 100 Taler (see Table 2 below). Note that complementarities between goods can also be present in the ordinal treatment. For instance, B and C are the two most valuable single goods for player 2 in Table 2;

Player 1			Player 2	
Monetary Payoffs	Bundles		Bundles	Monetary Payoffs
100	ABCD		ABCD	100
98	ABC		ABC	97
95	ABD		ABD	96
93	CBD		ACD	91
83	ACD		CBD	88
66	AB		BC	75
57	CD		AC	45
53	BC		BD	42
46	AD		CD	40
45	BD		AB	28
20	AC		AD	19
9	B		A	8
5	A		B	7
3	C		C	3
1	D		D	2
0	-		-	0

Table 1: Example of treatment CARD (EXP I, R3 = EXP II, R3)

Player 1		Player 2
Bundles		Bundles
ABCD		ABCD
BCD		ABC
ABD		BCD
ABC		ABD
ACD		ACD
AC		AB
AD		AC
AB		CD
BC		BC
CD		BD
BD		AD
D		B
C		C
A		A
B		D
-		-

Table 2: Treatment ORD (EXP III, R3)

nevertheless, the bundle BC is not the most valuable combination of two goods. Experiments EXP I and II involved the *CARD* and *ORD* treatments, and EXP III and IV only the *ORD* treatment.

3. The Bargaining Problems

We now describe the bargaining problems that we tested. Each experiment is summarized in a separate table that shows the data of the examples used in each of the five rounds in

R1				R2				R3				R4				R5			
n.a.	P1	P2	n.a.	n.a.	P1	P2	n.a.		P1	P2			P1	P2			P1	P2	
100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100
95	ABC	BCD	98	98	ABC	BCD	94	98	ABC	ABC	97	95	ABC	ABD	98	98	ABC	BCD	94
92	ACD	ABD	95	96	ACD	ABD	90	95	ABD	ABD	96	92	BCD	ACD	95	96	ACD	ABD	90
89	BCD	ABC	87	92	BCD	ACD	86	93	CBD	ACD	91	89	ABD	ABC	87	92	BCD	ABC	86
82	ABD	ACD	84	88	ABD	ABC	81	83	ACD	CBD	88	82	ACD	BCD	84	88	ABD	ACD	81
60	BC	BD	64	60	BD	CD	64	66	AB	BC	75	60	AB	AD	64	60	BC	BD	64
55	AB	BC	47	45	AC	BC	53	57	CD	AC	45	55	AC	AB	47	45	AB	BC	53
50	CD	AC	43	40	CD	AD	50	53	BC	BD	42	50	BD	BC	43	40	CD	AC	50
46	AD	CD	38	36	AB	AC	44	46	AD	CD	40	46	CD	BD	38	36	AD	CD	44
35	BD	AB	30	30	AD	BD	32	45	BD	AB	28	35	AD	AC	30	30	BD	AB	32
28	AC	AD	27	28	BC	AB	26	20	AC	AD	19	28	BC	CD	27	28	AC	AD	26
15	C	B	17	9	C	D	19	9	B	A	8	15	B	A	17	9	C	B	19
12	A	D	11	8	A	B	15	5	A	B	7	12	C	D	11	8	A	D	15
7	B	C	5	5	B	C	10	3	C	C	3	7	A	B	5	5	B	C	10
5	D	A	4	2	D	A	7	1	D	D	2	5	D	C	4	2	D	A	7
0	-	-	0	0	-		0	0	-	-	0	0	-	-	0	0	-	-	0

P1	P2	#		Δ	P1	P2	#		Δ	P1	P2	#		Δ	P1	P2	#		Δ	P1	P2	#		Δ
AB	**CD**	10	PO	2	**AB**	**CD**	10	PO	3	**BD**	**AC**	14		3,0	**CD**	**AB**	21	PO	2,1	**AB**	**CD**	21	PO	2,1
AD	**BC**	6	PO	2	**BD**	**AC**	8	PO	3	**AD**	**BC**	7	PO	3,29	**AC**	**BD**	1	PO	2,17	**CD**	**AB**	1		2,8
BD	**AC**	2		2	**AD**	**BC**	2		3	**AB**	**CD**	2	PO	3,26						**C**	**ABD**	1	PO	9,81
AC	**BD**	2	PO	5	**BC**	**AD**	1		3															
CD	**AB**	1		2	**AC**	**BD**	1		3															
ACD	**B**	1	PO	9																				
ABCD	**-**	1	PO	15																				
		23					22					23					22					23		

Table 3: EXP I

the upper part (rows 2 – 18) and summarizes the results in the lower part (rows 20 – 27).

Table 3 above refers to EXP I. In row 2, “n.a.” stands for “not available” and refers to the fact that in the *ORD* treatment the later Taler payments associated with the bundles of goods (printed in light gray in the tables) were not known by the two players at the time of bargaining. The columns P1 and P2 indicate the ordinal rankings of the bundles for player 1 and 2, respectively. The lower part of the table (rows 20 – 27) shows the chosen distributions of goods (column P1 indicates player 1’s bundle and column P2 player 2’s bundle). The frequency of the respective choices is shown in column 3 (#). Thus, for instance, the allocation (AB,CD) (i.e. the bundle AB for player 1, and the bundle CD for player 2) was chosen by 10 bargaining partners in R1 of EXP I. The fourth column indicates whether the respective distribution is Pareto optimal. Finally, the fifth column (Δ) gives the difference in the ranks of the respective bundles in the preference orderings of the two players, and, if applicable, also the difference in Taler payoffs. Thus, for instance in R3 of EXP I, the Pareto optimal distribution (AD,BC) was chosen seven times, it involves a difference of 3 ranks, and a payoff difference of 29 Taler.

EXP I involved both the *ORD* and the *CARD* treatments, as the first two rounds were without numerical payoff information. However, these two rounds were designed for a different purpose and are evaluated in Herreiner and Puppe (2004a); the same applies to the example in R5 in EXP III (see Table 5 below).[9] The distinctive feature of the division problem in R3 of EXP I is the tension between equality and efficiency. Indeed, the distribution (BD,AC) of goods results in the equal distribution (45,45) of monetary payoffs; however, this distribution is strictly dominated by the distribution (AD,BC) that yields payoffs (46,75). By contrast, the most equal distributions in the last two rounds of EXP I, i.e. the distribution (CD,AB) in R4 and the distribution (AB,CD) in R5, are both Pareto optimal.

