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DISCRIMINATING TO LEARN TO DISCRIMINATE*

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Abstract

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Keywords: Prisoner's dilemma; Cooperation; Exit; Experiments; Learning.

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JEL classification numbers: C14, C72, C78, C91

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1 Introduction

The Prisoner's dilemma game has been taken to the experimental laboratory well above 1000 times (see Sally (1995) for an overview). Its characteristic tension between the Pareto superiority of cooperation and the strict dominance of defection promises an interesting study of human behaviour. The game, indeed, deserves special attention since it formalizes many experiences of everyday life. Situations of friendship formation, information sharing, joint research, selling and buying among others can all be modeled by this simple game.

The standard prediction that defection is the only rational outcome in the finite version of the game leaves a feeling of unease: it cannot be reconciled with observed cooperation levels. Controlled experiments have tried to nail down reasons for cooperation or the failure to cooperate. Many different explanations have been put forward, reaching from framing and presentation effects[1] to postulates on the way how human beings are, reason and learn. E.g. persistent cooperation in one-shot experiments are often seen as evidence for the existence of intrinsic cooperative types. How well-founded are these explanations? Do existing experiments capture all aspects that might be important for cooperation?

The above examples of everyday prisoner's dilemmas reveal that human beings often play several such games simultaneously in the same time interval and that many of these games have a voluntary interaction structure. E.g. I can choose my friends or co-authors. Partner selection is a strategic choice and might therefore influence the outcome of the game.

In the last years, several theoretical papers have examined the influence of partner selection on cooperation levels[2]. Those papers confirm the strategic importance of partner selection and how it relates to cooperation; however, the extent to which cooperation is enhanced depends in a sensitive way on the model specifications and behavioural assumptions. The great majority of papers use finite automata to model behaviour within the prisoner's dilemma and some even use inbuilt stopping rules (e.g. if your game partner defects never interact with him again) to model partner selection. Thereby they implicitly assume the existence of player types who treat (identical) opponents with the same history of play in exactly the same way. Is it reasonable to assume that such types do indeed exist? The evidence experimental results have presented so far rests on shaky grounds since there are no experiments which allow subjects to play simultaneously with several players the two-person prisoner's dilemma game. Only if in a multiple game situation subjects choose the same strategy with identical opponents will the hypothesis of the

[1] e.g. the words chosen to instruct experimental subjects might encourage cooperation.

[2] these papers include among others Ashlock *et al.* (1996), Ghosh and Ray (1996), Morikawa *et al.* (1995) and (1996), Orbell and Dawes (1991), Peck (1993), Schluessler (1989), Smucker *et al.* (1994), Stanley *et al.* (1994), Tesfatsion (1995), Hauk (1997)

existence of "fixed" player types be well-founded.

The present paper addresses two different but related issues: (i) is the assumption of player types justified?. (ii) It examines partner selection in an environment without any ex-ante stopping rules or ex-ante player types. For this purpose three sets of experiments are conducted. In each setup every experimental subject plays 10 supergames of a 10 period finitely repeated two person prisoner's dilemma with 6 other subjects simultaneously. I.e. in every period each subject plays 6 prisoner's dilemma games, choosing one strategy for each opponent. Partner selection is modeled in the form of an outside option with zero payoff which results from the subjects' conscious decision not to enter (which is equivalent to exiting) a period of a game. The experimental setups differ in whether exit is possible and in the relative payoff this exit option yields. In the basic setup there is no partner selection. In the second setup the payoff from exiting is better than the mutual defection payoff[3] and in the third setup the payoff from exiting is worse.

The observations on individual behaviour in multiple game situations are very illuminating. They strongly reject the existence of player types: most subjects are not cooperatively or defectively inclined but use different types of behaviour against opponents with an identical history of play, i.e. they discriminate among equals. Discrimination is both common among subjects and persistent over time. This result seriously questions any type-dependent theoretical work and can affect strongly the learning dynamics in the game.

Indeed, discrimination leads to a very different cooperation patterns than the ones usually observed in finitely repeated prisoner's dilemma experiments (see e.g. Roth *et al.* (1978), Selten and Stoecker (1986) and Andreoni and Miller (1993)). Cooperation levels which normally tend to fall in early supergames rise immediately in the multiple game experiment. Both cooperation and endeffect behaviour[4] are learned sooner. Discrimination might seem arbitrary but has some optimal properties. This happens because experimental subjects are all the time experimenting with the different available strategies and thereby quickly learn which ones perform better. Discrimination serves as a way to resolve the intrinsic tension of the prisoner's dilemma between the Pareto superiority of cooperation and the strict dominance of defection. Two things have to be achieved: (i) cooperation (ii) at the minimal cost. Discrimination helps to spread the risk involved in cooperative attempts. Experimental subjects function like classifier systems: the same initial situation triggers different behavioural rules, with more successful rules being reinforced over time.

Experimental subjects behave in a very clever sophisticated manner, although they

[3]Therefore, I sometimes refer to this setup as "better".

[4]see Section 3 Def. 1

are left free to do anything. This implies that theorists have to be very careful which behavioural rules they impose on their economic agents. It is not reasonable to assume any rule. The experiment confirms the commonly shared belief among economists that human beings are not stupid and do not make consistent errors for ever.

Concerning partner selection, the experiment confirms that voluntary interactions favour the Pareto superior outcome. In both experimental setups partner selection clearly enhances cooperation[5] although the strategic role of the outside option is very distinct. In setup III exiting serves to sustain cooperation by constituting a harder punishment for defection than defection itself. In the experiment this non-subgame-perfect Nash equilibrium shows some strength. Moreover, exiting in early periods of a supergame is used as a less costly way than immediate cooperation to signal cooperative intentions. In setup II exiting constitutes the subgame perfect equilibrium path. However, complete inactivity is hardly ever observed. Players quickly learn to cooperate and become very efficient in excluding defectors.

Observed behaviour in the final period of setup II corroborates our hypothesis that experimental subjects function like classifier systems. An instance of *not learning by not doing* occurs. A considerable proportion of experimental subjects fails to learn to exit in the last period of a supergame[6]. Last period entry usually happens only with a limited number of game partners. Since last period entry only leads to a game if both opponents enter, in the majority of cases no game occurs and the negative consequences of last period entry do not arise. Having no consequence, last period entry is neither discouraged nor reinforced.

The remainder of the paper is organized as follows. In the next section the experimental setup is explained and justified. Sections 3 and 4 introduce the hypotheses, test them and discuss the results. Section 5 describes last period entry in setup II (not learning by not doing). The final section concludes.

2 Experimental setup

2.1 The underlying game

The experiments are based on the two person repeated prisoner's dilemma with the following bimatrix of the one-shot game.

[5]In the literature, the payoff structure of setup II has been examined for the *one-shot game*. Even in the one-shot game, experiments found that cooperation levels increased. (Orbell and Dawes (1993) for the 2-person case and Orbell, Schartz-Shea and Simmons (1984) for the n-person prisoner's dilemma.)

[6]Last period play can only lead to defection.

	cooperate	*defect*
cooperate	5 5	7 -6
defect	-6 7	∓ 1 ∓ 1

Prisoner's Dilemma

During the experiment defection was coded by a and cooperation by b. The $\mp$ entry for mutual defection represents the two payoff matrices used in the different experiments. The negative entry was used in the baseline experiment and in setup II, while setup III used the positive entry.

2.2 The baseline experiment

The baseline experiment (treatment I) concentrates on replicating the multiplicity of human activity. For this purpose the repeated prisoner's dilemma (without exit) was played in 6 parallel partnerships. 7 subjects were used, since 6 partnerships seems to be few enough to keep track of every individual match, and a big enough number to allow for experimentation and a wide experience in a short time period. Subjects did not have to commit to a supergame strategy before beginning to play nor were they forced to use the same strategy against everybody. In other words, when deciding how to play against two players with the same history, they could choose to defect against one and cooperate with the other. This freedom of choice allows us to test for the existence of fixed player types.

Treatment I is called baseline experiment since it only addresses one of the two issues to be examined and since - concerning the second issue (partner selection) - it additionally serves as a basis for comparison with experimental setup II.

2.3 Further experimental treatments

The two remaining experimental setups model *voluntary* interactions. Partner selection takes place in form of a conscious choice whether or not to play with a certain subject. Each period of a supergame has two stages: the matching stage, in which subjects express their willingness to get matched; and the game stage, in which matched pairs play the prisoner's dilemma. Not playing is equivalent to zero points.

Two treatments are examined using the negative and positive payoff entry for mutual defection respectively. The different payoff matrices affect the Nash equilibria of the model. In the first case (treatment II) the existence of the outside option shifts the only game theoretic (subgame perfect) equilibrium of the finitely repeated game from always defect to never play. Hence, if subjects play, they prefer the prisoner's dilemma over the safe outside option.

The second case (treatment III) has the same subgame perfect equilibrium as the baseline experiment, namely always defect, although this outcome is now positive in terms of payoffs. This changes the underlying incentive structure because subgame perfect play does no longer lead to losses but to a small but secure reward. Therefore treatment III does not allow for a direct comparison with treatment I. Treatment III is interesting in its own right, because it adds an outside option which is irrelevant for subgame perfect play since it is weakly dominated. One might therefore expect that experimental subjects will not use this option given that it is worse than the equilibrium payoff. However, the outside option adds an additional punishment for defection that is harder than defection itself. The non-credible background threat of exiting leads to the existence of further (non subgame perfect) Nash equilibria. Two of them are of special interest[7]:

1. mutual cooperation until period 9, in which both players switch to defection[8].
2. as before but exit in the first period of a supergame in order to signal cooperative intentions.

In both cases, defection before period 9 is punished via eternal exit. This punishment threat is not credible, since once defection has occurred reoptimization dictates to continue the game and to defect.

Treatment III serves to examine whether an "irrelevant" outside option nevertheless has some positive effects on cooperation levels. This would be the case if evidence for the above-mentioned non subgame perfect Nash equilibria could be found. This would be a very strong argument that the freedom whether or not to play to prisoner's dilemma is a key element in explaining observed cooperation levels in real data.

Table I summarizes the differences in the experimental setups.

[7]There are other Nash equilibria, which are not interesting for the problem under examination. E.g. exiting in every period is a Nash equilibrium, although it is weakly dominated.

