Indirect reciprocity in cyclical networks
- An experimental study -

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Abstract

A cyclical network of indirect reciprocity is derived organizing 3- or 6-person groups into rings of social interaction where the first individual may help the second, the second the third, and so on until the last, who in turn may help the first. Mutual cooperation is triggered by assuming that what one person passes on to the next is multiplied by a factor of 3. Participants play repeatedly either in a partners or in a strangers condition and take their decisions first simultaneously and then sequentially. We find that pure indirect reciprocity enables mutual cooperation, although strategic considerations and group size are important too.

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1. Introduction

According to Alexander (1987), networks of indirect reciprocity are crucial for understanding the evolution of large-scale cooperation among humans. Such networks arise whenever individuals help and receive help from different persons: *A* helps *B*, who helps *C*, who helps *D*, who finally helps *A*. Alexander calls this kind of interaction "indirect reciprocity" and considers two possibilities, among others. First, *A* helps *B* only if *B* helps *C*. Second, *A* helps *B* only if *A* receives help from *D*. In both cases, conditional behavior is based on local information. Each agent knows the behavior of the individuals with whom she interacts, but does not know what happens along the entire chain of indirect reciprocity.

So far the literature has focused to a large extent on direct reciprocity, which presupposes bilateral interactions.[1] Less attention has been paid to indirect reciprocity, usually interpreted as rewarding (punishing) people who were kind (hostile) toward others. In most experiments, the "social status" of the potential recipient affects the donor's decision, where the term social status normally refers to an image score, i.e., a record of the individual's past level of cooperation. Recent experimental studies of this form of indirect reciprocity include Wedekind & Milinski (2000), and Seinen & Schram (2001) who examine behavior in a 2-person repeated helping game[2] where donors can observe recipients' image score. They conclude that indirect reciprocity is important since many donors base their helping decision

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[1] Many experimental studies have observed direct reciprocal behavior, which can be either positive (rewarding kind actions) or negative (punishing unkind actions). Relevant studies include public goods games (Croson, 2000; Brandts & Schram, 2001), ultimatum games (Güth et al., 1982; Camerer & Thaler, 1995), investment games (Berg et al., 1995; Gneezy et al., 2000), and gift exchange games (Fehr et al., 1998b; Gächter & Falk, 2002).

[2] The helping game is a degenerate game in which a donor has the choice of either "helping" a recipient at a cost smaller than the recipient's benefit, or "passing," in which case both individuals receive zero.

on the image score of the recipient. Güth et al. (2001) also find evidence of indirect reciprocity in an investment game where, instead of repaying their own donor, recipients repay a different donor whose attitude to cooperation is commonly known.

In this paper, we investigate experimentally the second type of indirect reciprocity envisioned by Alexander. In our experiment, participants know only what happens to them and have no information about the cooperative attitudes of the person whom they may help, or of any other individual in their group.[3] We believe that this form of indirect reciprocity captures real-world situations better than one requiring knowledge about the recipients' image score. In general, one would expect that individuals have much better information about what others did to them than about others' interactions with third parties.

To implement networks of indirect reciprocity, we use a variant of the investment game introduced by Berg et al. (1995). We arrange individuals into a ring of n players and provide each of them with an initial endowment. Every individual i can receive an investment from her left-hand neighbor i-1 and, after learning about how much she has received, send an investment to her right-hand neighbor i+1, where everyone can only invest from her endowment. We close the ring by allowing individual n to return the investment to individual 1.[4] Cooperation is beneficial as individual i+1 (for all i = 1, ..., n) receives three times the investment of i, i.e., the social benefits of giving are greater than the social costs.

The hypothesis tested in this paper claims that people are nicer to others if third parties were nice to them. Boyd & Richerson (1989) view this as a generalization of tit-for-tat to

[3] This type of indirect reciprocity has been studied theoretically by Boyd & Richerson (1989) who investigated its evolutionary properties, and experimentally by Dufwenberg et al. (2001) who aimed at comparing it to direct reciprocity.

[4] The idea of modeling indirect reciprocity via a closed cycle goes back to Boyd & Richerson (1989)

the case of indirect reciprocity. Their model also suggests that the conditions necessary for the evolution of indirect reciprocity become more restrictive as group size increases. We test the effect of group size on indirect reciprocity by comparing 3- to 6-person cyclical networks.

Note that we explicitly define reciprocity as (conditional) behavior. Among the models designed to explain conditional behavior, two prominent classes can be distinguished: outcome-based models that focus on distributional concerns, and intention-based models that focus on the role of intentions that players attribute to one another (cf., McCabe et al., 2003). Examples of the former approach are Fehr & Schmidt's (1999) and Bolton & Ockenfels's (2000) models of inequity aversion as well as Levine's (1998) model of altruism. Falk & Fischbacher (2000), McCabe & Smith (2000), and Dufwenberg & Kirchsteiger (2004) represent the second approach. Understanding why people act reciprocally is a key but still open question. The issue of interpretation tends to become less important, however as long as one recognizes the stability of reciprocal behavior (cf., Fehr & Gächter, 1998).

In this paper, we mainly focus on what subjects do when they repeatedly interact in a (closed) loop. We do not intend to provide insights into the motivations underlying reciprocity.[5] Nonetheless, it needs to be emphasized that the motives driving behavior in our experimental setting are different from those in experiments (like, e.g., Seinen and Schram, 2001), where an individual knows whether the potential recipient has been generous in the past. Since in the latter case, donors know whether recipients deserve to be helped, reciprocity may rely on intentions. In our design, where donors know (and can

who refer to networks of indirect reciprocity as "interconnected loops of varying lengths."

[5] We have no treatment variable which allows for discrimination between alternative explanations of reciprocal behavior.

react to) how much they receive, intentions are superfluous unless donors take the investments received as a sign of the willingness to cooperate in the population. In principle, reciprocal behavior in our experiment may be due to equity concerns or the pursuit of efficiency gains throughout cooperation (see, e.g., Brandts & Schram, 2001).

It is widely acknowledged that indirect reciprocity works via reputation and status, and that the interaction of indirect reciprocity and strategic reasoning can have substantial impact on cooperation (cf., Alexander, 1987; see also Harbaugh, 1998, and Milinski et al., 2002). Based on Seinen & Schram's (2001) design, Engelmann & Fischbacher (2002) conducted an experimental helping game where in any period only half of the players had a public image score and hence a strategic incentive to help. In this way, the authors aimed to study pure indirect reciprocity uncontaminated by strategic concerns. They find clear evidence for pure indirect reciprocity as well as very strong effects of strategic reputation building: The average helping rate of donors with a public score is more than twice that of donors without.

To assess the interplay of indirect reciprocity and strategic reasoning, we repeat the game a finite number of times and vary the re-matching procedure. In particular, we distinguish between a partners condition (where the same group interacts for 10 periods) and a strangers condition (where groups are randomly reassembled after each period). While partners may have an incentive to play strategically in the sense of Kreps at al. (1982), strangers cannot be motivated by strategic considerations. Comparing the decisions by "partners" with those by "strangers", we can evaluate the impact of strategic concerns on investment rates.