[9] Specifically, these examples were designed to test the criterion of envy freeness. For instance, in R1 of EXP I the distribution (AB,CD) is *envy free* since each player prefers her own bundle to that of her partner. This property does not hold, say, for the distribution (AD,BC), where player 1 would rather have her partner’s bundle. In R1 and R2 of EXP I and R5 of EXP III there is no unambiguous distribution which may serve as reference point for the CPIES procedure; by consequence, the observed choices (see Tables 3 and 5) support the CPIES prediction in a trivial way. The results of all three examples do confirm the other findings reported here, in particular the result that Pareto optimality is often achieved in the ordinal treatment.

Table 4 below shows the examples and results of EXP II which involved only the *CARD* treatment. The example tested in R3 is identical to that in R3 of EXP I. In all other rounds, there is an unambiguous most equal distribution, Pareto optimal in each case.

R1				R2				R3				R4				R5			
	P1	P2			P1	P2			P1	P2			P1	P2			P1	P2	
100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100
95	ABC	BCD	98	98	ABC	BCD	94	98	ABC	ABC	97	97	ABC	BCD	95	96	ABC	BCD	97
92	ACD	ABD	95	96	ACD	ABD	90	95	ABD	ABD	96	95	ACD	ABD	91	91	ACD	ABD	93
89	BCD	ABC	87	92	BCD	ACD	86	93	CBD	ACD	91	93	BCD	ABC	86	90	BCD	ABC	88
82	ABD	ACD	84	88	ABD	ABC	81	83	ACD	CBD	88	87	ABD	ACD	82	83	ABD	ACD	86
60	BC	BD	64	60	BC	BD	64	66	AB	BC	75	60	BC	BD	64	60	BC	BD	64
55	AB	BC	47	45	AB	BC	53	57	CD	AC	45	47	AB	BC	52	56	AB	BC	46
50	CD	AC	43	40	CD	AC	50	53	BC	BD	42	42	CD	AC	51	52	CD	AC	41
46	AD	CD	38	36	AD	CD	44	46	AD	CD	40	35	AD	CD	46	45	AD	CD	39
35	BD	AB	30	30	BD	AB	32	45	BD	AB	28	33	BD	AB	32	39	BD	AB	35
28	AC	AD	27	28	AC	AD	26	20	AC	AD	19	29	AC	AD	28	31	AC	AD	30
15	C	B	17	9	C	D	19	9	B	A	8	9	C	B	18	14	C	B	16
12	A	D	11	8	A	B	15	5	A	B	7	7	A	D	17	13	A	D	14
7	B	C	5	5	B	C	10	3	C	C	3	6	B	C	11	8	B	C	7
5	D	A	4	2	D	A	7	1	D	D	2	3	D	A	6	2	D	A	4
0	-	-	0	0	-		0	0	-	-	0	0	-	-	0	0	-	-	0

R1 P1	R1 P2	#		Δ	R2 P1	R2 P2	#		Δ	R3 P1	R3 P2	#		Δ	R4 P1	R4 P2	#		Δ	R5 P1	R5 P2	#		Δ
AD	**BC**	19	PO	2,1	**AB**	**CD**	22	PO	2,1	**BD**	**AC**	11		3,0	**AB**	**CD**	23	PO	2,1	**AD**	**BC**	22	PO	2,1
AB	**CD**	2	PO	2,17	**BC**	**AD**	1	PO	5,34	**AD**	**BC**	9	PO	3,29	**CD**	**AB**	1		2,10	**BC**	**AD**	1	PO	5,30
ACD	**B**	1	PO	9,75																**AB**	**CD**	1	PO	2,17
BD	**AC**	1		2,8																				
CD	**AB**	1		2,20																				
		24					23					20					24					24		

Table 4: EXP II

EXP III and IV only involved the *ORD* treatment. Table 5 summarizes EXP III. R1 in EXP III was designed to test the impact of the number of goods on the choice of allocations. Note that the distributions (AB,CD) and (ABC,D) are both Pareto improvements from the reference distribution (AD,BC), and both involve a rank difference of 3. Nevertheless, (AB,CD) is chosen much more frequently (14 times versus 3 times). We conjecture that this is due to the more equal *number* of goods distributed. R2 and R3 illustrate the CPIES procedure: in R2 the distribution (AD,BC) is the unique

Pareto improvement relative to the reference distribution (AB,CD). In R3, the reference distribution (AB,CD) itself is Pareto optimal. In R4, there are several Pareto improvements relative to the reference distribution (BD,AC). Among these, (BC,AD) is

R1				R2				R3				R4				R5			
n.a.	P1	P2	n.a.	n.a.	P1	P2	n.a.	n.a.	P1	P2	n.a.	n.a.	P1	P2	n.a.	n.a.	P1	P2	n.a.
100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100	100	ABCD	ABCD	100
95	ACD	BCD	90	98	ABC	ACD	94	98	BCD	ABC	97	85	BCD	ACD	85	98	ABC	BCD	94
80	BCD	ACD	80	96	ABD	ABD	90	95	ABD	BCD	96	80	ACD	BCD	80	96	ACD	ABD	90
70	CD	ABD	70	92	BCD	BCD	86	93	ABC	ABD	91	60	CD	ABD	73	92	BCD	ACD	86
65	ABC	CD	48	88	ACD	ABC	81	83	ACD	ACD	88	40	ABC	CD	67	88	ABD	ABC	81
63	ABD	BD	45	60	AC	BC	58	66	AC	AB	75	38	ABD	AD	62	60	BD	CD	64
50	BD	AD	40	45	BC	AC	53	57	AD	AC	45	32	AD	BD	57	45	AC	BC	53
32	AB	D	35	38	AD	AB	50	53	AB	CD	42	24	BC	D	50	40	CD	AD	50
30	AC	ABC	32	36	AB	CD	44	46	BC	BC	40	23	AB	ABC	49	36	AB	AC	44
25	AD	BC	30	30	BD	AD	32	45	CD	BD	28	20	BD	AC	44	30	AD	BD	32
20	BC	AB	28	28	CD	BD	26	20	BD	AD	19	19	AC	AB	37	28	BC	AB	26
15	B	AC	20	9	B	D	19	9	D	B	8	9	A	BC	30	9	C	D	19
10	D	C	15	8	A	A	15	5	C	C	7	7	D	C	9	8	A	B	15
5	C	A	10	5	C	C	10	3	A	A	3	5	C	B	8	5	B	C	10
2	A	B	5	2	D	B	7	1	B	D	2	2	B	A	1	2	D	A	7
0	-	-	0	0	-		0	0	-	-	0	0	-	-	0	0	-	-	0

