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The poverty game and the pension game: The role of reciprocity

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Abstract

We examine the force of the reciprocity norm in gift giving experiments in which mutual gift giving is efficient but gifts are individually costly. Our main result is that we find almost no evidence for reciprocity. Gifts supplied are unrelated to gifts received. This applies equally to the Poverty Game (player 1 gives to player 2, player 2 gives to player 1) and the Pension Game (player 2 gives to player 1, player 3 gives to player 2, player 4 gives to player 3, etc.). Nevertheless, we do find substantial levels of gift giving. Furthermore, these levels are higher in the Pension Game than in the Poverty Game.

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1. Introduction

Experimental inquiry has produced a substantial body of evidence indicating that strategic decision-making is often at odds with the presumptions of strict gamesmanship. For instance, several experimental studies have shown a substantial degree of cooperation among players in social dilemmas. Both among economists and psychologists, these results have sparked a serious interest, both theoretically and experimentally, in the strength and consequences of ethical values and social norms. In spite of this acknowledged importance, social norms have rarely been the direct focus of research (see, e.g., Kerr, 1995). In this paper we examine more closely the norm of reciprocity. An important and well-recognized feature of reciprocity is that it sometimes allows a more efficient outcome to be achieved in situations with partially conflicting interests. Therefore, reciprocity has been called a "natural law" (Sugden, 1986) and one of the "cements of society" (Elster, 1989). If there is trust that a cooperative choice will be reciprocated, there is room for mutually beneficial cooperation.

A problem which sets itself at the outset is that reciprocity means different things to different authors (Kerr, 1995). Furthermore, some use other labels – like fairness, or interpersonal orientation – for concepts which are very close to what most scholars now call reciprocity. Most authors seem to agree that reciprocity refers to a conditional obligation, not an unconditional one, such as, for instance, under (pure or impure) altruism. Reciprocity refers to a quid pro quo; good behavior is rewarded and bad behavior is punished. In addition, most authors take it that reciprocity considerations apply in response to *observed* behavior of others. As Gouldner (Gouldner, 1960, p. 171) puts it: "we owe others certain things because of what they have previously done for us". Some authors, however, take a somewhat broader perspective and allow reciprocity considerations to be applied in situations where the behavior of others is (yet) unknown (e.g., Rabin, 1993). In these cases people reciprocate the *anticipated* behavior of others.

In the present paper we will concentrate on the first version of reciprocity which relates to responses to observed choices of others. Reciprocity is then only possible if two conditions are met. First, there must be sequentiality in the move structure: one player acts, a second player reacts. Second, the reacting player must be informed about the action of the first player. Budescu et al. (1995) refer to these two conditions as "priority in time" and "priority in information", respectively. If the player moving second does not know how the first player acted, she can act but not *re*act. In our design, we exploit this latter condition. We compare two treatments of the so-called Poverty Game (Hammond, 1975) and examine whether and to what extent reciprocity induces cooperative gift giving. In both treatments, gift giving is individually costly, but collectively efficient. In addition, both treatments have a sequential move structure. First, player 1 decides on his gift to player 2, then player 2 decides on her gift to player 1 (priority in time). The treatments only differ in the information provided to player 2. In one treatment, player 2 is informed about the gift by player 1, in the other (control) treatment player 2 is not informed about the gift of player 1 when she decides about her gift to player 1. Only in the first treatment there is priority in information. If reciprocity is to make a difference, this difference should show up in a comparison of the two information treatments. Notice, however, that reciprocating the *anticipated* gift of the other player is also possible in the (control) treatment without priority in time. To allow for a sharper view on the (relative) importance of such 'anticipating reciprocity', we also ask the subjects in the experiment to give their *expectations* about the gift of the other player.

A second contribution of this paper concerns the effect of the matching structure on the occurrence of cooperation. In a previous paper (Van der Heijden et al., forthcoming) we investigated gift exchange with an "overlapping" matching structure. There we had a series of experiments in which there was a succession of players, whereby each player decided on a gift to the preceding player. Player t decided on the gift to player $t-1$, player $t+1$ decided on the gift to player t, player $t+2$ decided on the gift to player $t+1$, and so on. Gift giving was again induced to be collectively efficient, but individually costly. Even in this so-called Pension Game (Hammond, 1975), reciprocity may induce gift giving. In this case reciprocity is not 'bilateral' but 'multilateral': "I keep agreements only with those who keep agreements with others" (Sugden, 1986, p. 164). If player $t+1$ conditions his transfer on the gift by player t to player $t-1$, player $t+2$ conditions her transfer on the gift by player $t+1$ and so on, then cooperative gift giving might be sustainable. [2]

The experimental data of our overlapping matching experiments displayed two clear results. Firstly, there were hardly any signs of reciprocity, in the sense that the level of the gift by player $t+1$ to player t was almost uncorrelated to the gift by player t to player $t-1$. Secondly, positive gifts did

[2] Overlapping matching structures have received widespread attention in the theoretical literature. In comparison to (repeated) bilateral matches, a sequence of overlapping matches poses special problems for cooperation. Applications include the sustainability of inter-generational transfers (e.g., Sjoblom, 1985), and cooperation in infinitely lived organizations with finitely lived agents (e.g., Crémer, 1986).

nonetheless occur. In fact, the average level of gifts was about halfway between the collectively efficient level and the individually rational level.

The present paper compares the results of the experiments with bilateral matches and those with overlapping matches. The potential for cooperation and reciprocity is quite different for the two matching structures. First, reciprocity in a bilateral match is more direct. Player 2 rewards or punishes player 1 in response to how player 2 *herself* was treated by player 1. With overlapping matches, however, player 2 rewards or punishes player 1 in response to how *someone else* was treated by player 1. Therefore, one would expect the force of reciprocity to be stronger in a bilateral relationship. Second, in a one-shot bilateral match an opportunistic second mover has a dominant strategy to make no return gift. In an (infinite) overlapping sequence, on the other hand, no player has a dominant strategy to make no gift. There is always a next player who might reciprocate, and thus each player in the sequence has to take into account the next player's reaction. [3] Consequently, gifts might be larger due to this absence of dominant strategies. This feature of overlapping generations of players is well recognized in the theoretical literature (e.g., Smith, 1992), but has never been put to an experimental test. In sum, potentially two opposite forces are at work in a comparison between bilateral and overlapping matches, which in our view makes this comparison non-trivial and interesting.

The paper is organized as follows. The next section presents the hypotheses and the experimental procedure of the bilateral gift giving experiment. Section 3 discusses the results. Section 4 discusses the effect of the matching structure: we compare gift giving with bilateral and overlapping matches. Section 5 presents a concluding discussion.

2. Hypotheses and procedure

A simple two-period Poverty Game forms the basis for the experiment. The crucial feature of the game is that gift giving is individually costly, but mutual gift giving is efficient. There are two players, player 1 and player 2. Each player is "rich" in one period and "poor" in the other period. In the first period, player 1 is rich and player 2 is poor. Player 1 decides about

[3] Of course, in an experiment one cannot have an infinite sequence of players. In a finite sequence of overlapping matches (only) the last player has a dominant strategy to defect.

his gift T_1 to player 2. In the second period the roles are reversed; player 2 is rich, player 1 is poor, and player 2 decides about her (return) gift T_2 to player 1. In the period a player is rich he has an endowment of 9, and in the period a player is poor he has an endowment of 1. Endowments and gifts together determine players' "consumption" levels in the two periods. If player P_i gives a gift of T_i when he is rich, then his "consumption" in that period is $9 - T_i$. If player i receives a gift of T_j when he is poor, then player i's consumption in that period is $1 + T_j$. The payoffs to player i are defined as the product of the consumption levels in the two periods,

$$U_i = C_{i\mathrm{D}} \times C_{i\mathrm{R}} = (9 - T_i)(T_j + 1). \tag{1}$$

Two information treatments are employed in the Poverty Game experiment. In treatment I (Information), player 2 is informed about player 1's gift T_1 in the first period, when he decides about his gift T_2 in the second period. In treatment N (No information), player 2 is not informed about T_1 when she decides about her gift T_2. Formally, in treatment N, both players choose a strategy T_i from the set $\{0, 1, \ldots, 7\}$ (we only allow natural numbers, and no more then 7 can be given away). In treatment I, player 1 chooses a strategy T_1 in $\{0, 1, \ldots, 7\}$ and player 2's strategy is a mapping τ_2: $\{0, 1, \ldots, 7\} \to \{0, 1, \ldots, 7\}$, which specifies her action T_2 as a function of player 1's action T_1.

The pay-off function implies that each player would like to smooth consumption over the two periods. The only way to achieve this is to exchange gifts: to give when rich and to receive when poor. However, though collectively efficient, gifts are individually costly. Without any enforcement mechanisms, each player would be tempted to set $T_i = 0$. Furthermore, the information condition does not affect this game-theoretical prediction. In treatment N, the players actually play a game with simultaneous moves. No player can react to the gift of the other player. Therefore, in this treatment both players have a dominant strategy to play $T_i = 0$. In treatment I, player 2 still has a dominant strategy to give nothing. Player 1 does not have a dominant strategy as he has to take account of the reaction τ_2 by player 2. However, player 1 should realize that player 2 will not play a strictly dominated strategy, which should lead him to the insight that player 2 will play $\tau_2(T_1) = 0$ irrespective of T_1. Player 1 should thus also play $T_1 = 0$. So, gamesmanship predicts no gift giving in either treatment, though the argument needed for this prediction is somewhat stronger in treatment I (iterated elimination of dominated strategies) than in treatment N (elimination of dominated strategies). We formulate this prediction as

Hypothesis 0 (*Strict gamesmanship*)**.** There are no gifts ($T_i = 0$, $i = 1,2$) in either treatment I or treatment N.

