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Transfers and reciprocity in overlapping-generations experiments[*]

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Abstract
We experimentally investigate the development of voluntary transfers in an overlapping generations environment. By varying the information conditions of the game, we study whether the development of transfers is related to the possibility of future generations to monitor and reciprocate transfers of past generations.

The results display a fairly high level of voluntary transfers across generations, even when players are experienced. Furthermore, the level of transfers does not seem to depend much on the possibility of monitoring. Weak signs of cross-generational reciprocity are observed. Apparently, the public-good features of the experiment dominate the possibilities for intertemporal rewarding and punishing.

Keywords: experiments, overlapping generations, transfers, reciprocity.

JEL-classification System Numbers: C90, D63, H55

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1. Introduction

Despite the growing financial strain caused by the ageing of the population, pay-as-you-go public pension systems enjoy wide public support in developed countries. This raises the question of the determining factors of intergenerational transfer systems. Factors such as the political power of generations and altruism towards other generations can be key elements.[1] The support for public pension schemes can also be motivated by the idea that older generations should receive a fair rate of return on the pension contributions paid during their working lives.[2] A critical aspect of the support, however, is the perception by current generations of the support the system will generate from future generations. There is no guarantee that today's decisions will not be overturned tomorrow. This temporal credibility problem implies that decisions taken today must, in some way, be related to decisions to be taken by future decision makers. In some sense, the system must be self-enforcing. In this paper we intend to study the presence and source of such self-enforcing mechanisms in an experimental environment.

One mechanism that could explain a stable public pension system is the presence of a voluntary "social contract" between successive generations. Even if generations are not altruistic towards other generations, (implicit) social contracts with positive intergenerational transfers can be supported as a Nash-equilibrium.[3] Ingredients of such a contract are, first, the obligation to provide the elderly with a transfer equal to some prescribed level if they have adhered to the contract themselves and, second, a punishment rule if the elderly broke the social contract. One problem with this approach is that many alternative social contracts leading to a stable pension system can be conceived of. Which of these alternatives will occur depends upon the expectations held by successive generations and the way they solve any coordination problems.

A less sophisticated but related explanation is that successive generations "build up confidence" in the maintenance of the system by looking at the past performance of the system. It can be shown that, in theory, public transfers can converge to socially efficient levels, if the confidence in the system grows when successive generations keep following the scheme.[4]

Under both the social contract approach and the confidence-building approach, the relation between past and present decisions plays a central role. By monitoring the behaviour of past decision makers, current decision makers decide whether to support a transfer system. In reality,

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[1] See Verbon (1986) for the fact that the effects of altruism and political power can be blurred. Veall (1986) demonstrates that altruism in itself is not sufficient to engender intergenerational transfers.

[2] See Van der Heijden, Nelissen and Verbon (1995).

[3] See Sjoblom (1985), and Kotlikoff *et al.* (1988) for a related model.

[4] See Verbon (1987) and Van Dalen and Van Praag (1992) for this approach.

preferences and decisions are aggregated in a complex (political) process. Therefore, in empirical data on the development of pension schemes, disentangling any of the above mentioned factors will not be easy, if not impossible.

As in many areas of economics, a more detailed inspection of the determining factors of decision-making is possible (only) in a controlled experimental environment. In the present paper we study individual decisions about transfers in an experimental overlapping-generations (OLG) setting. We examine the development of transfers in a setting where player (generation) P_t decides about a transfer to player P_{t-1}. In turn, player P_{t+1} decides about a transfer to player P_t; then player P_{t+2} decides about a transfer to player P_{t+1}, and so on. A voluntary transfer system is induced to be collectively efficient in the experiment. Furthermore, intergenerational transfers allow for the smoothing of consumption over time. On the other hand, transfers are individually irrational because the direct private benefits of giving a transfer are negative.

The central question of our investigation is whether the possibility of the present generation to monitor and reciprocate the transfers of the previous generation(s) facilitates the development and stability of a voluntary system of transfers. To that purpose we employ two information treatments. In one treatment, players-generations are supplied with information about the transfer levels of previous generations. In the second (control) treatment they are not supplied with this information. So, if monitoring and reciprocity across generations adds to the development and stability of a voluntary transfer system, then this should show up in the difference between the two treatments.

The type of experiment we are using is related to the experiments on the voluntary provision of public goods. In the public-goods experiments there is also a tension between collective and individual rationality. The typical finding then is that contributions are clearly bounded away from the individually rational level of zero, but fall short of the collectively efficient contribution level (see Ledyard, 1994 for a survey of the literature on public-goods experiments). Public-good experiments, however, lack the intertemporal structure that is characteristic to decision making on intergenerational transfers.

Our experiments are also related to studies on the role of altruism and reciprocity in bilateral decision-making. In gift exchange experiments, for example, the responder's return on the proposer's gift is often found to be increasing in the size of the gift (Berg *et al.*, 1993, Fehr *et al.*, 1993). Similarly, in ultimatum bargaining experiments the probability that a proposal is accepted by a responder is increasing in the share of the cake the proposer is prepared to give to the responder (Güth and Tietz, 1990). So, reciprocity can be observed in a bilateral relationship if both sides have some power. Experimental evidence suggests, furthermore, that the latter condition is not only sufficient but also necessary for gift giving to occur. If the receiving side of the relationship has no power at all, the proposer's gift decreases drastically. For example, in Forsythe *et al.* (1994), the modal proposal is about 50% of the $5 cake to a receiving player with veto power but less than 10% to a receiver with no reciprocal power (see also Güth and Van Damme, 1994). The experimental games studied in the present paper are, in some sense, in between the dictator games with no reciprocal power and the ultimatum or gift exchange games with full reciprocal power. Reciprocity may be anticipated by the sender of a gift, but reciprocity (if at all) is not supplied by the receiver of the gift but by a third party, namely the next generation-player.

Our main aim is to contribute to the literature concerned with the occurrence and stability of cross-generational transfer systems, or, more generally, with the development and efficiency of 'voluntary social contracts'. Of course, it is not obvious how our experimental results would carry over to the complex (political) process, which determines intergenerational transfers. Nevertheless, by controlling the information on the history of transfers, we try to get more insight how the history of a system influences today's decisions on the system.

