Cooperative Behavior and the Frequency of Social Interaction*

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Abstract: We report results from an experiment that examines play in an indefinitely repeated, 2-player Prisoner's Dilemma game. Each experimental session involves N subjects and a sequence of indefinitely repeated games. The main treatment consists of whether agents are matched in fixed pairings or matched randomly in each indefinitely repeated game. Within the random matching treatment, we vary the information that players have about their opponents. Contrary to a theoretical possibility suggested by Kandori (1992), a cooperative norm does not emerge in the treatments where players are matched randomly. On the other hand, in the fixed pairings treatment, the evidence suggests that a cooperative norm does emerge as players gain more experience.

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"Sometimes cooperation emerges where it is least expected" -Robert Axelrod, *The Evolution of Cooperation*

1. Introduction

Experimenters know that as subjects acquire experience with a game of strategy their choice of action often changes. Sometimes it is this learning behavior itself that is of direct interest. In other experiments, the experimenter is most interested in how subjects who are experienced with a given game play that game. In either case, it is necessary to give subjects repeated opportunities to play the same game. Of course, multiple iterations of a given game can create a 'supergame' with a much larger set of strategies than the stage game from which it is constructed. As Ellison (1994) notes, "in experimental economics it is a well-recognized concern that subjects who are asked to play a game several times may treat the situation as a repeated game. To avoid repeated game effects it is common practice to randomly match the players in an anonymous setting so that pairs of players do not meet repeatedly."[1]

Does random matching work to control supergame effects? The work of Kandori (1992) and Ellison(1994) has shown that there is no theoretical reason to believe such procedures will be sufficient to prevent the development of a supergame equilibrium that is different from the equilibrium of the stage game from which it is constructed. The central point of their work is that even if specific individuals in a population meet and play a stage game infrequently, their actions when they meet become part of their histories upon which future actions will be based. If the group is finite, these future actions directly or indirectly affect the behavior of individuals who will eventually be matched. Therefore, each agent must be concerned about the possibility of setting off a contagious reaction to his current action that will have long term consequences

[1] Managers may also be interested in preventing people from playing a supergame strategy in environments where each player plays the same game repeatedly Ickes and Samuelson (1987) suggest that the performance incentive of a target reward system could be enhanced by periodic, lateral job transfers. Such transfers would be analogous to changing the membership of the group to which an individual=s performance is being compared in a relative pay scheme. The objective of the lateral transfers is to break the incentives to develop cooperative strategies for playing the iterated game.

for himself, *even if no information about any individual's past actions is ever specifically identified with that individual*. Under some parametric conditions, this possibility of a contagious reaction can support a cooperative equilibrium in the iterated play of a Prisoner's dilemma. Of course, everyone playing non-cooperatively is also equilibrium of the repeated Prisoner's dilemma game. Therefore, as Ellison notes, "…(given the) moderate population sizes (in an experimental session) random matching may not solve the problem." We cannot be confident that such schemes do what they are supposed to do without direct empirical evidence comparing the repeated play of a game under different matching protocols. It is in response to this challenge that the experiment discussed below is designed.

2. Related Work

2.1 Finitely Repeated Stage Games

There is considerable evidence that when a finitely repeated number of plays of a Prisoners' dilemma stage game are done with fixed pairings the game is treated by subjects as a supergame. In many experiments using this setup, cooperative play is often observed in the initial stage games. Presumably, this reflects a belief that if the supergame is of sufficient length reciprocal acts of cooperation will occur with sufficient frequency to make it profitable to refrain from taking an opportunistic, non-cooperative action too soon. Of course, in the final stage game a belief that cooperation will be reciprocated cannot be credible. Consequently, if it is a belief in reciprocity that induces cooperation in these games, it is not surprising that the levels of cooperation observed drop dramatically as the end of these games draw near. (See, e.g., Selten and Stoecker (1986), Andreoni and Miller (1993), Cooper, et al. (1996) and Meyer and Roth (2003)).[2]

Cooper et al. (1996) introduced a matching protocol, called the 'turnpike protocol' to allow subjects to gain experience with playing a stage game in a way that makes it impossible for

[2] Kreps and Wilson (1992) provide a theory of reputation building that accounts for the initial belief that cooperation will be reciprocated by positing that there is some commonly known fraction of the population that is altruistic and will always play cooperatively. That theory predicts that the likelihood of the first non-cooperative play is an increasing function of the number of stage games already played, and that once a non-cooperative act is observed, it is no longer credible that the individual who defected will cooperate in the future. The behavior observed is consistent with the qualitative implications of the Kreps-Wilson model, but as Cooper, et al, show, at the individual level of observation, only a very small percentage of individuals behave as the theory predicts.

any subject's current choices to have any influence, either directly or indirectly, on the information set of any other player with whom s/he will be matched in subsequent rounds. With this matching protocol, a subject cannot develop a reputation for cooperation. Using this protocol with a prisoner's dilemma game, they observed that the modal behavior of individual subjects was to always 'defect'. Nevertheless, cooperation rates averaged 38 percent in the first ten periods, declined with experience, but remained above 20% in the last ten periods. The decline in cooperative play observed as subjects gained experience suggests that with enough experience the difference in cooperative play observed between these two matching protocols would get even larger. However, since N subjects can be matched only N/2 times under the turnpike protocol, there are practical limits to the number of rounds of experience that an experimenter can generate from any given subject.

With random anonymous matching, the size of the subject pool and the number of subjects in a given session place no limit to the number of times a given subject can be observed playing a game. This makes an anonymous random matching protocol attractive for studying how a play of a particular game evolves as players gain experience, *assuming that the protocol does not itself create a supergame.* The evidence of the effect of random pairings on cooperation in a game composed of a finitely repeated sequence of a stage game in which the dominant strategy is non-cooperation is mixed. Andreoni and Croson (2002) provide a summary of studies comparing cooperative play in fixed pairings ("partners" treatment) versus random re-matching ("strangers" treatment) in linear public goods experiments. They report that out of thirteen such studies, four find more cooperation in the strangers treatment, five find more cooperation in the partners treatment and four fail to find any difference. The lack of systematic difference in behavior under different matching protocols suggests that reputation effects do not play a large role in accounting for cooperative play under the fixed pairings treatment in these linear public goods games.[3]

[3] In a linear public goods game the marginal value to an individual of a unit s/he contributes to a public good may be composed of two parts: (1) the payoff that is controlled by the experimenter and (2) an uncontrolled subjective 'warm glow' component that an individual may secure from acting in the interest of the group. As long as these two components are additive, the optimal strategy for an individual is to contribute all, or none of his/her endowment to the public good. Palfrey and Prisbrey (1996) use a game in which the opportunity cost to each individual of contributing to a public good is drawn randomly and independently before each stage game from a fixed distribution. This allows them to estimate each individual's cut-point strategy for contribution from the individual's

2.2 Indefinitely Repeated Stage Games

By definition, indefinitely repeated stage games have no (predictable) last stage. Therefore, if cooperative play is reciprocated in the early stages, a belief in future reciprocity will be reinforced and cooperative behavior may be sustained indefinitely. The concept of an indefinitely repeated stage game is implemented experimentally by use of a randomization device to determine after each stage game is played whether the game has ended or another stage game is to be played. The probability of continuation determines whether or not there exists a cooperative equilibrium in the supergame. This device was introduced by Roth and Murnighan (1978)[4]. There are surprisingly few experiments that have been conducted with indefinitely repeated stage games.[5] To our knowledge, the only other experiment with an indefinitely repeated game played under different matching protocols is reported in Palfrey and Rosenthal (1994). They conducted an experiment using an indefinitely repeated N-person provision point

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contribution history. The distribution of estimated cut-point strategies is similar across matching protocols. However, the deviation between the actual and predicted contributions of individuals is larger when subjects are randomly re-matched after each stage game than if they remain in fixed pairs for a finite sequence of plays of the stage game. As Andreoni and Croson observe, the greater role of unsystematic individual behavior in the random matching protocol may itself account for the variation across studies in the levels of contributions made under different matching.

[4] In their experiments subjects played an indefinitely repeated Prisoners' Dilemma stage game against a pre-programmed strategy, either tit-for tat, or grim response. They were interested in testing how responsive cooperative play was to variations in the continuation probability. While they found the rate of cooperative play to be positively related to the continuation probability, the levels of cooperation they observed were quite far from 100%.

5 Dal Bo (2002) considers whether the responsiveness of cooperation to an increase in the continuation probability observed by Roth and Murnighan is simply a reflection of the increase in the expected number of repetitions of the stage game before a relationship is terminated, or whether behavior in games of indefinite length is fundamentally different from behavior in games of finite repetitions of a prisoners' dilemma stage game. He finds that the percentage of cooperative play in finitely repeated games of a given length is lower than indefinitely repeated games of the same expected length.

Van Huyck et al. (2002) report an experiment conducted with supergames constructed of an indefinite sequence of repetitions of dominance solvable stage games, followed by a small fixed number of repetitions of the same stage game. They observed that during the probabilistic continuation phase of a supergame whose stage game has an equilibrium in strictly dominant strategies, the cooperation level rose dramatically with experience. They also found that when subjects played only this stage game, with random matching after each play for a finite sequence of plays, the non-cooperative equilibrium action was chosen with a very high frequency right from the outset.

Aoyagi and Frechette (2003) report an experiment conducted with supergames constructed from an indefinite sequence of an oligopoly game with noisy public signals.

voluntary contribution game. This game was played under both fixed and anonymous random matching protocols. Unlike a Prisoners' Dilemma, non-cooperation is not a dominant strategy in their stage game and the stage game has a multiplicity of cooperative equilibria. In the parameterization Palfrey and Rosenthal use, the Bayesian equilibrium involves the use of a "cut-point" strategy. The equilibrium cut-point strategy is different in the indefinitely repeated game than in the one-stage game and implies higher contributions in the repeated game than in the one-shot game. They found that under random matching, subjects "adhere to cut-point decision rules that are, on average, very close to those predicted by the Bayesian equilibrium (of the one-shot game). Repetition (i.e., fixed matching) leads to more cooperative behavior (than observed with random matching)...(but) the observed magnitudes of improvement are much smaller than predicted (assuming that random matching corresponds to the play of a one-shot game)." The fact that subjects did not come close to fully exploiting the opportunities for coordination and cooperation under either the fixed or random matching protocols in the Palfrey-Rosenthal experiment is, perhaps, not surprising. Their stage game is a game of incomplete information. The symmetric cut-point strategy that maximizes expected joint profits is not transparent. That game has a multiplicity of equilibria in non-symmetric pure strategies and there is no evidence of any effort to coordinate on a pure strategy equilibrium.