[8]Let h represent the payoff achieved when defecting against a cooperator. d refers to the mutual defection payoff, c to the payoff received from mutual cooperation and l to the payoff resulting from cooperating against a defector. The incentive constraint is that n_c cooperative periods followed by mutual defection lead to a higher payoff than betraying the partner followed by being exited against forever. Formally, $n_c c + (10 - n_c)d \geq h + (n_c - 1)c$, which is fulfilled as long as $n_c \leq 9$ given the payoff matrix used.

setup	exit	payoff from mutual defection	SPE	further NE
I	no	-1	always defect	none
II	yes	-1	exit	none
III	yes	+1	always defect	yes

Table I

In all setups no restrictions on the strategy choice were made given the available options.

2.4 Matching mechanism

A match occurs *if and only if* two players have both explicitly stated their wish to interact. If only one person enters, no match occurs. Matches are reconsidered every period.

This simple matching mechanism isolates past behaviour and expectations as the causes of exiting. If the number of game offers and games were restricted exogenously, the non-occurrence of a match would no longer reveal a clear preference against this particular match, but could be due to capacity constraints. This would distort the effects of the exit option on the amount of cooperation.

2.5 Information

In each supergame every player is identified by a player number. This number remains the same during the supergame but changes when a new supergame begins. Throughout the experiment, players are able to look back into the history of all past supergames. This does not enable them to deduce a player's past identity but allows them to learn how to react to some reoccurring behaviour patterns.

Players will only be communicated whether a match occurred, but not the decision of the opponent. This implies that if player A said no to player B, player A will be communicated that he is not matched with B, but he will not know whether B wanted to be matched with him or not. Clearly, if B wanted to be matched with A, no match implicitly reveals to B that A did not want the match.

Players will only be told their own total payoff. They will be communicated their own score from each individual match as well as the opponent's score and action. The latter two pieces of information can be deduced form the payoff matrix and are hence redundant.

2.6 The end

Each supergame ends after 10 periods, which is known by the subjects.

Each experimental session consists of 10 supergames involving the same set of players.

2.7 Payment

Since some of the points in the prisoner's dilemma games are negative, players were given a starting capital of 1000 Pesetas. It was guaranteed to them that they did not have to give any money to the experimenter should they go bankrupt, which never happened. Points achieved in supergames were scaled by 15 when converted into Pesetas. Consequently, payoff incentives were very high in each supergame, as every player had 60 possible interactions[9]. Hence, in expected terms, every single decision mattered. As one experimental session consisted of 600 possible interactions, not every supergame could be paid using the before-mentioned desirable incentive structure. In order to ensure that subjects tried to do their best in every supergame, subjects were told that two randomly determined supergames will be paid. At the end of the experiment every subject was asked to draw once out of two urns. One urn contained supergames 1 to 5, the other urn contained supergames 6 to 10. Consequently, every supergame had equal probability to be paid. The subdivision into two groups rewarded for learning effects by guaranteeing the payment of some supergame in which learning could have occurred.

2.8 Experimental subjects and sessions

The subject pool consisted of students at the Universitat Pompeu Fabra Barcelona. They were either economists or business administration students in their first semester or students of humanities. Subjects were allowed to take part in only one experimental session and in only one experimental setup. The experiments were done in a period of two weeks. The baseline experiment lasted two hours, while the second and third treatment required 3 hours each. The experiments were conducted in a computerized laboratory and were programmed in $C++$. At the end of every experimental session subjects gave written reasons for their overall decisions. For every single experimental setup, three experimental sessions took place. Unfortunately in the last session, which was using the baseline experiment, the network broke down, hence the session could not be completed[10].

[9] 10 periods with each of the six other subjects.

[10] I only used available data to derive the experimental results, although the subjects' comments were detailed enough to simulate the continuation of the game. I decided not to do so, since experimental subjects did not behave 100% according to their own description.

3 Results I: the multiple game situation

The outcomes of the multiple game experiment strongly reject the existence of player types. At the same time they present a puzzle.

Observation 1 *experimental subjects discriminate among equals, i.e. they use different strategies against partners with an identical history of play.*

Observation 2 *Discrimination pays: compared to standard experiments learning is faster.*

The puzzle consists in that some apparently arbitrary way of behaviour (discrimination among equals) has some optimal properties. Before trying to resolve this puzzle some evidence for its occurrence will be shown.

Evidence for observation 1

In order to present evidence for observation 1 two types of discriminatory behaviour will be distinguished: (i) discriminating in initial periods of a supergame will be referred to as **divergence**. A player **converges** if he stops using diverse initial behaviour over the supergames. (ii) A player is said to be **mixing** if he reacts differently towards identical partners within the same supergame, i.e. if he discriminates among equals from period 2 of a supergame onwards[11].

Both completely unexperienced subjects and more experienced subjects discriminate. Divergence in the first period of the first supergame is the rule rather than the exception. $\frac{7}{9}$ of all experimental subjects diverged ($\frac{15}{21}$ in setup I, $\frac{18}{21}$ in setup II and $\frac{16}{21}$ in setup III). The binomial test rejects the hypothesis of equal probabilities of pure and diverging natural inclinations at a significance level which is basically zero[12]. This observation extends far beyond first encounters. Divergence and mixing behaviour does not disappear over time. $\frac{1}{3}$ of the subjects do not converge, while $\frac{8}{21}$ continue mixing (for details see appendix C). Moreover, there is no clear correlation between the proportion of subjects that diverged (mixed) in each supergame and the number of the supergame. Only setup II supports the hypothesis of falling mixing behaviour and falling divergence with increasing number of supergames. The Spearman rank order correlation coefficients r and their significance level α are summarized in Table II.

[11] From period 2 onwards some individual reputation exists.

[12] All test statistics used are explained in appendix A.

setup	mixing r	mixing α	divergence r	divergence α
I	-0.755	0.01	-0.318	insignificant
II	-0.91	0.0005	-0.831	0.0025
III	0.311	insignificant	0.25	insignificant

Table II

Apart from most coefficients being insignificant, some of them even have the wrong sign. Discrimination among equals is a persistent phenomenon.

Evidence for observation 2

To see that this seemingly arbitrary way of behaviour has nevertheless some optimal properties the multiple game situation has to be compared to standard experiments in which one supergame is played at a time. Figure 1 (see appendix B1 for underlying data) shows that percentage cooperation[13] in the present experiments, averaged over sessions, do not follow the typical well-known pattern as e.g. explained by Roth *et al.* (1978): in the typical pattern some cooperation occurs during the first supergame, but the strict dominance of defection immediately takes over. The possibility of reaching the Pareto superior outcome is noticed later and learned slowly. Consequently, cooperation levels fall in the second supergame and require several supergames before reaching and overtaking the original level of the first supergame[14].

Figure 1 illustrates that in the multiple game situation there is no drastic drop in cooperation levels in the second supergame. On the contrary, cooperation levels are either stable or increase immediately. This immediate increase in cooperation levels is mainly due to successful cooperative encounters. In all experimental setups the number of cooperative relationships[15] per subject is higher in supergame 2 than in 1, and increases further over several supergames (see appendix B3 and figure 3). Experimental subjects quickly learn not to waste their cooperative efforts. They cooperate if they find cooperation. In other words they cooperate only if cooperation pays. This is also confirmed by the development of always defective play in the setups (I and III) in which defection is subgame perfect. Although overall defection levels hardly increase or do not increase at all (see Figure 1), the number of defective relationships per subject increases heavily

[13]These are measured in average percentage (one-sided) cooperation levels. When the outside option exists, cooperation levels are conditional on mutual entry.

[14]E.g. in Selten and Stoecker (1986) it requires 12 supergames till cooperation is learned.

[15]A parnetship is COOPERATIVE (Selten and Stoecker (1986)) if both players choose the cooperative alternative at least during 4 subsequent periods.

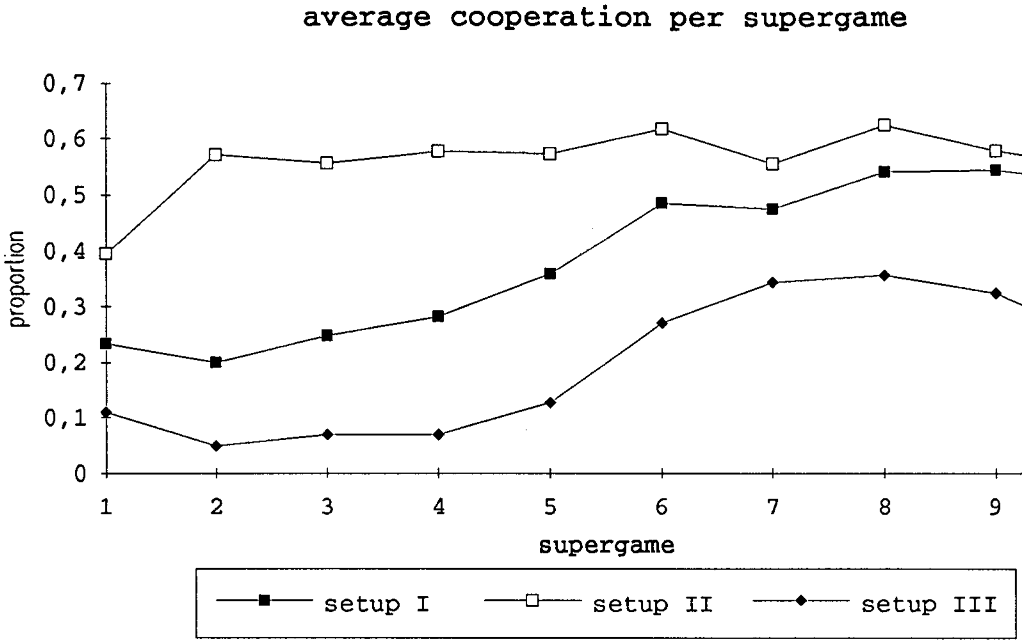

Figure 1:

from supergame 1 to 2. Figure 2 shows the percentage of (one-sided and mutual) always defective play in the different supergames (for underlying data see Appendix D). The pattern of always defective relationships implies that subjects learn to penalize defectors immediately. Given that at the same time subjects do increase their successful cooperative encounters, the incentives to move away from purely defective play are high. This explains the steady reduction in the number of always defective relationships over supergames.