The paper proceeds as follows. The next section presents the underlying model and the two main questions of the experiment. Section 3 describes the experimental design. Results are presented in Section 4. Because we wanted to examine the robustness of the results, we have run two additional series of experiments. Section 5 briefly discusses the results of these additional sessions. Finally, Section 6 contains a concluding discussion.

2. An OLG model with transfers

The model that forms the basis for our experiments is a simple two overlapping-generations (OLG) model in which it is assumed that each generation consists of one player. Each player born after period zero lives for two periods. In the first period (when young), players are endowed with a fixed transferable endowment E and a fixed non-transferable basic endowment ϵ_1. Old players only receive a fixed non-transferable basic endowment ϵ_2. Young players decide about the transfer T to the old player, $0 \leq T \leq E$. The remaining part of the endowment is used for consumption. So, first period consumption C_{1t} of player P_t is given by:

$$C_{1t} = E - T_t + \epsilon_1 \tag{3}$$

Second period consumption C_{2t} of player P_t is given by:

$$C_{2t} = T_{t+1} + \epsilon_2 \tag{4}$$

where T_{t+1} is the transfer player P_t receives from player P_{t+1} in the second period of his life. Total utility U_t of player P_t, $t \geq 1$, is given by the following utility or pay-off function:[5]

$$U_t = C_{1t} \times C_{2t} = (E - T_t + \epsilon_1)(T_{t+1} + \epsilon_2) \quad (5)$$

The form of the pay-off function reflects the fact that consumption in both the first and the second period matter. The multiplicative form, in addition, implies that it is optimal to smooth consumption over both periods.

In the model there exists a tension between individual rationality and collective rationality or Pareto efficiency. The socially efficient stationary optimum, T^*, of our model can be calculated to be:

$$T_t = T^* = 0.5\,(E + \epsilon_1 - \epsilon_2),\ \forall t \quad (6)$$

Can this socially efficient outcome be established without commitment and without the help of a social planner? Or, indeed, can *any* positive transfer level be sustained in this setting? Of course, this will depend on the motivations and expectations of the players.

In this paper we examine whether the possibility to monitor previous generations facilitates the realization of collectively desirable positive transfer levels. To that purpose we employ two information treatments: treatment I (information) and treatment N (no information). In treatment I, each player-generation P_t is provided with information about the transfers of previous generations $(T_1,\ldots,T_{t-1})$ in the sequence. In (control) treatment N, players are not provided with this information. The main two questions of our inquiry then are:

Q1. Does the possibility to monitor the transfer levels of previous generations facilitate the occurrence of positive transfers?

Q2. Do we observe a systematic relation between the transfer level of the present generation and that of the next generation(s) in information treatment I?

The two questions are, of course, interrelated. An affirmative answer to question Q1 requires an affirmative answer to question Q2 almost by necessity. If monitoring facilitates the occurrence of positive transfers, then this facilitating role should come about through some form of reciprocity, that is, some (positive) relation between present and future transfers. To be more formal, consider

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[5] First period consumption C_{10} of player P_0 is assumed to be equal to the basic endowment, i.e. $C_{10} = \epsilon_1$. Hence, utility of the very first player P_0 is given by $U_0 = C_{10} \times C_{20} = \epsilon_1 \times (T_1 + \epsilon_2)$.

the following. A (non-altruistic) player/generation t maximizing expected utility faces the problem:

$$\max_{0 \le T_t \le E} (E+\epsilon_1-T_t)(\epsilon_2+T^e_{t+1}) \tag{7}$$

where T^e_{t+1} is player P_t's expectation about the next player's transfer. It is easily seen that a necessary condition for a positive transfer to be the outcome of this maximization is: $\partial T^e_{t+1}/\partial T_t > 0$. If there is a positive relation between present and (expected) future transfers, then present transfers might be positive. For example, if 'strict reciprocity' is anticipated, that is, $T^e_{t+1} = T_t$, the individually rational level of transfers can be calculated to be equal to the socially optimal level of transfers: $T_t = T^* = ½(E+\varepsilon_1-\varepsilon_2)$.

A more general specification, suggested by Van Dalen and Van Praag (1992), is the following:

$$T^e_{t+1} = T_t + \sigma(T_t - T_{t-1}),\ \sigma > -1 \tag{8}$$

where σ denotes the degree of confidence or the support expected by the next generation. The current generation is expecting to be rewarded for its own transfer, and, in addition, it expects to receive a premium for increasing the transfer level compared with that of the previous generation.[6]

Finally, we consider a specification that is more in line with the social contract approach, mentioned in the introduction. In particular, a form of reciprocity that is more strategically motivated is conceivable. Strictly speaking, in a finite OLG game (that is, *t* is finite) the only Nash-equilibrium is for each player to transfer $T_t = 0$. As we will see below, our experimental OLG game is finite. 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 (see, e.g., Selten and Stoecker, 1986, Camerer and Weigelt, 1988). Similarly, if such trigger strategies are (learned to be) employed and anticipated in our finite OLG game, they might lead to positive transfer levels. Consider the following special candidate:

$$T_{t+1} = \begin{cases} T^* & if\ T_t = T_t(c) \\ 0 & if\ T_t \neq T_t(c) \end{cases} \tag{9}$$

where

[6] A problem with this specification is that it is not consistent with expected pay-off maximization. It is easily checked that maximization of (5) subject to (6) leads to a solution for T_t which is usually inconsistent with the hypothesized relation in (6). In other words, if players' expectations about the transfers of others are in accordance with (6), their own transfers will not be in accordance with (6) under expected pay-off maximization.

$$T_t(c) = \begin{cases} T^* & if\ T_{t-1}=T_{t-1}(c) \\ 0 & if\ T_{t-1}\neq T_{t-1}(c) \end{cases} \tag{10}$$

The transfer $T_t(c)$ denotes the transfer at time t specified in the social contract c. Generations are punished for not providing the socially efficient transfer level T^*, unless the deviation from T^* was in order to punish the previous (deviating) generation. In other words, deviators and non-punishers are punished, but punishers are not. Clearly, if such a strategy is anticipated, then following the implicit contract would be optimal (for all but the last generation). Of course, the backward-induction unravelling argument would destroy this to be an equilibrium strategy. Nevertheless, as mentioned earlier, in finitely repeated games experimental subjects are sometimes observed to learn to employ such strategies.