2.3 Information

The Kandori theorem implies that a cooperative norm can develop even in the absence of any information being transmitted about one player's past actions or experience to the other player with whom he is currently paired. Nevertheless, one might expect players to act differently if such information is transmitted than if it is not. Bolton, Katok and Ockenfels (2001) report an experiment with a finitely repeated stage in which each player may carry with him an *image score* that reflects some information about the past experience of that player. This information, but not the identity of the player, is observable to the other person with whom s/he is matched in the current stage game. After the game is played each individual is paired with someone s/he has not been paired with previously and the stage game is played again. In each stage game individuals are paired, and once paired a random draw determines the choice of dictator. The dictator can either ‘Give’ the receiver a large payoff and receive no payoff himself, or ‘Take’ a small payoff himself and give the receiver nothing. Because of the matching protocol, a person who ‘Gives’ cannot have that action reciprocated by the receiver of his generosity. They find that when the opportunity cost of ‘Give’ is small subjects will be ‘giving’ even when there is no image score. However, when the opportunity cost of being nice is high, ‘giving’ is much lower when there is no image score than when there is an image score. Because their stage game is finitely repeated, Kandori’s theorem does not directly apply. Indeed, in their game the only sequentially perfect equilibrium is to ‘Take’. This experiment suggests that information transmission may make a difference in the play of a game under a random matching protocol.

Another related experiment was conducted by Schwartz, Young and Zvinakis (2000). They use a modified Prisoners’ Dilemma as the stage game in their experimental design. The modification involves the addition of a strategy “Refuse”. If either player chooses “Refuse” at the beginning of the stage game then the stage game ends and both players get the same payoff that they would have gotten if neither chose refuse and both chose “Defect”. This stage game is played for an indefinite number of times. Before the beginning of each stage game there is a random pairing of subjects. Subjects remain anonymous. However, under different treatments different portions of a player’s past history is revealed to the person with whom s/he is currently matched. In this experiment, subjects do acquire experience, playing seven supergames of indefinite length under fixed information disclosure conditions. They find that disclosure condition has a large effect on the initial levels of cooperation observed. Under either information condition, they observe a decline in cooperation as subjects gain experience.

Like Bolton, et al., the results of Schwartz and his associates indicate that among inexperienced subjects, in environments where cooperation can only be reciprocated indirectly, information transmission can have significant effects on behavior. What is left open is whether these effects can sustain a cooperative equilibrium in a supergame composed of repetitions of a stage game in which non-cooperation is a dominant strategy. Conversely, it is still unknown whether anonymous random matching with no information transmission about one’s current partner’s history will reliably produce a non-cooperative equilibrium.

3. The Experiment

3.1 Experimental Design

Kandori's (1992) theorem applies to indefinitely repeated games of local interaction with minimal observation of the past actions of individuals with whom one is currently playing a stage game. Our experiment is designed, therefore, to study the behavior of individuals drawn from a fixed population who play an indefinite sequence of a two person –Prisoner's Dilemma under different matching protocols and different amounts of information transmission. The objective of varying the matching protocol (fixed pairings versus random matching) is to determine empirically how much difference in the level of cooperative play is associated with different matching protocols.

In all sessions of our experiment we use an indefinite repetition the following stage game shown in Figure 1.

		Column Player	
		X	Y
Row	X	20,20	0, 30
Player	Y	30, 0	10,10

Figure 1: The stage game

The infinite horizon supergame was constructed as follows: After a play of the stage game, a random draw is made from a uniform distribution over the range [1,100] . The draw was made by the computer program that was used to carry out the experiment (students made their choices and observed the outcomes on networked computer workstations) and the number chosen was displayed in a pop-up box on all player's computer screens to reinforce the random nature of the draws. If the draw was less than or equal to 90, players were matched according to the given protocol and the stage game was repeated. If the random draw exceeded 90 the supergame was ended. Thus, the probability, p, that a supergame continues is .90 and the expected number of future rounds to be played from the perspective of any round reached is always 1/(1-p) or 10. This is equivalent to an infinite horizon where the discount factor attached to future payoffs is

.90 per round. Once a supergame ended, depending on the time available, another supergame would begin with the same stage game, matching protocol and population of players used in all previous supergames of the experimental session.[6]

The parameters of the stage game are such that for the population size of players we consider, N≤14, there exists a perfect, sequential equilibrium that supports perfect cooperation even if there is anonymous random matching after each stage game and all information about an individual=s prior history is strictly private. Under the same parameters, these games also have perfect, sequential equilibria that support perfect coordination when some information about an individual=s history is transmitted to the individual with whom that person is currently matched. See the appendix for further details. Given that the expected length of a supergame is 10 repetitions of the stage game, subjects have experience with several supergames over the course of a two-hour session.

There are three treatment variables in our main design. The first treatment variable is the matching protocol (fixed pairings; random pairings). The second treatment variable is the size of the population (N=14 or N=6). The third treatment variable is the information conveyed to each member of a pair playing a stage game regarding the history of the other member of the pair. Since, in fixed pairings each member of a pair shares a complete history with the other member and is aware of this fact, the information treatment is varied only in the random matching protocol sessions.

3.2 Hypotheses

The basic hypothesis to be tested is that there is no significant difference in the level of cooperative play observed under anonymous-fixed and anonymous-random matching protocols. In a given session, the matching protocol is made public through the instructions that are read out loud. In most sessions, the protocol does not change during the course of a session. Subjects are either assigned to a fixed pairing at the beginning of a supergame or are randomly paired after

6 Theoretically it is possible for a supergame to continue indefinitely. We chose to truncate a supergame after the play of 40 rounds. Specifically, if the 40th round of a supergame was reached, the computer program chose a random number in the interval [91,100] so as to force an end to the supergame. This truncation rule was NOT revealed to subjects; they merely observed that the random number drawn following the completion of 40 rounds was greater than 90. In the 21 sessions we have conducted, only 1 supergame ever reached this upper bound of 40 rounds.

each stage of a supergame. In sessions in which a fixed pairing protocol is used the pairings change from one supergame to another in a round robin format so that no pair plays more than one supergame. In some sessions, one matching protocol is used at the beginning of a session and then the second protocol is used for the remainder of the session. In those sessions in which two protocols are used subjects are not informed of the change in protocol until the point in the session at which the switch is made. This treatment allows us to observe how a given group of subjects responds to a change in matching protocol.

A second hypothesis to be tested is that in an anonymous, random matching environment the relative frequency of cooperative play is unaffected by the amount of information about an individual's own history that is available to the person with whom that individual is matched. A competing hypothesis is that the more information an individual has available to label a player a non-cooperator, the greater the likelihood that individuals will refrain from non-cooperative play in the random matching environment. The amount of information about one player's history that is transmitted to the other play is a second treatment variable in our design. The amount of information about one player's history that is transmitted to the other play is a second treatment variable in our design. This variable, I, can take on one of three values: 0 (no information is transmitted); 1 (the average payoff in the individual=s last game is transmitted); 2 (the action chosen by the individual last period is transmitted). Under all conditions the matching and information transmission are done so as to preserve the anonymity of each person. When I = 0 each player can only condition his/her own strategy on his/her own history. Only an individual who has actually experienced non-cooperative play has any reason to update his/her own priors about the relative frequency of playing future games with another individual who has had the same experience. Intuitively, this is the condition least conducive to the development of a fear of contagion necessary to sustain cooperation. When I > 0, each player can condition his action on not only his/her own history, but on the information regarding the other player. When I>1, each player knows that the player with whom one will be matched next period will possess information that may (when I= 1) or will (I = 2) be sufficient to label him/her a >non-cooperator= if there is anything in the transmitted information that indicates that this person has experienced non-cooperation in the past. Intuitively, the more nearly complete the information, the greater the likelihood that past non-cooperative acts will be met with non-cooperation in the present game and, therefore, the greater the likelihood that one=s own past acts of non-cooperation will engender future acts by others of non-cooperation, thereby increasing the future cost of a deviation from cooperation and increasing the likelihood that cooperative behavior materialize and be sustained.

3.3 Results

We have conducted 27 experimental sessions involving a total of 306 subjects. A description of the characteristics of these experimental sessions is given in Table 1. In most sessions we used a populations size of N=14. In eight sessions we considered a smaller population of size N=6 as a robustness check on our results with the larger population size.

[Insert Table 1 here]

Our aim was to get approximately 100 rounds of data per session. As the length of each indefinitely repeated game should average 10 rounds, our goal of 100 rounds per session was satisfied by playing an average of 10 indefinitely repeated games per session. Of course, due to the random end of each indefinitely repeated game, there is some variation in the number of games and rounds as indicated in Table 1. Subjects were not told of our objective of 100 rounds, nor were they told in advance which indefinitely repeated game would be the last one played. Subjects were recruited for a two-hour session but our goal of 100 rounds was always achieved well before this two-hour limit, typically after around 90 minutes.

The subjects were recruited from the undergraduate population at the University of Pittsburgh. Each group of subjects had no prior experience participating in any treatment of our experiment. Subjects were read instructions pertaining to the single treatment they were participating in and then began playing, entering their choices, X or Y, on a computer screen when prompted. A copy of the instructions used in the fixed and the random pairings (I=0) treatments are attached. All treatments involved the same stage game as shown in Figure 1. Following their choice of action, X or Y, subjects were informed of the other player's action and their payoff. The payoff numbers for the game, as shown in Figure 1 were interpreted as monetary payoffs in terms of cents (US$). Thus, if two players chose Y,Y in a round, each player earned 10 cents, etc. Subjects were paid their payoffs from all rounds of all games played and in addition were given a show-up payment of $5. Average total earnings depended on the treatment. In the fixed pairings treatment, subject's total earnings (including the $5 showup fee) averaged $18.64. In the random pairings treatment (I=0, N=14) subjects' total earnings averaged $14.86.