Can indirect reciprocity play a role also when decisions are simultaneous and independent? To explore this issue, we enable all *n* players in the ring to decide not only successively but also simultaneously how much they want to invest. Though differently framed, both decision protocols can trigger mutual cooperation based on indirect reciprocity: Players can always condition their behavior on the amount that they receive.

Our findings reveal that, irrespective of the decision protocol, average amounts sent are positive for both partners and strangers. The latter provides evidence for pure indirect reciprocity. Yet partners are more cooperative than strangers (especially in case of sequential decisions), suggesting that strategic reasoning plays a crucial role, too. Moreover, we observe more cooperation in small groups. Our results are thus consistent with the argument by Boyd & Richerson (1989) that indirect reciprocity is likely to be more effective for small, close, and long-lasting loops.

The paper proceeds as follows. Section 2 describes our experimental procedures and formulates some hypotheses. Section 3 presents and discusses the results. Section 4 summarizes and concludes.

2. Experimental procedures and hypotheses

Let $N = \{1, 2, ..., n\}$ be an ordered group of players, each endowed with $e = 5$ ECU (Experimental Currency Unit). The only decision of player *i* (for all $i \in N$) is how much of *e* she wants to send to $i+1$, where $n+1=1$. Let x_i denote the integer amount sent, with $0 \le x_i \le 5$. As in the investment game of Berg et al. (1995), $i+1$ receives from *i* not just

x_i but $3x_i$. Thus, the final earning, U_i, of each player i depends on her own choice x_i, and the choice x_{i-1} of the left-hand neighbor i-1 via

$$U_i = e - x_i + 3x_{i-1}, \qquad \text{where} \quad i-1=n \quad \text{for} \quad i=1.$$

The game theoretic solution, assuming opportunistic (i.e., motivated by monetary rewards) players and common knowledge of opportunism, is to send zero, i.e., $x_i^* = 0$ for all $i \in N$.[6] This is also the per-period level of investment predicted by the unique subgame perfect equilibrium for the finitely repeated game. On the other hand, symmetric efficiency requires full cooperation in the sense that $x_i^+ = e$ for all $i \in N$.

Within this basic experimental setting, three aspects are varied in a systematic manner: the group size (n = 3 vs. n = 6), the re-matching procedure (partners vs. strangers condition), and the protocol specifying how decisions can be taken (*I*-protocol vs. *S*-protocol). Under the *I*-protocol, all players $i \in N$ decide independently and simultaneously how much they want to send, being informed of $3x_{i-1}$ from period 2 onward. Under the *S*-protocol, players decide sequentially, i.e., player 1 chooses x_1; then, being informed of $3x_1$, player 2 chooses x_2; and so on until finally, being informed of $3x_{n-1}$, player n chooses x_n. In both decision protocols, players get to know only how much they receive; they never learn about the investment decisions of the other group members. Participants successively faced both decision protocols (within-subjects factor), with the group size and the re-matching procedure as between-subjects factors.

The computerized experiment was conducted at the experimental laboratory of the Max Planck Institute in Jena using the software z-Tree (Fischbacher, 1999). Participants were undergraduate students from different disciplines at the University of Jena. After being seated at a computer terminal, they received written instructions. Questions regarding clarification of the rules were answered privately. Once the instructions were understood, the experiment started. Each session took about 1½ hours. We implemented an exchange rate of 100 ECU= €4.00. The average earning per subject was €16.42 (including a show-up fee of €2.50). At the end of the experiment, subjects were asked to fill in a questionnaire concerning the rationale of their choices in the game.[7]

In total, we ran nine sessions. Each session involved 24 participants and consisted of four subsequent phases of 10 periods each. In the first two phases we employed the *I*-protocol, and in the last two phases the *S*-protocol.[8] Participants kept the same position in the ring throughout the experiment.

There were two partners sessions with groups of size n=3, four partners sessions with n=6, and three strangers sessions with n=3. Hence only partners interacted in large groups.[9] In the partners sessions, subjects stayed in the same groups throughout an entire phase (i.e., groups were randomly re-matched every 10 periods). In the strangers sessions, new groups were randomly formed in each of the 40 repetitions. In the partners (strangers) sessions, we distinguished matching groups of 2n (4n) players, guaranteeing 8

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[6] Formally, this solution can be derived by repeated elimination of (weakly) dominated strategies.

[7] An English translation of the instructions and the questionnaire can be downloaded from http://experiment.uni-koeln.de/~bgreiner/supplements.

[8] We did not perform experiments with the *S*-*I* order because we predicted and observed no difference in the average amount sent between decision protocols. Hence, in order not to overburden our design, we simply ordered games according to their complexity by starting with the easiest one (namely, the *I*-protocol, which, due to the players' symmetry, seems less complex than the *S*-protocol).

[9] Although this means our design is not completely balanced, our main reason for varying the re-matching procedure is to separate strategic play by partners from nonstrategic play by strangers. In our view, comparisons based on one group size suffice for this purpose.

independent observations per each n for the partners condition, and 6 independent observations for the strangers condition. In order to discourage repeated game effects (especially among strangers), participants were not informed that random re-matching of the groups had been restricted in such a way.

As pointed out above, the unique subgame perfect equilibrium of our game predicts noncooperation. Nevertheless, theoretical as well as experimental studies have shown that trust in reciprocity and reciprocity allow for deviations from this inefficient prediction.[10] In our cyclical networks, trust and indirect reciprocity can play a major role in shaping the final outcome. Consider, for instance, the *I*-protocol. If, in period 1, player i (for all $i \in N$) trusts her left-hand neighbor (i.e., she expects a positive x_{i-1}), then $x_i > 0$. Since, from period 2 onward, player i+1 learns about i's decision in the previous period, she can indirectly reciprocate i's kindness and choose $x_{i+1} > 0$. Actually, one can see the *I*-protocol as made up of n different networks of indirect reciprocity: The n networks (one for each player) start in period 1, run parallel to each other for all the repetitions of the game, and end in T, the last period of interaction. In other words, the *I*-protocol entails "interconnected loops": Each person is part of n n-person loops.

Turning to the *S*-protocol, an improvement of the subgame perfect outcome can be achieved if player 1 trusts all her co-players and the trustees indirectly reciprocate. Since from period 2 onward, player 1 can condition her decision on the amount sent by player n in the previous period, the *S*-protocol can be represented as a unique network extending

[10] For theoretical studies on trust, see, e.g., Kreps (1990), and Güth & Kliemt (1994). For models of reciprocity, see Rabin (1993), Falk & Fischbacher (2000), and Dufwenberg & Kirchsteiger (2004). Examples of experiments in which trust and reciprocity are important are provided by Berg et al. (1995), Güth et al. (1997), Fehr et al. (1998b), Cochard et al. (2004), and Gneezy et al. (2000).

over all the repetitions of the game: The network of indirect reciprocity starts in period 1 with player 1 and ends in period *T* with player *n*.