Of course, other, more general or complex, forms of reciprocity are conceivable (cf. Sjoblom, 1985). Nevertheless, the main point remains: if monitoring is to facilitate the development and continuance of positive intergenerational transfers, then this must come about through some form of intergenerational reciprocity. Furthermore, if monitoring and reciprocity across generations adds to the development of voluntary transfers then this should show up as a difference between the two information treatments.

Three other remarks must be made. First, as we have argued, an affirmative answer to Q1 seems to require an affirmative answer to Q2. The reverse, however, need not necessarily hold. If we were to find a systematic positive relation between present and future transfers, then the average level of transfers might still be lower in treatment I than in treatment N. The reason for this is that with a positive relation between T_t and T_{t+1} low transfers in period t will be followed by low transfers in period $t+1$ (and $t+2$, $t+3$, etc.). So, if in the initial period or in some later period a low level of transfers is observed, this might lead to a low level of transfers after that period. Such a chain of low transfers owing to reciprocity can occur in information treatment I but not in information treatment N. However, if such an outcome were to be observed - that is, an affirmative answer to Q2 and a negative answer to Q1 - we would at least expect the (low) level of transfers in treatment I to be more "stable" than in treatment N. Therefore, in discussing the results, we will not only compare the average levels of transfers across the two treatments, but also the variability or stability of the transfers.

Second, it seems important to allow the subjects to learn and understand the structure of the OLG game. In our basic design we choose to have several repetitions (15) of an OLG game with a restricted sequence of generations (8) and no ’reincarnation’ (like Cadsby and Frank, 1990), rather than one OLG game with a long sequence of (say, 120) generations and reincarnation (like Marimon and Sunder, 1993). Consequently, the backward-induction argument of unravelling might apply to our design. The last generation in each OLG game might learn or realize that reneging is profitable. Because of backward unravelling, a decline of transfers within each OLG game might then be the result. Such a decline might become stronger with more experience. In discussing the results we will look whether such a pattern is visible in the data.

Finally, any design that allows for the possibility of learning or getting experience, simultaneously allows for the possibility that reputational considerations enter the picture. Usually, there is little hope to disentangle these two effects.[7] We will not pay too much attention to this matter since the purpose of our paper is to study the effect of monitoring and reciprocity as they reveal themselves in a comparison between treatment I and treatment N.

3. Design

Eleven experimental sessions, based on the model described above, have took place on January 9, 10 and 11, 1995. Five sessions employed information treatment N (no information) and six sessions employed information treatment I (information). Subjects were students recruited from Tilburg University with the announcement that the experiment would last for about an hour and that they would earn anywhere between 7 and 50 Dutch Guilders (i.e. between \$ 4 and \$ 29). No subject participated more than once, and most of them had no experience with economic experiments. Eight subjects participated in each session.

Upon arrival, subjects were randomly seated behind computer terminals, which were separated by partitions. Instructions were distributed and read aloud (see Appendix). Then subjects were given some three minutes to study the instructions more carefully and ask questions. After that, the experiment was run. Upon completion, subjects were asked to fill out an anonymous questionnaire about some background information (gender, age, major, motivation). Finally, subjects were privately paid their earnings in cash.

In each session the following parameters were used (see the model in the previous section): E=7, ϵ_1=2 and ϵ_2=1. Each session consisted of 15 rounds and one practice round. Each round consisted of a sequence of eight periods (0-7). Period 0 is an auxiliary period in which the first

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[7] Some clues about the relative effects of experience and reputation might be discernible in the data, however. For example, learning curves are typically steep in the beginning and become flatter with more experience. Effects of learning would then be strongest in the early rounds of the experiment. Effects of reputation, on the other hand, would reveal themselves most strongly in the last rounds of the experiment, as subjects would then be observed to ’cash in’ on their reputation.

'old' player was randomly selected from the eight participants. As no decisions are made in period 0, it will be excluded from the analysis. In each subsequent period (1-7), one of the remaining subjects was randomly selected to be the young player in that period.[8] The young player had to type a number T out of 0,1,...,7, which denoted his transfer to the old player. First-period consumption of the young player then was C_1=9-T. Second-period consumption of the old player was C_2=1+T. The old player was informed about the transfer received and her pay-off (in points) in the round: $U = C_1 \times C_2$. The young player became old in the next period and a new young player, chosen randomly from the remaining players, had to make a transfer decision. This procedure was repeated until period 7. Then all players had participated in the round, and a new round was started. After the last round, the points earned in the 15 rounds were added and converted into money at a rate of 1 point = 5 cents. In addition, each player received a lump sum (participation) payment of ƒ5. All aspects of the procedure were common knowledge.

The two information treatments differed as follows. In the no information treatment N, a player, when selected to enter the round and make a decision, was only informed about the period number t ε (1,..,7). In information treatment I, a player was also informed about the transfer decisions made by the players in the previous periods of the round (T_1,..,T_{t-1}). Note that in both treatments, a player was informed about the transfer made to him and his pay-off (in points) for the round when he left the round.

Some additional remarks have to be made with respect to the procedure. First, note that the player, selected to be old in period 0 of a round, did not play the role of the young generation in that round. Her first-period consumption (when young) was then fixed at C_1=2 (=ϵ_1). Similarly, the player, selected to be young in the last (7th) period of a round, did not play the role of the old generation of that round. His second-period consumption was then determined as C_2=1+T', where T' is the average transfer received by all previous old players in the round (rounded up). All of this was common knowledge. Second, to facilitate the computation of the pay-offs, subjects could use a pay-off table, which was included in the instructions. Thirdly, in each period 1-6 a player was also asked to type a prediction (0,..,7) regarding the transfer he expected to receive from the player in the next period. Subjects were not paid to make (accurate) predictions. Therefore, we do not intend to use these predictions extensively in the analysis. Finally, as mentioned earlier, the complexity of the OLG game requires the possibility for familiarization and learning by the subjects. In the (sparse) literature on OLG experiments, basically two designs can be distinguished.