3.3.1 Fixed Versus Random Pairings with No Information, 14 Subjects

[Insert Figure 2 here]

The left column of Figure 2 presents data on the aggregate frequency of cooperation in each round of each game played in four sessions that were conducted under a fixed pairings matching protocol with 14 subjects. The horizontal axis reports round numbers. A round number of 1, represented in Figure 2 by a vertical bar, indicates the start of a new supergame with new pairings. The right column of Figure 2 presents the aggregate frequency of cooperation in each round of each game played in the four sessions that were conducted under the random pairings matching protocol when subjects received no information (I=0) regarding the past experience of anyone with whom they were currently matched. While subjects who played under the random pairings protocol were randomly paired after each round of play, the procedure was to terminate a sequence of rounds with the same stopping rule as was used in the fixed pairings matching protocol sessions. When a sequence ended, the end of the 'game' was announced. If our criteria of obtaining 100 rounds of play had not yet been reached, we announced that a new game would begin. Therefore, a round number of 1 on these graphs also indicates when a new sequence of rounds was begun. In the graphs shown in Figure 2, we report both the aggregate frequency of cooperation—% choice of action X— together with a fitted line from running a regression of %X on a constant and time, t,=1,2…N (where N is the total number of rounds played in all games of a session). A tabular display of the aggregate frequencies of cooperation for the fixed and random (I=0) matching sessions with 14 subjects is presented in Table 2. For comparison purposes, Table 2 also reports statistics for the random pairings I=1 and I=2 treatments which are discussed later in the text.

[Insert Table 2 here]

The column in Table 2 labeled "Game 1, Round 1" reports the aggregate frequency of cooperative play (i.e., choice of X) in the first round of the first game played in each session involving 14 subjects. According to nonparametric, robust rank-order tests[7], there is no significant difference in the distribution of these Round 1 cooperation frequencies between the Fixed and Random (I=0) (or between the Fixed and Random (I=1)) treatments. Thus, the difference between the fixed and random matching protocols is not immediately taken into account by subjects.

While there is not much difference in the way inexperienced subjects first play these games, experience under the fixed protocol drives each group of subjects to a much higher level of cooperative play than is true under either of the random pairings (I=0,1) treatments. Under random pairings, as subjects gain experience the frequency of cooperation plummets towards zero. By contrast, under fixed pairings, as a session progresses the frequency of cooperative play increases.

More precisely, robust rank-order tests (of the null hypothesis of no difference between treatments) confirm that the aggregate cooperation frequencies over the first half, over the second half, and over all rounds of a session (as reported in Table 2) are significantly higher in the fixed pairings treatment than in either the random, I=0 or I=1 treatments (p =.014, smallest critical value for paired samples with 4 observations each). Furthermore, in the fixed pairings treatment, the cooperation frequencies in the second half of the sessions are significantly higher than those in the first half (p=.014). By contrast, in the Random I=0 treatment, the cooperation frequencies in the second half of the sessions are only marginally significantly lower than those in the first half (p=.10), and in the Random I=1 treatment, one cannot reject the null hypothesis of no difference in the cooperation frequencies between the first and second halves of sessions.

An interesting property of the data in all sessions is the increase in cooperation observed in the first round of many of the supergames relative to the level of cooperation in the final rounds of the preceding supergame. This 'restart' phenomenon shows up in all of our treatments

7 See Feltovich (2003) for a discussion of the nonparametric robust rank order test. We use critical values from his Table B.

as revealed in Figure 2. It can also be seen in Figure 3a, which shows the aggregate frequency of cooperation in the first round of all supergames as well as the aggregate frequency of cooperation in all other rounds, excluding the first round of all supergames across the fixed and random treatments with 14 subjects. The figure reveals that on average, cooperation is greater in the first round than over all subsequent rounds of each supergame.

[Insert Figures 3a-3b here]

In the random pairings treatment, the restart effect reflects repeated efforts by just a few subjects to encourage a social norm of cooperation. The presence of these cooperating types can be seen in Table 3, which reports the number of players in each fixed or random pairings session with 14 subjects whose individual frequencies of cooperation (over all rounds of all supergames played) fell below various threshold levels. For instance, in both Sessions 2 and 3 of the Random (I=0) pairings treatment, there were always 2-3 subjects who cooperated (chose action X) in 10 to 25 percent of all rounds played. In session 2 of the Random (I=1) treatment, one subject choose to cooperate in more than 75 percent of all rounds played. However, these efforts were ultimately unsuccessful as cooperation rates invariably diminish as a given supergame proceeds. This experience tends to dampen out the 'restart' phenomenon as additional supergames are played. This dampening out of the restart phenomenon is illustrated in Figure 3b, which shows the aggregate frequency of cooperation in the first rounds of supergame numbers 1-10 using pooled data from all sessions of a treatment.[8]

[Insert Table 3 here]

In the fixed pairings treatment, the aggregate level of cooperation within a given sequence of rounds (supergame) in which pairings are fixed tends to diminish as the number of rounds played in that sequence increases, as can be seen in Figure 2 or in the aggregate frequencies shown in Figure 3a. This is not true of the behavior of all pairs, some of whom sustain constant, or even increasing levels of cooperation over the course of a supergame. The decline in the aggregate frequency of cooperation over time is due to the presence of just a few

8 As noted above in the discussion of Table 1, some sessions had more than 10 supergames, and some had less. In Figure 3b, we have reported the average frequency of cooperation in supergame number 1,2,…10 for all sessions of a treatment for which that supergame was actually played.

player types, who very frequently chose to defect, despite being in the fixed pairings treatment. The presence of these defecting types can again be seen in Table 3. For instance, in Sessions 1, 2 and 3 of the fixed pairings sessions, we see that there was always 1 or 2 individuals who were choosing action X (cooperating) in less than 10 percent of all rounds played, (defecting more than 90 percent of the time). As in the random pairings treatment, there is a "restart" phenomenon where the aggregate level of cooperation increases at the beginning of a new sequence with new pairings, from the level observed at the end of the previous sequence. Unlike the random pairings treatment, however, this restart phenomenon does not dampen out. Instead, there is a trend increase in the aggregate level of cooperation observed the first time new pairings interact, in the first round of each supergame – see Figure 3b. The increase in the aggregate frequency of cooperation over time in the fixed pairings treatment appears to be largely due to the increase in cooperation in the *first round* of each new supergame. Within each supergame, there is typically, though not always, a decline in the aggregate frequency of cooperation following the first round.

[Insert Figure 4 here]

As Figure 4 makes clear, the decline in cooperation within each supergame does not go away with experience; *on average*, the aggregate frequency of cooperation is a little more than 10 percent *lower* at the end of each supergame relative to the start of that supergame. The reason for this finding is that in each fixed pairing session there is typically a small core of players – 'defectors' – who defect with a high frequency as can be seen in Table 3. In the first rounds of play of a new supergame, these defectors' impact on the aggregate frequency of cooperation is at its weakest. However, if the defectors are in *fixed matches* with non-defectors, i.e., subjects playing conditionally cooperative strategies, these conditional cooperators will quickly switch from cooperating to defecting, thereby further lowering the aggregate frequency of cooperation as the supergame proceeds. Nevertheless, the upward trend in the frequency of cooperation in the first round of each new supergame is sufficiently strong that the aggregate frequency of cooperation increases over time. Therefore, under fixed pairings there appears to develop a social norm of cooperation as a given group of subjects gains experience, while under the random pairings-no -information condition, experience tends to drive a group to a competitive norm.

3.3.2 The Effect of Group Size

A group size of 14 is, theoretically, sufficiently small for the existence of a cooperative equilibrium under random matching with no information transmission. Indeed, as detailed in Appendix A, our parameterization of the indefinitely repeated prisoner's dilemma game admits a cooperative equilibrium under random pairings and no information for any group of size 2-30. However, the threat of setting off a contagion process does not appear to be sufficient to sustain cooperation in random matching environments with a group of size 14. Figure 5 below displays the results observed in sessions in which a smaller group of 6 subjects were matched either in fixed pairings for the duration of a supergame or randomly in each round of a supergame with no information about their opponent's prior history of play. In the experimental sessions with groups of 6 subjects, we followed the same experimental procedures as in the sessions with 14 subjects. With a smaller group size, a contagion process will get back to its originator much more quickly and the threat of setting off such a process should provide a correspondingly larger incentive to cooperate. As the data in Figure 5 reveal, when there is no information feedback, under random matching the smaller groups behave as competitively as the larger groups.

[Insert Figure 5 here]

A tabular display of the aggregate frequency of cooperation in the eight sessions with 6 subjects is presented in Table 4.

[Insert Table 4 here]

As in the sessions with 14 subjects, robust rank-order tests reveal that the distribution of cooperation frequencies in the first stage game played in the fixed pairings treatment is no different than that observed for the random (I=0) pairings treatment. Under the fixed matching protocol, the aggregate frequency of cooperation increases with experience in all four sessions with 6 subjects, while under the random matching protocol the aggregate frequency of cooperation diminishes with experience in three of the four sessions.[9] Rank-order tests further confirm that the aggregate cooperation frequencies over the first half, over the second half, and over all rounds of a session (as reported in Table 4) are significantly higher in the fixed pairings treatment than in the random, I=0 treatment. ($p \leq .029$). A comparison of the aggregate cooperation frequencies (over the first half, second half, or all rounds of a session) achieved by groups of 14 subjects in the random (I=0) treatment with those achieved by groups of 6 subjects in the same treatment (cf. Tables 2 and 4) yields no significant differences. Similarly, a comparison of the cooperation frequencies achieved by groups of size 14 or 6 under the fixed pairings protocol also yields no significant differences. We conclude that group size has no statistically significant effect on aggregate cooperation rates.

3.3.3 The Effect of Prior Conditioning

A group of subjects who gain experience with the fixed pairings protocol tends to exhibit high degrees of cooperation as the number of supergames played increases. It is natural to ask whether the social norm of cooperation such a group had exhibited under fixed pairings will be sustained when the group is switched to a random matching protocol. Conversely, if a group has exhibited a social norm of non-cooperation under a random matching protocol will that experience inhibit the formation of a cooperative norm if they are switched to a fixed pairings protocol? To study the effect of prior conditioning on the nature of the social norm developed under a given matching protocol we conducted four sessions in which subjects were first matched under one protocol and then, sometime during the middle of each experimental session, they were switched to another matching protocol. This type of design is sometimes referred to as a "within-subjects" design and stands in contrast to the "between-subjects" design we have used up to now. The switch in matching protocols was not announced in advance. Rather, following the randomly determined end of an indefinitely repeated game sometime in the middle of the session, we handed out and read aloud a brief change in the instructions, which explained to subjects the new matching protocol that would be in effect in all subsequent rounds. We then

9 In session #4 of the random (I=0) treatment with 6 subjects, there was a slight increase the aggregate frequency of cooperation, and cooperation levels of 100% were achieved for a few rounds. However, these high levels of cooperation were never sustained as revealed in Figure 5. The slope of the fitted line for this session is positive, but not significantly different from zero.

played several supergames under this new protocol. All other procedures were as before. For this treatment we considered again the case of 14 subjects.