In our experimental setting, the multiplication of the amount sent by a factor of 3 (and, therefore, seeking efficiency gains) provides strong incentives to rely on trust and indirect reciprocity and engage in mutually beneficial cooperation.[11] Although the literature has mainly focused on direct trust and reciprocity (where the trustee can directly reciprocate the trustor), several authors have stressed that the concept does not need to be restricted to two individuals (e.g., Trivers, 1971; Sugden, 1986; Alexander, 1987; Binmore, 1992), and recent experimental studies have demonstrated that indirect reciprocity is an important phenomenon in the laboratory. Consequently, we expect trust and indirect reciprocity to be equally important in our context and test the following hypothesis:

Hypothesis 1 *Regardless of the decision protocol, subjects in both the partners and the strangers conditions send, on average, positive amounts.*

Despite differences in frame and players' characteristics,[12] both decision protocols allow agents to detect (and thus reciprocate) the predecessor's behavior from actual play, the only difference being when the information about the other's choice is revealed (either in the next or in the same period). Thus, if trust in indirect reciprocity and indirect reciprocity drive

[11] Two important design features may also induce people to deviate from opportunistic behavior: lack of double-blindness and corner-point solution. Hoffman et al. (1994, 1996), e.g., claim there is less rewarding in double-blind dictator experiments. Our experimental data, however, suggest that subjects do not feel ashamed not to reward properly. Hence, although quantitatively the lack of double-blindness might affect results, qualitatively it does not.

[12] Due to the simultaneity of decisions, players are symmetric in the *I*-protocol. By contrast, a clear asymmetry between player 1 and all other players is present in the *S*-protocol, where player 1's first

individuals' choices, the two decision protocols should be theoretically similar with respect to the level of cooperation that they can trigger.[13] Hence we test:

Hypothesis 2 *The I- and the S-protocols are equivalent in terms of average amounts sent whatever the re-matching procedure and the group size.*

Experimental evidence suggests that cooperation is higher when strategic reasoning interacts with indirect reciprocity than when the latter is uncontaminated by strategic concerns (cf., Engelmann & Fischbacher, 2002). In our setting this means that partners, who have strategic reasons for being cooperative, send higher amounts than strangers. Furthermore, most previous public goods experiments find that partners, on average, contribute significantly more than strangers (cf., Croson, 1996; Sonnemans et al., 1999; Keser & van Winden, 2000), albeit the evidence regarding the (dis)similarity in behavior between partners and strangers is far from being conclusive (see, e.g., the recent survey by Andreoni & Croson, forthcoming). Thus, in line with some previous studies and the evidence concerning the enforcement of indirect reciprocity by strategic reasoning, we expect less cooperation in case of the strangers condition, and test:

Hypothesis 3 *In both I- and S-protocols, strangers send, on average, lower amounts than partners.*

Our last hypothesis concerns the effects of group size variation. In Boyd & Richerson's (1989) model, increasing group size reduces the extent of cooperation. Here

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investment decision cannot be anchored in another person's behavior.

[13] The equivalence of the two decision protocols is especially true for the partners. Nevertheless, there is now little disagreement among researchers that reciprocal behavior is a widespread phenomenon even among anonymous subjects who interact only once (Roth et al., 1991; Fehr et al., 1998a; Gächter & Falk, 2002; on this issue, see also Fehr & Gächter, 1998).

this implies that the larger the group, the smaller the players' investment.[14] Thus, we claim:

Hypothesis 4 *In the partners treatment, regardless of the decision protocol, groups of size 6 send, on average, lower amounts than groups of size 3.*

3. Experimental results

The results are presented in two subsections. First, we present a general overview and analysis of investment behavior both over periods and across treatments. All statistical tests in this part of the analysis rely on the averages over players for each matching group.[15] In addition, we report on generalized linear mixed models describing the relationship between the individual sending decision and the most recently received amount (with the various treatments as dummies). Then we try to identify some features of individual behavior by studying participants' choices in more depth.

3.1. General results

Fig. 1 displays the time paths of the average amounts sent in the strangers and the partners conditions, the *I*- (first 20 periods) and the *S*-protocols (last 20 periods), and 3- and 6-person groups.

Insert Fig. 1 about here

The predictions of the subgame perfect equilibrium are clearly rejected. On average, all players, independently of the decision protocol, group size, and re-matching

[14] On this issue, see also Olson (1971) and Selten (1973).

[15] Due to our re-matching system, the numbers of statistically independent groups are 8 for each n in the

procedure, send positive amounts. Partners in 3-person groups send, on average, 3.68 (3.35) ECU in the first (second) 10-period phase of the *I*-protocol and 3.62 (3.61) ECU in the first (second) phase of the *S*-protocol. The respective averages for groups of size 6 are 2.12 (1.98) and 2.46 (2.00). As to strangers, they invest, on average, 2.87 (2.65) in the first (second) phase of the *I*-protocol and 2.32 (2.38) in the first (second) phase of the *S*-protocol. Hence we report the following result:

Result 1 *Regardless of the decision protocol, the group size, and the re-matching procedure, average amounts sent are positive.*

This finding provides immediate support for the hypothesis that players are indirectly reciprocal and, in particular, for the existence of pure indirect reciprocity uncontaminated by strategic concerns.

Fig. 1 shows a number of things. First of all, no decision protocol effect seems to be present in the data: Whatever treatment we consider, average amounts sent under the *I*-protocol (periods 1 to 20) do not appear to differ from those sent under the *S*-protocol (periods 21 to 40). Second, there is a clear order in investment decisions: Partners invest, on average, more than strangers, and groups of size 3 invest more than groups of size 6. Third, partners (especially in groups of size 3) exhibit a sharp end effect in each of the four 10-period phases, with average donations dropping drastically in the final period of each phase. No end effect seems to be present in the strangers condition. Finally, the figure reveals a "restart effect" (cf., Andreoni, 1988) in all treatments.

To check whether the two decision protocols are equivalent as to cooperation rates, we performed Wilcoxon signed-rank tests (two-sided) comparing average amounts sent

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partners condition, and 6 in the strangers condition.

over the first and the last 20 periods. The results show that the two decision protocols do not differ significantly, with the lack of significance being prominent in the case of partners (for partners p=0.38 if n=3, and p=0.46 if n= 6; for strangers p=0.07).

Result 2 *The I- and the S-protocol do not differ significantly in terms of average amounts sent regardless of the re-matching procedure and the group size.*

Next, we compare the two between-subjects treatments. Wilcoxon rank sum tests (one-tailed, with averages over players and periods) indicate that groups of size 3 invest significantly more than groups of size 6 (p<0.01 for both the *I*- and the *S*-protocol) and that, within 3-person groups, partners invest significantly more than strangers (p<0.05 for both decision protocols). Therefore, (even with a conservative non-parametric test) we find treatment effects in the sense that both the group size and the re-matching procedure affect choices. This influence goes in the direction conjectured by Hypotheses 3 and 4, implying that partners try indeed to behave strategically.