Endeffect behaviour[16] shows a further striking difference with "standard" experiments. Endeffect play arises nearly simultaneously with mutual cooperation while in standard experiments cooperation is learned before endeffect play is understood. In the present experiments endeffect play spreads fast, since one subject's understanding is sufficient to affect everybody else. Often a chain reaction is observed. Endeffect play by one partner

[16] Endeffect behaviour (play) is defined as follows:

Def. 1 *The play of a supergame is called* END-EFFECT PLAY *(Selten and Stoecker (1986)) if*

1. *both players choose the cooperative alternative in at least four consecutive periods* $k, \ldots, m$.
2. *In period* $m+1$ *for* $m < 10$ *at least one player chooses the non-cooperative alternative.*
3. *In all periods* $m+2$ *- if there are any - both players choose the noncooperative alternative*

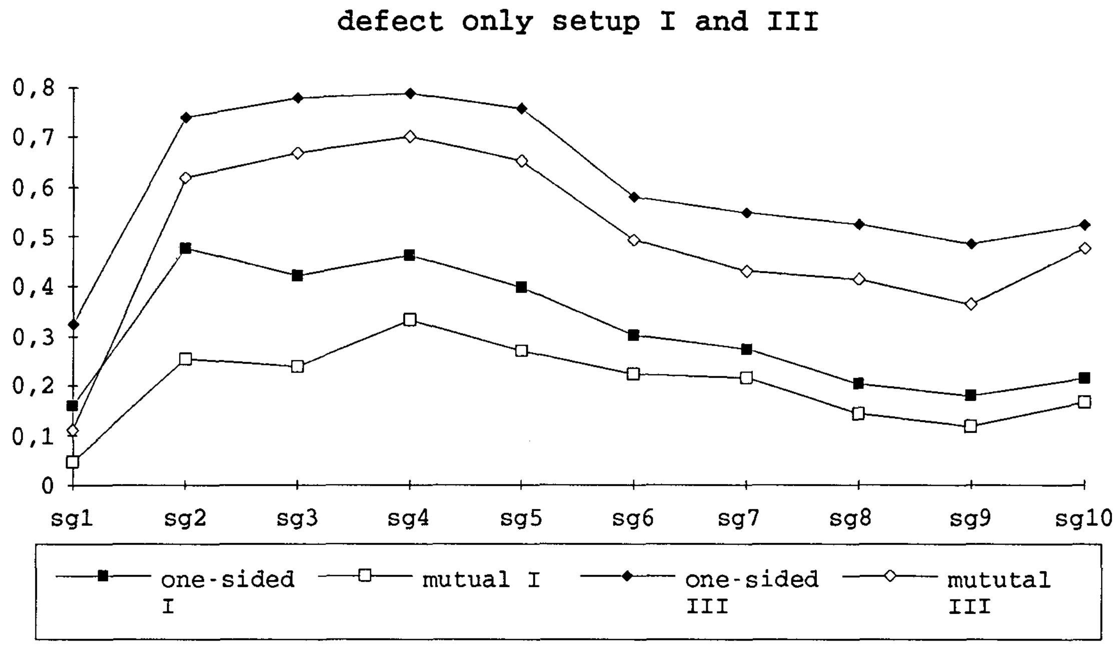

Figure 2:

provokes endeffect play with others. A detailed characterization of endeffect periods and the immediate shift of the endeffect to earlier periods can be found in appendix E.

The puzzle resolved

Discrimination pays in all three setups since it is a sophisticated way to experiment with reasonable available strategies which allows for quick learning. The finitely repeated prisoner's dilemma is characterized by its fundamental tension between the Pareto superiority of cooperation and the strict dominance of defection. Mutual cooperation is desirable but making a cooperative move is risky and might be costly. Mixing and divergence are used because they are a way to resolve this tension, to spread the risk involved in cooperation, and to search for the right balance between the two conflicting forces. Experimental subjects are not of a specific fixed player type but function like a classifier system, in which the same initial situation can trigger a variety of behavioural rules. The probability of each rule being triggered is updated via reinforcement learning: rules yielding a higher payoff become more likely over time[17] Mixing and divergence simply serve to

[17]Roth and Erev (1995a,b) have successfully simulated observed behaviour in several experiments on extensive form games and on repeated games whose strategy set can be restricted to the stage game actions using a simple model of reinforcement learning. Their model cannot be applied in the present context since our experimental subjects can use different stage game actions in the same supergame.

gather as much experience as possible within the same supergame in order to discover an optimal strategy. Experimental subjects learn in a sophisticated clever manner.

Observation 3 confirms that mixing and divergence have the same origin.

Observation 3 *A player who stops mixing is very likely to converge and vice versa.*

If there was no correlation, the number of subjects who neither mix nor converge, or mix and diverge, and the number of subjects who either mix and converge, or do not mix and diverge, should be more or less equal. The binomial test rejects this null hypothesis in favour of the hypothesis of correlation in all three experimental setups[18].

Why does discrimination not disappear over time? Should subjects not have discovered the optimal strategy? What is optimal changes with what subjects have learned. Two things have to be learned: (i) to reach cooperation (ii) at the minimal risk and lowest cost. While in the early periods of discrimination (first phase) subjects try to escape the trap of strict dominance and learn to cooperate, in the second phase they search for the profits from slight exploitation without risking the break-down of cooperation. If, however, cooperation breaks down, they have to learn to cooperate again and return to the first phase of discrimination. These different phases are reflected in the following observations.

1. $\frac{2}{7}$ of experimental subjects show sudden jumps from one type of converging behaviour to the other. These switches are mainly from always cooperate (AllC) to always defect (AllD). Sudden jumps from AllD to AllC occurred only if the subjects had jumped from AllC to AllD before.

2. 8 subjects smoothly switched from AllD to AllC convergence[19]. 6 of them had made a sudden jump from AllC to AllD convergence earlier on. A smooth change from AllC to AllD convergence never happened.

3. Falling cooperation levels are consistently observed in the last supergame in all experimental setups (see figure 1).

4. In setups I and III the number of relationships in which players use the subgame perfect equilibrium "always defect" rises in the final supergames. At the same

[18]For 21 observations the probability of observing at most as many subjects whose mixing and divergence behaviour is not correlated as were observed during the experiments are $P[k \leq 3] = 0.001$, $P[k \leq 4] = 0.004$ and $P[k \leq 6] = 0.039$ in the first, second and third experimental setup respectively.

[19]A smooth change means a switch in convergence behaviour via several supergames with divergence, moving each supergame more towards the opposite convergence behaviour. E.g. in supergame 1 DDDDDD, in supergame 2 DDCDCDD, in supergame 3 DDCCCCD and in supergame 4 CCCCCC.

time the number of cooperative relationships falls (or remains constant) in all three experimental setups (see Figure 2 and Appendix B3).

Switches from AllC to AllD divergence occur when cooperation is common. Subjects have learned that cooperation is commonly conceived as the best outcome and try to maximize periods of exploitation. They believe that defecting in the first period will not endanger the Pareto superior cooperative outcome. However, once everybody experiments with the possibility of slight exploitation cooperation becomes risky again and the risk is spread. People will only gradually return to cooperate, once they are confident that the others also do so.

To summarize: in the prisoner's dilemma there is no single optimal strategy. Moreover, what is optimal changes with what other agents have learned. Cooperation is neither reached easily nor easily preserved. Experimental subjects are not of a specific fixed type but permanently experiment with different strategies, i.e. they are aware of the changing environment and are always trying to learn. This has important implications on how to model economic agents: one lesson can be learned by these experiments. Economic agents have to be sophisticated; no model denying this sophistication by making economic agents unaware of their own consistent errors can be any good.

4 Results II: Partner Selection

This section examines the second issue of the paper: how does the freedom whether or not to play the prisoner's dilemma influence the outcome of the finitely repeated prisoner's dilemma?

Hypothesis 1 *Introducing the possibility of not playing some or all the periods of a finitely repeated prisoner's dilemma increases total cooperation levels.*

4.1 Baseline experiment compared to experimental setup II

Total cooperation levels[20] (see Appendix B2) were compared in three different ways:
(i) total cooperation levels of the single players were averaged.
(ii) total cooperation levels of the experimental sessions were averaged over sessions.
(iii) the overall total cooperation levels where calculated without distinguishing players or sessions[21].

[20] As before cooperation levels are conditional on entry.

[21] This corresponds to the cooperation rate as used in Sally (1995) who explains the influence of different factors on this dependent variable in his meta-analysis of experiments from 1958-1992.

For each of these levels the Wilcoxon-Mann-Whitney test and the robust rank order tests were performed. For the Wilcoxon test the probability p of the null hypothesis of equal cooperation levels in both experimental setups is reported while for the rank order test the significance level α with which the alternative hypothesis of higher cooperation levels in setup II than in the baseline experiment is accepted is displayed. The results are summarized in table III:

per player	p=0.0262 ($W_x = 79$)	U=-2.174 $\alpha = 0.025$
session	p=0.0116 ($W_x = 75$)	U=-2.611 $\alpha = 0.025$
total	p=0.0045 ($W_x = 71$)	U=-3.43 $\alpha = 0.01$

Table III

The probability of the Wilcoxon test corresponds to an acceptance of the alternative hypothesis at significance level $\alpha = 0.05$.

Hypothesis 2 *The percentage of mutual cooperative relationships out of active relationships is higher in setup II than in the baseline experiment. This is due to experimental subjects becoming more effective in excluding game partners.*

Def. 2 *A relationship is* **active** *if the individuals interact in at least 5 periods*[22] *during a supergame.*

Let the null hypothesis H_0 be that the number of active relationships is the same in the two experimental setups. The alternative hypothesis H_1 is that the number of active relationships is higher when the outside option is the subgame perfect equilibrium. The Wilcoxon-Mann-Whitney test attributes probability $p = 0.0093$ to H_0 against H_1 at significance level $\alpha = 0.05$. The robust rank order test also accepts H_1 at significance level $\alpha = 0.01$. To test the second part of hypothesis 2 we have to show

1. experimental subjects become more efficient in excluding defectors.
2. disregarding partner exclusion, the percentage of cooperative relationships in the second experimental setup lies below the baseline experiment after a certain supergame.

[22]5 periods are chosen because a cooperative relationship requires at least 4 periods of mutual cooperation. Moreover, 5 happens to be half of the periods of a supergame.

To show the first statement we calculate the Spearman rank order coefficient which relates the number of the supergame with the percentage of defective play that occurs in the supergame. Including all three experimental sessions the coefficient is $r = -0.766$. As required, it is negatively significant at $\alpha = 0.01$ (one-tailed test). The second statement is proven by graphical inspection. Figure 3 shows that it holds after supergame 4.