Figure 6 shows data on cooperation frequencies from 5 within subjects sessions we conducted with 14 inexperienced subjects. The left column of Figure 6 displays data from three sessions in which subjects were first matched according to the fixed pairings protocol and then, without prior announcement switched to a random pairings protocol in the manner describe above. The right column of Figure 6 displays data from two sessions with the opposite order of use of protocol.

[Insert Figure 6 here]

When subjects are first matched under fixed pairings, they quickly achieve a high level of cooperation. However, the switch to the random matching protocol produces an immediate, dramatic decline in the rate of cooperation and as the session continues under the random matching protocol the rate of cooperation quickly tends to zero. In short, there is no evidence that a group of people who have learned to cooperate under fixed pairings will develop a social norm of cooperation that persists when matched randomly. Conversely, experience with random matching that has led members of a group to behave competitively does not prevent the group from immediately making a marked increase in the cooperation rates in response to a switch to the fixed pairing protocol and, with experience, achieving very high sustained levels of cooperation. Indeed, the data suggest that a group that has experienced the competitive outcomes under random matching may learn to cooperate under fixed pairings even more rapidly than groups who have not had such experience.

3.3.4 The effect of information transmission

In the random pairings information treatment I=1, each player in a given stage game is told the average of the payoffs (10, 15, or 20) received by his opponent and the person with whom his opponent was matched in the last stage game. If the report is 10 (20), then it is known that the opponent played Y (X) in the last stage game. If the report is 15, then it is known that either the opponent, or his matched pair, but not both, played Y last period. Figure 7 displays the aggregate frequency of cooperation achieved in four sessions conducted under this information condition with groups of 14 subjects who were matched under the random matching protocol.

[Insert Figure 7 here]

The aggregate cooperation frequencies for this treatment were reported earlier in Table 2. As we noted in the discussion of Table 2, a comparison of the behavior of subjects in their first round of experience under the fixed matching protocol and the random matching protocol with information feedback (I=1) indicates that there is no statistically significant difference in initial behavior (game 1, round 1). As in the random matching protocol sessions with no information feedback (I=0), there is no indication of any trend increase in cooperation rates with experience and the level of cooperation achieved is quite low relative to that observed under the fixed pairings matching protocol. While cooperation frequencies in the fixed pairings treatment are significantly higher than those achieved in the random (I=1) treatment, the cooperation frequencies in the random (I=1) treatment are significantly higher than those achieved in the random I=0 treatment (over the first half, second half and all rounds of a session - probability of rejecting the null of no difference, $p = .014$, lowest value possible with four observations). These findings do not appear to change if the group consists of just 6 rather than 14 subjects. Figure 8 shows aggregate cooperation frequencies in a single session we conducted with 6 subjects under the random matching protocol with information on past average payoffs (I=1). The aggregate cooperation frequency in this session looks very similar to that shown in Figure 5. We conclude that the additional information given in the I=1 treatment yields some increase in cooperative play, but that the trend in cooperative behavior as subjects gain experience is much more similar to the random I=0 treatment than to the fixed pairings treatment, regardless of group size.

[Insert Figure 8 here]

A similar finding obtains for the random pairings protocol under information condition I=2, which corresponds to the case where players are informed of the action (X or Y) that their opponent chose in the previous round of play when matched with another player. This is a different kind of information than is given in the I=1 treatment; in the I=2 treatment, there is no ambiguity about what a player's opponent chose in the previous round (as there might be in the I=1 treatment when the average payoff is revealed to have been 15). However, in the I=2 treatment, the player does not know the payoff that his opponent received in the previous round from the action that he played in that round.

We conducted just one session of the random pairings, I=2 treatment with 14 subjects, and the outcome is revealed in Figure 8. Again we see that there is no indication of any trend increase in cooperation rates as subjects gain experience.

[Insert Figure 9 here]

Aggregate statistics for this session are given in Table 2. While we do not have enough observations of this treatment to make any definitive conclusions, it appears on the basis of the single observation that we do have that there is not much difference in aggregate outcomes between the random pairings (I=2) and (I=1) treatments. As was the case with the random (I=1) treatment, cooperation levels might be higher in the random I=2 treatment relative to the random, no information treatment (I=0). Nevertheless the random I=2 treatment gives rise to the same trend decrease in cooperation rates over time that is observed in both the random I=0 and I=1 treatments.

4. Concluding Observations

Random matching does appear to be sufficient to prevent the development of a cooperative norm in the controlled conditions of the laboratory. Some inexperienced subjects will play cooperatively, and the restart phenomenon described above may be interpreted as a deliberate attempt to promote the development of such a norm. But the experience of frequent defection of others dampens out this effort, at least under the information transmission conditions of our experiment. This should provide some comfort to experimenters who have used the anonymous, random matching protocol as a method for controlling for repeated game effects while collecting multiple observations from a given group of subjects.

A second result of our experiment is to establish empirically that in indefinitely repeated PD games played with fixed pairings the level of cooperation grows over time and most pairs are likely to achieve the cooperative equilibrium outcome with sufficient experience. While this result may not be surprising to theorists, to our knowledge, this is the first empirical demonstration that the cooperative equilibrium does serve as a focal point in such games.

Appendix A

In this appendix we explain how we verified the existence of a "contagious" equilibrium as described in Kandori (1992, section 4) for the parameterization of the prisoner's dilemma game we examined in the pilot experiment and which we intend to use in further experimental sessions. We also establish that under this same parameterization, the cooperative outcome can be supported as equilibrium of the indefinitely repeated game if both players in a fixed pairing adhere to a grim trigger strategy.

Let the stage game be described by the following symmetric payoff table showing the payoffs to the row player only

	C	D
C	w	x
D	y	z

Here C is the cooperative action and D is the defect action (labeled X and Y in the experiment).

In our experimental environment (unlike Kandori (1992)), we restricted w, x, y and z to be strictly nonnegative. Specifically, as noted in the text, we chose $w=20, x=0, y=30$ and $z=10$ so that the game is a prisoner's dilemma. To translate into Kandori's notation, the gain from defection $g = y - w$, and the loss when cheated $\ell = z - x$ Given our parameterization, $g = \ell = 10$.

A.1 Cooperative Equilibrium With Random Pairings

As in Kandori, let δ be the period discount factor and let M denote the population size. The M players are randomly paired in each round of an indefinitely repeated game. Suppose there are just two types of players in the population. Type c players are those whose history of play includes no defections; otherwise, a player is a type d player forever. The "contagious strategy" is for players to play the action corresponding to their type, i.e. type c's play C and type d's play D. Kandori (1992 Theorem 1) shows that the contagious strategy is a sequential equilibrium strategy for any given g and M provided that δ and ℓ are sufficiently large.

Following Kandori's (1992) notation, let X_t be total number of type d players in period t and let A be an $M \times M$ transition matrix with elements $a_{ij} = \Pr\{X_{t+1} = j \mid X_t = i\}$. Similarly, let B be an $M \times M$ transition matrix with elements $b_{ij} = \Pr\{X_{t+1} = j \mid X_t = i \;\; \textit{and} \text{ one type d player deviates to playing C at time } t\}$. The matrix $H=B-A$ characterizes how the diffusion of d types is delayed if one d type unilaterally deviates from the contagious strategy. The conditional probability that a type d player randomly meets a type c player when there are i d types is given by the ith element of the column vector

$$\rho = \frac{1}{M-1}[M-1, M-2, \ldots, 1, 0]^T .$$

Finally, let e_i be a $1 \times M$ row vector with the ith element equal to 1 and all other elements equal to 0. Using the notation given above, we restate Kandori's Lemma.

The contagious equilibrium constitutes a sequential equilibrium if, first, a one-shot deviation from the equilibrium is unprofitable, i.e. if

$$\frac{w}{1-\delta} \geq \sum_{t=0}^{\infty} \delta^t \left[e_1 A^t \rho \ y + (1 - e_1 A^t \rho) z \right]$$

The left hand side is the expected payoff from cooperating forever and the right hand side is the expected payoff from defecting forever. The term $e_1 A^t \rho$ is the probability of meeting a type c player at time t given that the player was the first to defect at *t=0*. The above expression can be simplified to yield

$$\frac{w-z}{y-z} \geq (1-\delta) e_1 (I - \delta A)^{-1} \rho, \qquad (1)$$

which is comparable to equation (1) in Kandori (1992) under his normalization of *w=1, z=0* and using the definition *y=w+g*.

A second, sufficient condition for the contagious equilibrium strategy to be equilibrium is that a one-shot deviation off the equilibrium path (a type d plays C) is unprofitable under any consistent belief. Specifically, the condition is that a type d player finds a one-shot deviation from playing D forever to be unprofitable given $X_t = k$, for all $k = 2,3,...,M$:

$$\sum_{t=0}^{\infty} \delta^t \left[e_k A^t \rho \ y + (1 - e_k A^t \rho) z \right] \geq \left(\frac{M-k}{M-1} \right) w + \left(\frac{k-1}{M-1} \right) x + \delta \sum_{t=0}^{\infty} \delta^t \left[e_k B A^t \rho \ y + (1 - e_k B A^t \rho) z \right].$$

The left hand side is the expected payoff from defecting forever when there are *k* d-type players including the player himself. The right hand side is what the player earns by deviating in the current period --playing C -- and then playing D forever; $(M-k)/(M-1)$ is the probability of meeting a type c player and $(k-1)/(M-1)$ is the probability of meeting a type d player. Finally, $e_k B$ is the distribution of the number of type d players in the next period given that in the current period there are k type d players and one of them (the player under consideration) deviates to playing C in the current period. The above expression can be simplified to yield

$$\left(\frac{M-k}{M-1} \right)(y-w) + \left(\frac{k-1}{M-1} \right)(z-x) \geq \delta \, e_k H (I - \delta A)^{-1} \rho \, (y-z) \ \text{ for } k = 2,3,...M, \qquad (2)$$

which is again comparable to equation (2) in Kandori (1992) under his normalization of *w=1*, *z=0* and using the definition $g = y - w$ and $\ell = z - x$.