Result 3 *Independently of the decision protocol, players are significantly more cooperative when they interact in a partners, rather than strangers, condition.*

Result 4 *Independently of the decision protocol, partners are significantly more cooperative when n = 3 than when n = 6.*

Statistical corroboration of the final round effect for the partners condition is provided by a series of Wilcoxon signed-rank tests (one-tailed) comparing averages in the first nine periods and in the last period for each of the four phases. The tests show that, whatever phase and group size, partners send significantly higher amounts in the first nine periods (p<0.05). On the contrary, for strangers the end effect is significant only for the first phase of the *S*-protocol (p<0.05). This confirms that positive amounts sent by partners in earlier periods are mainly motivated by strategic reasoning; namely, by sending positive amounts, partners invest in the reputation for cooperativeness toward their group members, whom they try to exploit strategically by defecting at the very end. Interestingly enough, the sharp decline in cooperation leaves partners in groups of size 3 worse off than strangers in three out of the four final periods. Averaging over the four final periods and over players, the average payoff of partners in 3-person groups is 8.49 ECU, whereas that of strangers is 9.34 ECU.

Concerning the restart effect, for each decision protocol, group size, and re-matching procedure, we applied a one-tailed Wilcoxon signed-rank test to compare the average amounts sent in periods 10 and 11. For partners, whatever group size and decision protocol, we can reject the hypothesis that they send the same or more in period 10 than in period 11 at the 1%-significance level. For strangers, the restart effect is significant under the *S*-protocol ($p<0.05$) and insignificant (but present) under the *I*-protocol ($p=0.076$). These results are inconsistent with the learning hypothesis (according to which subjects learn the incentives of the game, and thus the equilibrium, throughout the experiment), but consistent with the hypothesis of strategic behavior by partners, who have a strategic incentive to send more in early periods of phase 2.

To further analyze the relationship between available information and sending decisions, Table 1 reports the results of generalized linear mixed regressions with individual *i*'s sending decision (x_i) as dependent variable and the amount most recently received (x_{i-1}), *Period* (which takes values 1 to 10 for each of the four phases), and treatment dummies as independent variables.[16] *Matching* equals 0 for partners and 1 for

[16] Due to censored observations, we assume a quasi-Poisson distribution so as to model over-dispersion.

strangers. *DProt* takes value 0 for the *I*-protocol and 1 for the *S*-protocol. *GrSize* is 0 if n=6 and 1 if n=3. The models have random effects at two levels: the effects for the 22 independent matching groups – to allow for dependency of observations – and the effects for all 216 individual subjects. The estimation method accounts for first-order autocorrelation in the within-(matching) group residuals. In comparison to Model 1, the specification of Model 2 contains two additional terms representing the interaction of *DProt* and *Period* with *Matching*. This allows assessing whether decision protocol effects are different for partners and strangers, and whether the different matching procedure results in different time trends.[17]

Insert Table 1 about here

In both models, the amount received has a significantly positive effect on the amount sent, implying that indirect reciprocity is important. The coefficient of *Matching* is always negative and significant, i.e., (in line with Result 3) strangers invest less than partners. The coefficient of *GrSize* is positive and significant, i.e., (as already summarized by Result 4) partners tend to send higher amounts if they interact in smaller groups. The coefficient of *Dprot* is not significant in both models, meaning (as suggested by Result 2) that the difference in decision protocol has no effect *per se*. However, the

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We excluded from the regressions the decisions x_i which could not be conditioned on x_{i-1}, i.e., the amount sent in period 1 by all n players (player 1) for the *I*- (*S*-) protocol. Note that, in the *I*-protocol, x_i and x_{i-1} refer to periods t and t-1 (t = 2, …, 10), respectively. In the *S*-protocol, the same holds for player 1. For all other players i in the *S*-protocol (i = 2, …, n) x_i and x_{i-1} refer to the same period.

[17] We estimated several models to test the interaction between the various explanatory variables. We report Model 2 which fits better the data on the basis of both the Bayesian Information Criterion (BIC) and the Akaike Information Criterion (AIC). In particular, the model including the interaction term *DProt*×*Period* (BIC= 30276.56; AIC= 30191.79) reveals that the coefficient of the latter is not significant (p= 0.1462). This suggests that the two decision protocols do not exhibit different time trends.

coefficient of *Dprot*×*Matching* is significantly negative, implying that strangers tend to send less in the *S*-protocol. Finally, the amounts sent decline over time (the coefficient of *Period* is significantly negative in both models), but this behavior is less pronounced for strangers. This is indicated in Model 2 by the significantly negative coefficient of the variable *Period* and the significantly positive coefficient of the interaction effect between *Period* and *Matching*. The total effect of *Period* on strangers is still negative, but its size is (in absolute terms) smaller than the effect on partners.

While confirming Results 3 and 4, the regression analysis seems to contradict Result 2 as strangers are found to behave differently in the two decision protocols. We note, however, that the non-parametric tests (establishing Result 2) include both the unconditional and the conditional decisions, whereas the regressions take into account only the latter. Performing further Wilcoxon signed-rank test (two-sided) by considering, for each independent strangers group, only the decisions that can be anchored in another person's behavior, we can reject the null hypothesis of no difference between decision protocols at the 5%-level. Analogous tests for partners confirm that they send, on average, the same amount under both the *I*- and *S*-protocols (p=0.84 if n=3, and p=0.94 if n=6). It seems therefore that strangers tend to indirectly reciprocate less in the *S*-protocol. A detailed analysis of individual choices will help to shed more light on what drives strangers' decisions.

3.2. Individual choices

In this section, we will study individual choices in more detail. We will classify choices depending on the relationship between the amount received and the amount sent.

In the postexperimental questionnaire, we asked subjects how they made their decisions. Most participants declared that their guiding principle was "to pass on an amount equal to that received". Following Boyd & Richerson (1989), we refer to this kind of behavior as "indirect tit-for-tat."[18] Table 2 shows the proportion of decisions following indirect tit-for-tat for each decision protocol, group size and 10-period phase, separately for partners and strangers.[19]

Insert Table 2 about here

Although the reported proportions convey no information about the actual (absolute) amounts sent, the table indicates that indirect tit-for-tat reasoning is more frequent when subjects interact among partners than among strangers, when they play sequentially rather than simultaneously, and when n equals 3 instead of 6. Wilcoxon tests confirm that all these differences are significant at the 5% level. Hence the *S*-protocol seems to trigger more indirect tit-for-tat reasoning. This appears in line with the work of, e.g., Schotter et al. (1994), Rapoport (1997), and McCabe et al. (2000), who observe attempts to reciprocate occurring more frequently in sequential play than in simultaneous play.[20]

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[18] Actually, the strategy space in Boyd & Richerson's model is smaller than ours. In their model, individuals have to choose only between cooperation and defection. However, we can think of indirect tit-for-tat as requiring player i to act exactly like player i-1, which in our context means to pass on an amount identical to that received.

[19] As for the regression analysis, to calculate this variable we have to exclude the unconditional decisions.