[8] Of course, in the experiment we did not use terms like 'young' and 'old' generation, but referred to these as Decider and Receiver, respectively.

Lim *et al.* (1994) and Marimon and Sunder (1993) use a design that consists of one single, long OLG game in which subjects enter several times. Here, one could speak of a 'single OLG game with reincarnation'. Cadsby and Frank (1990) use a design that consists of a repetition of shorter OLG games, where in each game subjects enter only once. Hence, one can speak of 'repeated OLG games without reincarnation'. The advantage of the former design is that an OLG sequence has to be started and stopped only once. However, effects of previous generations' transfers, which are our prime interest, might be better discernable across the periods when there are more repetitions of the OLG game. In our basic design we have opted for a repeated OLG game without reincarnation.[9] To check for the robustness of the results, however, we have also run a series of five experimental sessions with one long OLG game with reincarnation (see Section 5).

4. Results

First, we will present the results regarding the level, development and stability of the transfers for both treatments (Q1). Then we will look more closely at the relation between present transfers and previous transfers (Q2). Finally, we discuss results regarding end-effects, experience and idiosyncrasy.

4.1 Development and variability of transfer levels

Recall that we have five (six) sessions with treatment N (I), and that in each session we have 15 repetitions of an OLG game consisting of 8 periods-generations with 7 transfer decisions. The overall average transfer, that is averaged over sessions, rounds and periods (1-7), is 1.90 in treatment N (no information) and 1.83 in treatment I (information). So, at the aggregate level, hardly any difference between the information treatments is visible. In fact, the transfer level is somewhat lower in treatment I, but the difference is not significant (p=0.93, with a two-tailed Mann-Whitney U test with session averages as observations, n_N=5, n_I=6).

The average level of transfers of about 2 might seem low compared with the socially efficient stationary level of T^*=4, but in terms of pay-offs the level of efficiency is rather high. A stationary level of transfers of T^*=4 leads to a pay-off (in periods 1-7) of 25 points. A level of transfers of T=0 leads to a pay-off of 9 points. The actual overall average pay-off (in periods 1-7) is 20.5 in treatment N and 20 in treatment I. Hence, a stationary transfer level of T^* leads to an efficiency gain of 16 (=25-9) points, of which about 11 (=20-9) are actually realized in the experiment. In

[9] Two differences with the design of Cadsby and Frank are (a) that in our experimental setting eight, instead of two, generations participated in each OLG game, and (b) that the order in which the subjects participated in each OLG game was random in our design but fixed in Cadsby and Frank's.

other words, the voluntary transfers observed in the experiment achieve an overall efficiency level of almost 70%.

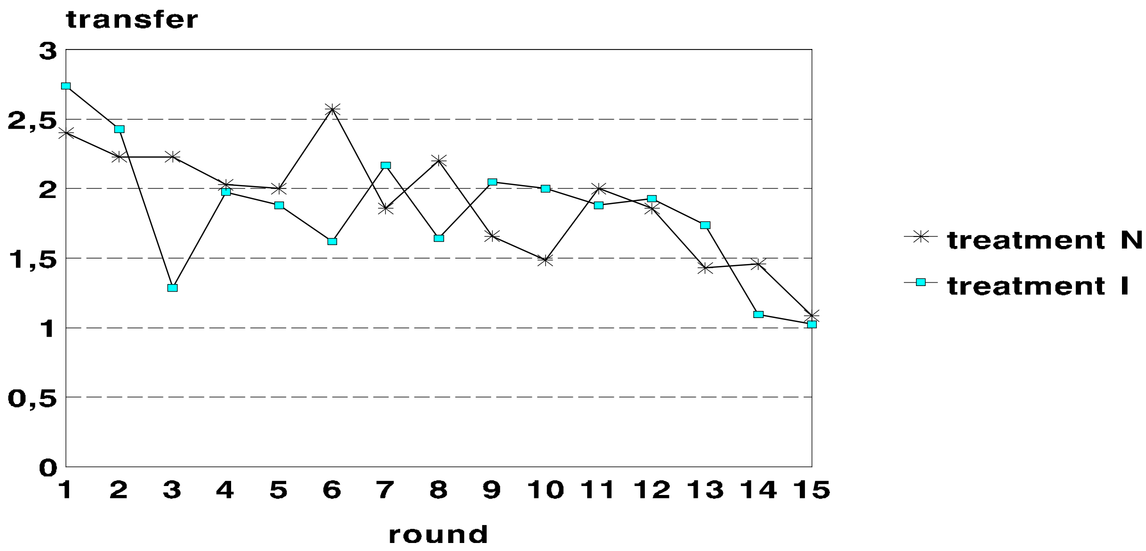

Figure 1: Average transfer per round per treatment

Figure 1 presents the level of transfers for each round of the OLG game (averaged over periods and sessions). The development of the average level of transfers over the rounds appears to differ barely between the two treatments. For both treatments, the average transfers seem to decrease in the early rounds (1-3), remain almost constant during the middle rounds (4-12), and decrease again in the final rounds (13-15). The average transfer levels in these three subsets of rounds are 2.29, 1.96, and 1.32 for treatment N, and 2.15, 1.90, 1.29 for treatment I. The decline over time is statistically significant. That is, the average transfers in rounds 1-3 differ significantly from those in rounds 4-12, which, in turn, differ significantly from those in rounds 13-15 (Wilcoxon matched-pairs signed-rank tests with session averages as observations). Differences between the two treatments are not significant in any subset of rounds.