To check whether conditions (1-2) are satisfied under our parameterization of the stage game and for our choices of M and δ, we require the transition matrices *A* and *H*. Formulas for constructing these matrices are provided in Kandori (1989) and for completeness we reproduce these formulas here.

First, define the number of different ways of forming *M/2* pairs out of *M* individuals,

$$S(M) = \prod_{m=1}^{M/2} (2m-1).$$

Using this definition, a closed form solution for the $M \times M$ transition matrix *A* is given by the following formula. For *j=i, i+2, i+4,..., min[2i, M]*, if *i* is even and for *j=i+1, i+3, i+5,..., min[2i, M]* if *i* is odd,

$$a_{ij} = \frac{\binom{i}{j-i}\binom{M-i}{j-i}(j-i)!\,S(2i-j)\,S(M-j)}{S(M)},$$

otherwise

$$a_{ij} = 0.$$

A closed form solution for the $M \times M$ transition matrix $H = (h_{ij}) = B - A$ is given by the following formula. For *j=i+2, i+4,..., min[2i, M]*, if *i* is even, and *for j=i+1, i+3,..., min[2i, M]* if *i* is odd,

$$h_{ij} = \left(\frac{j-i}{i}\right)a_{ij} \text{ and } h_{i,j-1} = \left|h_{ij}\right|,$$

otherwise

$$h_{ij} = 0.$$

Using these definitions for the matrices *A* and *H*, we have verified that conditions (1-2) are satisfied for our parameter choices $\delta = .90$, *w=20, x=0, y=30 z=10* for even integer values of *M* over the range $2 \le M \le 30.$[10] (The maximum number of computers we have available in our computer laboratory is 30).

A.2 Cooperative Equilibrium with Fixed Pairings

When players remain paired with the same player for the duration of an indefinitely repeated game, a strategy where each player plays C in all rounds of the game is an equilibrium under our parameterization if players adhere to a "grim trigger" strategy, i.e. begin by cooperating and if the history of play ever includes a defection, defect forever, otherwise continue cooperating.

Specifically, consider a player who decides to deviate from playing C in the current round. His one time gain from doing so, *g=y-w*. Since the other player is playing a grim trigger strategy, the deviant player faces a loss of *w-z* in the following period and forever after. Hence, the cooperative strategy is equilibrium provided that:

$$y - w < \delta \sum_{t=0}^{\infty} \delta^t (w - z), \text{ or}$$

$$y - w < \frac{\delta}{1-\delta}(w - z).$$

This is simply the condition that a deviation from the grim trigger strategy is unprofitable. Since *y-w=w-z=10* in our parameterization, this condition reduces to $.50 < \delta,$ which is readily satisfied by our choice of $\delta = .90.$ Hence, the grim trigger strategy supporting cooperative play is an equilibrium in the fixed pairings environment that we consider.

10 A Mathematica program that checks these conditions is available at http://www.pitt.edu/~jduffy/pd/

Appendix B

This appendix provides the written instructions used in the two main treatments of the experiment, the fixed pairings treatment and the random matching (I=0) treatment.

B.1 Instructions used in the fixed pairings treatment

Overview

This is an experiment in decision-making. The University of Pittsburgh has provided funds for this research. During the course of the experiment, you will be called upon to make a series of decisions. If you follow the instructions carefully and make good decisions, you can earn a considerable amount of money which will be paid to you in cash at the end of the experiment. We ask that you not talk with one another for the duration of the experiment.

Specifics

The experiment is divided into a series of games. A game will consist of an indefinite number of rounds. At the beginning of each game you will be paired with someone else in this room. You will be paired with this player for one game. In each round both of you will play the game described in the upper center portion of your screen. In this game each of you can make either of two choices, X or Y. The points you earn in a round depends upon both the choice you make and the choice made by the other person with whom you are matched. As the payoff table on your screen indicates:

If both of you choose X this round then: you both earn ***20*** points.

If you choose X this round and the other person chooses Y then: you earn ***0*** points and the other person earns ***30*** points.

If you choose Y this round and the other person chooses X then: you earn ***30*** points and the other person earns ***0*** points.

If you both choose Y this round then: you both earn ***10*** points.

To make your choice in each round, click the radio button next to either X or Y. You may change your mind any time prior to clicking the submit button by simply clicking on the button next to X or Y. You are free to choose X or Y in every round. When you are satisfied with your choice, click on the submit button. The computer program will record your choice and the choice made by the player with whom you are matched. After all players have made their choices, the results of the round will appear on the lower portion of your screen. You will be reminded of your own choice and will be shown the choice of the player with whom you are matched as well as the number of points you have earned for the round. Record the results of the round on your RECORD SHEET under the appropriate headings.

Immediately after you have received information on your choice and the choice of the person with whom you are matched for a given round, the computer program will randomly select a number from 1 to 100. The selected number will appear on a popup box in the middle of your screen. If this random number is less than 91, the game will continue into the next round. If the number selected is greater than 90 the game is over. Therefore, after each round there is a 90% chance that you will play another round with the same individual and a 10% chance that the game will end.

Suppose that a number less than 91 has been drawn. Then you click on the OK button, eliminating the popup box, and the next round is played. You will play the same game with the same individual as in the previous rounds Before making you choice, you may review all the outcomes of all of the prior games in the sequence by scrolling down the history record. You then choose either X or Y. Your choice and the choice of the person with whom you are matched are recorded and added to the history record at the lower portion of your screen. You record the outcome and your point earnings for the round. The computer then randomly selects a number between 1 and 100 to determine whether the game continues for another round.

If the number drawn is greater than 90 then the game ends. The experimenter will announce whether or not a new game will be played. If a new game is to be played then you will be matched with someone different from any of the people you have been matched with in prior games. You will be matched with that person for all rounds in the new game.

Earnings

Each point that you earn is worth 1 cent ($.01). Therefore, the more points you earn the more money you earn. You will be paid your earnings from all rounds played today in cash, and in private, at the end of today=s session.

Final Comments

First, do not discuss your choices or your results with anyone at any time during the experiment.

Second, your ID# is private. Do not reveal it to anyone.

Third, remember that you are paired with the same individual for the entire sequence of rounds in a given game. Since there is a 90% chance that at the end of a round the sequence will continue, you can expect, on average, to play 10 rounds with the same individual. However, since the stopping decision is made randomly, some sequences may be much longer than 10 rounds and others may be much shorter.

Questions?

Now is the time for questions. Does anyone have any questions before we begin?

B.2 Instructions used in the random pairings, no information (I=0) treatment

Overview

This is an experiment in decision-making. The University of Pittsburgh has provided funds for this research. During the course of the experiment, you will be called upon to make a series of decisions. If you follow the instructions carefully and make good decisions, you can earn a considerable amount of money which will be paid to you in cash at the end of the experiment. We ask that you not talk with one another for the duration of the experiment.

Specifics

The experiment is divided into a series of games. A game will consist of an indefinite number of rounds. At the beginning of each round you will be paired with someone else in this room. You will be paired with this player for one round. In each round you will play the game described in the upper center portion of your screen. In this game each of you can make either of two choices, X or Y. The points you earn in a round depends upon both the choice you make and the choice made by the other person with whom you are matched. As the payoff table on your screen indicates:

If both of you choose X this round then: you both earn ***20*** points.

If you choose X this round and the other person chooses Y then: you earn ***0*** points and the other person earns ***30*** points.

If you choose Y this round and the other person chooses X then: you earn ***30*** points and the other person earns ***0*** points.

If you both choose Y then: you both earn ***10*** points.

To make your choice in each round, click the radio button next to either X or Y. You may change your mind any time prior to clicking the submit button by simply clicking on the button next to X or Y. You are free to choose X or Y in every round. When you are satisfied with your choice, click the submit button. The computer program will record your choice and the choice made by the player with whom you are matched. After all players have made their choices, the results of the round will appear on the lower portion of your screen. You will be reminded of your own choice and will be shown the choice of the player with whom you are matched as well as the number of points you have earned for the round. Record the results of the round on your RECORD SHEET under the appropriate headings.

Immediately after you have received information on your choice and the choice of the person with whom you are matched for the round, the computer program will randomly select a number from 1 to 100. The selected number will appear on a popup box in the middle of your screen. If this random number is less than 91, the game will continue into the next round. If the number selected is greater than 90 the sequence is over. Therefore, after each round there is a 90% chance that you will play another round and a 10% chance that the game will end.

Suppose that a number less than 91 has been drawn. Then you press the OK button eliminating the popup box and the next round is played. You will play the same game as in the previous round, but **with an individual selected at random from all the individuals in the room**. Before making your choice, you may review all the outcomes of all of the prior games in the sequence by scrolling down the history record. You then choose either X or Y. Your choice and the choice of the person with whom you are matched this round are recorded and added to the history record at the lower portion of your screen. You record the outcome and your point earnings for the round. The computer then randomly selects a number between 1 and 100 to determine whether the game continues for another round.

If the number drawn is greater than 90 then the game ends. The experimenter will announce whether or not a new game will be played. If a new game is to be played then you will be matched with someone drawn and at random from the other people in the room. The new game will then be played as described above.

Earnings

Each point that you earn is worth 1 cent ($.01). Therefore, the more points you earn the more money you earn. You will be paid your earnings from all rounds played today in cash and in private at the end of today=s session.

Final Comments

First, do not discuss your choices or your results with anyone at any time during the experiment.

Second, your ID# is private. Do not reveal it to anyone.

Third, since there is a 90% chance that at the end of a round the sequence will continue, you can expect, on average, to play 10 rounds in a given game sequence. However, since the stopping decision is made randomly, some sequences may be much longer than 10 rounds and others may be much shorter.

Fourth, remember that after each round of a game you will be matched randomly with someone in this room. Therefore, if there are N people in the room the probability of you being matched with the same individual in two consecutive rounds of a game is 1/(N-1).

Questions?

Now is the time for questions. Does anyone have any questions before we begin?