[20] McCabe et al.'s explanation of this is that reciprocity is more difficult to achieve via intentionality detection in the normal form while the extensive form facilitates the mutual reading of intentions. It should be noted, however, that McCabe et al. study 2-person games and focus on direct reciprocity.

However, particularly in light of the previous regression results, we note that our characterization of indirect tit-for-tat comprises the choice of "sending 0 in response to 0." Thus, higher compliance with indirect tit-for-tat under the *S*-protocol may be due to more opportunistic behavior in case of sequential decisions. Excluding the cases in which people send zero in response to zero reduces the proportion of choices that in the *S*-protocol comply with indirect tit-for-tat to 58% (32%) for partners in 3 (6)-person groups and to 25% for strangers. The decline is much less dramatic for the *I*-protocol (where the percentages decrease by less than 10 percentage points for both partners and strangers). Further Wilcoxon signed-rank tests using the independent average proportions of indirect tit-for-tat play *without* the cases "sending 0 in response to 0" reveal no statistically significant difference between decision protocols, with the lack of significance being eminent in the case of strangers (p=0.09 for partners and p=0.22 for strangers).[21]

Besides indirect tit-for-tat, our design allows us to provide a complete categorization of individuals' choices based on the comparison between x_i (i.e., *i*'s sent amount) and x_{i-1} (i.e., *i*'s received amount). Dufwenberg & Kirchsteiger (2004) define the reference point of zero kindness of player *i* to player *j* as the average between the minimum and maximum payoff of *j* that is compatible with the space of *i*'s efficient strategies.[22] Applying this definition to our game, an amount sent of at least 3 ECU is kind, while an

[21] Later in this section, we will provide clear evidence that indirectly reciprocating 0 by 0 occurs more often in the *S*- than in the *I*-protocol.

[22] In Dufwenberg & Kirchsteiger's model, kindness relates to beliefs so that intentions and possibilities define the kindness of an action. In our game, *i*'s kindness is measured only in terms of *i*'s behavior. Since *i* knows how much *i*-1 has sent him and has no information about *i*+1, no belief-dependent motivations are involved in our game.

amount smaller than 3 ECU is unkind. Using this definition, we can distinguish the following types of choices:

I) *Pure altruistic*: if a kind amount is sent in response to an unkind one.[23]

II) *Positive indirect reciprocal*: if a kind amount is sent when receiving a kind one.

III) *Negative indirect reciprocal*: if an unkind amount is sent when receiving an unkind one, which is the counterpart to II.

IV) *Defecting*: if an unkind amount is sent in response to a kind one.

Of course, we need to distinguish positive indirect reciprocal choices by partners from those by strangers as the former are contaminated by incentives for strategic reasoning. In particular, since strangers should not be motivated by strategic concerns, we can attribute the difference in indirect reciprocity between partners and strangers to strategic behavior.

We are aware that our categorization may be somewhat artificial.[24] However, it facilitates the descriptive analysis of the distribution of amounts sent for all possible amounts received, as described in Tables 3 and 4 for partners and strangers, respectively. The tables are split into different sub-tables, which refer to the different experimental treatments.[25] Each sub-table is in turn divided into four symmetric panels, which are defined by the kindness of the amount sent compared with the kindness of the amount received. Hence each panel corresponds to one of the four aforementioned types of

[23] One may think of such a choice also as “warm-glow” (cf., Andreoni, 1990).

[24] For instance, “sending 0 in response to 0” is regarded as a negative indirect reciprocal choice. Nonetheless, it may be an (unconditional) defecting choice aimed at maximizing one’s own monetary payoff. Moreover, “sending 3 in response to 4 or 5” is identified as a positive indirect reciprocal choice. This may seem strange especially taking into account that player i is paid $3x_{i-1}$.

[25] We report averages across all 20 periods of a decision protocol. A more detailed analysis separating the two 10-period phases does not yield any significant difference for each treatment.

choices. The cases lying on the main diagonal of each sub-table represent the numbers of choices complying with indirect tit-for-tat.

<u>Insert Tables 3 and 4 about here</u>

For each amount received, the proportion of indirect reciprocal choices is higher than the proportion of pure altruistic and defecting choices, regardless of the decision protocol, the group size, and the re-matching procedure. In addition, in accordance with previous findings, the tables make clear that the occurrence of "0 in response to 0" is higher under the *S*- than the *I*-protocol, especially for strangers. It may be argued that this is a consequence of the asymmetric position of player 1 with respect to players 2 to *n* in the *S*-protocol. The player that must decide first without being able to condition her choice on the others' behavior may perceive such asymmetry as unfair. Since unfair procedures are assumed to trigger negative feelings,[26] player 1 may be induced to act opportunistically more often when decisions are sequential rather than simultaneous. To test for this conjecture, we computed the percentages of players 1 sending zero in the two decision protocols. As to strangers, these percentages are not significantly different (20.21% in the *I*-protocol vs. 19.79% in the *S*-protocol). As to partners, they are slightly higher in case of independent decisions if *n*=3 (16.6% in the *I*-protocol vs. 14.75% in the *S*-protocol), while the opposite holds if *n*=6 (23.75% in the *I*-protocol vs. 33.44% in the *S*-protocol). Hence, as long as 3-person groups are concerned, trust in reciprocity does not seem to be affected by the different information structure underlying the two decision protocols. The observation that strangers are less cooperative in the *S*-protocol may

[26] See, e.g., Lind & Tyler (1988) and Bolton et al. (2003) for the notion (and evidence) of procedural fairness.

therefore be caused by untrustworthy players 2 and 3 who take advantage of their more favorable information condition and do not reciprocate properly.

This is corroborated by the percentage of choices which comply with a specific typology in each experimental treatment (cf., Table 5). The performance of the four different types of choices in the different treatments can be visualized in Fig. 2.

<u>Insert Table 5 and Fig. 2 about here</u>

The table and the figure point out that the distribution of choices among the four cases depends on both re-matching procedure and group size. In particular, under both decision protocols indirect reciprocity fares better in partners than in strangers groups, and when partners interact in 3- instead of 6-person groups. Between the two indirect reciprocity categories, positive indirect reciprocity outperforms negative indirect reciprocity for partners in groups of size 3 under both protocols, and for strangers under the *I*-protocol. The latter confirms that simultaneous moves trigger more cooperative thinking than sequential moves among strangers.

The percentage of the strangers' choices which are kind in response to kindness remains nonetheless quite substantial in both decision protocols (36% on average). The frequency of positive indirect reciprocity increases by 26 percentage points for the strategic partners. If we accept the assumption that strangers cannot be motivated to cooperate out of strategic considerations (since cooperative decisions cannot be carried over different groups), these results imply that positive indirect reciprocity is behaviorally important, although cooperative choices are also influenced by strategic reasoning. The interaction of indirect reciprocity and strategic play appears thus to have substantial impact on cooperation.[27]

Studying the participants' behavioral patterns reveals a huge amount of dynamics. Hence it is interesting to look at the evolution of the different choices over time. Fig. 3 displays the average proportion of decisions in line with each type across the 40 periods separately for partners and strangers, and 3- and 6-person groups.