The gamesmen forego considerable pay-off opportunities. Each player earns only 9. If, for example, a binding agreement were possible then gifts would be optimally set at $T_i = T_j = 4$. This would give perfect consumption smoothing and almost triple the payoffs to 25. Hence, there are significant incentives to arrive at some form of implicit cooperation.

Several recent experimental studies (e.g., Berg et al., 1995; Bolle and Ockenfels, 1990; Fehr et al., 1993; Güth et al., 1993; Morris et al., 1995) suggest that reciprocity allows gains from cooperation to be realized. These experiments employ a sequential move structure, which allows the second mover to reward or punish the first mover. These studies observe a degree of cooperation and efficiency that is at odds with the hypothesis of strict gamesmanship. In addition, the data reveal signs of reciprocity, that is, a positive relation between action and reaction (though it must be admitted that the evidence is sometimes weak here).

Berg et al. (1995), for instance, study a two-stage investment game. In stage one, a player has to split $10 between a second player and herself. In the second stage, the amount given to the second player is tripled by the experimenter, and the second player has to decide how much of the total amount he wants to return to the first player. In contrast with game-theoretic predictions, the authors found that 92% of the first players transferred money and that 85% of the second players who received money actually returned some money. Berg et al. (1995) define a reciprocal second player as one who gives back enough money to make player one better-off than in case player one had kept all the money herself. According to this definition, they can classify 46% of the second players who received money as reciprocal.

The present inquiry can partly be seen as an attempt to investigate the robustness of these findings. Hence, we formulate:

Hypothesis 1 (*reciprocity*)**.** (a) Gifts are positive in treatment I and larger than in treatment N.

(b) In treatment I the gift by the second player (T_2) is positively (cor)related to the gift by the first player (T_1).

Part (b) of the hypothesis can be based on a 'weak', as well as a 'strong' form of reciprocity. A weak form of reciprocity, in line with the definition by Berg et al. (1995), could work out as follows. For any value of

$T_1 \in \{1, \ldots, 4\}$, player 2 should at least give $T_2 = 1$ to make player 1 earn more than the payoff of 9 which she can minimally earn by choosing $T_1 = 0$. For example, if player 1 chooses $T_1 = 4$ and player 2 responds with $T_2 = 1$, then player earns 10 (>9). Hence, if player 2 is weakly reciprocal then he should give $T_2 > 0$ whenever player 1 chooses $T_1 \in \{1, \ldots, 4\}$. [4] If the players adhere to this weak form of reciprocity, we may perhaps only expect moderate transfer levels. Nevertheless, even transfers of $T_1 = T_2 = 1$ would allow both players to earn 16 points, which is a considerable improvement over the payoff of 9 which they would earn with zero gifts.

A stronger form of reciprocity would be one in which the gift by player 2 is monotonically increasing in the gift by player 1. [5] In particular, if player 1 believes that 'what you give is what you can expect to get' then the collectively optimal level of gift exchange might be achieved. It is easily verified that the payoff in Eq. (1), subject to $T_j = T_i$, is maximized by $T_i = 4$.

Before we spell out our experimental procedures, it is useful to note some differences with the above-mentioned studies. First, our game has a fully symmetric setup in the sense that pay-off functions and action sets in the game are the same for both players (contrary to, e.g., Berg et al., 1995; Güth et al., 1993). Therefore, with a reciprocal outcome ($T_i = T_j$) both players will have the same payoff. We expect this feature to be conducive for reciprocity since it cannot interfere with concerns for income equality or equity.

Second, the Poverty Game is very simple. In our game it should be fairly obvious to the player how to reciprocate. For example, the reciprocity norm cannot be strengthened or weakened by competitive pressures like in the market experiments of Fehr et al. (1993).

Third, subjects play the game repeatedly (contrary to, e.g., Berg et al., 1995; Bolle and Ockenfels, 1990). This allows subjects to learn, and perhaps to learn to reciprocate. Of course, this also opens the possibility of reputation formation as a mechanism to support gift exchange, but the development of gifts over time will give us a hint at the relative importance of repeated-game considerations. Furthermore, after each round the players are rematched randomly and anonymously.

[4] We concentrate on values of $T_1 > 0$ in the set $\{1, \ldots, 4\}$, since that seems to be the more relevant action set given that the collectively optimal level of gifts is 4.

[5] Without explicitly saying so, this stronger form of reciprocity seems to underlie Hypothesis A3 in Berg et al. (1995), in which a positive correlation between the money sent by player 1 and the money returned by player 2 is hypothesized. See, also Dufwenberg and Gneezy (1996).

Fourth, our subjects are paid in cash and paid according to their achievements in the experiments (contrary to Morris et al., 1995). Hence, the incentive to arrive at a cooperative outcome, as well as the danger of being exploited are 'real' and not just hypothetical. Moreover, we do not employ the 'strategy method' as Bolle and Ockenfels (1990) do. In their experiment, subjects are asked to submit a complete action plan: How will you act if your opponent chooses to cooperate and how will you act if your opponent chooses not to cooperate. Since it is often observed that subjects make different choices in a state of certainty than in a state of uncertainty (violating the so-called sure thing principle, see, e.g., Shafir and Tversky, 1992), we chose to observe just subjects' choices rather than having them predict their choices in several states.

Finally, like Bolle and Ockenfels (1990), we employ an experimental control treatment (N) which precludes reciprocity, but gives the same incentives to arrive at some form of cooperation. Especially this latter feature is important. The mere fact that there is more cooperation than predicted by game theory is not a sufficient indicator for reciprocity. It is the control treatment that enables a sharper view on the extent to which reciprocity is responsible for any positive gift exchange and not, for instance, (pure or impure) altruism.

Note that Hypothesis 1 concentrates on reciprocity by player 2 based on the *observed* gift by player 1. As was discussed above, some authors allow reciprocity also to be based on *expected* gifts by the other player. This latter type of reciprocity can also be applied by the players in treatment N. We expect this type of reciprocity to have weaker force than one based on observed gifts. Therefore, we expect gifts to be higher in treatment I than in treatment N. Nevertheless, in the analysis we will investigate also whether there are signs for this form of reciprocity based on anticipated gifts.

2.1. Procedure

Ten experimental sessions based on the Poverty Game described above were run in March 1995, five with each of the two information treatments I and N (and 11 with an overlapping matching structure in January 1995, see below). In each session eight subjects participated. Students from Tilburg University were recruited as subjects. An announcement in the University Bulletin solicited participants for a 1 h decision-making experiment, which would earn them money. No subject had previously participated in any related experiment, and no subject participated more than once.

Upon arrival, subjects were randomly seated behind computer terminals, which were separated by partitions. Instructions (see Appendix) were distributed and read aloud by the experimenter. After that, subjects were given several minutes to study the instructions more carefully and ask questions (few questions were asked).

Of course, in the experiments we did not use expressions like "consumption", rich and poor. In the period a player is rich, he is called Decider, when poor a player is called Receiver. The gifts T_i and T_j were referred to as "transfer from you to the Receiver when you are the Decider", and "transfer to you from the Decider when you are the Receiver". [6] Consumption levels $C_{i\mathrm{D}}$ and $C_{i\mathrm{R}}$ were referred to as "final amounts". Earnings in each round (U_i) were denoted in points and calculated according to Eq. (1). Subjects were also provided with a table, which gave U_i as a function of T_i and T_j. Subjects knew that points would be transferred to money earnings at a rate of 1 point = 5 cents. In addition, they earned a fixed show-up fee of 5 guilders. [7]

After one practice round, subjects played 15 repetitions of the bilateral gift giving game. In each round, subjects were randomly and anonymously assigned to one of four couples, and also randomly assigned to be the Decider in either the first or the second period. For each couple, the first Decider chose a transfer (T_1) to the first Receiver. Then the two switched roles, and the second Decider chose a transfer (T_2) to the second Receiver. The only difference between the two information treatments was that in treatment I the second Decider was informed about the transfer T_1 by the first Decider before she had to decide about T_2, whereas in treatment N the second Decider was not informed about T_1 when deciding on T_2. At the end of a round in both treatments, subjects were informed about their own payoffs in that round. Hence, at that moment all subjects knew the size of the transfer given to them.

At the end of round 15, the points earned were accumulated and transferred into money earnings. Then an anonymous questionnaire asked for some background information (gender, age, major, motivation). Finally, subjects were privately paid their earnings in cash.

One final remark has to be made with respect to the procedure. Both first and second Deciders in treatment N and first Deciders in treatment I were

[6] The terms 'gift' and 'transfer' are taken as synonyms and used interchangeably here. In the experiment we used the term transfer ('overdracht' in Dutch) because it is more neutral than gift, which may have a somewhat positive connotation.

[7] At the time of the experiments, one Dutch Guilder exchanged for about 0.65 US Dollars.

Table 1
Average gifts (and standard deviation) by player and information treatment

	Treatment N	Treatment I	Significance
Player 1 (T_1)	0.99 (0.44)	2.10 (0.35)	$p = 0.01$
Player 2 (T_2)	1.03 (0.35)	0.72 (0.30)	$p = 0.12$
Significance	$p = 0.69$	$p = 0.04$	

asked to type their expectation regarding the transfer $\{0, 1, \ldots, 7\}$ to be received in the next period. Although we will make some use of these stated expectations, it is important to realize that the subjects were not paid to make (accurate) predictions.