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Table 1: Characteristics of Experimental Sessions

Session Number	Treatment	Number of Subjects	Number of Supergames	Number of Rounds
1	Fixed	14	11	59
2	Random, I=0	14	10	112
3	Random, I=1	14	9	75
4	Random, I=2	14	14	110
5	Fixed	14	10	96
6	Random, I=0	14	12	104
7	Random, I=1	14	9	106
8	Random, I=0	14	8	125
9	Fixed	14	13	131
10	Random, I=1	14	16	99
11	Fixed	14	10	115
12	Random, I=1	14	9	105
13	Random, I=0	14	8	97
14	Random, I=0	6	12	104
15	Random, I=1	6	9	97
16	Fixed Then Random, I=0	14	15	134
17	Random, I=0 Then Fixed	14	13	113
18	Random, I=0 Then Fixed	14	11	133
19	Fixed Then Random, I=0	14	15	127
20	Fixed	6	10	109
21	Random, I=0	6	11	101
22	Fixed Then Random, I=0	14	9	118
23	Fixed	6	9	108
24	Random, I=0	6	12	100
25	Fixed	6	13	108
26	Random, I=0	6	17	129
27	Fixed	6	8	116
		Sum=306	Avg.=11	Avg.=108.6

Table 2: Aggregate Frequencies of Cooperation
All Sessions with 14 Subjects and a Single Matching Protocol

Fixed	Game 1, Round 1	All Rounds of All Games	First Half of the Session	Second Half of the Session
Session 1	0.786	0.548	0.478	0.617
Session 2	0.286	0.576	0.457	0.695
Session 3	0.571	0.477	0.408	0.545
Session 4	0.286	0.608	0.520	0.695
All Sessions	0.482	0.549	0.462	0.634

Random I=0	Game 1, Round 1	All Rounds of All Games	First Half of the Session	Second Half of the Session
Session 1	0.214	0.022	0.034	0.010
Session 2	0.429	0.042	0.080	0.004
Session 3	0.286	0.116	0.132	0.100
Session 4	0.429	0.063	0.097	0.029
All Sessions	0.339	0.063	0.087	0.039

Random I=1	Game 1, Round 1	All Rounds of All Games	First Half of the Session	Second Half of the Session
Session 1	0.429	0.135	0.197	0.075
Session 2	0.500	0.185	0.168	0.202
Session 3	0.571	0.144	0.143	0.144
Session 4	0.571	0.225	0.236	0.214
All Sessions	0.518	0.176	0.186	0.166

Random I=2	Game 1, Round 1	All Rounds of All Games	First Half of the Session	Second Half of the Session
Session 1	0.571	0.158	0.203	0.109

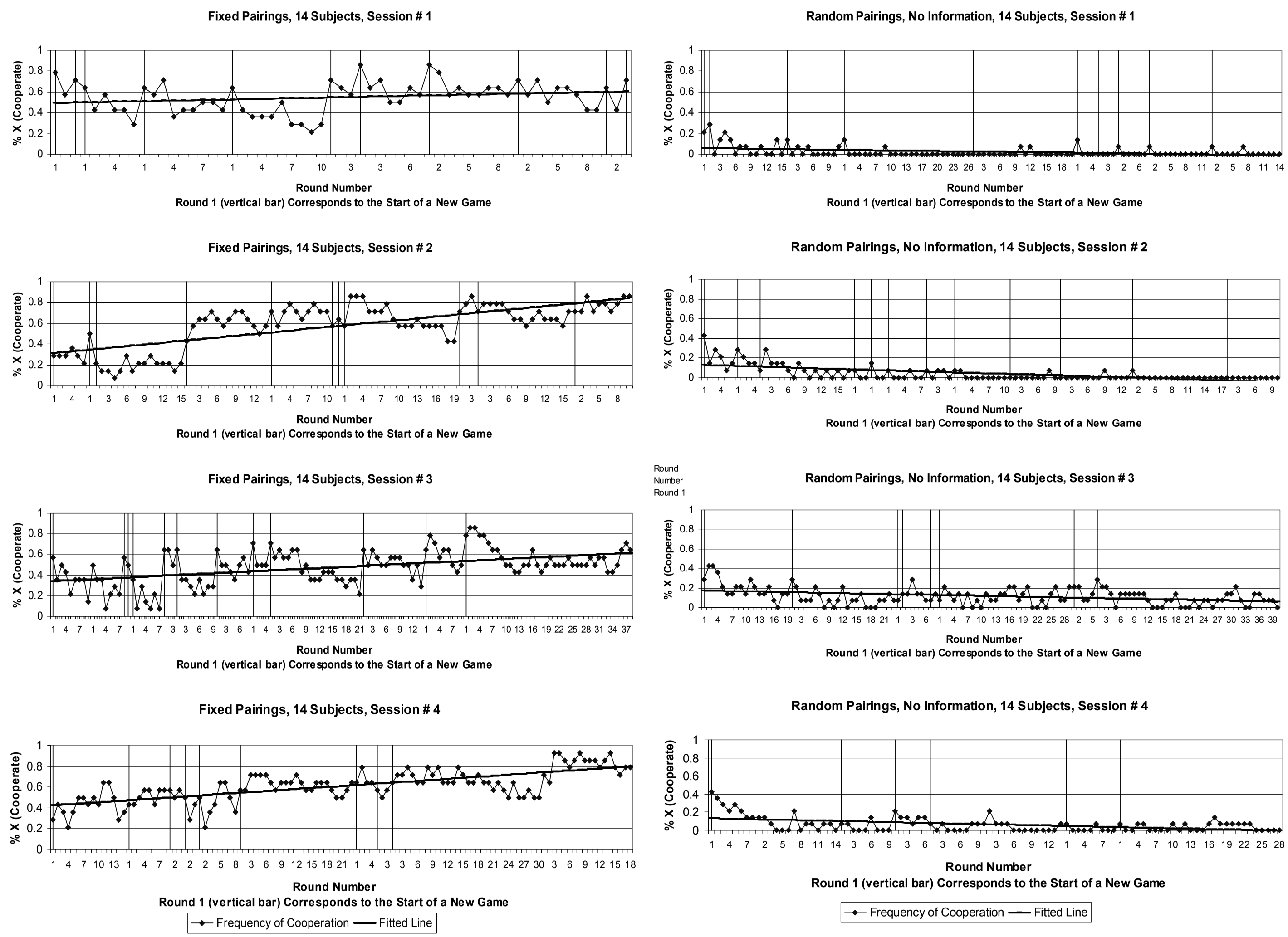


Figure 2: Aggregate Frequency of Cooperation in Fixed (left column) and Random (I=0, right column) Matching Sessions with 14 Subjects

Figure 3a: Frequency of Cooperation in First Rounds Versus All Remaining Rounds of All SuperGames, Pooled Data, All Sessions of a Treatment

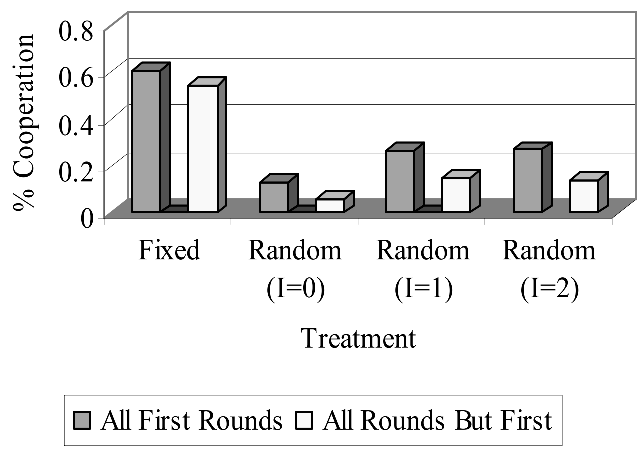


Figure 3b: Frequency of Cooperation in First Rounds of Supergames 1-10, Pooled Data, All Sessions of a Treatment

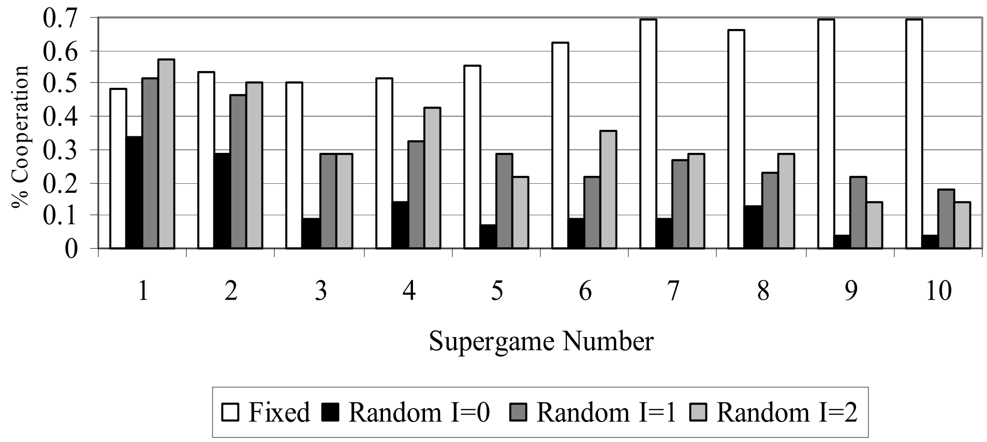


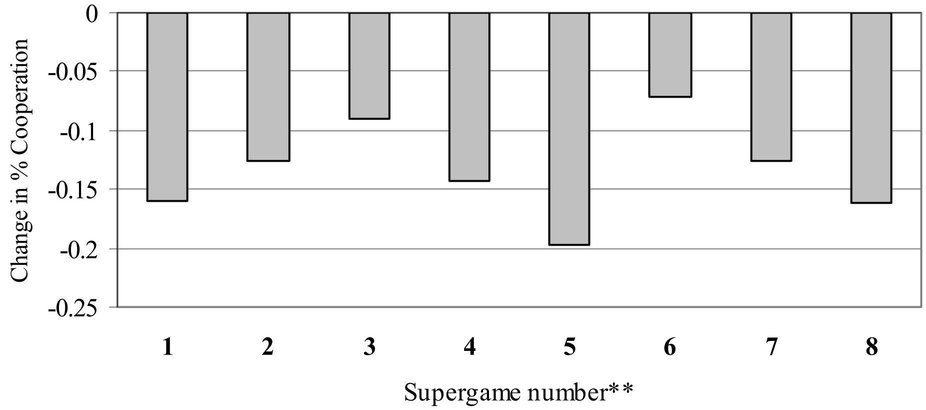
Table 3: Individual Frequencies of Cooperation