<u>Insert Fig. 3 about here</u>

The total amount of positive and negative reciprocal choices stays rather constant over time for both partners and strangers. Yet, in the last (tenth) period of each phase, partners (but not strangers) exhibit a kink in the proportion of negative reciprocal choices. Noticing that the proportion of defecting choices starts increasing in the second to the last period, we have a further corroboration of the partners' strategic play.

In the first two periods of every phase, the share of indirect positive reciprocal choices is higher than the share of the other types of choices, especially for partners in 3-person groups and for strangers in the *I*-protocol. However, the presence of defecting choices lowers the amount of positive reciprocity and increases the share of negative reciprocity, which is reflected in the observed sending behavior.

[27] Seinen & Schram (2001), and Engelmann & Fischbacher (2002) report results quite similar to ours. The numbers are, however, not directly comparable because we investigate a different type of indirect reciprocity and use different classifications.

4. Conclusions

We have conducted an experimental investment game where each player is located at a different position in a ring of *n* players and must decide how much to send to her right-hand neighbor after learning about how much she receives from her left-hand neighbor. By this means, we aimed to study whether *n*-person cyclical networks of indirect reciprocity can sustain cooperation.

There exists an abundance of evidence showing that direct reciprocity is behaviorally important. There are also experimental studies showing that individuals act kindly (hostile) toward those who were kind (hostile) toward others. This paper has shown that individuals act kindly (hostile) toward others if someone else was kind (hostile) to them.

In our experiment, indirect reciprocity shows up in the sensitivity of the amount sent by a player to the amount that she receives. Both, in analyzing responses to the postexperimental questionnaire and estimating individual choices, we observe that many subjects decide how much to send on the basis of how much they received.

However, we find significant treatment effects. Namely, small groups are more cooperative than large ones, and partners are more cooperative than strangers. Thus, we find support for the hypothesis that cooperation rates are higher when individuals have strategic incentives to send positive amounts. This is in line with earlier experiments indicating that the interplay of indirect reciprocity and strategic reasoning favors cooperation (e.g., Seinen & Schram, 2001; Engelmann & Fischbacher, 2002).

Furthermore, we find that strangers tend to be less cooperative in case of sequential decisions. This result may be due to the different motivations triggered by the different treatments. In the partners condition (where the same group interacts for 10 periods), the efficiency gains deriving from cooperation seem to outweigh the incentive to defect regardless of the decision protocol. Thus, as long as partners are concerned, the simultaneous and the sequential protocols actually differ only as to when the information about the other's choice is revealed.

By contrast, in the strangers conditions the different information structure of the sequential protocol, as compared to the simultaneous one, gives a strategic advantage to players 2 and 3 over player 1. Because of random re-matching of the groups after each period, players who can condition their choice on another in the same period have no strategic incentive to reciprocate properly the predecessor's behavior.

The motivations underlying behavior in the various treatments may therefore differ, but our experiments are not intended to provide insights into what motivates reciprocity. This study is meant to contribute to the recently growing literature maintaining that mutually beneficial cooperation does not require bilateral exchanges, but can be sustained also in situations where reciprocity has to be indirect.

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Table 1
Generalized linear mixed-effects regression on individual sending decisions

	Model 1	Model 2
Independent variable		
Constant	1.702***	1.882***
	(0.14)	(0.070)
x_{i-1}	0.449***	0.467***
	(0.008)	(0.010)
Matching	-0.515***	-0.717***
	(0.228)	(0.119)
GrSize	0.770***	0.653***
	(0.216)	(0.055)
Dprot	-0.005	-0.043
	(0.028)	(0.041)
Period	-0.102***	-0.141***
	(0.005)	(0.008)
Dprot × *Matching*		-0.311***
		(0.080)
Matching × *Period*		0.103***
		(0.015)
Information criteria		
Akaike	27506.30	27604.30
Bayesian	27569.28	27681.29

Note: Std. errors are reported in parentheses. The stars indicate significance at the 1% level.

Table 2
Proportion of decisions following indirect tit-for-tat reasoning

	Partners		**Strangers**
	n = 3	*n = 6*	
***I*-protocol**	0.54	0.41	0.27
Phase 1	0.50	0.37	0.27
Phase 2	0.58	0.45	0.27
***S*-protocol**	0.69	0.55	0.41
Phase 1	0.68	0.55	0.40
Phase 2	0.70	0.55	0.41

Table 3
Empirical distribution of amounts sent for each possible amount received; partners condition

***I*-protocol**

n = 3

Amount received	*0*	*1*	*2*	*3*	*4*	*5*	*Mean*	*Mode*	ν[a]
0	0.54	0.08	0.10	0.05	0.02	0.22	1.58	0	105
1	0.38	0.16	0.13	0.19	0.03	0.13	1.72	0	32
2	0.25	0.13	0.09	0.22	0.25	0.05	2.25	0	55
3	0.13	0.03	0.08	0.31	0.26	0.19	3.11	3	104
4	0.09	0.04	0.08	0.17	0.24	0.39	3.61	5	105
5	0.09	0.01	0.04	0.05	0.07	0.74	4.21	5	463

n = 6

Amount received	*0*	*1*	*2*	*3*	*4*	*5*	*Mean*	*Mode*	ν
0	0.61	0.16	0.09	0.04	0.03	0.06	0.89	0	491
1	0.33	0.30	0.19	0.08	0.02	0.08	1.43	0	271
2	0.24	0.18	0.25	0.17	0.06	0.10	1.90	2	272
3	0.16	0.08	0.18	0.27	0.14	0.16	2.63	3	216
4	0.13	0.14	0.16	0.12	0.23	0.23	2.87	4	136
5	0.20	0.06	0.06	0.08	0.10	0.49	3.30	5	342

***S*-protocol**

n = 3

Amount received	*0*	*1*	*2*	*3*	*4*	*5*	*Mean*	*Mode*	ν
0	0.65	0.06	0.05	0.05	0.03	0.16	1.24	0	154
1	0.46	0.18	0.21	0.04	0.00	0.11	1.25	0	28
2	0.27	0.14	0.30	0.20	0.07	0.02	1.73	2	44
3	0.17	0.04	0.19	0.23	0.25	0.13	2.73	4	48
4	0.06	0.03	0.05	0.09	0.31	0.46	3.95	5	80
5	0.07	0.01	0.01	0.02	0.05	0.84	4.52	5	574

n = 6

Amount received	*0*	*1*	*2*	*3*	*4*	*5*	*Mean*	*Mode*	ν
0	0.71	0.09	0.06	0.04	0.02	0.08	0.82	0	612
1	0.31	0.37	0.19	0.06	0.02	0.04	1.23	1	214
2	0.18	0.18	0.30	0.18	0.08	0.09	2.05	2	210
3	0.11	0.09	0.13	0.40	0.17	0.11	2.76	3	217
4	0.09	0.08	0.12	0.16	0.38	0.17	3.16	4	186
5	0.10	0.03	0.05	0.06	0.09	0.67	4.03	5	449

Note: The four symmetric panels in each sub-table correspond to our four types of choice. Starting from the upper left panel and moving round, we have: negative indirect reciprocity, pure altruism, positive indirect reciprocity, and defection.