3. Results

This section discusses the experimental results of the Poverty Game experiment. Table 1 presents the average transfer, averaged over the 15 rounds and the five sessions, made by the first player (T_1) and the second player (T_2) for treatments N and I. [8] The final row and column show whether the transfers differ across the two players and across the two treatments, respectively. For that purpose we employ non-parametric tests (Wilcoxon and Mann–Whitney tests, respectively) and use the ten session averages as units of observation (because of the dependency of observations within each session). Small p-values indicate that the transfers differ significantly.

Additional information can be obtained from the development of the gifts over the 15 rounds of play. For each round, Figs. 1 and 2 present the average transfers of the first and second Decider (T_1 and T_2) in treatment N and treatment I, respectively. Recall that each treatment consisted of five sessions with eight subjects, who formed four different pairs in each round. Each data point in the figures thus represents an average of 20 transfer decisions.

The figures and the table allow us to make five main observations. First, contrary to hypothesis 0 (strict gamesmanship) we observe positive gift levels. The average gift level (averaged over all ten sessions) is 1.21. Although this level is at only 30% of the efficient gift level of 4, subjects are able to capture

[8] Data for each session separately can be found in Table 4 of Appendix A; Table 5 and Table 6 present a frequency table of all $T_1 - T_2$ combinations in treatments N and I, respectively.

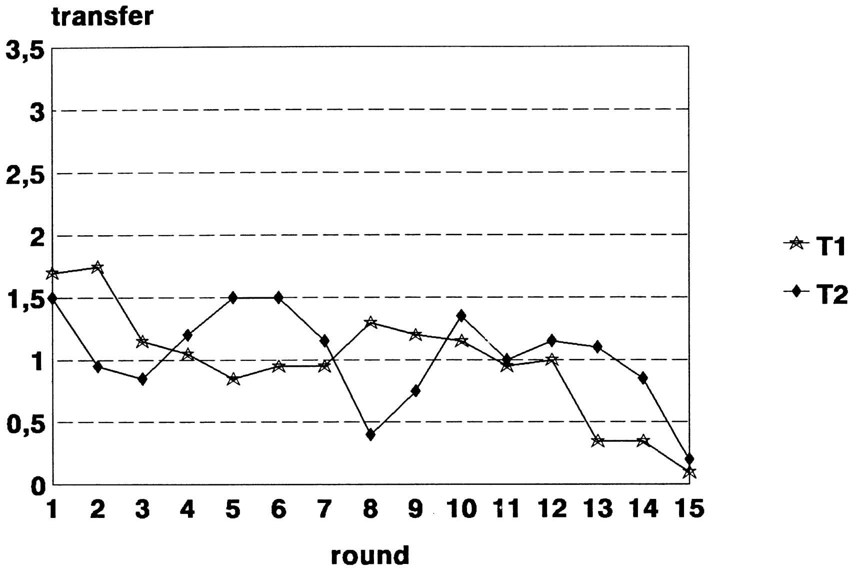

Fig. 1. Average transfer T_1 and T_2 by round in treatment N.

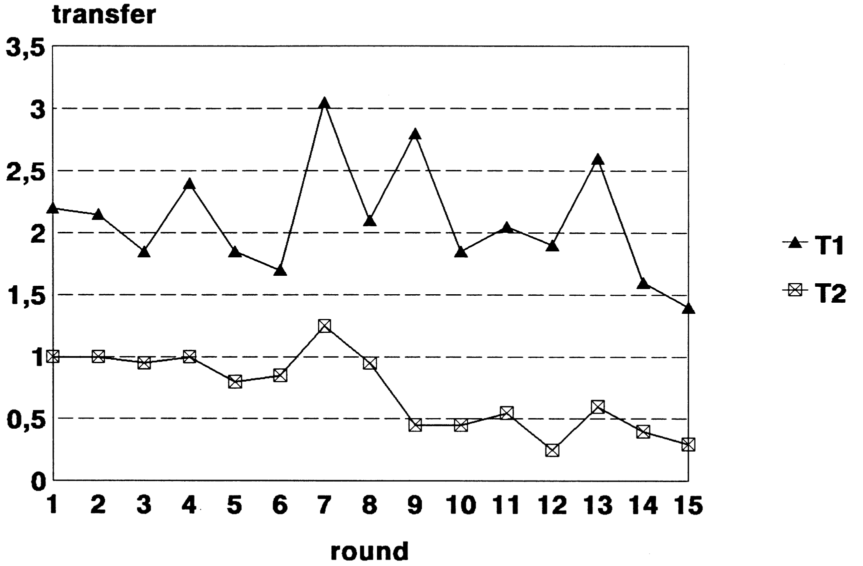

Fig. 2. Average transfer T_1 and T_2 by round in treatment I.

a major part of the possible gains from trade. Without gifts ($T_1 = T_2 = 0$) they would earn 9 points; with efficient gift exchange ($T_1 = T_2 = 4$) they would earn 25 points. Averaged over the two treatments, subjects' earnings are 17.38. Hence, on average, 52% ((17.38 − 9)/(25 − 9) × 100) of the possible efficiency gains from gift exchange are actually realized. Although a strict comparison is hazardous, this by and large conforms to the efficiency gains realized in Berg et al. (1995) and Fehr et al. (1993). For example, in Berg et al. (1995) the average gift by the first mover is $5.14 implying an efficiency gain of 51% of the maximal possible gain.

Second, in relation to the hypotheses formulated in the previous section, we observe that the average levels of gifts are higher in treatment I than in treatment N. This difference rests entirely on the average transfer of the first player (T_1), which is significantly larger for treatment I than for treatment N. The average transfer of the second player (T_2) does not differ significantly across the two treatments. Nevertheless, the average gift $\frac{1}{2}(T_1 + T_2)$ is 1.01 for treatment N and at 1.41 about 40% higher in treatment I (this difference is significant at $p = 0.08$ with a two-tailed Mann–Whitney test). As a consequence, the average payoff in treatment I is somewhat higher (18.75) than in treatment N (16.02). This outcome contrasts with Hypothesis 0 (gamesmanship), which predicted no difference in the level of transfers between the two treatments. The outcome is in line with part (a) of Hypothesis 1 (reciprocity) which predicted larger gifts in treatment I than in treatment N. Priority in information does increase the average level of gifts. Yet, the difference, though significant, is not overwhelming.

Third, the average levels of T_1 and T_2 are almost identical for treatment N. In treatment N the players take symmetrically strategic positions. Although the players move sequentially, neither player is informed about the move of the other player. In game-theoretical terms, the normal form of the game does not depend on who moves first. Hence, in effect the game is identical to one with simultaneous moves, and the subjects can be seen to act accordingly. [9]

[9] Interestingly, this result is in contrast with recent experiments that found a clear effect of 'priority in time' even without 'priority in information' (Budescu et al., 1995; Morris et al., 1995; Rapoport, 1993, Shafir and Tversky, 1992). For example, Morris et al. report that subjects opt for cooperation in a prisoner's dilemma more often if they move first than when they move second even without either player being informed about the move of the other player. This effect is sometimes attributed to 'causal illusion'. Even though the first player should know that his choice cannot influence the choice of the second player, the fact that he moves first activates a decision heuristic of acting so as to influence the opponent. In the first three rounds we find some evidence for causal illusion, but this quickly disappears with experience.

Fourth, being the second mover appears to be the more favorable position in treatment I. Over the 15 rounds the average gift from the first to the second Decider (T_1) is 2.10, whereas the average gift from the second to the first Decider (T_2) in only 0.72. This difference is significant ($p = 0.04$ with a Wilcoxon matched-pairs signed-ranks test, with the five session averages as observations), and does not show a tendency to become smaller or larger over the rounds. So, although the average level of gifts $\frac{1}{2}(T_1 + T_2)$ is larger in treatment I, it is mainly player 2 who gains from this. On average the gift returned (T_2) is a mere 35% of the gift received (T_1). As a consequence, the average payoff is 11.85 for player 1 and 25.65 for player 2.

Finally, in treatment N the gift levels display a clear tendency to move toward zero as the experiment proceeds. Transfers start at a level of about 1.6, then quickly drop to about 1, staying there till about round 12, and drop to about 0.2 in the final round. Towards the end of the experiment, the subjects are able to capture only a very small portion of the potential gain from cooperation. At least this suggests that altruism (pure or impure) is not a particularly strong concern to the subjects. Also in treatment I the transfers have a tendency to decline over time, but here the effect is somewhat less pronounced. The difference between first and last round transfers is about 0.7 for both T_1 and T_2.

To sum up, the average picture looks as follows. The data show that the possibility of monitoring and reciprocating previous gifts plays a facilitating role for the occurrence of gifts. Average gifts are larger in treatment I than in treatment N. However, the benefits mainly accrue to the player moving second. On average the gift returned is much smaller than the gift received. It seems that the player moving first places considerable trust in reciprocity. His gift is much larger than the gifts observed in treatment N. The player moving second though, does not seem to reciprocate these gifts.

A closer examination of Hypothesis 1(b) corroborates this picture. Remember that we made a distinction between a 'strong' and a 'weak' form of reciprocity. A strong form of reciprocity would require a systematic positive relationship between T_1 and T_2 in treatment I. No such relation is visible in the data, however. The Pearson correlation coefficient between T_1 and T_2 over all the 300 paired observations (5 session $\times$ 15 rounds $\times$ 4 matches) is only 0.006 and it is not significantly different from zero (Table 6 in Appendix A gives a frequency table of all paired observations of T_1 and T_2). Furthermore, the correlation coefficient declines over the rounds. In fact, it is even (non-significantly) negative in the final rounds.