Fixed Pairings	Cumulative Number (Cum %) of the 14 Subjects Whose Frequency of Cooperation Falls Below Various Thresholds				Sum Total	All 4 Sessions Combined Cumulative
Freq. Coop. is:	Session 1	Session 2	Session 3	Session 4	Of Row	Frequency
<.05	1 (.071)	0 (.000)	0 (.000)	0 (.000)	1	0.018
<.10	1 (.071)	1 (.071)	2 (.143)	0 (.000)	4	0.071
<.25	2 (.143)	1 (.071)	3 (.214)	0 (.000)	6	0.107
<.50	4 (.286)	5 (.357)	7 (.500)	3 (.214)	19	0.339
<.75	11 (.786)	11 (.786)	10 (.714)	10 (.714)	42	0.750
<=1.00	14 (1.00)	14 (1.00)	14 (1.00)	14 (1.00)	56	1.000

Random (I=0) Pairings	Cumulative Number (Cum %) of the 14 Subjects Whose Frequency of Cooperation Falls Below Various Thresholds				Sum Total	All 4 Sessions Combined Cumulative
Freq. Coop. is:	Session 1	Session 2	Session 3	Session 4	Of Row	Frequency
<.05	13 (.929)	9 (.643)	5 (.357)	4 (.286)	31	0.554
<.10	13 (.929)	12 (.857)	8 (.571)	14 (1.00)	47	0.839
<.25	14 (1.00)	14 (1.00)	11 (.786)	14 (1.00)	53	0.946
<.50	14 (1.00)	14 (1.00)	14 (1.00)	14 (1.00)	56	1.000
<.75	14 (1.00)	14 (1.00)	14 (1.00)	14 (1.00)	56	1.000
<=1.00	14 (1.00)	14 (1.00)	14 (1.00)	14 (1.00)	56	1.000

Random (I=1) Pairings	Cumulative Number (Cum %) of the 14 Subjects Whose Frequency of Cooperation Falls Below Various Thresholds				Sum Total	All 4 Sessions Combined Cumulative
Freq. Coop. is:	Session 1	Session 2	Session 3	Session 4	Of Row	Frequency
<.05	5 (.357)	4 (.286)	5 (.357)	1 (.071)	15	0.268
<.10	7 (.500)	6 (.429)	6 (.429)	3 (.214)	22	0.393
<.25	10 (.714)	10 (.714)	11 (.786)	8 (.571)	39	0.696
<.50	14 (1.00)	13 (.929)	14 (1.00)	14 (1.00)	55	0.982
<.75	14 (1.00)	13 (.929)	14 (1.00)	14 (1.00)	55	0.982
<=1.00	14 (1.00)	14 (1.00)	14 (1.00)	14 (1.00)	56	1.000

Figure 4: Change in the Aggregate Frequency of Cooperation from the First to the Last Round of Each Supergame*
(Pooled Data from 4 Fixed Pairing Sessions with 14 Subjects Each)



* Change is: Avg. % Cooperation in Final Round of Supergame - Avg. % Cooperation in First Round of Supergame. ** Supergames are in the order played, excluding any 1-round supergames.

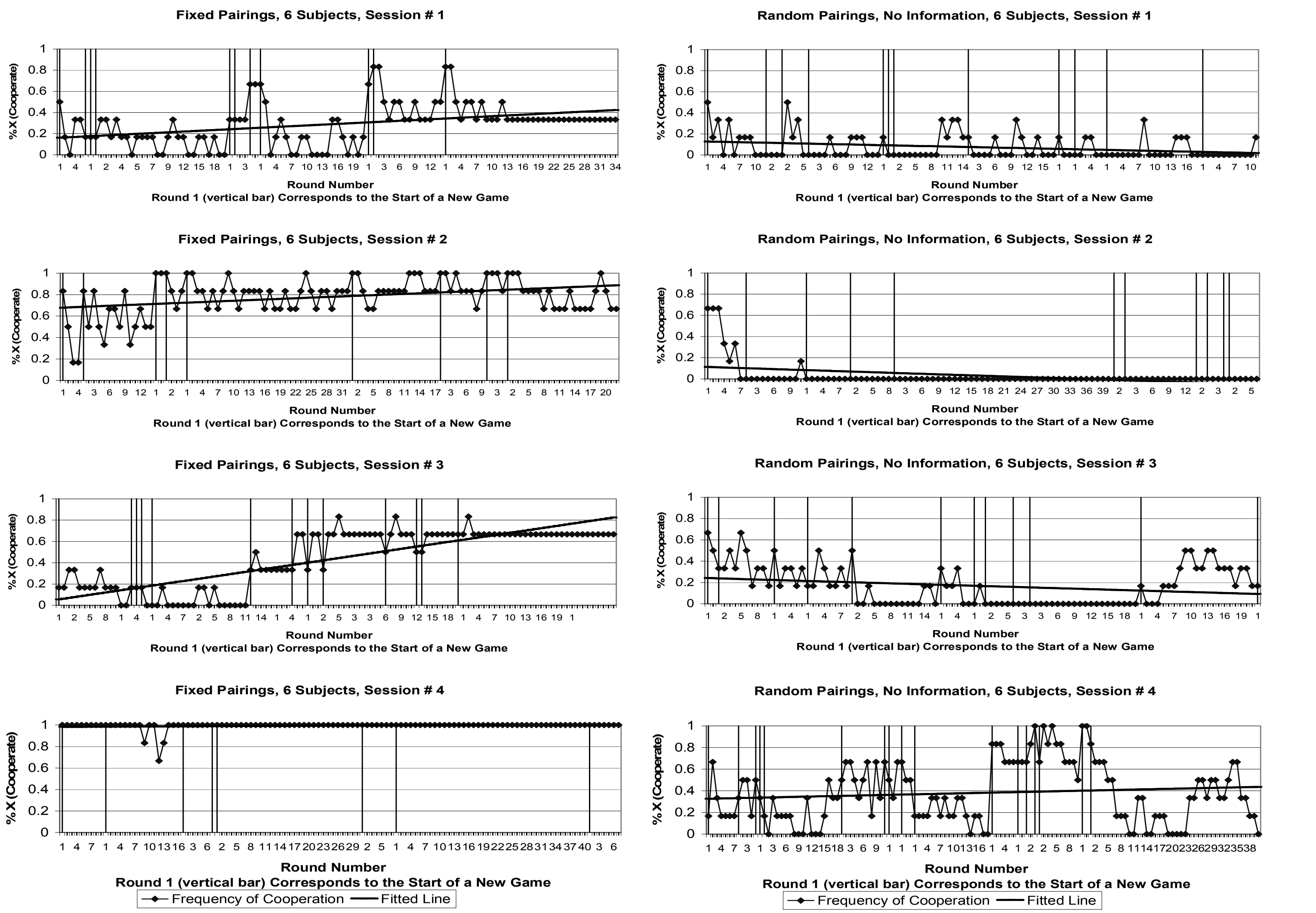


Figure 5: Aggregate Frequency of Cooperation in Fixed (left column) and Random (I=0, right column) Matching Sessions with 6 Subjects

Table 4: Aggregate Frequencies of Cooperation
All Sessions with 14 Subjects and a Single Matching Protocol

Fixed	Game 1, Round 1	All Rounds of All Games	First Half of the Session	Second Half of the Session
Session 1	0.500	0.292	0.189	0.418
Session 2	0.833	0.782	0.729	0.840
Session 3	0.167	0.440	0.280	0.663
Session 4	1.000	0.994	0.989	1.000
All Sessions	0.625	0.627	0.547	0.634

Random I=0	Game 1, Round 1	All Rounds of All Games	First Half of the Session	Second Half of the Session
Session 1	0.500	0.072	0.102	0.045
Session 2	0.667	0.030	0.061	0.000
Session 3	0.667	0.167	0.217	0.117
Session 4	0.167	0.381	0.290	0.476
All Sessions	0.500	0.162	0.168	0.160