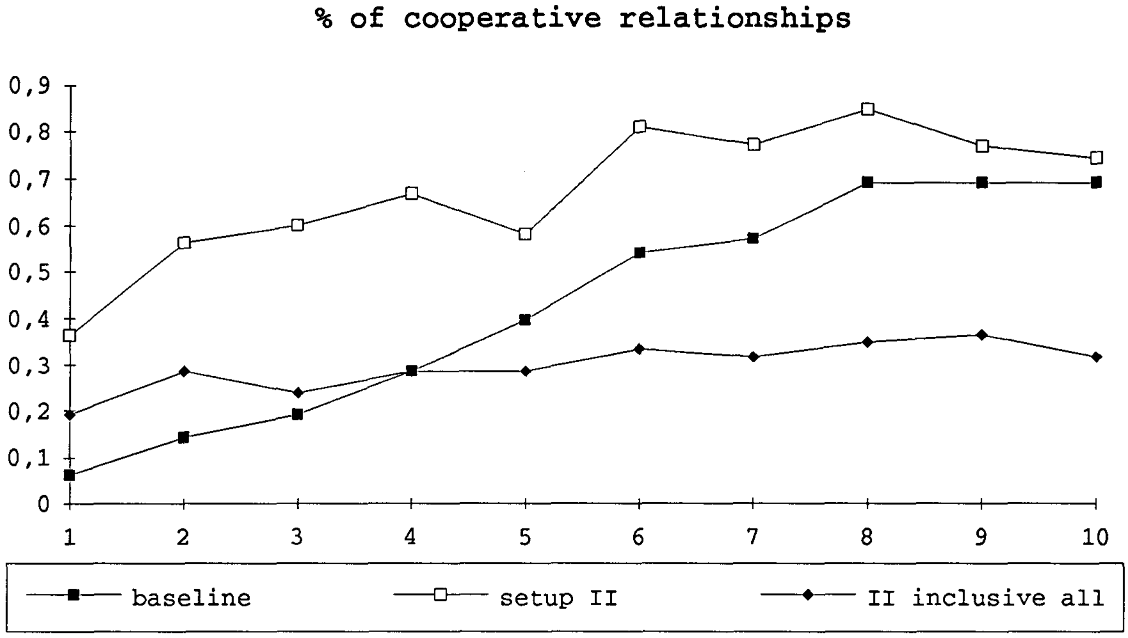

Figure 3:

4.2 Cooperation in experimental setup III

For reasons which will be explained in detail below and were already hinted at in Section 2.3, setup III cannot simply be compared to setup I. Nevertheless, treatment III is important since it introduces an outside option which is irrelevant for subgame perfect equilibrium play. The outside option could serve as an effective punishment for defective behaviour, but this punishment threat is non-credible since it involves inflicting harm on one-self and fails to be optimal in the moment of its implementation. Therefore, the possibility that players can choose whether or not to play the prisoner's dilemma seems of no importance since not playing is unattractive relative to mutual defection. However, it turns out to matter a lot. As will be seen below, the outside option is used. Furthermore subjects use it in order to enhance cooperation. Hypothesis 1 is also corroborated in setup III.

This subsection is organized as follows. First it will be explained why the baseline experiment cannot be compared directly to setup III. Secondly, evidence for the use of the outside option will be provided. Then it will be shown that its use is to increase cooperation. The latter is confirmed by showing similarities in cooperation patterns in setup II and III.

Why setup I and III are not comparable

A direct comparison with setup III would be misleading due to the difference in the payoff matrices: apart from the defect-defect payoff differing in magnitude, it differs in sign, which is the major problem. In both setups, mutual defection in all periods is the subgame perfect equilibrium; however, while playing the SPE leads to losses in setup I, subjects in setup III get a constant, small but secure reward. The incentive to risk cooperation is therefore reduced considerably. The following example illustrates why.

In the worst case a player who always defects gets ten times the defective payoff d. Imagine that in the case of mutual cooperation the endeffect would strike in period 9. Also suppose it were mutual[23]. How high has the probability p that a player responds to a cooperative first move in the next period to be for a risk neutral player to consider this cooperative first move to be worthwhile? The incentive constraint with $d = \mp 1$, $l = -6$ and $c = 5$ is as follows.

$$10d < l + p(7c + 2d) + (1 - p)(l + 8d)$$

which requires $p > \frac{10}{47}$ for the baseline experiment while $p > \frac{2}{5}$ is required in setup III. Clearly the latter probability is considerably higher (more than double). Thus the incentive to start cooperation is lower.

Moreover, former research has found considerable psychological differences in how gains and losses are conceived. Tversky and Kahneman (1991) show that people tend to have a loss-aversion, i.e. they dislike losses more than they like equal-sized gains. The authors also observe the phenomenon of *reflection*: people avoid risks that can yield gains but often seek risks to avoid equal-sized losses.

These results from individual choice find their game-theoretic cousin in different principles of loss avoidance. In its most general form, the principle states that people choose strategies that *might* result in gains and *expect* others to do the same. Cachon and Camerer (1996) find substantial evidence for the use of this principle in experiments on coordination games. For our setup the variance of loss avoidance which Cachon and Camerer

[23] These assumptions are made because they correspond to the break-down period of cooperation in the non-subgame-perfect Nash equilibrium of setup III.

(1996) refer to as *losing-equilibrium avoidance* is of interest: subjects avoid strategies with negative equilibrium payoff[24]. While in the baseline experiment, the sure loss of the subgame perfect equilibrium might be avoided by risking cooperation, in setup III a sure gain is put under risk once subjects move away from subgame perfection. Therefore, it is not surprising that lower cooperation levels are observed in setup III than in setup I. (see Figure 4)

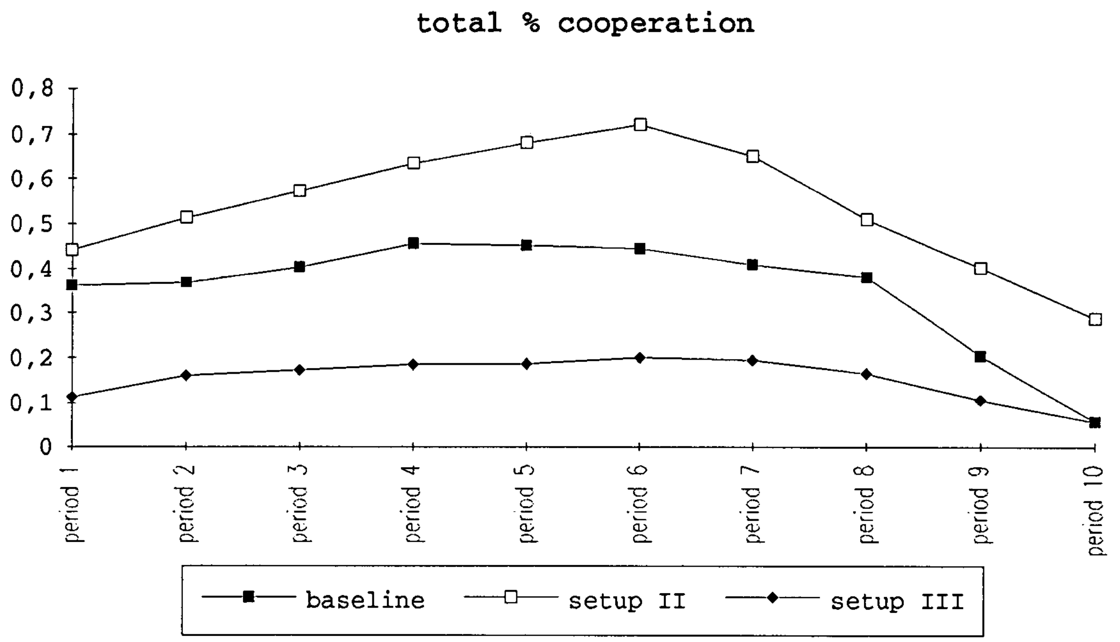

Figure 4:

The use of the outside option in setup III

The evidence below shows that the outside option does not turn out to be irrelevant because it is used and its use does not disappear and sometime even increases over time.

18 out of 21 subjects exited at some point during the experiment. Figures 5 and 6 show the percentage of all relationships per supergame, and per player in which some opting out occurred. In the latter case the overall experiment is contrasted with the last 5 supergames only. It can be seen that for $\frac{1}{3}$ of the subjects the number of relationships affected by the outside option did not diminish or even increased over time.

In the light of the above numbers the importance of the outside option cannot be denied given that its use is weakly dominated.

[24]Cachon and Camerer examine games with multiple equilibria whereas I use the principle also in situation with only one equilibrium.

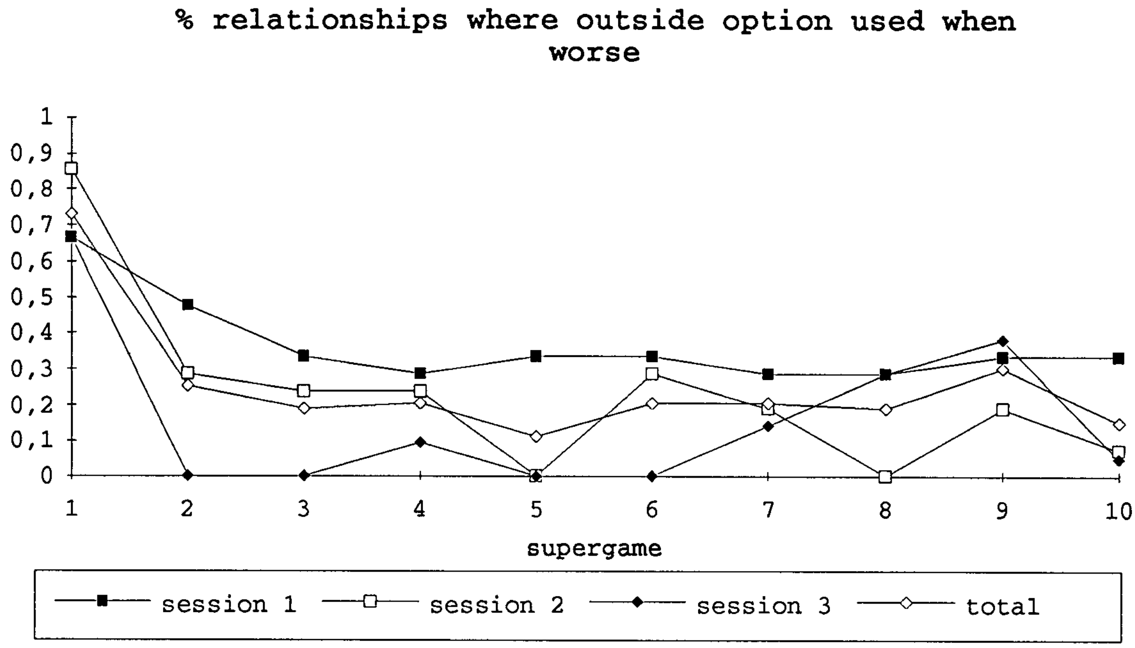

Figure 5:

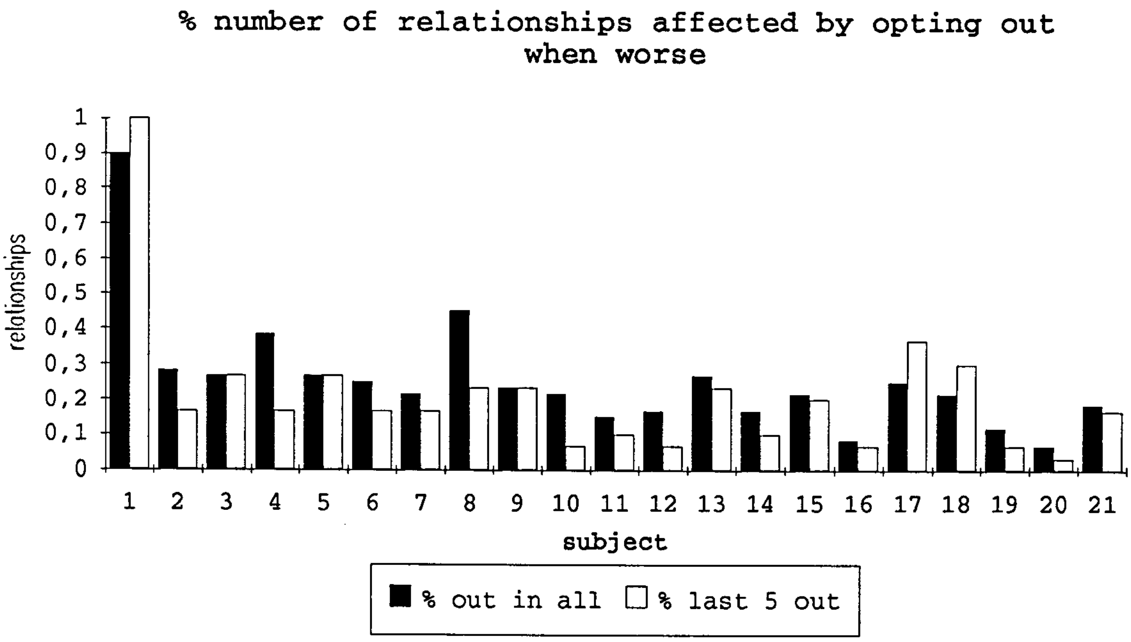

Figure 6:

Non-subgame perfect equilibrium play

The outside option is used systematically in order to sustain cooperation. Both non-subgame perfect equilibria mentioned in Section 2.3 occur. The use of the outside option serves two purposes:

1. *severe punishment for defection*
   As explained in Section 2.3. cooperation can be sustained till period 8 (inclusive) by threatening eternal punishment via exit. Since in equilibrium this threat is not implemented, it cannot be assessed directly how much cooperation arose due to its existence[25]. Its impact is seen indirectly when examining endeffect behaviour. If the above equilibrium is relevant, cooperation should not break down before period 9. If it does, it should give rise to self-inflicted punishment via exit. Indeed

   Observation 4 *The actual endeffect period of the third experimental setup lies above the one in the baseline treatment*[26].

   The Wilcoxon-Mann-Whitney test rejects the hypothesis H_0 of the same endeffect period in favour of the alternative hypothesis H_1 that the endeffect period of the third experimental setup lies above the baseline experiment. At a significance level $\alpha = 0.05$ the probability that H_0 is true is $p = 0.0217$ (for underlying data see Appendix E1).

   Moreover, 11 out of 41 cases of too early endeffect behaviour were punished by exiting[27].

2. *signal for cooperative intentions*
   The Nash equilibrium that allows for mutual cooperation is based on the threat of self-inflicted punishment, which is fairly weak. One way to increase the credibility of the threat is to opt out in the first period and to start to cooperate in the second. It is easily seen that opting out in the first period, followed by mutual cooperation until period 8 inclusive, and mutual defection afterwards, is a Nash equilibrium sustained by the now more credible background threat of quitting[28]. Opting out in early periods of a supergame serves as an equilibrium selection device. It reveals that the player in question is willing to use self-inflicted punishment. Moreover, opting out is a less costly signal for cooperative intentions than cooperation itself[29].

[25]exit after first period defection is common, but eternal exit is rare ($\frac{1}{21}$).

[26]In this case the comparision with the baseline experiment is justified since the two games have the same subgame perfect equilibrium.

[27]If we separate the different experimental sessions, it was used with frequency $\frac{1}{6}$ in session 1, $\frac{4}{5}$ in session 2 and $\frac{1}{5}$ in session 3. Averaging over the sessions yields a frequency of $\frac{7}{18}$.

[28]Formally, $n_c c + (9 - n_c)d \geq h + (n_c - 1)c$.

[29]Only subjects who are willing to risk a cooperative first move opted out in early periods.

7 out of 21 experimental subjects used and understood this signal. In the first experimental session, nearly all the cooperation arose out of this behaviour[30].

To sum up, the outside option is used, and the data reflects a positive effect of the existence of the outside option on cooperation levels.

Comparison with setup II

Since the use of the outside option mainly affects per period cooperation levels observation 5 further confirms hypothesis 1.

Observation 5 *Per period cooperation levels have the same distributional shape in setup II and III.*

In order to test this we rank the observations in both experimental setups according to the degree of cooperativeness. The Spearman rank order correlation coefficient accepts observation 5 at significance level $\alpha = 0.0005$ (r=0.9758). The cooperation patterns of setup II and III are identical.

The higher cooperation levels of setup II compared to setup III (see Figure 4) were expected before running the experiments and are easily explained. First of all setup II has the same prisoner's dilemma matrix than setup I and therefore higher incentives to move away from mutual defection. Secondly, in setup II the outside option is not irrelevant for subgame perfect equilibrium play. Therefore its impact is stronger. However, the results of setup III seem more interesting because they emphasize the importance of whether or not interactions are voluntary, since this turns out to matter even in the case when theory predicts its irrelevance.

The experiment clearly shows that voluntary interaction structures might be a key element in explaining observed cooperation levels in real data. The willingness to interact when there exists an option not to do so is based on the belief that the interaction is worthwhile. In setup II this can only be the case for both game partners if they cooperate. The way cooperation is reached confirms further the sophistication of experimental subjects; in particular the use of the outside option in setup III to signal cooperative intentions reveals how highly imaginative subjects are and that they continuously search for a better strategy.

[30] 100% cooperation was due to signalling by opting out from supergame 6 onwards.

4.3 Subgame perfect play in experimental setup II

This subsection examines a minor point which is of interest because it serves to illustrate the difference in subgame perfect play in setup II and III and it relates the present experiments to existing experiments with an exit option.

While the outside option in the setup III is weakly dominated, in setup II it is strictly dominant and constitutes the subgame perfect equilibrium path.

Observation 6 *The subgame perfect strategy of never entering a game is hardly ever observed. This stands in sharp contrast to the number of occurrences of the subgame perfect equilibrium of always defect in experimental setups I and III.*

Only one subject stopped playing completely[31]. Two more subjects played the subgame perfect equilibrium in some relationships in early supergames but stopped once they had learned the possibility of mutual cooperation. Compared to setup I and III (see figure 2) but also to one-shot experiments as conducted by Orbell *et al.* (1993) this percentage of subgame perfect equilibrium play is very low. Why does this result of the one-shot game not extend to the finitely repeated game?

Orbell *et al.* (1993) interpret their results in lines with the *false consensus literature.* They hypothesize that the choice whether or not to exist is determined by the subject's intention how to play the prisoner's dilemma. People are supposed to project their own intentions onto others and expect others to behave like themselves. Given the payoff matrix, an experimental subject exits if and only if he expects defective play. Subgame perfect play is mainly due to intending defectors. Cooperation levels increase because intending cooperators are observed to exit much less frequently than intending defectors.

In the multiple game situation subgame perfect play is rare for the following reasons: payoff incentives are such that it pays to enter for somebody who intends to defect always as long as he believes that at least one player will make a cooperative move. The section on divergence behaviour showed that purely defective intentions are exceptional. Moreover, although the experimental subjects could project their own probabilities of their different behavioural rules onto others, the present setup allows for an additional predictor concerning future play; given that it is known that the set of experimental subjects is the same in all supergames, each subject can built up some well-founded beliefs about the average behaviour of his potential game partners. This "reputation effect" ensures that the past discovery of one cooperator is sufficient to destroy future subgame perfect play.

[31]The subject explained that the only possible outcome in the game is mutual defection which is worse than staying out.

5 Not learning by not doing

We have argued that experimental subjects are very sophisticated. They discriminate in order to learn and constantly search for a better strategy. Past errors are corrected quickly. Experimental setup II presents an observation which seems to reveal the opposite: $\frac{1}{3}$ of the subjects fail to learn not to enter the last period of a supergame[32]. But strikingly, this observation does not carry over to the other experimental setups in which players learn not to reverse their play after the endeffect has occurred.[33] How can this failure to learn be reconciled with the postulated sophistication of experimental subjects?

Hypothesis 3 Not learning by not doing: *(Inconsequentiality of last period entry) Choosing to enter the game in the last period in setup II has a consequence only if both players do so. As only very few of the last period game entries actually lead to a match, experimental subjects are not aware of the negative consequences of their entering the game*[34].

Subjects who fail to learn not to enter in the last period of the supergame enter only sporadically. Rather than endangering our postulate of sophisticated behaviour, this confirms the hypothesis that experimental subjects work like classifier systems. Due to its sporadic nature last period entry has no consequences, therefore it is neither discouraged nor reinforced. Moreover, players who enter the game in the last period of later supergames normally do not play in preceding periods. This makes mutual last period entry unlikely, hence any rule recommending entry in the last period after periods of inactivity will be triggered with positive probability since it neither benefits nor harms the player.

In appendix F tables for each subject's behaviour in the last period can be found. The table lists how many times a subject entered the last period in each supergame, how many of these entries were justified[35], how many directly followed former exit periods and

[32]Notice that the only rational outcome of last period entry is mutual defection

[33]In the third experimental setup, only 4 subjects play once cooperatively after the endeffect has occurred. This implies that in general, players understand right from the beginning the consequences of the endeffect. In the baseline experiment, one experimental subject (subject 11) did not even learn endeffect behaviour. He only learned never to cooperate in period 10. 2 players never entered a cooperative relationships. 5 subjects never played cooperatively after the endeffect, 6 subjects only once in the first five supergames. The remaining 7 subjects tried to reverse the endeffect in some later supergame in which the endeffect had occurred relatively soon (period 4 to 8).