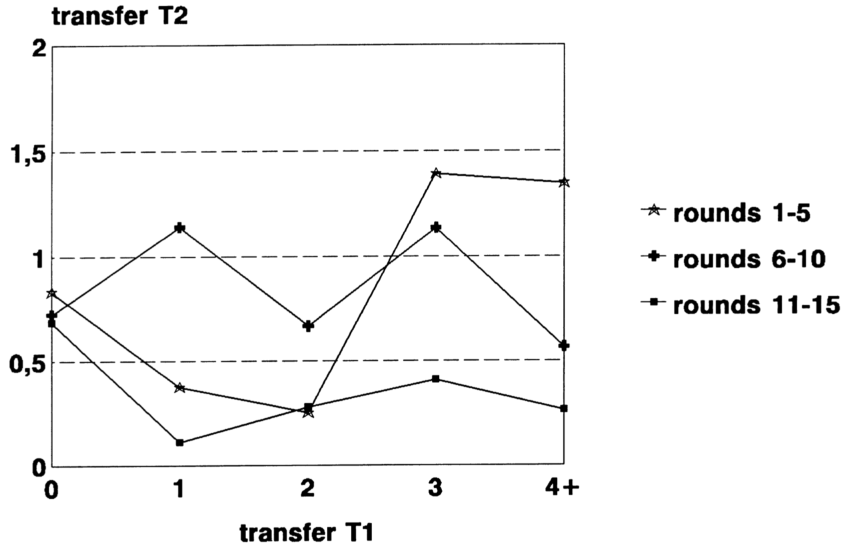

Fig. 3. Transfer conditioned upon the previous transfer in I.

A graphical representation of the relationship between T_2 and T_1 in treatment I gives the same picture. Fig. 3 depicts the "average reaction function" of players 2, that is, the average level of the gift returned (T_2) as a function of the gift received (T_1). As gift levels of $T_1 \in \{5, 6, 7\}$ are rare, we pool the response to these gifts with those to a gift of $T_1 = 4$. Furthermore, the figure displays the average reaction function separately for rounds 1–5, rounds 6–10, and rounds 11–15.

The figure shows few signs of reciprocity. In the earlier rounds 1–5, we observe that the average value of T_2 in response to values of $T_1 \in \{3, \ldots, 7\}$ is somewhat larger than in response to values of $T_1 \in \{0, 1, 2\}$. The difference is not very pronounced (about 0.8) and it is not significantly different from zero. Furthermore, the reaction function in the middle rounds (6–10) and the later rounds (11–15) is almost flat. On average, the gift returned is almost independent of the gift received. No systematic and significant relationship between gifts received and gifts returned is observed, as a strong form of reciprocity would require. These results are in line with the results of Berg et al. (1995) (in their no-history treatment), and the replication by Ortmann et al. (1996).

How about signs for a weaker version of reciprocity, more in line with the definition in Berg et al. (1995) p. 126? This would require that, for any value of $T_1 \in \{1, \ldots, 4\}$, player 2 makes player 1 at least as well-off as when player 1 had played $T_1 = 0$. By playing $T_1 = 0$ player 1 ensures herself of a minimum payoff of 9. It appears that in 68.7% of the cases in which the first player plays $T_1 \in \{1, \ldots, 4\}$, the first player actually earns less than 9. Hence, in 31.3% of the cases the second player responds to $T_1 \in \{1, \ldots, 4\}$ with $T_2 > 0$. This compares somewhat unfavorably to the 46% reciprocal plays in Berg et al. (1995). [10] The *average* pay-off to player 1 when playing $T_1 \in \{1, \ldots, 4\}$ is 10.54 which is more than the *minimum* pay-off of 9 which player 1 can get by playing $T_1 = 0$. However, the *average* pay-off to player 1 of playing $T_1 = 0$ is 15.73. Given the average reaction of the second player to the gift of the first player, it would be in player 1's self-interest to give a transfer of zero. Moreover, in treatment N the percentage of players 1 who earn more than 9 when playing $T_1 \in \{1, \ldots 4\}$ is 39%, which is even higher than in treatment I. In treatment N, by construction, reciprocity cannot be the reason for this outcome. This in turn makes it doubtful that (weak) reciprocity is a main force in treatment I.

Hence, we find no signs for 'strong' reciprocity by player 2 in treatment I, and some signs for 'weak' reciprocity (though fewer than in Berg et al., 1995).[11, 12] At the same time, we find that the average level of gifts by player 1 is significantly higher in treatment I than in treatment N. This could suggest that players 1 in treatment I at least place considerable *trust* in reciprocal gift giving by players 2. The data on expectations lend only limited support for such trust, however. For example, on average players 1 in treatment I expects a return gift of $T_2^e = 1.46$ when they play $T_1 = 0$ and they expect $T_2^e = 1.87$ when they play $T_1 \in \{1, \ldots, 4\}$; a positive but very moderate effect. A similar

[10] It does, however, compare favorably to the mere 15% of reciprocal players 2 found by Ortmann et al. (1996) in their replication of Berg et al. (1995).

[11] The difference between the two treatments cannot be explained by reciprocity based on expected gifts by the other player (rather than observed gifts by the other player). If players feel an obligation to reciprocate the *expected* gift by the other player, then this should apply to the (first) players in both treatments. However, the average expected gift by the first player in treatment I (1.79) is not significantly different from the average expected gift in treatment N (1.57) and cannot explain the large difference of T_1 in treatment I (2.10) and treatment N (0.72).

[12] Another way in which reciprocity could manifest itself in *both* treatments, is through a positive (cor)relation between the gift given in a particular round and the gift received in the *previous* round. After all, a subject has a positive probability of meeting the same subject in the next round(s), and might feel obliged to reciprocate earlier gifts (even though a player cannot know to whom he is actually matched in any round). For this form of reciprocity *across rounds* we find limited support. For example, in both treatments the average gift in round t is about 0.4 larger if in round $t-1$ a gift $T_{t-1} \in \{1, \ldots, 4\}$ was received rather than a gift of $T_{t-1} = 0$.

finding is reported in Dufwenberg and Gneezy (1996), who elicit (and reward) beliefs in a game similar to Berg et al. (1995). In their study only 14 out of the 31 players who give away money expect to be rewarded by player 2 (and only 11 out of 31 actually get rewarded by player 2).

Furthermore, the average Pearson correlation coefficient between the gift provided (T_1) and the expected return gift (T_2^e) is significantly positive in treatment I ($r = 0.34$, $p < 0.01$). [13] This positive correlation, however, rests entirely on the initial rounds of the experiment. It is as high as $r = 0.62$ in rounds 1–5, but drops to $r = 0.08$ in rounds 11–15. Hence, in the early rounds there seems to be significant trust in reciprocity, but this trust disappears with repetition.

In view of these results it is interesting, and perhaps surprising, that the decline of the first gift (T_1) over the rounds is not more pronounced in treatment I. Though the subjects seem to have become well aware of the fact that gifts are not being reciprocated, it is as if they nevertheless keep trying. This remarkable result is reminiscent to the one found in Forsythe et al. (1995); see also Fehr et al. (1995). Forsythe et al. study an experimental market in which a seller is endowed with an asset, the quality of which is private information to the seller. Sellers can send a cheap talk message to buyers about the quality of the asset. The results reveal that sellers are quite willing to lie about the quality. In fact, the messages contain almost no information. More striking, however, is that buyers continue to place considerable trust in the messages of sellers. Consequently, buyers buy at too high prices and sellers gain at the expense of the buyers. This result explains the title of their paper: "Half a sucker is born every minute".

It should be noted that in our experiment (as in Forsythe et al., 1995), the subjects switch roles between rounds. The same subject who is the "exploitee" in round *t*, may be the "exploiter" in round *s*. The net effect of being exploitee in round *t* and exploiter in round *s* is positive compared with the outcomes in treatment N where the average gifts are lower: the average payoffs in treatment I (18.75) are somewhat higher than in treatment N (16.02). One might conjecture that this fact could explain the relatively slow decline of the level of T_1 over the rounds in treatment I. Subjects in the role of player 1 do not mind to be exploited by player 2, since they have a good change to be in the role of player 2 in the next round(*s*).

[13] In Treatment N the average correlation coefficient between gift T_i and expected return gift T_j^e is much lower at 0.15. We will come back to this in the discussion of Section 4.

To test this conjecture, we conducted five follow-up sessions with treatment I, in which the subjects did *not* switch roles. A subject was either the first or the second Decider in each of the 15 rounds. It turned out that the pattern and level of gifts in this treatment were almost identical to those in the treatment with random switching of roles. The average transfer of the first Decider was 2.14 and the average gift of the second Decider was 0.99. As a result the subjects playing the first Decider earned a lot less (13.06) than the subjects playing the second Decider (24.59). Thus, these additional sessions give no support for the hypothesis of 'alternating exploitation', and corroborate our conclusion that the (moderate) trust that the first player seems to put in the second player's obligation to reciprocate is too a large degree exploited by the second player. [14]

In summary then, we appear to be in the rather awkward position to reject both Hypothesis 0 (gamesmanship) and Hypothesis 1 (reciprocity). On the one hand, the possibility of monitoring a received gift and reciprocating with a return gift appears to have a significant (though moderate) positive effect on gift exchange, which is contrary to Hypothesis 0 but in line with Hypothesis 1(a). On the other hand, and contrary to Hypothesis 1(b), this positive effect is not mainly due to gifts being actually reciprocated. In other words, there seems to be (some) trust but hardly any reciprocity.