Figure 2 gives some more details on the distribution of the transfers in both treatments. From the histogram it follows that transfers of 0 are most frequent in both treatments, namely more than 30%. Furthermore, all transfers between 1 and 4 occur rather often. The distributions are very similar, although some differences exist between the distribution of the transfers in both treatments. Compare, e.g., the frequencies of the transfers 2 and 3.

Finally, we look at the stability of the voluntary transfer system. As argued in Section 2, the possibility of monitoring previous generations might affect the stability of the transfers, be it at a high or at a low level. If, in treatment I, the early generations start with a low (high) level of transfers, then this low (high) level might be followed by subsequent generations. No such following pattern is possible in treatment N.

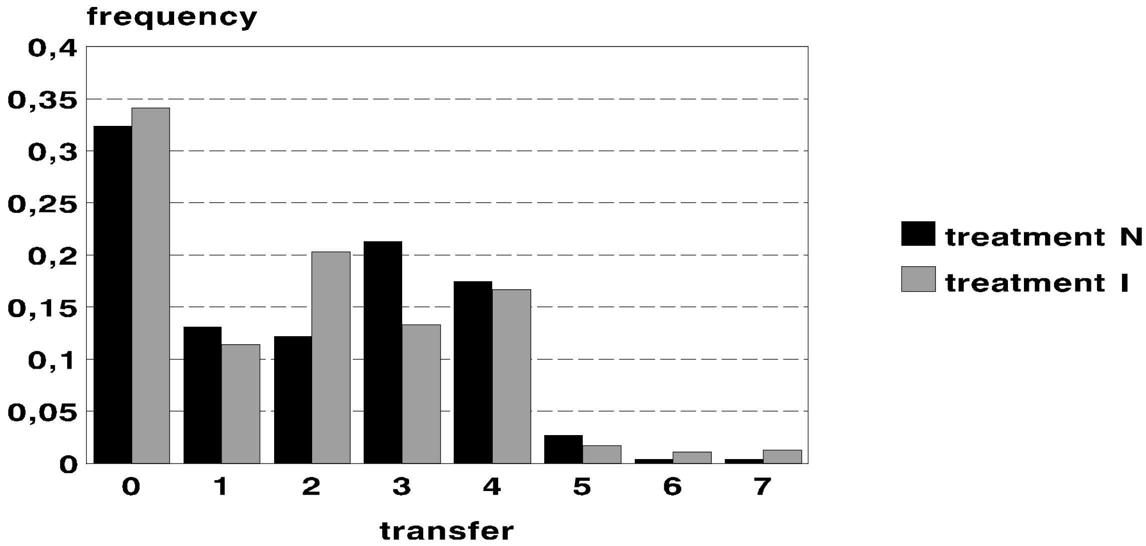

Figure 2: Histogram of the transfers in treatments N and I

Figure 3 presents the average coefficients of variation of the transfers per round, averaged over the sessions in each treatment. No systematic difference in the variability between the two treatments is found. The weak increase of variability is due to the combination of the (weak) decline in the transfer levels and an almost constant standard deviation over the rounds.

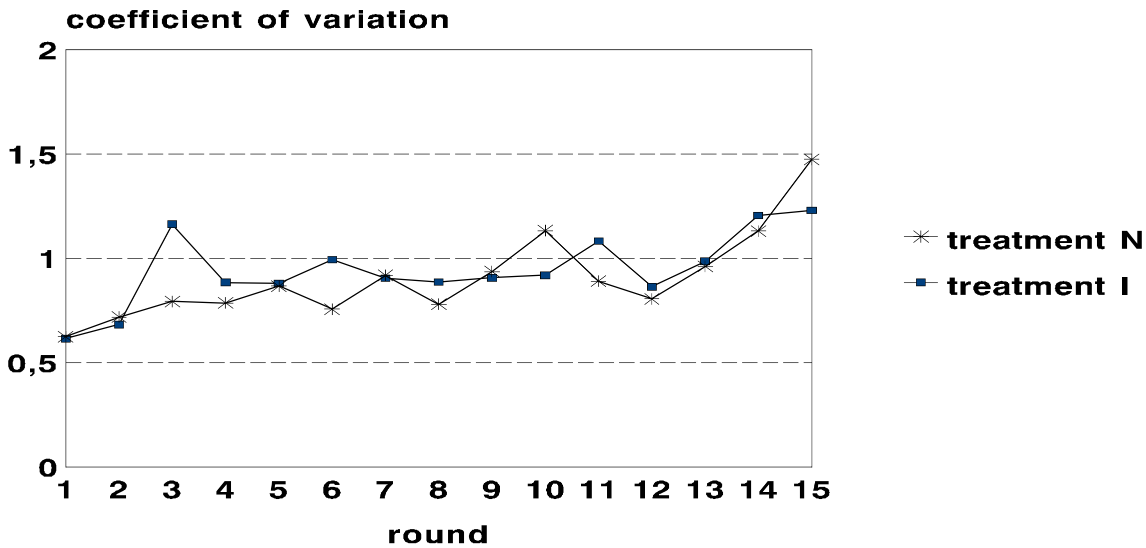

Figure 3: Average coefficient of variation per round

In summary, no difference in the level, development, distribution or stability of the transfers between the two information treatments is detectable in the data. The answer to question Q1, whether information facilitates the occurrence of positive transfers, is clearly negative. Nevertheless, the average transfer level, of about 50% of the collectively efficient level, is rather high. Even in later rounds, the average transfer remains positive.

4.2 A close look at reciprocity

The results of the previous section do not point to strong indications of reciprocity across generations. The present section presents analyses that confirm this intuition.