[34]A similar phenomenon is observed in experiments on second price auctions. The dominant strategy argument is not learned, since bidding above once own price does not incur any loss if one does not get the object (c.f. J. Kagel and D. Levin (1993)).

[35]Due to a cooperative relationship or a defect-cooperate result in period 9.

failed attempts to enter by the player, how many actually lead to a game, and the points resulting from this game.

Table IV summarizes the overall learning behaviour. It displays how many subjects failed to learn or learned only in the last supergames and the underlying reasons.

	no learning	delayed learning	learning
total	7	4	10
positive total points	2	1	
no game in 10	4	3	
no game in <10	6	3	
no explanation	1	0	

Table IV

The behaviour of all but one subject can indeed be explained by hypothesis 3. The failure to learn does not make experimental subjects less sophisticated since it only occurs when it has no consequence. In setup I and III subjects do learn the theoretical right move in the last period since the consequences are immediate.

6 Conclusion

The present paper has addressed two separated issues: (i) how good or harmful are certain behavioural assumption like the existence of player types and certain rules of thumb or learning schemes that allow agents to make eternal consistent errors? (ii) what is the importance of voluntary versus involuntary interaction structures in the finite prisoner's dilemma?

The experiments conducted to examine these issues allow us to draw the following conclusions: (i) player types do not exist. Experimental subjects turn out to be sophisticated in their use of different strategies and do not make consistent errors. (ii) partner selection enhances cooperation and thus might be a key element in explaining high cooperation levels in real data. For the analysis of the prisoner's dilemma this suggests that situations with and without partner selection have to be distinguished carefully and that any fixed type models in order to explain cooperation are not founded on human behaviour. Moreover, fixed type models might result in very wrong learning dynamics in the game. The implication for economic modelling in general is that any reasonable model has to recognize the sophistication of agents and allow them to avoid consistent errors. Recently, some authors have called for the need to impose certain "consistency" or "quasi-rationality" requirements on behavioural rules used by agents to avoid using learning schemes that cause agents to make the same mistakes forever (see e.g. Evans and Ramey (1994), Brock and Hommes (1995), Fudenberg and Levin (1995), Marcet and Nicolini (1995)). The present experiments confirm that such consistency conditions are an important ingredient in describing agents' behaviour in the laboratory.

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A Statistical tests used

To test the hypotheses 5 different tools from nonparametric statistics were used which shall be explained below. Siegel and Castellan (1988) served as the basis.

- *The binomial test*
  This test can only be used if the data can be seen as a binary population, i.e. falls into two discrete categories[36]. The null hypothesis is that the two categories occur with equal probability. The probability that the actual number of cases falling into one of the categories is at most as big as the observed number is calculated under this hypothesis. If this probability is less than a significance level α H_0 is rejected.

- *The Wilcoxon-Mann-Whitney test*
  This test serves to examine whether two independent samples have been drawn from the same population. It works as follows: let m be the number of observations from sample X and n be the number of observations from sample Y. The observations of both samples are combined and ranked according to algebraic size, starting from the lowest values. Then the sum of the ranks for each groups is calculated. If these sums lie to far apart, the hypothesis of equal distribution is rejected.

[36]My categories are confirming or not confirming to a certain behavioural pattern.

- *The robust rank order test*

  This test also examines whether the median of two independent samples is the same. Unlike the Wilcoxon-Mann-Whitney test it does not require the same underlying distribution. The test is performed as follows. The observations in the two samples are ordered from the smallest to the biggest. Then for each observation of the first sample X, the number of observations of the second sample Y with a lower rank are counted. It is called the placement of X and denoted by $U(YX_i)$. Similarly, the placement of Y is $U(XY_i)$. Then the mean (U(XY) and U(YX)) and the variability V_x and V_y of the mean of the placements is calculated with

$$V_x = \sum_{i=1}^{m}[U(YX_i) - U(YX)]^2$$
$$V_y = \sum_{j=1}^{n}[U(XY_i) - U(XY)]^2$$

  with m and n being the total number of observation from the first and second sample respectively. The test statistic is

$$U = \frac{mU(YX) - nU(XY)}{2\sqrt{V_x + V_y + U(XY)U(YX)}}$$

- *The Spearman rank order correlation coefficient*

  This coefficient reveals the association between two variables that are at least measurable on ordinal scales. It is based on the idea that the difference d_i between the ranks of the two variables indicates their disparity. If there are no ties in the data it is calculated as

$$r = 1 - \frac{6\sum_{i=1}^{N} d_i^2}{N^3 - N}$$

  where N is the number of observations of each variable. If there are ties in the data, each observation is assigned the average of the ranks that would have been assigned in the absence of ties. If there are many ties, the coefficient has to be corrected and is calculated as

$$r = \frac{\sum x^2 + \sum y^2 - \sum d^2}{2\sqrt{\sum x^2 \sum y^2}}$$

  where $x = X - \overline{X}$, i.e. the observed rank minus the mean. Similarly, $y = Y - \overline{Y}$.

B Cooperation

B.1 Percentage cooperation per supergame

setup	session	supergame 1	2	3	4	5	6	7	8	9	10
I	1	0.2214	0.2429	0.3167	0.3452	0.5214	0.6405	0.6357	0.6857	0.6619	0.6524
	2	0.2214	0.2119	0.2619	0.3	0.3310	0.4214	0.3129	0.3929	0.4238	0.3881
	3	0.2595	0.1429	0.1643	0.1976	0.2214	0.3905				
	average	0.2341	0.1192	0.2476	0.2810	0.3679	0.4841	0.475	0.5393	0.5429	0.5202
II	1	0.4444	0.5940	0.4658	0.5882	0.5234	0.5728	0.5921	0.6652	0.6598	0.6327
	2	0.1734	0.0945	0.18	0.1512	0.1512	0.5128	0.2255	0.5	0.3846	0.3810
	3	0.5405	0.8203	0.8532	0.7569	0.7820	0.7460	0.7232	0.7023	0.6277	0.5902
	average	0.3947	0.5707	0.5550	0.5760	0.5722	0.6169	0.5536	0.6236	0.5773	0.5484
III	1	0.1152	0.0783	0.0624	0.0634	0.1069	0.2087	0.2568	0.2630	0.2356	0.1709
	2	0.1070	0.0597	0.0719	0.0487	0.1143	0.1695	0.2611	0.3	0.2798	0.1105
	3	0.1053	0.0106	0.0732	0.0941	0.1589	0.4338	0.5082	0.5043	0.4533	0.4247
	average	0.1091	0.0495	0.0692	0.0687	0.1267	0.2707	0.3421	0.3558	0.3229	0.2354

Table V

B.2 Total cooperation levels per period

type	setup	period 1	2	3	4	5	6	7	8	9	10
total	I	0.3619	0.3690	0.4047	0.4569	0.4524	0.4460	0.4093	0.3810	0.2042	0.0586
	II	0.4428	0.5134	0.5730	0.6337	0.6804	0.7197	0.6505	0.5089	0.4033	0.2871
	III	0.1129	0.1616	0.1736	0.1860	0.1869	0.2002	0.1960	0.1661	0.1069	0.0573
per session	I	0.3619	0.3690	0.4047	0.4293	0.4275	0.4235	0.3917	0.3661	0.200	0.0629
	II	0.4013	0.4608	0.5157	0.5648	0.6052	0.6312	0.5720	0.4610	0.3561	0.2340
per player	I	0.3860	0.3935	0.4282	0.4293	0.4275	0.4235	0.3917	0.3661	0.2002	0.0629
	II	0.3999	0.4426	0.4936	0.5164	0.5455	0.5489	0.4905	0.4002	0.3259	0.2168
	III	0.1113	0.1624	0.1735	0.1843	0.1847	0.1993	0.1942	0.1661	0.1075	0.0595

Table VI

From table VI it can be seen that in the baseline treatment, total cooperation starts to decrease by period 4 while in setup II it only starts falling in period 6. Furthermore, cooperation levels increase less and fall more strongly in the baseline treatment.

B.3 Percentage of cooperative relationships

In experimental setup II, I distinguish between the percentage of cooperative relationships out of active ones referred to as II(active) and the percentage of cooperative relationships out of all possible partnerships.

setup	supergames 1	2	3	4	5	6	7	8	9	10
I	0.0635	0.1429	0.1905	0.2857	0.3968	0.5397	0.5714	0.6905	0.6905	0.6905
II(active)	0.3636	0.5625	0.6000	0.6667	0.5806	0.8077	0.7692	0.8462	0.7667	0.7407
II(all)	0.1905	0.2857	0.2381	0.2857	0.2857	0.3333	0.3175	0.3492	0.3651	0.3175
III	0	0.0317	0.0476	0.0794	0.1270	0.2698	0.3175	0.3333	0.2857	0.2540

Table VII

C Mixing and divergence behaviour

$\frac{1}{3}$ of the subjects do not converge while $\frac{8}{21}$ continue mixing. The exact numbers per setup are summarized in the following two tables. "No divergence" (no mixing) means that the subject never diverged (mixed) in any supergame. "Partial convergence" (partial stop) describes convergence (no mixing) over several supergames with a return to diverging (mixing) behaviour.

setup	no divergence	convergence	partial convergence	no convergence	total
I	5	10	2	4	21
II	3	9	0	9	21
III	2	13	0	6	21

Table VIII

setup	no mixing	stop mixing	partial stop	mixing	total
I	4	11	1	6	21
II	0	7	2	12	21
III	2	8	5	6	21

Table IX

The proportion of subjects that diverged (mixed) in each supergame are represented below[37].