4. Bilateral versus overlapping gift giving

This section compares the results of the Poverty Game (i.e., bilateral gift giving), described in the previous section, with the results of our earlier Pension Game (i.e., overlapping gift giving, see Van der Heijden et al., forthcoming). A prime motive for studying the latter games, is the theoretical attention for overlapping matching structures, on the one hand, and the lack of empirical insight into their effect, on the other hand (Lucas, 1986). Though over-

[14] One might wonder whether there are large individual differences between the subjects, obscuring the aggregate picture. Perhaps some act as gamesmen, while others act reciprocally. Clearly, classifying the different subjects is a tedious exercise. Nevertheless, some 20 out of 80 subjects could be typified as strict gamesmen (12 in treatment N and 8 in treatment I); they choose a gift of 0 in at least 13 of the 15 rounds. On the other hand, 11 subjects (out of 40) in treatment I could be classified as strong reciprocators. In their role as second deciders in treatment I the correlation coefficient between gift received and gift returned was at least 0.50. However, according to this definition we also found 6 strong reciprocators in treatment N, which suggests that such occasionally high correlations may be a 'statistical coincidence'.

lapping matching structures are deemed important in many areas (see, e.g., Crémer, 1986; Sandler, 1982), they figure most prominently in the literature on inter-generational transfers. An issue that has received particular attention is the credibility problem of transfer schemes and pensions (Hammond, 1975; Kotlikoff et al., 1988; Sjoblom, 1985). Even if a transfer scheme, or any cooperative arrangement, is collectively optimal ex ante, its establishment may be hindered by suboptimality ex post. There is no guarantee that today's decisions will not be overturned tomorrow. You may take a cooperative attitude toward others today, but which mechanism will ensure you that others will take a cooperative attitude toward you tomorrow?

The question of cooperation with overlapping matches is, of course, closely related to the question of cooperation in bilateral relationships. Under both matching structures, cooperation seems to require a systematic relationship between your decision now and the decision of others later. It has been argued that also in real life, reciprocity is a prerequisite for the political and public support of particular social security and pension plans (e.g., Waller, 1989). In this respect it is interesting to note that Hammond (1975) describes the Poverty Game as one in which the players in turn are rich and poor, and the Pension Game as one in which the players are first "young" and then "old". In other words, the Poverty Game studies the support for unemployment or disability insurance schemes, whereas the Pension Game addresses the support for pay-as-you-go pension schemes. Hence, if it is true that also in reality the popular support for particular schemes depends on the presence of reciprocity, then it is interesting to study this support in the two different matching schemes experimentally. There are at least two features which make such a comparison non-trivial.

On the one hand, relationships in the Poverty Game are more direct than relationships in the Pension Game, which offers a better chance for reciprocity to be important. That is, it is more likely that subjects reward or punish in response to how they themselves have been treated than in response to how someone else has been treated (see also Güth and van Damme, 1994). A more direct relationship might thus lead to larger transfers in the bilateral setting.

On the other hand, in a (one-shot) bilateral game it is a dominant strategy for the second player to transfer nothing, while in an overlapping sequence no player has a dominant strategy to transfer nothing. There is always someone next, who might reward or punish you for how you treated the previous player. This absence of a dominant strategies in the overlapping game might thus lead to larger transfers.

Hence, (at least) two opposing forces could affect the transfer decisions in the two matching structures. A comparison of the experimental results can shed some light on the relative strengths of these forces.

4.1. Design of the pension game experiment

Eleven sessions of the experiment with an overlapping (OL) matching structure were run in January 1995. The procedure in the Pension Game experiment was similar to that in the Poverty Game experiment as much as possible (see Van der Heijden et al., forthcoming, for details), that is, subjects were recruited from the same pool, eight players participated in each session, and each session consisted of 15 rounds of play. Each round, the sequence of the eight players has been determined in a random way. Payoffs are again determined by Eq. (1), as the product of the two consumption levels (as Decider and as Receiver). The differences between the two games are most easily explained with the following picture. The arrows show who gives to whom, and the numbers indicate the order in which the players act (are the Decider). In a sense, with the bilateral structure a round consists of four times two periods, whereas with the overlapping structure it consists of eight periods, in seven of which a transfer decision is made (see below).

$$\text{Bilateral}: \quad P_1 \rightleftarrows P_2 P_3 \rightleftarrows P_4 P_5 \rightleftarrows P_6 P_7 \rightleftarrows P_8$$
$$\text{Overlapping}: \quad P_1 \leftarrow P_2 \leftarrow P_3 \leftarrow P_4 \leftarrow P_5 \leftarrow P_6 \leftarrow P_7 \leftarrow P_8$$

The two differences referred to above are evident from the picture. First, in the Poverty Game the relationship is more direct in the sense that you give to the same person who gives to you. We expect this feature to be conducive to reciprocity. Second, the Pension Game ties more players to each other. There is always someone next, who can reward or punish you for the way you have treated another player. In game-theoretical terms, no player has a dominant strategy to give nothing. This feature has been shown to make cooperation supportable (Nash) in an infinite sequence of finitely interacting players (Hammond, 1975; Salant, 1991; Smith, 1992).

In an experiment, of course, one cannot have an infinite sequence of players. So, the last player in the overlapping experiment has a dominant strategy of giving nothing. The backwards induction unraveling argument would then again predict all transfers to be zero. However, in finitely repeated games experimental subjects are sometimes seen to (learn to) employ 'trigger-like' strategies to support outcomes that are non-Nash in the stage game (e.g., Axelrod, 1984; Selten and Stoecker, 1986; Camerer and Weigelt, 1988).

Table 2
Average gifts (and standard deviation) by matching structure and information treatment

	Treatment N	Treatment I	Significance
BL (bilateral)	1.01 (1.06)	1.41 (1.16)	$p = 0.08$
OL(overlapping)	1.90 (0.72)	1.83 (0.51)	$p = 0.93$
Significance	$p = 0.08$	$p = 0.27$	

Similarly, if such strategies are employed and anticipated in our game, they can lead to positive transfers even with a finite sequence of overlapping players. [15]

Again, two information treatments were run in the Pension Game experiment, namely with and without information about the previous Decider's gift. In the five sessions of the OL treatment without information (labeled OL-N) players were not informed about the transfers made by previous players in a round. In the six sessions of the OL treatment with information (labeled OL-I) players were informed about all transfers made by previous players in a round, before they made their own decision. For ease of reference, the bilateral treatments with and without information will now be denoted by BL-I and BL-N, respectively.

4.2. Results

Table 2 presents the transfer levels for the two matching structures and the two information treatments. The middle block presents the overall average transfer level by treatment (averaged over subjects, rounds, and sessions). The last column and the last row present the significance levels for the difference of the average transfer level across the information condition and the matching structure, respectively. [16] Figs. 4 and 5 show the development of

[15] Of course, in the experiment the sequence of players has to be started and stopped. The first player in the sequence, P_1, only plays the role of the receiving player. The final player in the sequence, P_8, only plays the role of the giving player. No experimental standard has been developed yet on how to deal with this issue. In the experiment we chose to set player P_1's first period consumption equal to the basic endowment of 2. Player P_8's received gift was set equal to the average transfer to all previous Receivers (rounded up). To a large extent this starting and stopping rule is an arbitrary matter.

[16] If not otherwise indicated all tests are two-tailed Mann–Whitney tests with session aggregates as units of observation. These sessions aggregates for the OL experiments can be found in Table 7. Frequency tables of all $T_t - T_{t-1}$ combinations can be found in Tables 8 and 9 for treatment OL-N and OL-I, respectively.

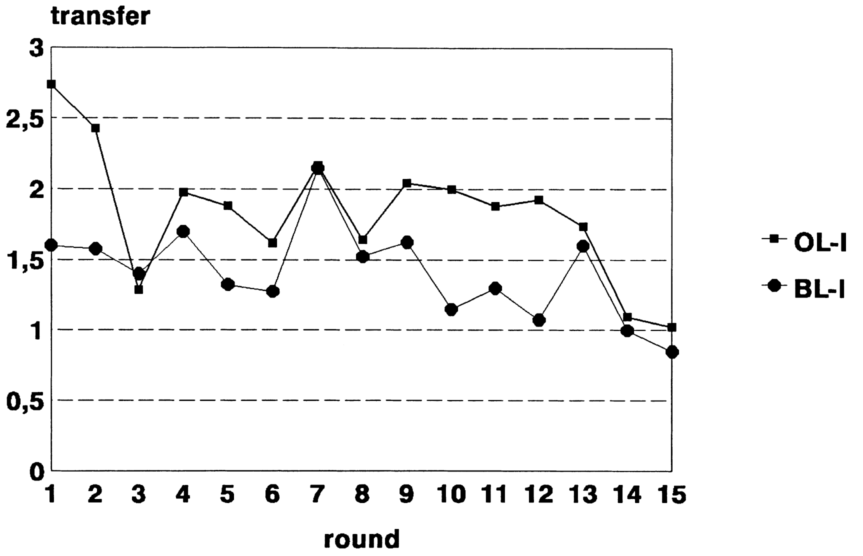

Fig. 4. Average transfer by round in treatment BL-I and OL-I.