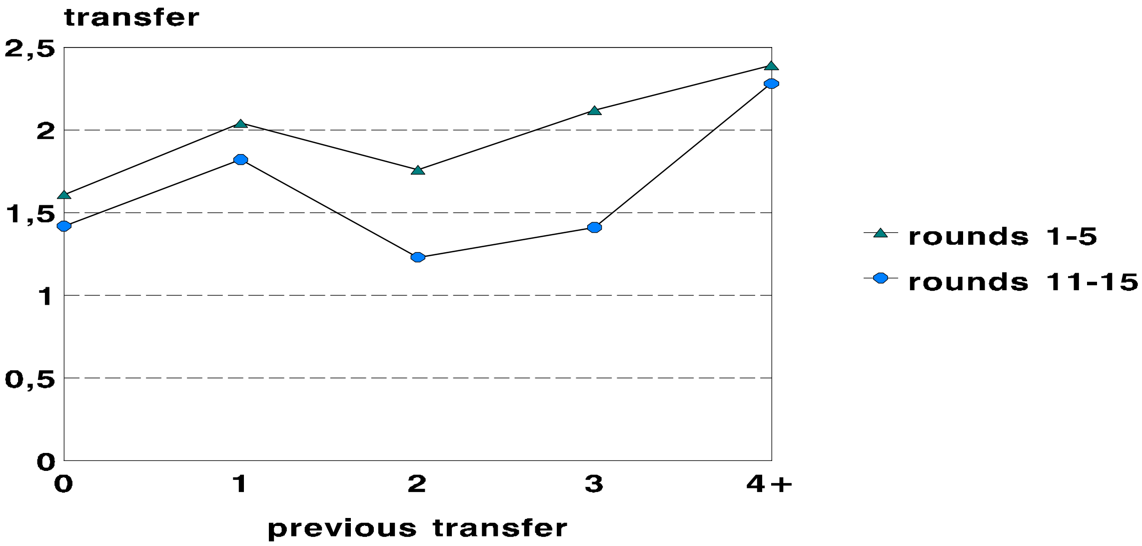

Figure 4: Transfer conditioned upon the previous transfer in the same round (treatment I)

For treatment I, Figure 4 displays the average levels of T_t as a function of the transfer of the previous generation T_{t-1}, $t \geq 2$. Because of their small number of observations, levels of T_{t-1} larger than four are pooled with transfers of four. Furthermore, we have performed this analysis separately for rounds 1-5 and rounds 11-15 in order to control for the decline of the average transfer level over the rounds, and in order to see how reciprocity develops over the rounds. Figure 4 suggests a weak positive relation between the transfers of the previous and the present generation.[10] The average transfer level after T_{t-1}=4+ is about 50% higher than the transfer level after T_{t-1}=0. However, the difference is insignificant at p=0.17 for rounds 1-5 and at p=0.14 for rounds 11-15 (two-tailed Wilcoxon matched-pairs signed-rank test with the (6) session averages as

[10] Note that Figure 4 by itself could be misleading. That is, given the fact that the average transfer is about 2, it could be that the (individual) data-points are concentrated around the points where the previous transfer is 2, so that no fair conclusions about the presence of reciprocity could be drawn. However, as we have seen in Figure 2, transfers are not clustered around 2. On the contrary, the modal transfer is zero, whereas the frequency of the transfers between 1 and 4 is about 10% to 20%. Finally, notice that Figure 4 presents the average value of the (present) transfers conditioned on the previous transfer, i.e. although the average transfer after a previous transfer of 0 is about 1.5, individual transfers between 0 and 4 are observed. Hence, it makes sense to talk about a weak positive relation in Figure 4.

observations). In addition, no monotonic positive relation between T_{t-1} and T_t is observed. For example, the average transfer after T_{t-1}=2 is lower than after T_{t-1}=1.

A comparison of reciprocity in the early rounds (1-5) and in later rounds (11-15) does no reveal a systematic difference (also, the relation for rounds 6-10 is similar). Although, as already observed above, the average level of transfers decreases over the rounds, the weak positive relation between T_t and T_{t-1} observed in rounds 1-5 becomes stronger nor weaker in rounds 11-15. Hence, there are weak signs of reciprocity, but the effect is not strongly 'intrinsic' (it is weak in the early rounds), nor is it 'learned' increasingly during the experiment (it remains weak in the final rounds).

One might argue that a monotonic relation between T_{t-1} and T_t is not what we should be looking for. If subjects are mainly strategically motivated, we should expect to observe 'trigger-like strategies'. As argued in Section 2, trigger strategies suggest that present transfers must be conditioned on the transfers of *all* previous generations. No version of trigger-like strategies is visible in the data, however.

Consider, for example, the following punishment rule (cf. equation 7). If player P_t observes a transfer of T_{t-1}=0, then he should not punish the previous player if $T_{t-2}<T_{t-2}(c)$, but he should punish the previous player if $T_{t-2}\geq T_{t-2}(c)$, where $T_{t-2}(c)$ is defined by eq. (8). We compared the average value of T_t after T_{t-1}=0 and $T_{t-2}<T_{t-2}(c)$, with the average value of T_t after T_{t-1}=0 and $T_{t-2}\geq T_{t-2}(c)$. Under trigger strategies, the least we should expect is that, on average, the former value is larger than the latter value. However, for each T^* ε {1,2,3,4} we find that the average transfer in case there should be a punishment is not distinguishable from the transfer in case there should be no punishment.

We also looked for more 'loose' versions of a relation between present transfers and transfers of the previous two generations. Again, no strong relations are detectable in the aggregate or individual data. A version which gives some positive results is the following (cf. equation 6). On average, transfers levels T_t are lower in case the previous player has decreased the transfer level ($T_{t-1}<T_{t-2}$), than in case she has not decreased the transfer level ($T_{t-1}\geq T_{t-2}$). The average transfer in the former case (1.68) is significantly lower than the average transfer in the latter case (1.81). The effect, though significant, is relatively weak and shows no strong development over time.

In summary, as expected from the negative answer to question Q1 in the previous section, only a moderate relation between present and previous transfers is found in the data of treatment I. Moreover, this relation seems to become stronger nor weaker with more experience.[11]

[11] In the experiment we also asked subjects' expectations about the transfer of the next generation (see Section 3). Two relations could be interesting here. Firstly, the relation between one's own transfer and the expected transfer (as a measure for 'anticipated reciprocity'), and, secondly, the relation between the

4.3 End effects, experience and idiosyncrasy

As argued before, in addition to the transfers of previous generations in the round, several other factors may affect the level of transfers and its development over time. In this subsection we briefly analyze end effects, experience, and individual idiosyncrasy.