P1	P2	#		Δ	P1	P2	#		Δ	P1	P2	#		Δ	P1	P2	#		Δ	P1	P2	#		Δ
AB	**CD**	14	PO	3	**AD**	**BC**	17	PO	2	**AB**	**CD**	19	PO	0	**BC**	**AD**	13	PO	2	**BD**	**AC**	11	PO	3
ABC	**D**	3	PO	3	**AC**	**BD**	4	PO	5	**AD**	**BC**	1	PO	2	**ABC**	**D**	7	PO	3	**AB**	**CD**	7	PO	3
AC	**BD**	3		3	**B**	**ACD**	1	PO	10	**CD**	**AB**	3	PO	4	**AB**	**CD**	1	PO	4	**BC**	**AD**	1		3
CD	**AB**	2	PO	7	**AB**	**CD**	2		0						**AC**	**BD**	1		4	**ABC**	**D**	1	PO	10
															ABD	**C**	1		7					
		22					24					23					23					20		

Table 5: EXP III

the unique rank-difference-minimizing distribution, whereas (ABC,D) is the lexicographic refinement of the "Rawlsian" solution (first minimize the rank of the worst off player; if this is not unique, minimize among all such distributions the rank of the better off player).[10] In R5 there is no unambiguous reference distribution since the minimal rank difference of 3 is achieved by all distributions involving two goods for each

[10] Note also that, in contrast to the distribution (BC,AD), the distribution (ABC,D) is envy free. In this example, there are thus several conflicting criteria of quite heterogeneous nature.

player. The CPIES procedure is thus not applicable here. Nevertheless, also this example confirms the prediction of a rank-difference-minimizing distribution among all Pareto optimal distributions (chosen in 90% = 18/20 of all cases).

R1			
n.a.	P1	P2	n.a.
100	ABCD	ABCD	100
98	BCD	ACD	87
92	ABC	ABD	77
86	ABD	BCD	62
77	ACD	ABC	60
60	BC	CD	45
55	BD	AB	43
42	AB	BD	40
39	AC	AD	39
35	AD	BC	36
31	CD	AC	33
12	B	D	24
9	C	A	15
3	A	C	7
2	D	B	2
0	-	-	0

P1	P2	#		Δ
AB	CD	14	PO	2
AC	BD	4		1
AD	BC	3		0
BC	AD	2	PO	3
		23		

R2			
n.a.	P1	P2	n.a.
100	ABCD	ABCD	100
92	BCD	ABD	97
87	ACD	ABC	87
81	ABC	BCD	72
70	ABD	ACD	70
63	CD	BD	49
53	BD	AD	43
49	BC	BC	40
42	AC	AB	36
35	AB	CD	32
30	AD	AC	29
16	C	B	24
12	D	D	19
9	B	A	15
8	A	C	8
0	-		0

P1	P2	#		Δ
BC	AD	21	PO	1
BD	AC	2		4
AB	CD	1		0
		24		

R3			
n.a.	P1	P2	n.a.
100	ABCD	ABCD	100
95	ABC	ABCD	85
90	ACD	BCD	75
83	ABD	ABD	66
77	BCD	ACD	62
56	AB	AD	51
50	AC	BC	46
46	BC	AC	42
39	AD	BD	35
32	CD	CD	31
24	BD	AB	28
19	A	B	27
13	C	CD	17
9	B	A	10
5	D	D	7
0	-	-	0

P1	P2	#		Δ
BC	AD	18	PO	2
AC	BD	4	PO	2
		22		

R4			
n.a.	P1	P2	n.a.
100	ABCD	ABCD	100
98	BCD	ABD	87
92	ABC	ACD	77
86	ACD	BCD	62
77	ABD	ABC	60
60	BC	AB	45
55	BD	BD	43
42	CD	BC	40
39	AB	AD	39
35	AC	CD	36
31	AD	AC	33
12	C	D	24
9	B	A	15
3	D	B	7
2	A	C	2
0	-	-	0

P1	P2	#		Δ
CD	AB	22	PO	2
BC	AD	1	PO	3
AB	CD	1		1
		24		

R5			
n.a.	P1	P2	n.a.
100	ABCD	ABCD	100
92	BCD	ABD	97
87	ABC	ABC	87
81	ACD	ACD	72
70	ABD	BCD	70
63	CD	BD	49
53	BD	AD	43
49	BC	AC	40
42	AC	CD	36
35	AB	AB	32
30	AD	BC	29
16	CD	A	24
12	B	B	19
9	D	D	15
8	A	C	8
0	-	-	0

P1	P2	#		Δ
BD	AC	14	PO	1
BC	AD	8	PO	1
AC	BD	2	PO	3
		24		

Table 6: EXP IV

Table 6 summarizes EXP IV. The distinctive feature of the example in R1 is that there are several Pareto improvements from the reference distribution (AD,BC), not all of which are Pareto optimal. Indeed, the rank difference minimizing Pareto improvement is (AC,BD) which is itself not Pareto optimal. As the examples R3 of EXP I and EXP II, also this example illustrates a tension between inequality and efficiency since 30% (= 7/23) of the observed agreements are not Pareto optimal. Interestingly, the effect is less pronounced here than in the cardinal treatment with full payoff information (see Section 4 for further discussion). The results in R2 very clearly illustrate the role of the rank difference, since the feasible Pareto improvements (AC,BD) and (CD,AB) from the reference bundle (AB,CD) are never chosen; both involve a rank difference of 3. Besides R4 of EXP III, the example R3 in EXP IV is the only example where a rank difference minimizing Pareto optimal distribution does not automatically coincide with the "Rawlsian" solution, i.e. the maximin solution that minimizes the rank of the worst off player. Indeed, both (AC,BD) and (BC,AD) involve the same rank difference of 2 and are both Pareto optimal, but of course, only (BC,AD) minimizes the rank of the worst off player. Incidentally, it is chosen much more frequently, namely in 82% (= 18/22) of all cases.[11] The two final rounds R4 and R5 once again confirm the role of rank difference, Pareto optimality and superiority relative to the reference distribution. Note that in R5, just as in R1 of EXP IV, there is a Pareto improvement from the equal split distribution (AD,BC) that is itself not Pareto optimal, namely the distribution (AB,CD) which is dominated by all of the chosen distributions; in contrast to (AC,BD) in R1 the distribution (AB,CD) is never chosen in R5.

4. Results

The average earning over all experiments and all subjects was about € 9 (in approximately one hour).[12] In less than 5% of our observations (23 of 480), bargaining partners did not reach an agreement and thus earned zero in the respective round. The majority of rounds lasted 9 minutes or longer since some subject pairs bargained until shortly before the deadline. However, many agreements were reached much quicker. For instance, in EXP II the median bargaining time ranged from 2 minutes 10 seconds (in R5) to 5 minutes 40 seconds (in R3).