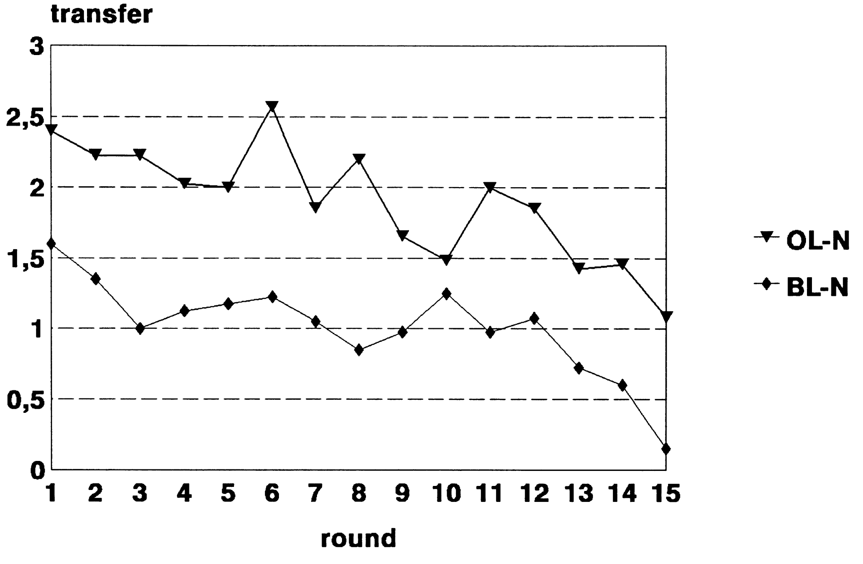

Fig. 5. Average transfer by round in treatment BL-N and OL-N.

the average transfer level by round in the treatments with and without information, respectively.

One can make two main observations. First, the transfer levels in the OL experiment are higher than in the BL experiment. Table 2 shows that at the session level the difference is significant only for the No-information treatments (BL-N versus OL-N). Fig. 4, however, shows that also in the Information treatments, the transfers in OL-I are higher than in BL-I in almost every round. [17] As a consequence, the average efficiency gain in the OL treatments is 70.6% (of the maximum of $16 = 25 - 9$), whereas it is 52% for the BL treatments. The OL structure is more conducive for transfers than the BL structure.

The second observation is that, contrary to the Poverty Game experiment, the information condition does not seem to have any impact on the average level of gifts in the Pension Game experiment. That is, it does not make any difference whether or not players are informed about the decision of the previous players in the sequence. This finding would suggest that the monitoring possibility (allowing for reciprocity) does not play an important role in treatment OL-I.

In examining the effect of monitoring we make again a distinction between strong and weak reciprocity. A closer examination demonstrates that hardly any signs of strong reciprocity are detected in the data. Analogously to Fig. 3, the link between the transfer of a player and the transfer of the previous player is only very weakly positive in treatment OL-I (see Van der Heijden et al., forthcoming, for more details). The average reaction functions do not exhibit a clear positive slope, neither in earlier rounds nor in the later rounds. Furthermore, although the Pearson correlation coefficient between a transfer by player P_t and the transfer made by the previous player in the round P_{t-1} is significantly positive, the overall correlation coefficient of 540 observations is rather small, 0.14. It moreover decreases across the rounds and is not significantly different from zero anymore in rounds 11–15. Signs for strong reciprocity are thus fairly small in treatment OL-I and cannot explain why transfers are higher than in treatment BL-I.

Regarding weak reciprocity it turns out that in treatment OL-I in 68% of the cases in which a player chooses $T_t \in \{1, \ldots, 4\}$ he earns a payoff greater

[17] A less conservative test with the average transfer by round or by subject as unit of observation would yield that the average transfer in Treatment OL-I is significantly higher than in Treatment BL-I at a level of significance $p < 0.01$.

than 9 (which he could ensure himself of by playing $T_t = 0$), which is more than double the rate of weakly reciprocal plays in treatment BL-I. Whether this is in fact due to weak reciprocity by the next player is again doubtful in view of the results in treatment OL-N. Also in this (control) treatment, in which by construction reciprocity cannot play a role, 70% of the players who play $T_t \in \{1, \ldots, 4\}$ earn a payoff larger than 9.

The absence of dominant strategies in the Pension Game cannot be a strong force either. Note that the absence holds only for treatment OL-I and not for treatment OL-N. In treatment OL-N all players in the sequence have a dominant strategy to supply a transfer of zero. However, as we have seen, the transfers in treatments OL-N and OL-I are not significantly different. Apparently, the absence of dominant strategies does not affect the level of the transfers in overlapping setting. [18]

Like in the previous section, the results put us again in a rather awkward position. We do find a significant effect of the matching structure, but the effect is not attributable to any of the two hypothesized features: the effect of reciprocity or the absence of dominated strategies. In the next section we put forward some possible, admittedly rather speculative explanations.

4.3. Discussion

One consequence of the OL structure is that in each round all players are connected to each other, either directly or indirectly. P_1 is affected by the action of P_2, who is in turn affected by the action of P_3 who is in turn affected by P_4, and so on. In the BL treatment the players interact in pairs and only two out of eight players are linked. Therefore, in the OL treatment the subjects actually play a repeated game; in each round they are in a game with the very same subjects. In the BL treatment, the subjects also play the game repeatedly, but the probability of being in a game with the very same subject from one round to the next is only $\frac{1}{7}$. It is possible that this feature induces repeated game considerations (reputation formation) in the OL game to a larger extent than in the BL treatments. There is some indication for this, but the evidence is not particularly strong. For example, the average decline in the transfer level from the first five rounds to the last five rounds is 0.61 in

[18] It is useful to mention here that there is no clear pattern of transfers within each round. In both treatment OL-I and OL-N, the average levels of the gift by the different players ($P_2, \ldots, P_8$) are almost identical and not significantly different from each other (with pairwise Wilcoxon tests).

Table 3
Average Pearson correlation coefficient between players' own gift and expected gift

	Treatment N	Treatment I	Significance
BL (bilateral)	0.15	0.34	$p=0.10$
OL (overlapping)	0.47	0.42	$p=0.93$
Significance	$p=0.01$	$p=0.54$	

the OL experiments and 0.45 in the BL experiments. The difference between the two treatments is not statistically significant, however. Furthermore, the difference in the *decline* of the transfer levels mainly rests on the difference between OL-I and BL-I, whereas the main difference in the *average* transfer level is the one between OL-N and BL-N (see Table 2).

Since the development of the transfers over time cannot explain the difference between the treatments, perhaps a closer examination of the reported expectations gives a hint at a possible explanation. Table 3 presents the average correlation coefficient between a player's own gift and the gift a player expects to receive for the two matching structures and the two information treatments. [19]

It turns out that the correlation coefficient is significantly higher in treatment OL-N than in treatment BL-N. At the same time, these average correlation coefficients for treatment OL-I and BL-I are much closer and not significantly different. Furthermore, the difference between OL-N and OL-I is not significant, whereas the difference between BL-N and BL-I is (marginally) significant. Interestingly, this pattern of correlations across the four treatments coincides exactly with the pattern of average transfer levels (see Table 2).

The question then is: How should these differences in expectations be interpreted and explained? In the information treatments (OL-I and BL-I), a positive correlation between a player's own gift and the expected gift could be interpreted as "trust in reciprocity": when I give more, I expect to receive more. In the no-information treatments (OL-N and BL-N) such an interpretation makes no sense from a rational point of view. The next player is

[19] Correlations are calculated for each session separately and then averaged. For the OL treatments we look at the correlation between T_t and T^e_{t+1} ($t = 2, \ldots, 6$). In the BL treatments we look at the correlation between T_i and T^e_j (for $i \neq j = 1, 2$), where for BL-I we can only use the correlation between T_1 and T^e_2, since player 2 does not need to give an expectation since he is informed about T_1.

not informed about your transfer; hence he cannot reciprocate. An interpretation that makes sense, even in no-information treatments, is that subjects to some extent try to match the gift they expect to receive (cf. Liebrand et al., 1986; Rabin, 1993; Sugden, 1984). Hence, the question then becomes: why would subjects try to match the expected gift in the OL experiments to a stronger extent than in the BL experiments? A speculative explanation is the following.

As we noted above, the eight players in the OL experiments are in one game in each of the 15 rounds, whereas the players in the BL experiments switch opponent in each round. Furthermore, in the OL experiments a chain of players is tied together, whereas in the BL experiments the players act in pairs and there is less 'social structure'. It is possible that because of this feature the subjects in the Pension Game experiments consider themselves to be part of a group to a larger extent than the subjects in the Poverty Game experiments do. As social-identity theory suggests (e.g., Tajfel and Turner, 1986), when subjects consider themselves to be members of a group, more or less sharing a common fate, they are more inclined to take account of the group-interest than when they consider themselves as single individuals. If subjects in the OL experiments are more group-oriented this could explain why they are more inclined to try and match the gift they expect to receive than are the subjects in BL experiments. In group dilemmas it is often observed that subjects' degree of cooperation is strongly correlated with the expected degree of cooperation of the other group members. For example, Offerman et al. (1996) find that a subject's degree of cooperation is strongly correlated with her expectation of the degree of cooperation of other group members (see also Wit and Wilke, 1992). Interestingly, Offerman (1996) also finds that this correlation between action and expectation is stronger when subjects interact with the same partners repeatedly (like in our OL experiments), than when they switch partners after each round (like in our BL experiments). Hence, if the OL experiments elicit a more group-oriented attitude than the BL experiments then this is much in line with the observed difference in the correlation between gift expected and gift provided.