[a] "ν" denotes the number of times in which each possible amount received was observed. The total numbers of observations are 960 for n = 3 and 1920 for n = 6. We do not report the cases in which the amount sent cannot depend on the others' behavior. The unconditioned choices are 32 for the *S*-protocol, and 96 or 192 for the *I*-protocol depending on whether n equals 3 or 6.

Table 4: Empirical distribution of amounts sent for each possible amount received; strangers condition

Amount received	***I*-protocol** ***Amount sent*** *0*	*1*	*2*	*3*	*4*	*5*	*Mean*	*Mode*	*ν*
0	0.28	0.08	0.16	0.21	0.09	0.17	2.27	0	241
1	0.28	0.14	0.15	0.17	0.05	0.20	2.18	0	113
2	0.18	0.08	0.22	0.21	0.11	0.19	2.57	2	168
3	0.19	0.08	0.17	0.25	0.11	0.21	2.66	3	253
4	0.22	0.09	0.07	0.16	0.22	0.25	2.82	5	187
5	0.11	0.07	0.08	0.16	0.20	0.37	3.38	5	334

Amount received	***S*-protocol** ***Amount sent*** *0*	*1*	*2*	*3*	*4*	*5*	*Mean*	*Mode*	*ν*
0	0.54	0.09	0.11	0.06	0.09	0.11	1.40	0	417
1	0.31	0.28	0.18	0.11	0.06	0.06	1.54	0	127
2	0.22	0.12	0.27	0.15	0.10	0.14	2.19	2	175
3	0.23	0.09	0.07	0.33	0.13	0.14	2.47	3	178
4	0.18	0.06	0.13	0.17	0.20	0.26	2.93	5	163
5	0.20	0.04	0.05	0.08	0.12	0.51	3.41	5	332

Note: As in Table 3, the four panels in each sub-table correspond to the four individual choices. "ν" has the same interpretation as in Table 3.

Table 5: Percentage of choices in accordance with each type (I: pure altruism, II: positive indirect reciprocity, III: negative indirect reciprocity, IV: defection)

	Partners				**Strangers**	
Strategy	*I-protocol*		*S-protocol*		*I-protocol*	*S-protocol*
	N = 3	*n* = 6	*n* = 3	*n* = 6	*n* = 3	*n* = 3
I	0.08	0.12	0.06	0.1	0.19	0.15
II	0.65	0.25	0.67	0.34	0.39	0.32
III	0.14	0.48	0.19	0.45	0.21	0.37
IV	0.13	0.15	0.08	0.11	0.21	0.16

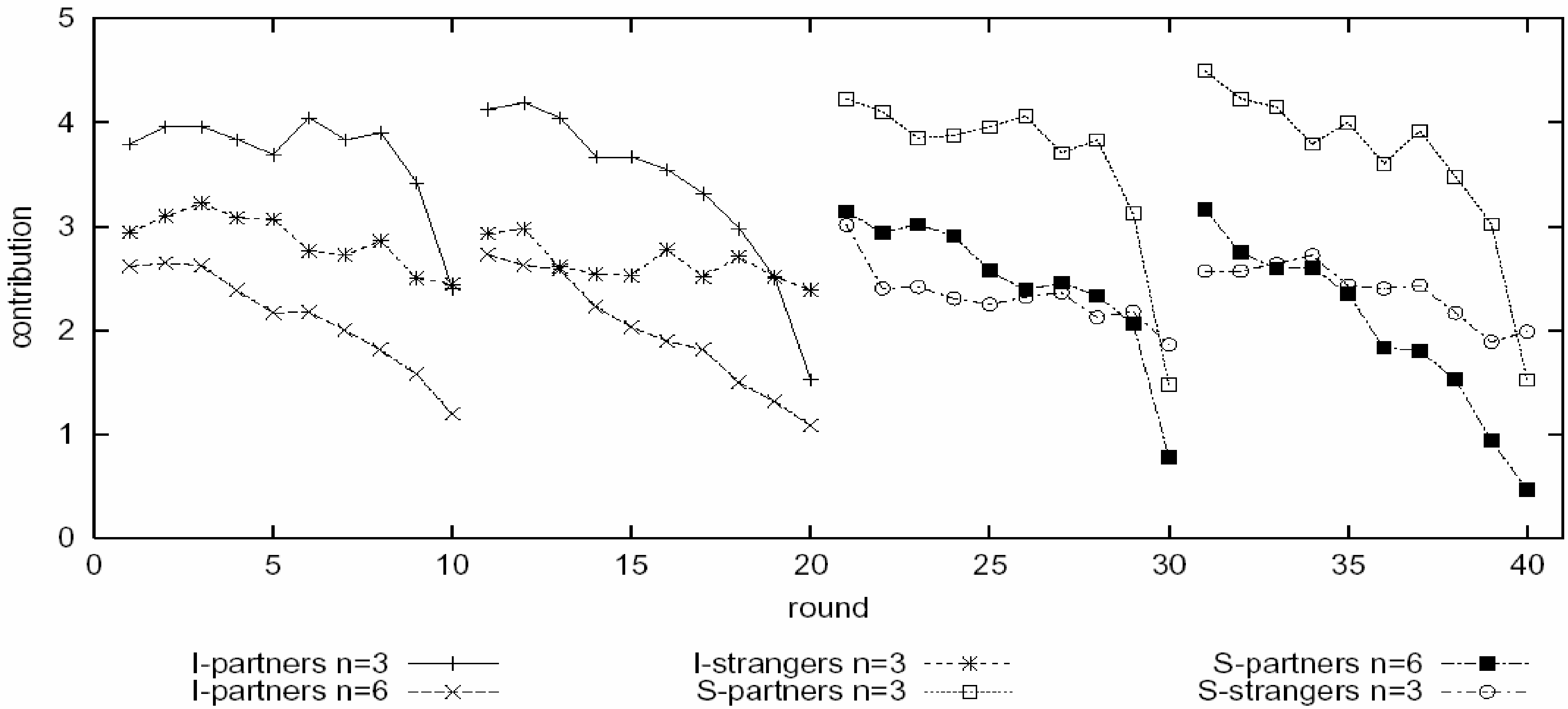


Figure 1: Average amounts sent in each period of the *I*- and *S*-protocols separately for strangers and partners, and for n = 3 and n = 6