The following table summarizes the results in treatment *CARD.* Each entry in the matrix gives the percentage of final distributions in the corresponding round (column) that satisfy the criterion corresponding to the row. The first row quantifies how many distribution were Pareto optimal. The second row gives the share of chosen distributions

[11] Note that, compared to (AC,BD), the Rawlsian distribution (BC,AD) is both envy free and, obviously, has a smaller *sum* of the ranks for the two players. Therefore, it is not evident which criterion is responsible for the result.
[12] Average earnings were thus comparable to the usual wage for student jobs at the University of Bonn.

with minimal difference in numerical payoffs (unique in each case), while the third row quantifies how many distributions were consistent with the CPIES procedure. Finally, the last row indicates the total number of agreements reached in the respective round (maximally 24).

	Exp I			Exp II					Total
	R3	R4	R5	R1	R2	R3	R4	R5	
Pareto Optimal	39%	100%	96%	92%	100%	45%	96%	100%	84%
Min Diff (“Reference Point”)	61%	95%	91%	79%	96%	55%	96%	92%	84%
CPIES	91%	95%	91%	79%	96%	100%	96%	92%	92%
Number of Agreements	23	22	23	24	23	20	24	24	183

Table 7: Results in treatment CARD (EXP I & EXP II)

Of particular interest are R3 in EXP I and R3 in EXP II in which the same division problem was given (we could have aggregated the data of these two rounds as well). Here “equal split” involves the allocation (BD,AC) with a resulting payoff distribution of (45,45), cf. Tables 3 and 4. In R3 of EXP I, 61% of subject pairs choose this allocation, and 55% in R3 of EXP II. The interesting fact is that this allocation is not Pareto optimal. Indeed, the allocation (AD,BC) Pareto dominates the equal split with a resulting payoff distribution of (46,75). Note that, in particular, both the “Rawlsian” maximin criterion and the utilitarian criterion (“maximize the sum of the individual payoffs”) favor (AD,BC) over the equal split. Thus, since most subjects settled on the distribution (BD,AC), our observations do not seem to lend support to C&R’s model of social preferences. The reason for a clear majority of subjects rejecting the Pareto improvement (AD,BC) seems to be the considerable payoff difference of 29 Talers, confirming the presence of inequality aversion as modeled by F&S and B&O.

Observe that either the choice of (BD,AC) and the choice of (AD,BC) is consistent with the CPIES procedure. In R3 of EXP II these two distributions were the only observed outcomes, whereas in R3 in EXP I two subject pairs agreed on the Pareto optimal allocation (AB,CD), a choice which is *not* consistent with the CPIES prediction since (AB,CD) is not a Pareto improvement relative to the equal split distribution (BD,AC), see Table 3.

	EXP III				EXP IV					Total
	R1	R2	R3	R4	R1	R2	R3	R4	R5	
Pareto optimal	86%	92%	100%	91%	70%	88%	100%	96%	100%	91%
Rank Diff Min (“Reference Point”)	0%	0%	83%	0%	13%	4%	0%	4%	0%	11%
Pareto optimal & Rank Diff Min	77%	71%	83%	57%	61%	88%	100%	92%	92%	80%
CPIES	91%	79%	83%	91%	100%	92%	100%	100%	100%	93%
Number of Agreements	22	24	23	23	23	24	22	24	24	209

Table 8: Results in treatment ORD (EXP III & IV)

Table 8 summarizes the results of treatment *ORD*. As before, the first row gives the percentage of Pareto optimal agreements. The second row quantifies the percentage of agreements on the distribution displaying the minimal difference in the respective ranks in the two players’ rankings of bundles. Just as the distribution with minimal payoff difference in the cardinal treatment, this is the distribution that serves as the reference point for the CPIES procedure in the ordinal treatment. For instance, in R1 of EXP III it is the distribution (AD,BC) since the bundle AD has the same rank in player 1’s ranking as the bundle BC in player 2’s ranking. The data from R5 of EXP III have been left out in Table 8, since there was no unambiguous reference point, as already noted above. In all other rounds there was a unique reference distribution.[13] The third row gives the percentage of chosen distributions with minimal rank difference *among all Pareto*

[13] In R3 of EXP III, the reference distribution was Pareto optimal and chosen in 83% of all cases (cf. Table 8). In all other examples of the ordinal treatment, the reference distribution was not Pareto optimal and chosen only in 3% (=5/209) of all cases.

optimal distributions.[14] This criterion almost always coincides with the "Rawlsian" criterion of maximizing the welfare of the worst off individual, i.e. in our case of minimizing the maximal rank of a bundle in the player's preference ordering.[15] Observe also that any distribution that minimizes the rank difference among all Pareto optimal distributions either coincides with the reference distribution (in R3 of EXP III) or is a Pareto improvement relative to the reference distribution (in all other examples of EXP III and IV). The fourth row gives the percentage of agreements that are consistent with the CPIES prediction. In EXP III, fewer distributions in which each player received two goods were consistent with the CPIES prediction, therefore the corresponding numbers in the fourth row are lower for EXP III than for EXP IV. As is apparent from the numbers in Table 8 and the results reported in Tables 5 and 6, the allocations according to the CPIES rule were not chosen randomly. We tested this using a binomial test and found the results to be highly significant; the *p*-values are given in Appendix 2.

A comparison of the first rows in Tables 7 and 8, respectively, suggests that the failure of Pareto optimality is less prominent in the ordinal than in the cardinal treatment. This becomes particularly transparent by comparing R3 in EXP I (treatment *CARD*) with R2 EXP III (treatment *ORD*). In both cases, there is an unambiguous "equal split" reference distribution, (BD,AC) in R3 of EXP I and (AB,CD) in R2 of EXP III, respectively, and exactly one distribution that Pareto dominates this reference distribution; in both cases it is the distribution (AD,BC). In R3 of EXP I, the reference distribution was chosen in 61% (= 14/23) of all cases, whereas in R2 of EXP III only in 8% (= 2/24) of all cases. We conducted a Fisher-test and found the differences in behavior in the two treatments to be highly significant.[16]

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[14] In R2, R3, R4 of EXP III and in R1, R2, R4 of EXP IV this criterion yields a unique prediction. In the other rounds, there were two rank difference minimizing distributions.