Furthermore, if the subjects' orientation in two matching structures differs, this could also explain why the impact of the information treatments works out differently. Generally, in bilateral (bargaining) situations, the strategic positions of players are strongly dependent on the information they receive (cf. Güth and van Damme, 1994). Also in the Poverty Game experiment, information turns out to have a significant impact. If the second player is informed about the first player's gift, this fact puts him in a more advantageous position. So, the monitoring possibility raises the average level of gifts. The trade-off between individual and group interest in a group dilemma, however, does not depend much on the information condition. In our Pension Game experiment the possibility of monitoring hardly affects the average level of gifts. [20] Even without information about previous players' gifts, subjects in an overlapping sequence still seem oriented toward voluntary gift giving.

5. Summary and conclusion

It is often argued that the reciprocity norm is one of the main vehicles that allow gains from cooperation to be realized, even in situations in which non-cooperation seems the more attractive alternative in terms of private incentives.

In the present study we have examined the force of the reciprocity norm in supporting cooperative gift exchange in experiments in which gift giving was mutually beneficial but individually costly. One innovation of the present study was to examine the extent to which reciprocity induces cooperation by comparing two information treatments in the Poverty Game. In both treatments, the subjects acted one after the other. In one treatment (I) the subject moving second was informed about the gift of the subject moving first. In the other treatment (N), however, the subject moving second was not informed about the subject moving first, and by design reciprocity was physically impossible because the second player could not react (although there is priority in time, there is no priority in information).

A second innovation was that we did not only study cooperative gift giving in bilateral relationships (Poverty Games) but also examined situations with an overlapping matching structure (Pension Games). In a bilateral match player 1 receives a gift from player 2 and player 2 receives a gift from player 1. In an overlapping match player 1 receives from player 2, who in turn receives from player 3, who in turn receives from player 4, and so on.

[20] Also in Erev and Rapoport (1990), the number of cooperative choices in a public-goods experiment does not depend on whether the players decide sequentially (with priority in time and information) or simultaneously. What is affected though, is the number of times the step-level public good is actually provided. The sequential move structure mainly acts as a coordination device, but does not affect the strategic positions of the players.

Overlapping matching structures pose special problems for cooperation, and have (only) received widespread theoretical attention.

Our analysis displayed the following main results. First, in line with the reciprocity hypothesis, and contrary to the hypothesis of strict gamesmanship, average gifts were (about 40%) higher in treatment I than in treatment N of the Poverty Game. The monitoring possibility (priority in information) increased the average level of gifts. This increase in average gifts mainly rested on the first player of each match, however. The first player seemed to place considerable trust in the second player's obligation to reciprocate gifts, but the second player basically decided to exploit this trust. Moreover, no systematic or significant (cor)relation between the gift received and the gift returned was found. Very few signs for reciprocity (of either a weak or a strong form) were visible in the data.

Second, in the Pension Game experiment the possibility of monitoring the gifts of the previous players in the chain did not have any impact on the average level of gifts. And, again, no significant or systematic (cor)relation between the present gift and the previous gift was found. Nevertheless, average gift levels were substantial and, moreover, higher than in the Poverty Game.

An admittedly speculative explanation for the latter result relates to social identity theory. In the Pension Game, all players in each round of the game are tied together, either directly or indirectly. In the Poverty Game, players interact in pairs, and there is less social structure. Perhaps these features elicit a more group-oriented behavior in the Pension Game than in the Poverty Game. This conjecture is in line with our finding that the correlation between subjects' own gift and expected gift is larger in the Pension Game than in the Poverty Game. Subjects appear to have a stronger inclination to match the gift they expect to receive in the former game. A more group-oriented attitude in the Pension Game is also in line with our finding that the information treatment – which affects the strategic position of the players – has a much smaller impact in the Pension Game than in the Poverty Game.

Irrespective of the validity of the latter explanation, we believe that our study at least warrants two recommendations. First, we found little force for reciprocity in the sense that 'what you give is related to what the other gave'. We did find some evidence, particularly in the Pension Game, for 'what you give is related to what you expect to get'. This also relates to a conditional obligation and Sugden (1984) calls it reciprocity. Although, of course, these two forms of reciprocity are related, they are not quite the same thing. One form of reciprocity may be possible in situations in which the other form is impossible. Furthermore, different situations may be conducive for a particular form of reciprocity. So, the issue of whether reciprocity exists as a social norm is rather complicated. As Dufwenberg and Gneezy (1996) remark: "The issue of when and in what sense reciprocity is important is apparently a delicate one, and more research seems necessary in order to disentangle the various aspects". The situation becomes even more complicated as terms like cooperative egoism, fairness, tit-for-tat, kindness, reciprocal altruism, and anticipated reciprocity are sometimes used synonymously and interchangeably, but at other times are intended to convey more or less subtle differences. As Kerr (1995) remarks "such terms often mean different things to different investigators and communication and comparison of findings become difficult".

Second, the vast majority of experimental inquiry focuses on bilateral interaction or interaction in fixed groups. In many situations, however, people's interactions partly overlap. This holds for inter-generational relationships, but also for interaction within and between organizations and networks. Theoretically, such interactions have been shown to raise special questions for the possibilities of cooperation (for example, with respect to social contracts, transfer schemes, sustainability of the environment). Our results demonstrate that these questions are even more intricate than (game) theoretical analysis suggests, and that they are worthy of further experimental investigation.

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Appendix A

The session data and frequency tables for the Poverty Game are shown in Tables 4–6 and those for the Pension Game are given in Tables 7–9.

Table 4
Average transfers and pay-offs (and standard deviations) for the Poverty Game sessions

	Session	T_1	T_2	P_1	P_2
BL-N	1	0.40 (0.82)	0.43 (0.83)	12.35 (7.41)	12.02 (7.00)
	2	1.53 (2.03)	1.32 (1.84)	17.70 (15.48)	19.87 (17.54)
	3	1.28 (1.35)	1.20 (1.30)	16.77 (9.99)	17.60 (11.13)
	4	0.78 (1.74)	1.17 (1.87)	18.22 (17.19)	14.38 (15.04)
	5	0.93 (1.02)	1.03 (1.09)	16.13 (8.42)	15.13 (8.01)
BL-I	6	1.87 (2.35)	0.47 (1.64)	10.78 (13.78)	24.78 (21.39)
	7	2.28 (1.50)	0.42 (0.72)	9.12 (3.88)	27.78 (12.28)
	8	1.63 (1.65)	1.10 (2.27)	15.82 (19.31)	21.15 (14.45)
	9	2.22 (1.52)	0.65 (1.09)	10.68 (6.26)	26.35 (12.63)
	10	2.50 (1.44)	0.97 (1.44)	12.87 (10.34)	28.20 (12.97)

Table 5
Frequency table of first and second transfers in treatment BL-N

T_1	T_2						Total (%)
	0	1	2	3	4	5–7	
0	100	32	22	11	4	6	175 (58.3)
1	20	7	7	5	2	0	41 (13.7)
2	16	4	9	6	3	0	38 (12.7)
3	12	3	9	3	0	0	27 (9.3)
4	5	0	1	0	0	1	7 (2.3)
5–7	7	4	0	1	0	0	12 (4.0)
Total (%)	160 (53.3)	50 (16.7)	48 (16.0)	26 (8.7)	9 (3.0)	7 (2.3)	300 (100)

Table 6
Frequency table of first and second transfers in treatment BL-I

T_1	T_2						Total (%)
	0	1	2	3	4	5–7	
0	78	0	2	1	3	7	91 (30.3)
1	19	3	1	0	0	1	24 (8.0)
2	35	7	3	0	1	0	46 (15.3)
3	42	3	7	14	0	1	67 (22.3)
4	40	7	4	4	6	0	61 (20.3)
5–7	9	2	0	0	0	0	11 (3.6)
Total (%)	223 (71.3)	22 (7.3)	17 (5.7)	19 (6.3)	10 (3.3)	9 (3.0)	300 (100)

Table 7
Average transfers and pay-offs (and standard deviations) for the Pension Game sessions

	Session	$T = \sum_{i=2}^{8} T_i$	$P = \sum_{i=2}^{8} P_i$
OL-N	1	1.23 (1.55)	17.84 (12.70)
	2	2.37 (1.47)	22.24 (9.65)
	3	1.00 (1.44)	16.29 (10.91)
	4	2.43 (1.59)	23.28 (11.65)
	5	2.47 (1.58)	23.23 (11.89)
OL-I	6	1.23 (1.20)	17.17 (9.05)
	7	1.68 (1.85)	19.98 (13.90)
	8	2.11 (1.85)	20.80 (12.28)
	9	1.31 (1.27)	18.41 (9.99)
	10	2.51 (1.56)	21.65 (9.69)
	11	2.13 (2.00)	21.90 (15.14)

Table 8
Frequency table of transfer and previous transfer in treatment OL-N

T_{t-1}	T_t						Total (%)
	0	1	2	3	4	5–7	
0	51	24	22	23	19	7	146 (32.4)
1	28	9	6	8	4	1	56 (12.4)
2	17	9	2	19	5	3	55 (12.2)
3	16	11	14	23	27	2	93 (20.7)
4	26	5	11	22	19	0	83 (18.4)
5–7	7	1	1	5	3	0	17 (3.8)
Total (%)	145 (32.2)	59 (13.1)	56 (12.4)	100 (22.2)	77 (17.1)	13 (2.9)	450 (100)

Table 9
Frequency table of transfer and previous transfer in treatment OL-I

T_{t-1}	T_t						Total (%)
	0	1	2	3	4	5–7	
0	69	26	33	19	23	4	174 (32.2)
1	22	11	15	8	2	6	64 (11.9)
2	39	12	37	10	10	3	111 (20.6)
3	23	6	10	20	9	4	72 (13.3)
4	25	8	11	11	35	4	94 (17.4)
5–7	9	3	6	1	6	0	25 (4.6)
Total (%)	187 (34.6)	66 (12.2)	112 (20.7)	69 (12.8)	85 (15.7)	21 (3.9)	540 (100)

Appendix B. Instructions for the Poverty Game experiment [21]

(instructions for the Pension Game experiment can be found in Van der Heijden et al., forthcoming).