First, our design consists of a repetition of finite OLG games. Subjects who enter an OLG game in the last period, know that they are last in the sequence of that round. Hence, backward-induction reasoning might induce them to renege on any (implicitly agreed upon) positive transfer level by previous generations. Moreover, due to the backward unravelling argument, we might expect to observe a gradual decline of the transfer level over the periods in each round of the OLG game.

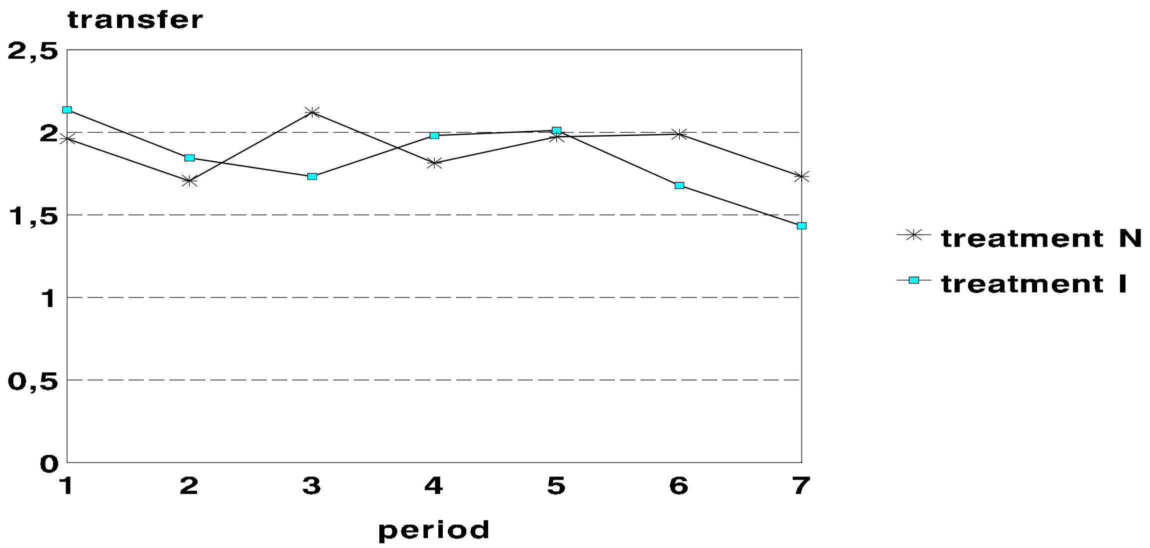

Figure 5: Average transfer per period per treatment

A weak final period end effect is indeed visible in the data. For both treatments, Figure 5 presents the level of transfers for each period of the OLG game (averaged over rounds). The average transfer level in period 7 is somewhat smaller than the average level over periods 1-6. When considering both treatments together, the difference is just significant (Wilcoxon matched-pairs signed-rank tests, $p<0.10$), but it is not significant for both treatments separately. Further-

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expected transfer and the actually received transfer (as a measure for the accuracy of the expectation). Overall, it appears that the first (cor)relation is stronger for treatment N (.57) than for treatment I (.41). Hence, reciprocity is anticipated more strongly if, by construction, it cannot be provided. What is more interesting, perhaps, is that this (cor)relation shows no systematic development over the rounds, that is, it becomes stronger nor weaker. The second relation is very weak (a correlation coefficient of about .10) in both treatments. Hence, subjects seem to be bad predictors. Again, the correlation shows no systematic development over the rounds. As subjects were not rewarded for making (accurate) predictions we do not wish to put much weight on these results, however.

more, no monotonic development over the periods of the OLG game exists. On average, earlier and later generations in the OLG game transfer about the same amount of their endowment.[12]

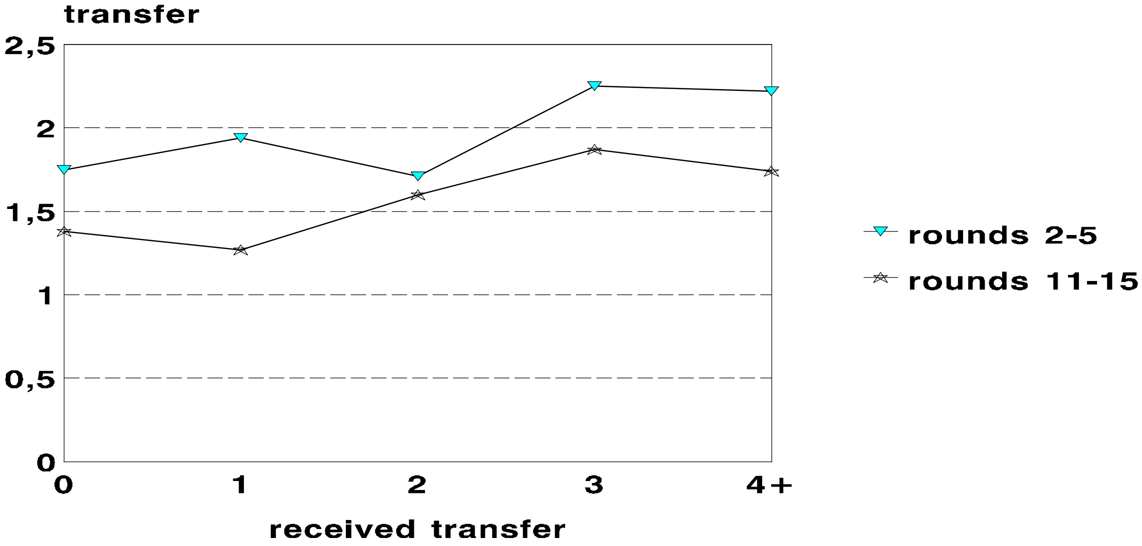

Figure 6: Average transfer as a function of the transfer received in the previous round (both treatments)

Second, it is to be expected that subjects adapt their behaviour in response to their experience in the previous round(s) of the experiment. A representative picture of the size of the effect of experience is given in Figure 6, which combines both treatments. Average transfer levels in round *R* (= 2,...,15) are related to the transfer level received in the previous round (*R*-1) of the OLG game. The figure shows that the average subject tends to increase her transfer somewhat if she has been ’treated well’ herself in the previous round. To control for the gradual decline of the transfer level over rounds (see Figure 1) the effect is displayed separately for early (2-5) and later rounds (11-15). This separation enables us to conclude that the increase is not substantially different in earlier or later rounds. Although in both treatments a slight positive relation can be observed, no significant difference can be found between the transfers given after receiving 0 in the previous round and after receiving 4, if the two treatments are considered together.[13]

Third, until now we have mainly focused on aggregated data. A look at the individual data, however, reveals that some 28 subjects followed an almost constant strategy. That is, in each

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[12] In both treatments, no strong development across the rounds can be observed. Therefore, the end effect does not become more important when players are experienced.