[15] The only exception is R4 in EXP III in which there are two "Rawlsian" solutions, namely (BC,AD) and (ABC,D). While both are Pareto optimal, the first of these distributions involves a rank difference of 2, whereas the second distribution represents the lexicographic refinement of the Rawlsian solution with a rank difference of 3. Incidentally, the first distribution was chosen much more often.

[16] Specifically, we asked whether subjects chose the Pareto dominated equal split distribution more often in the cardinal treatment than in the ordinal treatment. We tested the equal split distribution both against *all* Pareto optimal choices (case i), and against only the Pareto improvements from equal split (case ii). Moreover, we tested R2 of EXP III both against the data of R3 in EXP I (p=0.0002 in case i, p=0.0003 in case ii) and against the aggregated data from R3 in EXP I and R3 in EXP II (p=0.0000 in case i, p=0.0002 in case ii). In each case, the difference in behavior in the ordinal and cardinal treatments is thus significant at least at the 1% level.

5. Conclusion

Our experimental results suggest a particular qualitative description of how agents reach agreements in bargaining problems with indivisibilities, the CPIES procedure. The key element is the role of "equal split" as the reference point for the bargaining procedure. Pareto improvements are implemented *provided* that they do not create too much inequality. Indeed, our most striking finding is that a clear majority of 58% of bargaining partners reject the payoff distribution (46,75) in favor of the Pareto inferior equal split distribution (45,45) (aggregated data from EXP I, R3 and EXP II, R3). This is in sharp contrast to the results of C&R, who "find a strong degree of respect for social efficiency" (p.849). It also conflicts with Kritikos and Bolle's (2001) experiments in which the majority of participants were efficiency- rather than equity-oriented. However, the experiments in these studies consisted of simple dictator games and not of dynamic bargaining games as in our present study. Indeed, inspecting the chat protocols we found strong evidence that procedural aspects play a crucial role in our experiments. For instance, in the above example, it seems to matter *who* is the first to propose the Pareto improvement. A detailed analysis is beyond the scope of the present paper, but generally the partners agree more easily on the payoff distribution (46,75) if the first individual suggests it to the second individual than if the second individual proposes it first (see Herreiner and Puppe (2004b)). It seems to matter whether the individual suggesting the Pareto improvement benefits more or less than the other person.

The failure of Pareto optimality due to equity concerns is much less pronounced in the ordinal treatment, even though we do find evidence that the ranks of bundles in the preference orderings serve as substitutes for the unknown monetary payoff. A possible explanation is that the quantification of inequality is *uncertain* in the ordinal treatment; by consequence, the "equal split" distribution (in the sense of equal ranks) looses some of its attractiveness as reference point – an issue, we plan to investigate further.

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Appendix 1 : Matching of Bargaining Partners

The following table shows how individuals were matched in each round of each session. The numbers in the table refer to the 8 individuals. The columns indicate the rounds. Each row shows the bargaining partners in the respective round with the individual in the role of player 1 named first.

R1	R2	R3	R4	R5
1 5	1 8	1 2	7 1	6 1
2 6	2 5	3 4	8 2	7 2
3 7	3 6	5 6	5 3	8 3
4 8	4 7	7 8	6 4	5 4

Table A1 : Matching of Bargaining Partners

Appendix 2 : Binomial Test of the CPIES prediction

The following table indicates *p*-values for a one-tailed test. The rows indicate the different treatments. The columns show how many allocations ("#") are compatible with the CPIES prediction and how frequently any of them were chosen ("Occ"). The probabilities ("Prob") have been calculated under the assumption that all allocations where each player receives two goods occur with a probability of 1/6 (ignoring any other allocations that may have been chosen). In R1 and R4 of EXP III the probability assigned to each allocation has to be adjusted to 1/7 because the allocations with two-good bundles span ranks 4-12 (instead of 6-11) and for 7 allocations[17] both bundles are within that range. The same calculation can be done based on all 16 allocations, assuming that they all are equally likely. Those probability values are obviously even lower than the ones shown in the table.

			#	Occ	Prob
CARD	EXP I	R3	2	21/23	$1.1249 \cdot 10^{-8}$
		R4	1	21/22	$8.4333 \cdot 10^{-16}$
		R5	1	21/22	$8.4333 \cdot 10^{-16}$
	EXP II	R1	1	19/24	$2.9488 \cdot 10^{-11}$
		R2	1	22/23	$1.4689 \cdot 10^{-16}$
		R3	2	20/20	$2.8680 \cdot 10^{-10}$
		R4	1	23/24	$2.5536 \cdot 10^{-17}$
		R5	1	22/24	$1.4817 \cdot 10^{-15}$
ORD	EXP III	R1	3	20/22	$3.5396 \cdot 10^{-6}$
		R2	2	19/23	$1.6667 \cdot 10^{-6}$
		R3	1	19/23	$7.2964 \cdot 10^{-12}$
		R4	3	21/23	$1.6561 \cdot 10^{-6}$
	EXP IV	R1	4	23/23	$8.9105 \cdot 10^{-5}$
		R2	2	22/24	$4.0824 \cdot 10^{-9}$
		R3	2	22/22	$3.1866 \cdot 10^{-11}$
		R4	3	24/24	$5.9605 \cdot 10^{-8}$
		R5	3	24/24	$5.9605 \cdot 10^{-8}$

Table A2 : p-values for the CPIES prediction

[17] The relevant allocations are (AB,CD), (AC,BD), (AD,BC), (BC, AD), (BD, AC), (CD,AB), and (ABC,D).

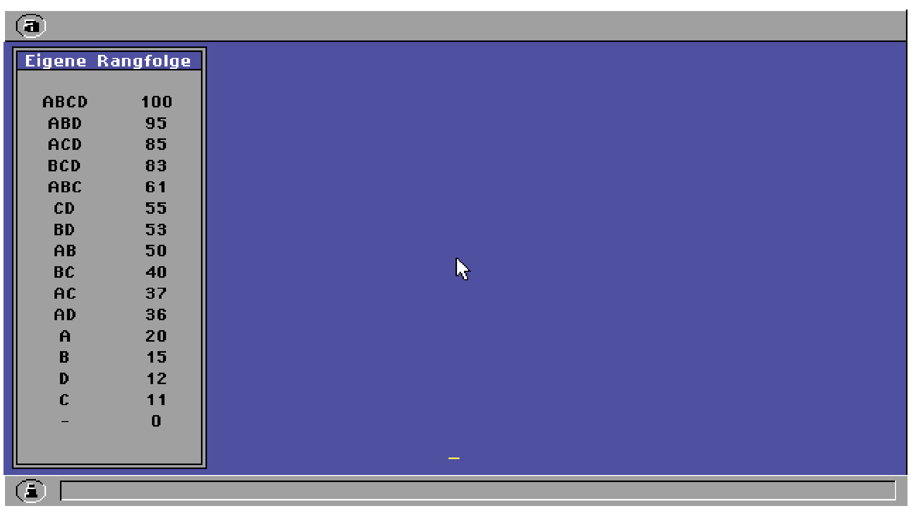
Appendix 3: Instructions and Screen Examples

(The following is a translation of the German instructions for EXP 1 – as close as possible to the German original. The original instructions are available upon request from the authors.)