B.1. Introduction (read aloud only)

You are about to participate in an experimental study of decision-making. The experiment will last for about 1 h. The instructions of the experiment are simple and if you follow them carefully and make good decisions you may earn a considerable amount of money. All the money you earn will be yours to keep and will be paid to you, privately and confidentially, in cash right after the end of the experiment.

{For the experiment it is of crucial importance to have 8 participants. However, experience shows that often 1 or 2 persons do not show up or do not show up in time. Therefore, we need to have 10 instead of 8 subscriptions. This sometimes has, as now, the consequence that too many participants are present and that 1 or 2 persons cannot participate in this experiment. These persons can still put their name down for one of the following experiments and receive *f* 10 for any inconvenience. These persons are determined by lot because one or two blank envelopes are added to the box with seating numbers, unless one of you checks in voluntarily not to participate in the experiment and receive *f* 10 instead.}

Before we go on with the instructions, I would like to ask all of you to draw an envelope from this box and open it. The number denotes the terminal you have to be seated. {If you draw a blank envelope you cannot participate in the experiment and you receive *f* 10.}

We will distribute the instructions of the experiment now and read through them together. After that, you will have the opportunity to ask questions. From now on, you are requested not to talk to, or communicate with, any other participant.

[21] The text between square brackets ([ ]) was added in information condition I. The text between brackets ({ }) was added when more than 8 participants showed up.

B.2. Instructions (distributed and read aloud)

B.2.1. Decisions and earnings

The experiment exists of 15 separate *rounds*. In every round, each of you will earn a certain amount of *points*. At the end of the experiment the points earned in the 15 rounds are added up for each participant separately. Every point earned is worth 5 cent (≈\$0.028) at the end of the experiment. In addition to this, all participants receive a fixed extra amount of *f* 5. Your total earnings will thus be equal to *f* 5 plus the number of points earned times 5 cent. Now, we describe how the points earned in each round will be determined.

In each round you will be matched with another participant. Each round will consist of two *periods*. In every round you have in one period the role of *Decider* and in the other period the role of *Receiver*. The earnings of a participant in a round are determined by the final amount of a participant in the period in which he or she is a Decider, and by the final amount of the participant in the period in which he or she is a Receiver. We denote the final amount when Receiver by E_{O} and the final amount when as Decider by E_{B}. The earnings in points of a participant in a round are determined by the product of the final amount when Receiver and the final amount when Decider. The earnings of a participant in a round are thus equal to $E_{\mathrm{B}} \times E_{\mathrm{O}}$ points. Next, we describe how the final amount when Decider E_{B} and the final amount when Receiver E_{O} are determined.

In each round the participants are first randomly matched two by two. After that the computer determines for each couple who will be the Decider and who will be the Receiver in the first period. In the second period the roles are reversed: the Decider in the first period is thus the Receiver in the second period and the Receiver in the first period is the Decider in the second period. The Receiver starts with an endowment of 1, whereas the Decider starts with an endowment of 9. The Decider has to decide which part of his or her endowment that he or she wants to transfer to the Receiver. This transfer, which we will denote by T, is 0 at the minimum, and 7 at the maximum. After the Decider has decided on the transfer T to the Receiver, the final amount of the Receiver is $E_{\mathrm{O}} = 1 + T$, and that of the Decider is $E_{\mathrm{B}} = 9 - T$. After the Decider has decided on her or his transfer to the Receiver, the second period of the round will be started, in which the roles are reversed.

In the second period, the other participant of the couple, who is the Decider now, will have to make a decision. The determination of the final amounts of the new Receiver and Decider in this period is similar to the previous period. The Receiver starts with an endowment of 1 and the Decider starts with an endowment of 9. The Decider decides again on the part of her or his endowment that will be transferred to the Receiver. This transfer T determines the final amounts of both participants in the second period: $E_O = 1 + T$ for the Receiver and $E_B = 9 - T$ for the Decider.

As said, your earnings in a round are determined by the product of your final amount E_B in your role of Decider and the final amount E_O in your role of Receiver. Your amount E_B depends on your transfer to the Receiver in the period you are Decider and your amount E_O depends on the transfer from the Decider to you in the period you are Receiver. To facilitate the determination of your earnings, you may use the table below.

The table states your earnings in points in a round dependent on the transfer *from you* to the Receiver when you are Decider and the transfer *to you* by the Decider when you are Receiver. In this table the rows present the transfer from you as Decider to the Receiver and the columns present the transfer to you as Receiver from the Decider. When you first look for the transfer *from you* in the row and then go to the right to the column stating the transfer *to you*, you can read your earnings in points, $E_B \times E_O$, for the round. The earnings in money are determined by multiplying the amount stated in points by 5 cents.

		Transfer *to you* from the Decider when you are Receiver							
		0	1	2	3	4	5	6	7
Transfer *from you* to the Receiver when you are Decider	0	9	18	27	36	45	54	63	72
	1	8	16	24	32	40	48	56	64
	2	7	14	21	28	35	42	49	56
	3	6	12	18	24	30	36	42	48
	4	5	10	15	20	25	30	35	40
	5	4	8	12	16	20	24	28	32
	6	3	6	9	12	15	18	21	24
	7	2	4	6	8	10	12	14	16

When the two periods of a round are over, so when both participants have decided on a transfer, a new round will be started.

B.2.2. Procedure and usage of the computer

After we have gone through the instructions, first a practice round will be run. After the practice round, the fifteen rounds will be run, which determine your earnings for this experiment.

In every round the computer, in a completely random manner, first determines who will be matched to whom. Then the computer determines, again in a random manner, for each couple who will get the role of Receiver and Decider in the first period. On the upper left part of the screen the Decider will see the number of the current round and the message "You are now Decider in the first period". Underneath the Decider will see the question "How much of your endowment do you transfer (0–7)?" The Decider has to type an integer from 0 up to and including 7. The number typed is the transfer T to the Receiver with whom she or he has been matched in this round.

Next, the current Decider will be asked the question "How much do you expect to receive?". Here, the Decider types an integer from 0 up to and including 7, dependent on her or his expectation about the transfer she or he expects to receive as Receiver in the next period. This expectation is used by us when analyzing the experiment, but your earnings will be unaffected by it. Besides, the other participants are not informed about your expectations stated.

After all Deciders have made a decision, the first period is over. In the second period the Receivers of the first period are now the Deciders. Every new Decider will see on the screen that in this round he or she is Decider in the second period [and how much he or she has received in the previous period]. Underneath the question is asked "How much of your endowment do you transfer (0–7)?". The Decider has to type an integer from 0 up to and including 7. The number typed is the transfer T to the Receiver with whom he has been matched in this round. When all Deciders of the second period have made a decision all participants will see how much they have received and what their earnings for the rounds are. These earnings are in points and are equal to the product of the final amount when Decider and the final amount when Receiver: $E_B \times E_O$. After one has been informed about this, the round is over and a new round will be started.

In the new round, the computer again determines first who will be matched with whom and next for each couple who will be the first Decider. So, you will not know with whom you will be matched in a particular round and whether you will be the first or the second Decider.

B.2.3. Summary

The experiment consists of 15 rounds, and every round consists of 2 periods. In each round the participants are randomly matched two by two by the computer. In each round every participant has in one period the role of Decider and in the other period the role of Receiver. When you are Decider your endowment is 9 and your final amount depends on your transfer T to the Receiver: $E_B = 9 - T$. When you are Receiver your endowment is 1 and your final amount depends on the transfer T by the Decider to you: $E_O = 1 + T$. Your earnings in points in a round are determined by the product of your final amount when Decider and your final amount when Receiver: $E_B \times E_O$. [After the first period of a round is over the new Deciders are informed about the transfer T which they have received in the first period.] After both periods of a round have been finished, everybody is informed about the transfer T to him or her and his or her earnings in that round.

The matching of the participants and the order in which participants are Decider in the two periods of a round are determined by the computer in a completely random way time after time. You will never be able to know whether you will be the first or the second Decider in a particular round, or with whom you are matched in a particular round.

B.3. Final remarks

After the last round, you will first be requested to answer some questions to evaluate the experiment. This questionnaire is anonymous. We can link your answers to your seat number but not to your name. After that, you will be called by your seat number to receive your earnings privately and confidentially. Your earnings are your own business; you do not need to discuss with anyone. It is not allowed to talk to or communicate with other participants during the experiment in either way.

On your table you will find an empty sheet, which you can use to take notes. Additionally, you will find a sheet labelled "REMARKS". On this sheet you can make remarks about the instructions or your decisions.

You get a couple of minutes to go through the instructions and to ask questions. When you want to ask something, please raise your hand. One of us will come to your table to speak to you.

After that we will start the practice round.

Are there any questions?

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