[37]The different denominator is due to some missing observations in setup I from supergame 8 onwards due to a network breakdown during the last experimental session.

proportion of mixing subjects

setup	supergames 1	2	3	4	5	6	7	8	9	10
I	34/42	22/42	22/42	12/42	18/42	16/42	18/42	15/42	15/42	16/42
II	20/21	18/21	17/21	18/21	17/21	15/21	13/21	10/21	13/21	13/21
III	13/21	8/21	6/21	4/21	8/21	8/21	8/21	9/21	10/21	10/21

Table X

proportion of diverging subjects

setup	supergames 1	2	3	4	5	6	7	8	9	10
I	26/42	10/42	10/42	6/42	8/42	8/42	6/42	3/42	9/42	12/42
II	18/21	14/12	14/21	13/21	11/21	12/21	13/21	12/21	11/21	9/21
III	15/21	3/21	5/21	3/21	5/21	7/21	7/21	7/21	8/21	6/21

Table XI

Table XII displays how often divergence and mixing did not go hand in hand.

setup	convergence but mixing	stop mixing but divergence	total
I	3	0	3
II	3	1	4
III	3	3	6

Table XII

D Percentage of only defective play in experimental setup I and III

type	setup	supergames 1	2	3	4	5	6	7	8	9	10
one-sided	I	0.1587	0.4762	0.4206	0.4603	0.3968	0.3016	0.2738	0.2024	0.1786	0.2143
	III	0.3254	0.7381	0.7778	0.7857	0.7540	0.5794	0.5476	0.5238	0.4841	0.5238
mutual	I	0.0476	0.2540	0.2381	0.3333	0.2698	0.2222	0.2143	0.1429	0.1190	0.1667
	III	0.1111	0.6190	0.6667	0.6984	0.6508	0.4921	0.4286	0.4127	0.3651	0.4762

Table XIII

E Endeffect behaviour

E.1 Actual (observed) endeffect period

	supergames									
setup	1	2	3	4	5	6	7	8	9	10
I	9.25	8.7	9.3333	9.5294	8.8	9.2059	8.375	8.2222	7.75	7.8276
II	10.8182	10.25	9.8667	9.6667	9.1667	8,4286	8.05	7.7727	7.5417	7.15
III		9.5	10.3333	10.2	10	10	9.45	9.2174	7.9473	7.6471

Table XIV

E.2 Intended endeffect period

The intended endeffect period is the period in which an experimental subject intend to break up the cooperative relationship. This period cannot always be observed directly, but is easily inferable from the subjects' detailed statements and their overall endeffect behaviour. The intended endeffect period reveals when a subject considers it worthwhile to break up a cooperative relationship. Mixing behaviour in this period is very common[38]. A shift of the endeffect to earlier periods ought to be reflected in intended endeffect periods. The hypothesis that the endeffect period is negatively correlated with the number of the supergame is supported in all three experimental setups[39]. Table XV shows the mean and standard deviation of intended deviation period in end-effect plays for supergames and experimental sessions separately. It also shows the Spearman rank correlation coefficient between the mean[40] of the intended deviation period and the number of the supergame. The values in brackets refer to the one-tailed and two-tailed level of significance respectively.

[38] Tables on intended and observed endeffect periods of each subjects can be obtained from the author upon request.

[39] Selten and Stoecker (1986) get slightly higher correlation coefficients. However, they look for correlation from supergame 13 onwards while I start with the first supergame.

[40] If players had different endeffect periods with different partners they were averaged in the calculation of the mean.

experimental session	Supergames 1	2	3	4	5	6	7	8	9	10	Spearman rank correlation coefficient
baseline I											
mean	10.875	10.125	9.9	9.44	9.239	9.47	9.407	9.24	8.314	8.29	-0.903
standard dev.	0.217	1.023	0.49	0.46	0.25	0.75	0.628	0.603	0.757	0.683	(0.0005) (0.001)
baseline II											
mean	11	9.5	10.33	10	9.7	9.61	7.56	8.875	9.42	8.83	-0.794
standard dev.	0	1.47	0.47	0.63	0.871	0.45	2.307	1.078	0.607	1.344	(0.005) (0.01)
baseline III											
mean		10	9.75	9.77	9.45	9.55					-0.8
standard dev		1	0.829	0.696	1.487	0.748					(0.1) (0.2)
better I											
mean	10.6	10	10	9.3	9.4	8.3	7.96	8.17	8.13	7.3	-0.96
standard dev.	0.49	0.894	0.707	0.56	0.8	1.599	1.80	1.07	0.39	0.417	(0.0005) (0.001)
better II											
mean		11	10	10		10.33	9	9	8.67	7.67	-0.9157
standard dev.		0	1	0		0.47	0	0	0.47	1.88	(0.0025) (0.005)
better III											
mean	11	10.83	10.42	10.03	9.4	8.7	8.08	7.84	7.21	7.667	-0.994
standard dev.	0	0.37	0.449	0.58	0.447	0.5	0.61	1.32	0.74	0.408	(0.0005) (0.001)
worse I											
mean		9.17		10		9.5	9.5	9.5	9	8.5	-0.891
standard dev.		0.63		0		0.5	0.5	0.5	0	0.5	(0.01) (0.02)
worse II											
mean		11	11	11	11	10.8	11	10.8	10	9.67	-0.844
standard dev.		0	0	0	0	0.4	0	0.4	0.89	0.47	(0.0025) (0.005)
worse III											
mean			10.333	10.667	10.25	10.12	9.07	8.82	7.98	7.357	-0.976
standard dev.			0.4714	0.4714	0.433	0.33	0.678	0.437	0.846	0.58	(0.0005) (0.001)

Table XV

F Last period entries setup II

player	activity	sg1	sg2	sg3	sg4	sg5	sg6	sg7	sg8	sg9	sg10
1	yes	3	5	1	2	0	0	0	0	0	2
	justfied	1	2	1	2	0	0	0	0	0	0
	after no	1	2	0	0	0	0	0	0	0	0
	after yes-no	1	0	0	0	0	0	0	0	0	2
	games	1	3	1	2	0	0	0	0	0	0
	points	5	-2	-6	-2	0	0	0	0	0	0
2	yes	4	3	5	4	2	3	3	3	4	0
	justified	0	1	1	0	0	0	0	0	0	0
	after no	0	0	0	0	0	0	0	0	0	0
	after yes-no	2	2	2	3	2	3	3	3	4	0
	games	0	1	2	2	0	0	0	0	0	0
	points	0	-1	-2	-2	0	0	0	0	0	0
3	yes	1	2	0	1	0	0	0	0	0	0
	justified	1	2	0	1	0	0	0	0	0	0
4	yes	3	2	0	1	1	0	0	0	0	0
	justified	1	0	0	0	0	0	0	0	0	0
	after no	1	0	0	1	0	0	0	0	0	0
	after yes-no	1	4	0	0	0	0	0	0	0	0
	games	2	3	0	0	0	0	0	0	0	0
	points	-2	-3	0	0	0	0	0	0	0	0
5	yes	4	6	5	6	0	6	0	0	0	1
	justified	3	2	2	1	0	0	0	0	0	0
	after no	0	0	0	2	0	6	0	0	0	1
	after yes-no	0	0	1	0	0	0	0	0	0	0
	games	3	5	3	4	0	1	0	0	0	0
	points	4	-7	1	-6	0	7	0	0	0	0
6	yes	6	6	6	6	2	6	6	1	6	6
	justified	1	4	0	2	1	1	0	0	0	0
	after no	0	0	0	0	0	2	1	0	0	6
	after yes-no	3	2	2	1	1	1	1	0	6	0
	games	2	3	3	3	0	2	1	0	0	1
	points	4	5	-3	5	-7	-1	-1	0	0	-1
7	yes	4	0	4	6	2	3	1	3	1	1
	justified	2	0	1	0	0	0	0	0	0	0
	after no	0	0	1	0	0	0	0	3	1	1
	after yes-no	1	0	0	5	1	3	0	0	0	0
	games	3	0	2	2	0	1	1	0	0	1
	points	5	0	6	-2	0	-1	-1	0	0	-1
8	yes	2	1	2	2	2	1	1	2	2	2
	justified	0	0	0	0	0	0	0	0	0	0
	after no	1	1	1	2	1	0	1	0	1	1
	after yes-no	1	0	1	0	1	1	0	2	1	1
	games	1	0	0	0	0	1	0	0	0	1
	points	-1	0	0	0	0	-1	0	0	0	-1
9	yes	4	3	3	2	1	2	0	0	0	6
	justified	0	1	0	1	1	1	0	0	0	0
	after no	0	0	0	1	0	0	0	0	0	4
	after yes-no	1	1	0	0	0	1	0	0	0	1
	games	2	2	1	2	1	1	0	0	0	2
	points	-2	4	-1	-2	-1	-1	0	0	0	-2
10	yes	0	2	3	2	1	2	0	0	0	0
	justified	0	1	0	1	0	2	0	0	0	0
	after no	0	1	0	0	0	0	0	0	0	0
	after yes-no	0	0	2	2	0	0	0	0	0	0
	games	0	2	1	2	1	2	0	0	0	0
	points	0	5	-1	-2	-1	6	0	0	0	0

player	activity	sg1	sg2	sg3	sg4	sg5	sg6	sg7	sg8	sg9	sg10
11	yes	0	0	0	0	0	0	0	0	0	0
12	yes	6	3	1	3	1	1	0	2	2	1
	justified	0	0	0	0	0	0	0	0	0	0
	after no	4	0	0	1	0	1	0	1	1	0
	after yes-no	1	1	1	2	1	0	0	1	1	1
	games	2	1	0	1	0	1	0	0	0	0
	points	-2	-1	0	-1	0	-1	0	0	0	0
13	yes	3	2	1	6	5	2	0	1	1	3
	justified	0	0	1	0	0	1	0	0	0	0
	after no	0	1	0	2	1	1	0	0	0	0
	after yes-no	2	0	0	4	3	0	0	1	1	3
	games	1	0	0	2	1	1	0	0	0	0
	points	-1	0	0	-2	-1	-6	0	0	0	0
14	yes	3	3	2	6	5	6	6	6	6	6
	justified	0	0	0	0	0	0	0	0	0	0
	after no	0	2	1	4	3	0	2	4	0	0
	after yes-no	3	0	1	2	1	6	2	2	5	5
	games	0	1	0	2	1	0	0	0	0	1
	points	0	-1	0	-2	-1	0	0	0	0	-1
15	yes	6	4	4	0	4	3	0	6	6	0
	justified	1	4	4	0	1	2	0	0	0	0
	after no	0	0	0	0	0	1	0	0	0	0
	after yes-no	4	0	0	0	0	0	0	3	4	0
	games	2	4	4	0	2	2	0	1	2	0
	points	4	28	4	0	-2	-2	0	-1	-2	0
16	yes	5	6	4	4	6	0	0	0	0	0
	justified	5	5	4	3	3	0	0	0	0	0
	after no	0	0	0	0	1	0	0	0	0	0
	after yes-no	0	0	0	1	1	0	0	0	0	0
	games	4	5	4	2	3	0	0	0	0	0
	points	20	14	12	6	-3	0	0	0	0	0
17	yes	3	4	4	4	3	3	0	0	0	0
	justified	3	4	4	4	2	0	0	0	0	0
	after no	0	0	0	0	1	1	0	0	0	0
	after yes-no	0	0	0	0	0	1	0	0	0	0
	games	3	4	4	3	2	2	0	0	0	0
	points	15	9	8	3	-2	6	0	0	0	0
18	yes	1	3	1	0	1	0	1	3	1	0
	after no	0	0	1	0	0	0	1	3	1	0
	after yes-no	0	3	0	0	1	0	0	0	0	0
	games	1	1	0	0	1	0	1	1	1	0
	points	-1	-1	0	0	-1	0	-1	-1	-1	0
19	yes	3	3	2	5	0	0	0	0	0	0
	justified	3	2	2	4	0	0	0	0	0	0
	after no	0	1	0	0	0	0	0	0	0	0
	after yes-no	0	0	0	1	0	0	0	0	0	0
	games	3	3	2	3	0	0	0	0	0	0
	points	15	9	-1	-7	0	0	0	0	0	0
20	yes	6	6	3	1	2	6	2	6	6	6
	justified	1	2	3	0	2	0	0	0	0	0
	after no	0	0	0	1	0	4	0	0	0	0
	after yes-no	3	3	0	0	0	1	2	5	5	5
	games	1	2	3	0	0	3	1	2	1	0
	points	-1	-1	-3	0	0	-8	-1	-2	-1	0
21	yes	3	3	6	3	4	3	0	1	1	0
	justified	3	3	3	3	3	0	0	0	0	0
	after no	0	0	0	0	0	0	0	0	1	0
	after yes-no	0	0	3	0	0	1	0	1	0	0
	games	3	3	2	3	3	1	0	0	0	0
	points	15	4	-7	6	-3	-1	0	0	0	0