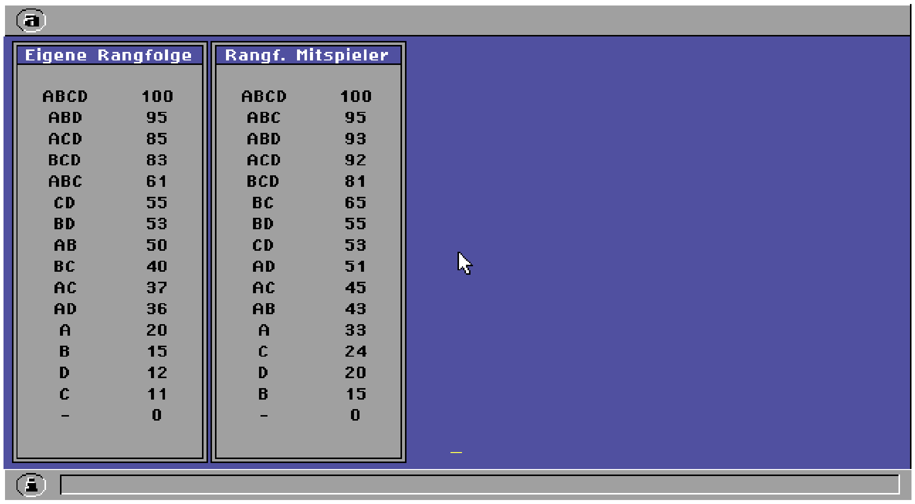
In this experiment you will repeatedly have to distribute several goods between yourself and a partner. The experiment has five independent rounds, each of which you will play with a different partner. In each round you will be given four goods, and you will have to agree with your partner on a distribution of these goods.

There will be four new goods in each round. The goods are referred to as A, B, C and D, respectively. You can think of any kind of object and any kind of division problem. The goods themselves are indivisible, i.e. each good can either be given to you or to your partner. All goods have a positive value. The more goods you receive, the better. However, the value of the goods is different for you and your partner. In each round, we give you a ranking of the bundles of goods in which the values of the bundles are listed in descending order. In each round, you will be given a new ranking. The ranking gives the value of each bundle of goods in Taler (T), our experimental currency. If you agree with your partner on a distribution of goods, you will receive the Taler amount corresponding to your bundle of goods. At the end of the experiment, these Taler amounts will be converted in Deutsche Mark (DM) and paid out to you.

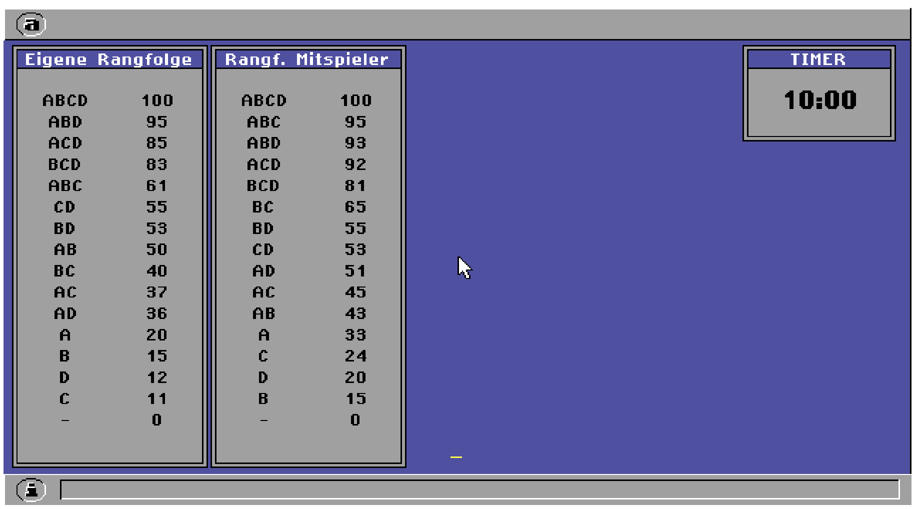
For example, your ranking could look as follows:



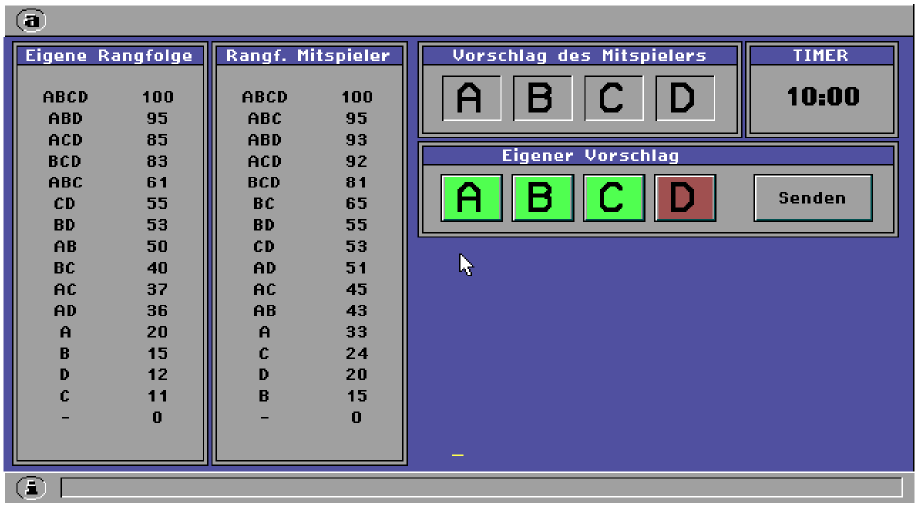
In this case, your most preferred bundle consists of goods A, B, C and D; it is worth T 100. Thus, if you and your partner agreed that he gets nothing and you get all four goods, then you would receive T 100. Your second best bundle is ABD, for which you would receive T 95 if you agreed with your partner that you get ABD and he gets C. Observe that the value of bundles of goods cannot be derived from the values of the single goods. For instance, good C alone is worth T 11 and good D alone is worth T 12, but both goods combined (CD) are worth T 55 to you. It is also possible that a good is worth little when added to another bundle, e.g. the bundle ABD is worth T 95 to you and adding C increases the value of the bundle only to 100 (ABCD), although good C alone is worth 11. In this case, good C does not add much value to the bundle ABD. The goods complement each other in different ways depending on the specific goods with which they are combined. Therefore, for all evaluations in this experiment you have to look at all bundles of goods and not only at the values of single goods. Your partner also gets a ranking of his valuations. On the screen, you will see your partner's ranking next to your own. This may look as follows:



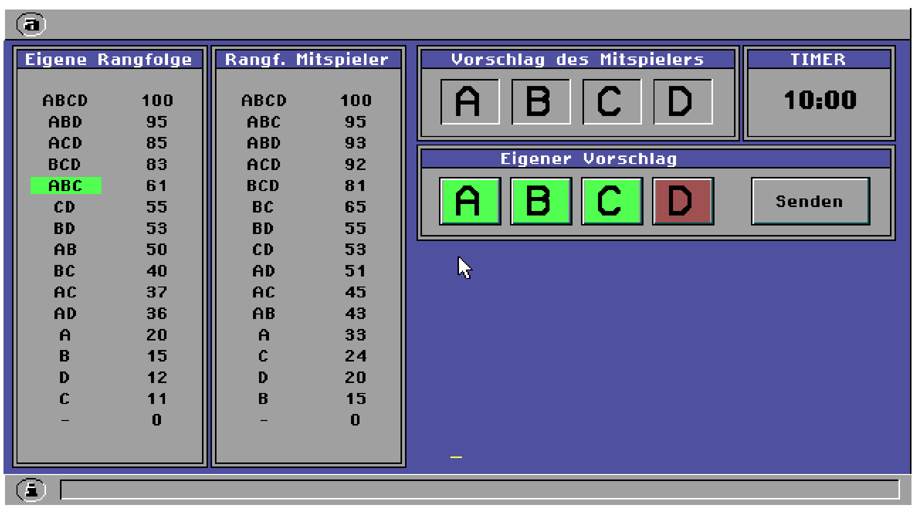
Please start each round by carefully looking at both rankings. The rankings will be different in each round. Each round of this experiment lasts 10 minutes at most. This time is indicated at the top right side of the screen and will be counted down to 0:00 during the round. Within this time span you have to reach an agreement with your partner on who gets which good. If you do not agree within 10 minutes, neither of you will receive anything in this round.



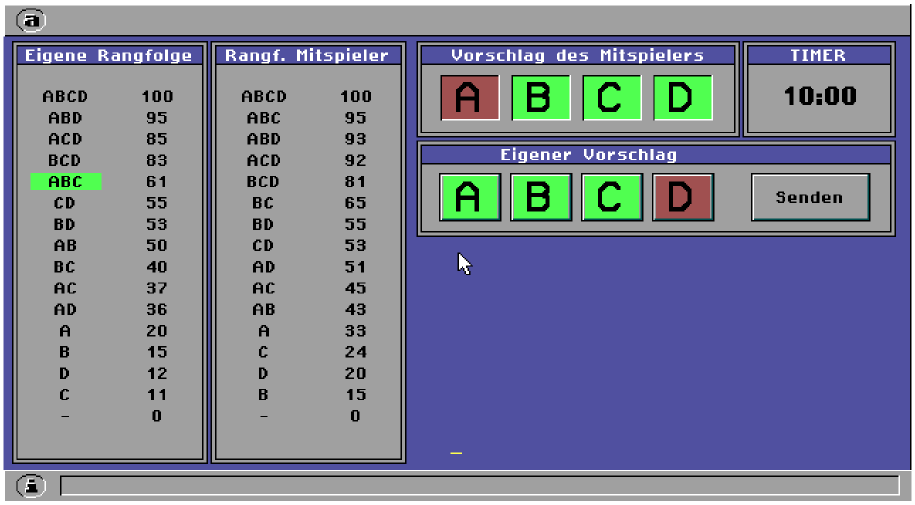
You reach an agreement with your partner by sending him a proposal or by waiting for his proposal. Each of you can make a proposal at the same time. Your partner's proposal appears in the top middle section and your own proposal appears directly beneath. In *both* proposal lines, the goods you get appear in *green*, those received by your partner in *red*. To make a proposal, select the goods you want to receive by clicking on the corresponding buttons, and then send the proposal by clicking on the "send" button.



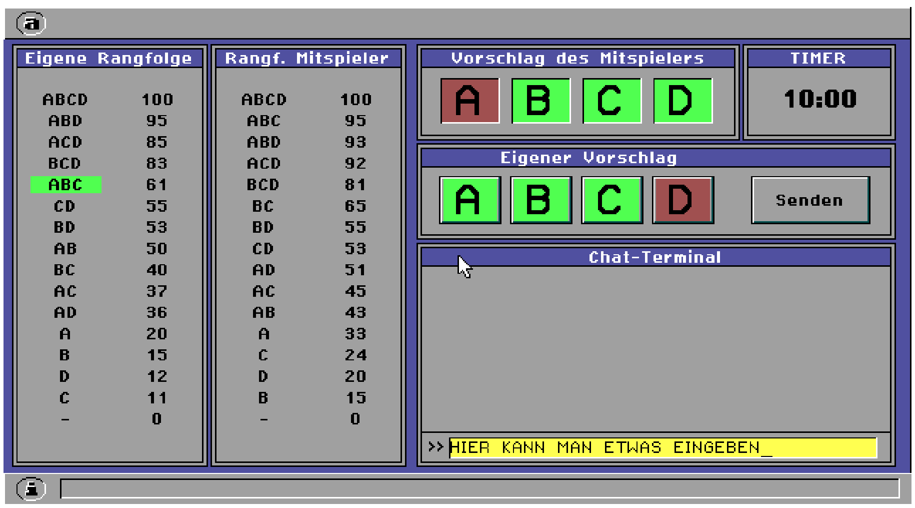
You can change your proposal at any time by clicking on the A, B, C, D buttons. Every click changes the color of the button and therefore moves the good from you (green) to your partner (red) or vice versa. Unless you send your proposal, your partner cannot see your current selection. The most recent proposal you sent can be seen in your ranking on the left – your corresponding bundle is shown in a green box.