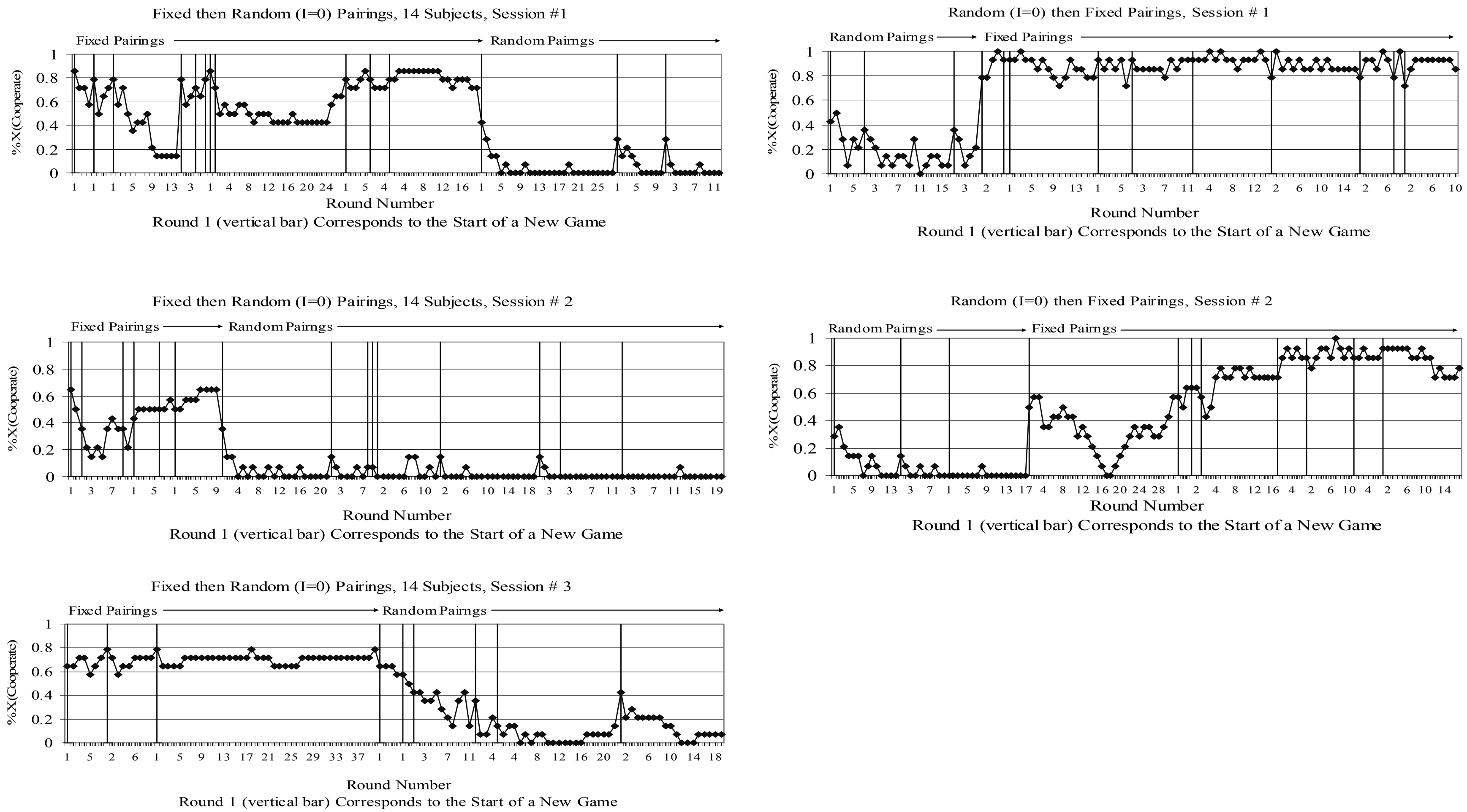


Figure 6: Within Subjects Treatments. 3 Sessions of Fixed followed by Random (I=0) Matching, 14 subjects each (left column) and 2 Sessions of Random (I=0) followed by Fixed Matching, 14 subjects each. The Timing of Each Matching Protocol is Indicated on Each Figure

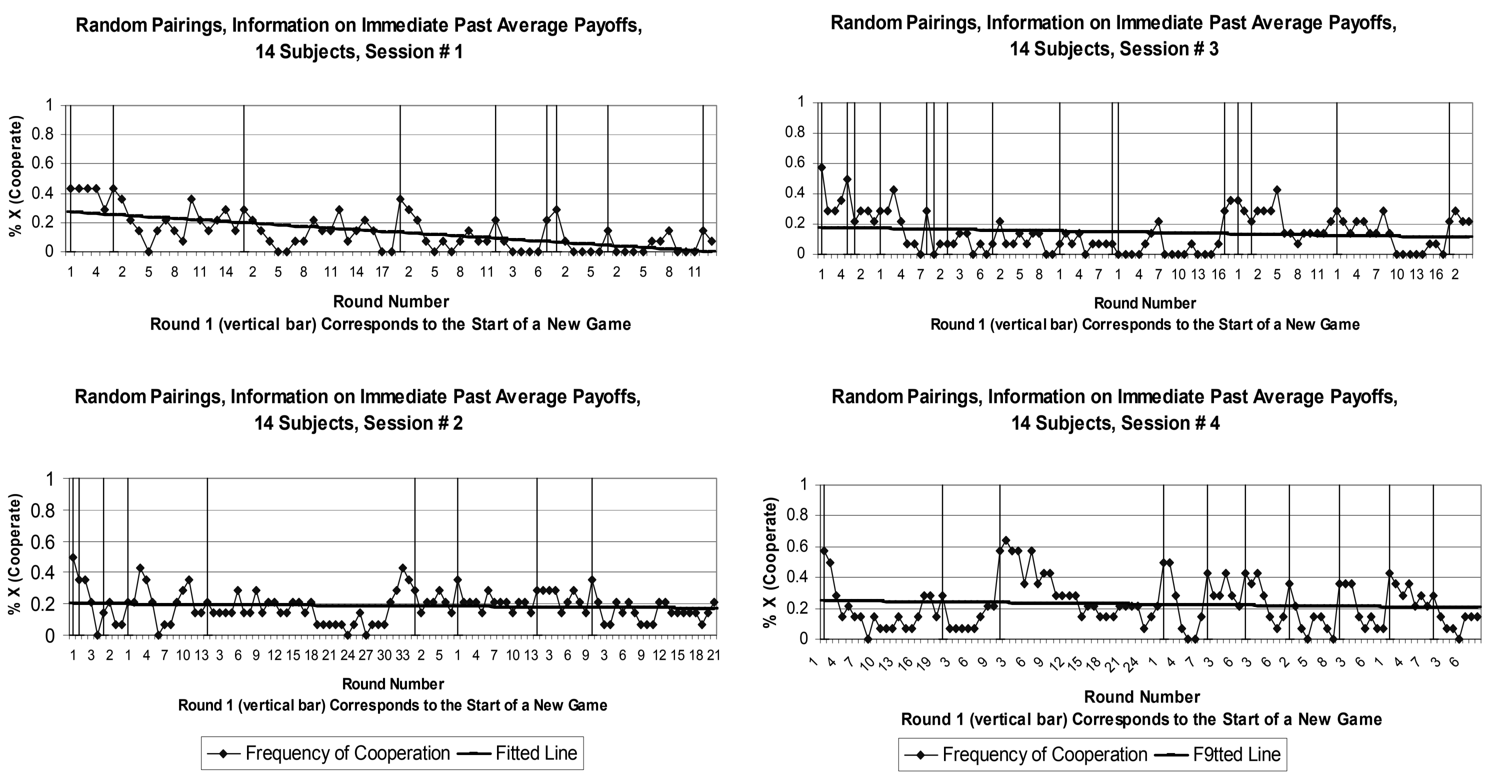


Figure 7: Aggregate Frequency of Cooperation in Four Random (I=1) Matching Sessions with 14 Subjects

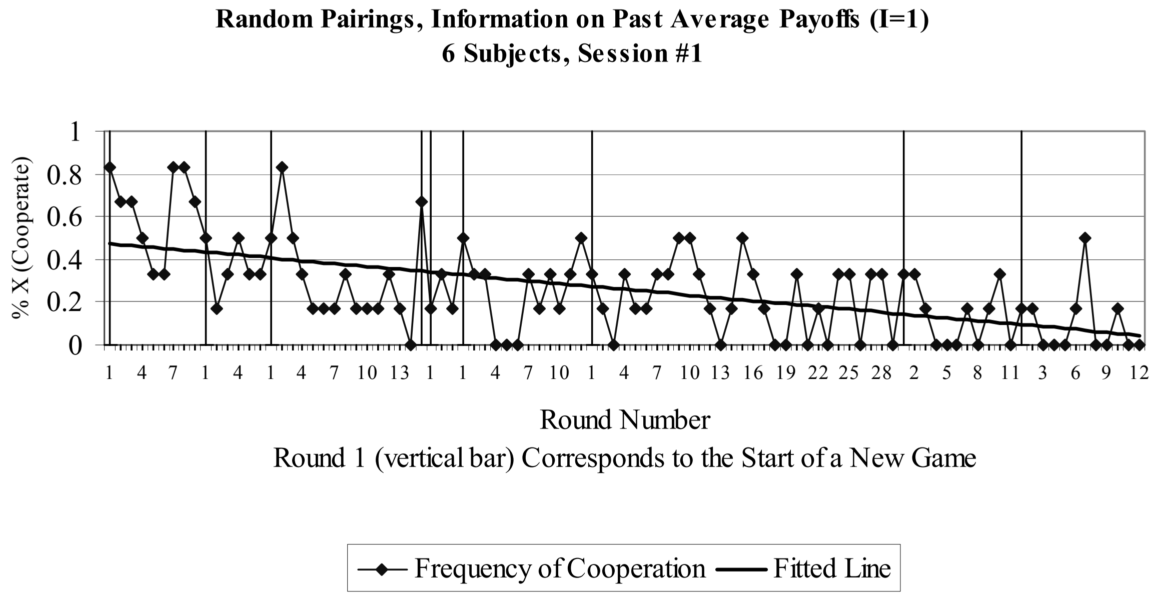


Figure 8: Aggregate Frequency of Cooperation in One Session of Random Pairings with Information on Past Average Payoffs (I=1), 6 Subjects

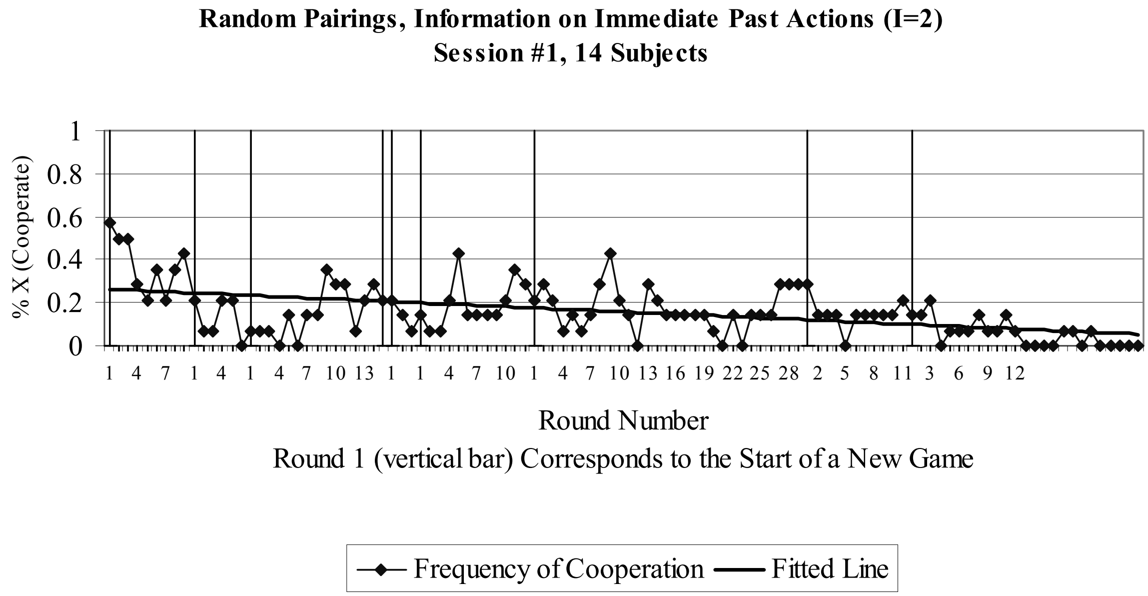


Figure 9: Aggregate Frequency of Cooperation in One Session of Random Pairings with Information on Past Actions of Opponents (I=2), 14 Subjects