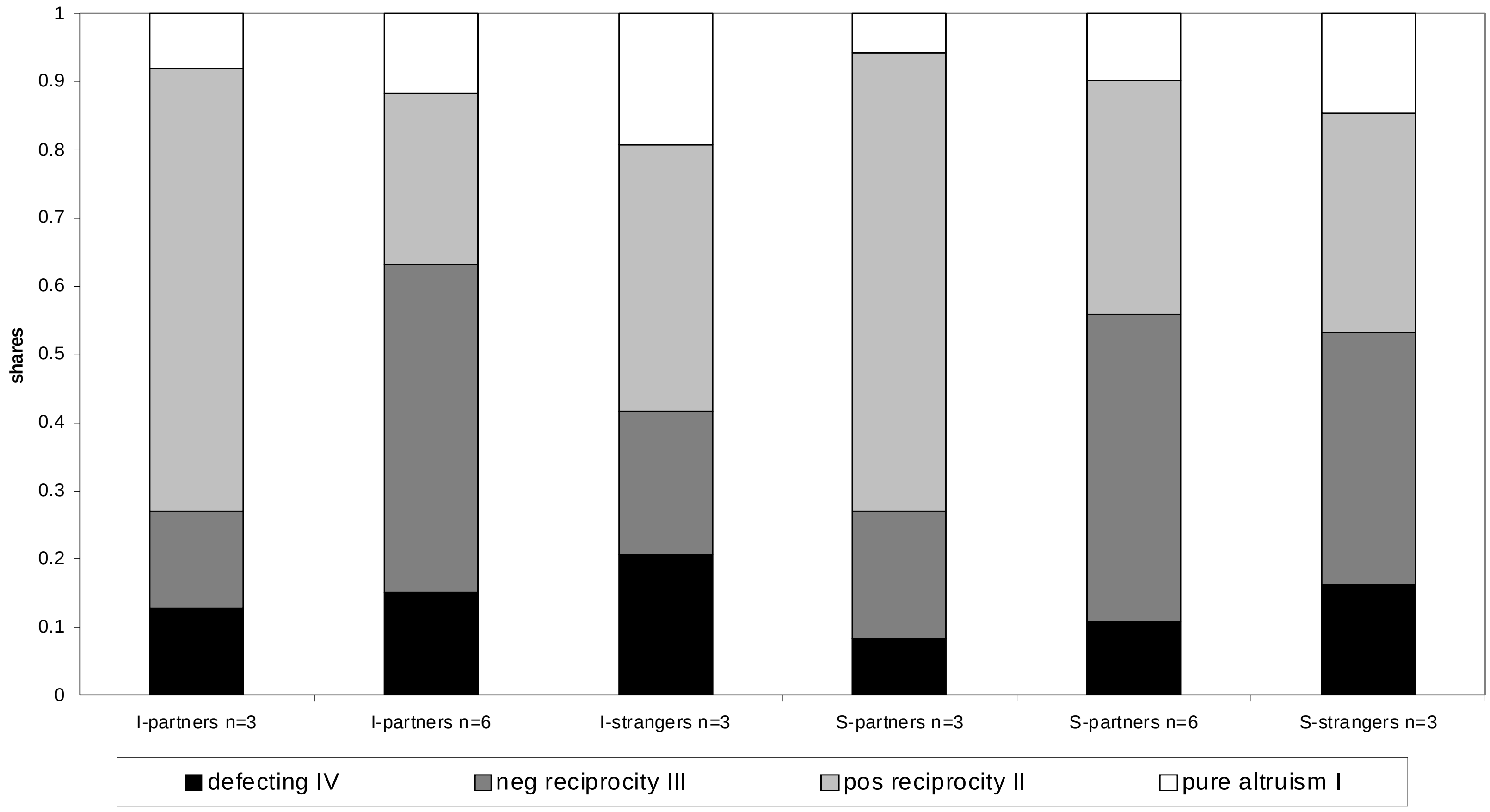
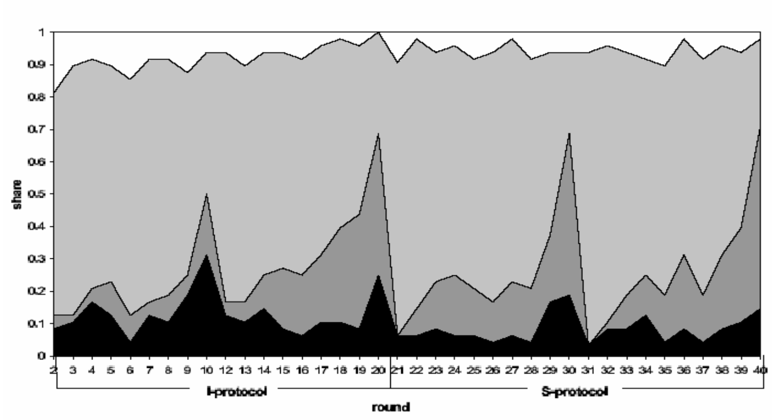


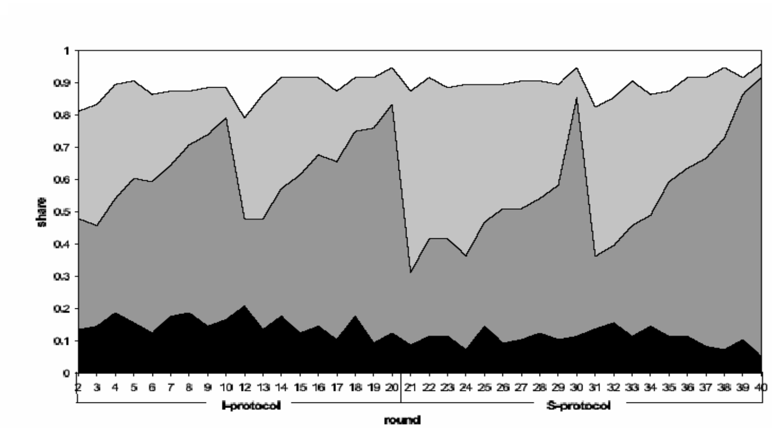
Figure 2: Relative performance of the four individual choices in the different experimental treatments

Partners

n = 3



n = 6



Strangers

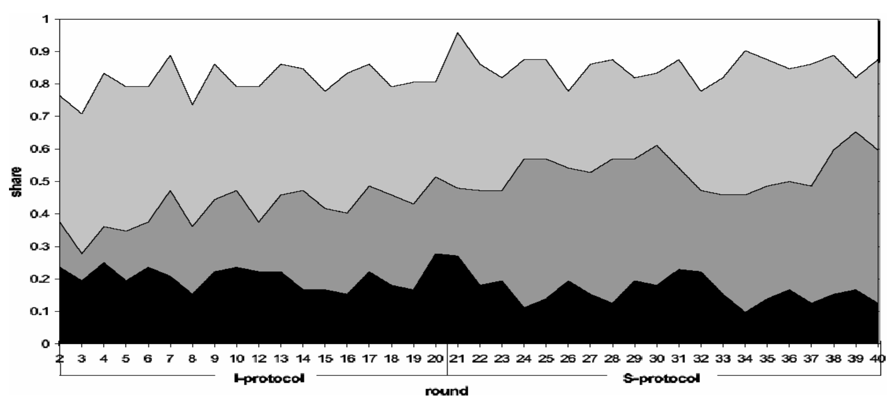


Figure 3: Average relative shares of the four individual choices over periods separately for strangers and partners, for *n* = 3 and *n* = 6 (colors as in Fig. 2)