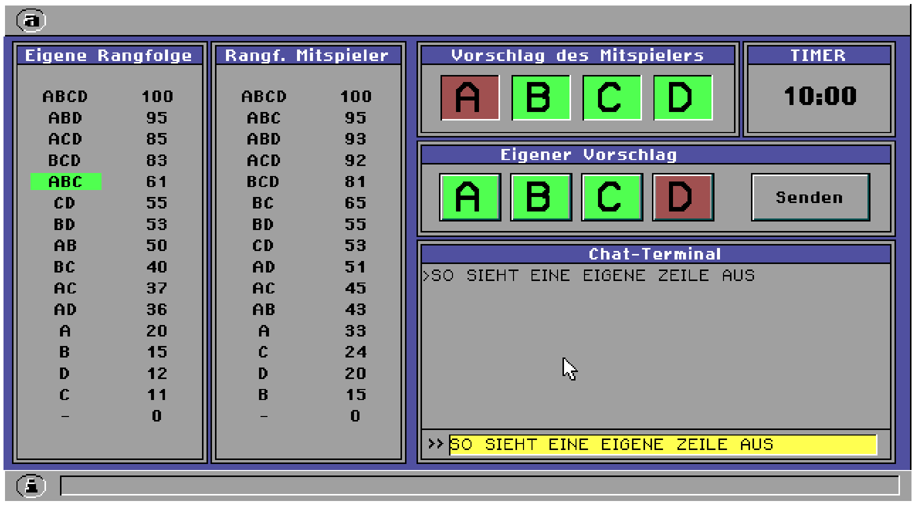
Do not delay sending your proposal because your partner will otherwise not know what you propose. You can change your mind at any time and send a new proposal.



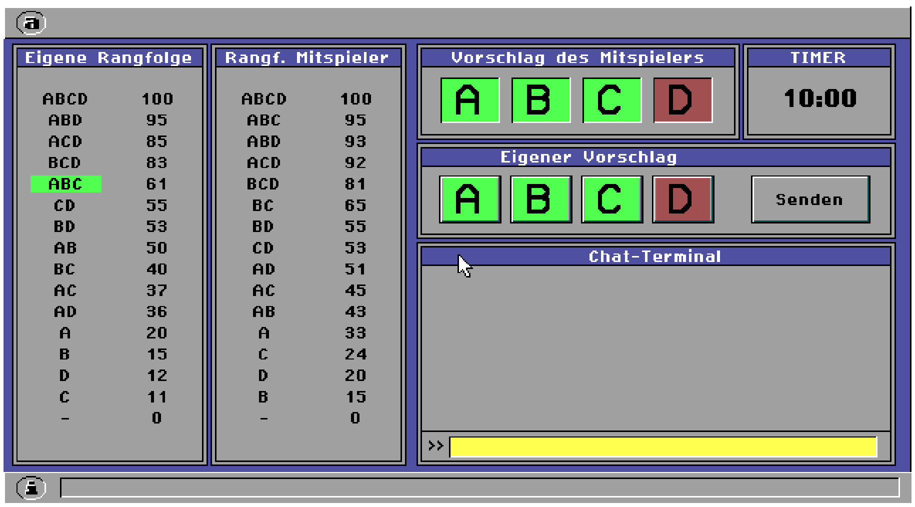
In order to convince a partner to accept your proposal, you can exchange messages in a "chat" window at the bottom right by commenting on your or your partner's proposal. To write in the chat line (max. 80 characters), you have to click on it with the mouse. Press the "enter" key to send a comment. If you want to leave the chat line without writing anything or without sending a comment, you have to press the "Esc" button. If you want to change your proposal after having sent a comment, you will need to leave the chat line first.



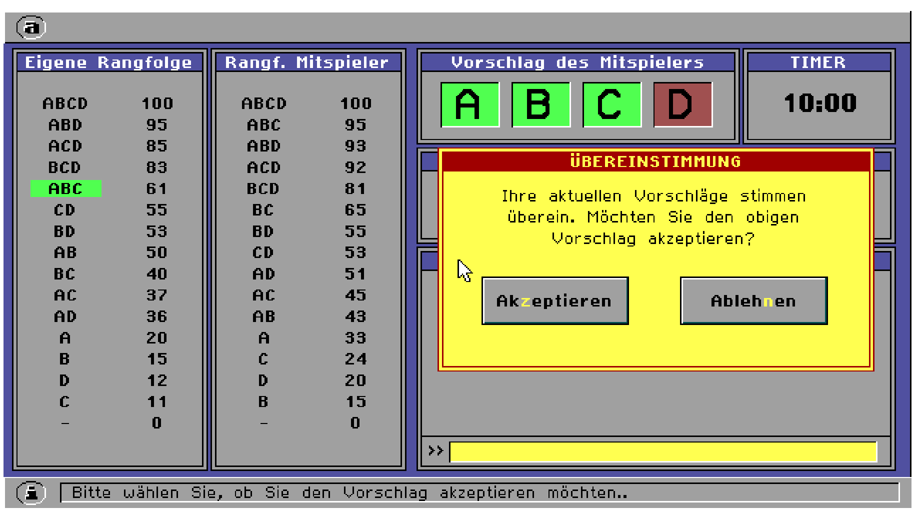
Your own comments appear in the chat terminal window with a leading ">" sign; your partner's comments are shown without any additional sign at the beginning.



If the colors of all buttons in your proposal coincide with the colors of the buttons in your partner's proposal, then you have made identical proposals.



You will then be asked whether you want to accept that proposal.



If you and your partner select “Accept” the proposal is accepted and the round is over. If neither or only one of you accepts the proposal, then the round continues, i.e. you can make new proposals or repeat old proposals, and chat. A round is over either if you have both accepted a proposal or if the time limit is reached. If the round has ended before the time limit, you will have to wait until the round is over for all other players – this will be indicated by an acoustic signal. Then, the next round starts for everybody.

At the end of each round you receive the Taler amount corresponding to your bundle of goods. If you did not reach an agreement with your partner you receive no bundle of goods and therefore no Taler amount. The Taler amounts you received will be added over the rounds and converted into DM at the end of the experiment. T 12 equal DM 1.

You will play with a different player in each round of the experiment, hence you *never* play with someone you have already played with. You and your partner *do not know* with whom you play; you will be matched anonymously. What proposals you make, what comments you send, and what bundle of goods you receive in any given round has *no* impact on your or your partner’s ranking of bundles, or on the matching of partners in future rounds.

Please do not mention your name and do not make any comments that could reveal your identity. If you violate this rule you will receive no payment!

All relevant information will appear on the screen. A status line at the bottom of the screen indicates the current state of the experiment. Before starting the experiment, you receive a number that corresponds to your computer terminal and you will be paid at the end based on your number.

Do you have any questions?
Please switch off your cell phones for the duration of the experiment.
Thank you for your cooperation.
Good Luck.