Table XVI

G Instrucciones

G.1 Introducción

Éste es un experimento de toma de decisiones. Las instrucciones son simples y, si las sigues y las aplicas con atención, puedes ganar unas pesetas. Te damos 1000 pesetas para empezar. Conseguirás aumentar o disminuir esta suma en función de tus decisiones a lo largo del experimento. Si pierdes más de esta suma inicial, no ganarás nada.....y, por supuesto, no nos tendrás que pagar nada.

G.2 Contenido de una sesión

- Sesión
  - una sesión está compuesta por diez períodos
  - hay siete participantes en cada sesión
  - durante la sesión cada participante está identificado por un número, del 1 al 7, que será elegido al azar
  - este número será tu identificación durante los diez períodos de la sesión

- Decisiones
  En cada período tienes que tomar dos decisiones

  1. La primera decisión es la de *elegir a las personas* con las cuales quieres emparejarte en el experimento. Puedes elegir desde ninguna hasta un máximo de seis personas. Cada participante tomará esta decisión simultáneamente.

     Cuando todo el mundo haya tomado esta decisión, se te informará de quienes van a ser tus parejas. Una pareja se forma cuando dos participantes se eligen mutuamente. Por lo tanto, puedes tener de cero a seis parejas.

  2. Tu segunda decisión será la de *elegir una acción* para cada pareja que tengas.
     - en cada caso puedes escoger entre dos acciones diferentes a y b, al igual que tu pareja.
     - tú y tu pareja teneis que decidir simultáneamente qué acción quereis tomar sin saber la decisión del otro.
     - como sólo podeis escoger entre a y b, hay cuatro combinaciones de acciones posibles. La tabla siguiente describe los puntos que consigues en cada una de ellas.

tu decisión	la decisión de tu pareja	tus puntos	los puntos de tu pareja
a	*a*	1	1
a	*b*	7	-6
b	*a*	-6	7
b	*b*	5	5

La tabla se interpreta como sigue:

* cuando eliges la acción *a* y tu pareja elige la acción *a*, tú consigues 1 punto y tu pareja consigue 1 punto
* cuando eliges la acción *a* y tu pareja elige la acción *b*, tú consigues 7 puntos y tu pareja consigue -6 puntos
* cuando eliges la acción *b* y tu pareja elige la acción *a*, tú consigues -6 puntos y tu pareja consigue 7 puntos
* cuando eliges la acción *b* y tu pareja elige la acción *b*, tú consigues 5 puntos y tu pareja consigue 5 puntos
* si no estás emparejado con un participante, consigues 0 puntos y este participante consigue 0 puntos

G.3 Repetición de la sesión

Tal como hemos dicho, durante una sesión (10 períodos) cada participante está identificado por un número. Durante estos 10 períodos tienes que tomar las decisiones descritas anteriormente. En cuanto hayas terminado los 10 períodos, te será asignado un nuevo número de identificación, elegido al azar, que estará vigente durante una *segunda* sesión de diez períodos, durante la cual tienes que repetir el procedimiento descrito arriba. En total habrá *diez* sesiónes de 10 períodos con un nuevo número de identificación en cada sesión.

G.4 Ganancias

Las 10 sesiones se dividen en dos grupos de 5 sesiones cada uno. Las ganancias que recibirás se calcularán como sigue:

1. Se eligirán aleatoriamente 2 sesiones, una perteneciente a cada uno de los grupos antes mencionados. La probabilidad de seleccionar una sesión, dado el grupo a que pertence, es igual a $\frac{1}{5}$. Te conviene, por lo tanto, jugar bien en todas y cada una de las sesiones porque 2 de ellas serán elegidas para calcular tus ganancias finales.

2. Los puntos obtenidos en las sesiones elegidas se convertirán en pesetas. Un punto equivale a 15 pesetas; de tal modo que, por ejemplo, 50 puntos equivalen a 750 pesetas.

3. Tus ganancias finales serán las ganancias/perdidas obtenidas mas la cantidad fija por participar (1000 pesetas).

G.5 El uso del ordenador

G.5.1 Las dos decisiones

1. *elegir a tu pareja*
   Para tu primera decisión el ordenador proyectará una pantalla pidiéndote que indiques con quién quieres emparejarte. Te comunicará también tu número de identificación, el período en el cual estás y el total de los puntos que has conseguido en la sesión hasta ese momento. Tienes que contestar utilizando y (yes) para elegir una pareja y n (no) para rechazarla. Un ejemplo de pantalla:

	1	2	3	4	5	6	7
oferta	x						

   La x debajo de 1 significa que eres participante 1. Como no puedes emparejarte contigo mismo, no te deja espacio debajo del 1. En la fila $oferta$ tienes que responder si quieres emparejarte o no con cada participante. Para ello tienes que saltar al campo que quieres modificar utilizando las flechas de los cursores. Una vez que estés en el campo, pulsa ENTER, escribe tu respuesta - y o n - y pulsa ENTER de nuevo. Date cuenta de que tienes que pulsar ENTER dos veces.

   Cuando hayas tomado todas las decisiones, pulsa F10 para comunicarlas. Una vez pulsado F10, tus decisiones son irreversibles. Así pues, asegúrate de que has escrito lo que querías escribir antes de pulsar F10.

2. *elegir tu acción*
   Cada vez que tienes que elegir una acción, el ordenador proyectará la tabla de puntos que las diferentes combinaciones de acciones reportan. Te comunicará también tus parejas, utilizando la estructura de la tabla siguiente

	1	2	3	4	5	6	7
pareja	x						
acción	x						

La x en la fila *pareja* significa que eres participante 1 en el ejemplo. La línea *pareja* indica las parejas que hayan resultado de tu primera decisión. Si hay una n debajo de un número, esto significa que **no** estás emparejado con ese participante. Si hay una y tienes entonces que llenar el campo vacío en la fila *acción*, elegiendo entre a y b. La tabla siguiente muestra todas las combinaciones posibles que pueden ocurrir.

	1	2	3
pareja	x	n	y
acción	x	x	

Observa que sólo en el último caso tienes que tomar una decisión. Para escribir una decisión, salta al campo que quieres modificar utilizando las flechas, pulsa ENTER, escribe tu decisión y pulsa ENTER de nuevo. Acuérdate que puedes elegir acciones diferentes con diferentes parejas. Cuando hayas tomado todas las decisiones pulsa F10 para confirmar y continuar.

¡Atención! En el caso de que no tengas ninguna pareja, tienes también que pulsar F10. No lo olvides o harás esperar a todos inútilmente.

G.5.2 Historia

Después de cada período el ordenador te dirá los resultados del período anterior y te mostrará la historia del experimento. La siguiente tabla te muestra los resultados posibles que pueden aparecer en la historia y cómo interpretarlos.

per		1	2	3	4	5	6	7
1	o\a	$x\ x\ x$	$n\ x\ x$	$y\ x\ x$	$y\ a\ a$	$y\ a\ b$	$y\ b\ a$	$y\ b\ b$
	pun	0 0	0 0	0 0	1 1	7 -6	-6 7	5 5

per significa período, *o* oferta, *a* la acción elegida y *pun* los puntos recibidos. Eres participante 1. Detrás de $o\backslash a$ el ordenador te dice si has hecho una oferta($y\backslash n$), la acción que tomaste y la acción que tomó tu pareja. x indica que no estás emparejado con este participante. *pun* te comunica los puntos que tú y tu pareja habeis recibido. Los puntos son 0, si no estais emparejados.

En los primeros tres casos no has tenido pareja: en el primer caso porque no puedes emparejarte contigo mismo, en el segundo porque tú no querías la pareja y en el tercero porque el otro no quería ser tu pareja.

En los cuatro casos que quedan tienes pareja y, por ello, aparecen tus acciones, las acciones de tu pareja y los puntos conseguidos en la tabla.

Puedes consultar la historia cada vez que tengas que tomar una decisión o que estés esperando a que los demás tomen sus decisiones. Sólo tienes que pulsar las teclas correspondientes. F1 te mostrará la historia de la primera sesión, F2 la de la segunda, F3 la de la tercera, F4 la de la cuarta, F5 la de la quinta, F6 la de la sexta, F7 la de la séptima, F8 la de la octava, F9 la de la novena y q la de la décima.

Si estás consultando la historia no podrás darte cuenta de cuándo terminan los demás. Por ello te pedimos que no dejes la historia abierta si no estás consultándola. Date cuenta de que si quieres consultar la historia cuando estás esperando, el ordenador reacciona lentamente. Después de haber pulsado F1 o F2 o F3 o F4 o F5 o F6 o F7 o F8 o F9 o q, tardará unos segundos antes de mostrarte la historia.

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