[13] When considering the treatments separately, only for rounds 2-5 in treatment N we found that the transfer made after receiving 0 in the previous round is significantly lower than that made after receiving 4.

round, they chose the same transfer level (0, 1, 2, 3 or 4) with at most three exceptions.[14] The presence of these 'obstinate' players could blur the (quantitative) effects of reciprocity, end-effects or learning. Therefore, we have repeated all the previous analyses for the subset of 60(=88-28) players with a non-constant strategy. The overall conclusions, however, hardly change. Some of the above effects become quantitatively stronger, but the differences with the full-group analysis are surprisingly small.

At the conclusion of this section, having some overall picture of the relative effects of the factors analyzed separately above seems useful. The easiest, and perhaps only, way to give such a picture is to regress transfer levels on all these factors. In particular, we ran OLS-regressions of the following behavioural equation:[15]

$$T_t^R = \alpha_0 + \alpha_1 R + \alpha_2 T_{t-1}^R + \alpha_3 (T_{t-1}^R - T_{t-2}^R) + \alpha_4 t + \alpha_5 R^{R-1} + \alpha_6 T^{R-1} + \eta \qquad (9)$$

The equation reads as follows. A subject's transfer T_t^R in period t (= 1,..,7) is supposed to depend on a constant (α_0), the round number (R), the transfer of the previous generation in the round (T_{t-1}^R), the difference of the transfers of the previous two generations in the round (T_{t-1}^R-T_{t-2}^R), the period number (t), the transfer received in the previous round (R^{R-1}), the transfer given in the previous round (T^{R-1}) and an error term (η). In sequence, these are the factors analyzed separately above.

Table 1 presents the regression results, for each treatment separately. The exogenous variables are in the first column. Parameter estimates and corresponding significant levels of the t-statistic are in the next two columns.[16] In treatment N, T_{t-1}^R and $T_{t-1}^R - T_{t-2}^R$ have not been included, as this information was not available for the decision-makers. The possibility of monitoring appears to influence the decision-making process and results in some reciprocity; in treatment I, T_{t-1}^R has a significantly positive effect, although its quantitative effect is small. On the other hand, we do not find any indication for the existence of a confidence parameter as suggested by Van Dalen and Van Praag (1992); the estimated coefficient of variable $T_{t-1}^R - T_{t-2}^R$ is insignificant. In treatment N, owing to the lack of information, decisions are mainly based on the transfer received in the previous round (R^{R-1}) and the transfer given in the previous round (T^{R-1}), the latter indicating some

[14] These 28 subjects are spread almost evenly over the two treatments. Furthermore, 12 of them chose for a zero transfer in each round (with at most three exceptions).

[15] In the analysis, it is assumed that all observations are independent, which is not the case, strictly speaking. Running regressions with individual data is rather common in experimental research, though. Furthermore, this analysis is mainly performed for illustrative purposes.

[16] Because of the regression specification, only the results in periods 3 to 7 of rounds 2 to 15 can be used.

presence of personal inertia or idiosyncrasy. The latter variable has also a significant effect under treatment I, whereas the former has no effect.

Table 1: Regression results of the behavioural equations for the two treatments

	treatment N		treatment I	
	value	p-value	value	p-value
α_0	0.92	0.01	1.17	0.00
R	-0.01	0.47	-0.01	0.68
T^R_{t-1}			0.15	0.02
$T^R_{t-1} - T^R_{t-2}$			-0.06	0.19
t	-0.05	0.32	-0.09	0.09
R^{R-1}	0.20	0.00	0.03	0.58
T^{R-1}	0.48	0.00	0.38	0.00
# obs.	350		420	
$\bar{R}^2$	0.30		0.17	

Concluding, the regression results show some small impact of monitoring and some presence of reciprocity. However, in line with our findings in the foregoing analysis, the effects seem to be very small. On average, subjects seem to balance the tension between the collective efficiency of a transfer scheme and the individual temptation to defect on such a scheme somewhere ’halfway’, with marginal adjustments in response to personal experience.

5. Two additional designs

To check on the robustness of the results, we ran two additional sets of experiments with information treatment I (in March 1995, with eight inexperienced subjects in each of the nine sessions). As these additional sets of experiments confirm the picture presented in the previous section, we do not dwell on them extensively.

First, the results described above might be sensitive to our choice for a ’repeated OLG games without reincarnation’. Therefore, we had five additional experimental sessions with ’one long OLG game with reincarnation’. These experimental sessions consisted of one OLG game of 120 (=15x8) periods. In this design, the OLG sequence has to be started and finished only once. The young player entering in each period was determined randomly with two restrictions (which were told to the subjects). First, each subject would enter the game fifteen times, and, second, a player who entered in a particular period could not enter in the next two periods. Furthermore, in line with information treatment I, a player entering a period was informed about the transfer levels in the preceding eight periods.

The results show that the average transfer level in this design (1.38) is lower than in our basic design (1.83). The difference, however, is not significant (Mann-Whitney *U* test with session averages as observations). Also in other respects the results are similar. For example, in each session the transfer level is relatively volatile, there is a slow decline with experience, and there are weak signs of reciprocity.

A second worry we had regarding our design, was that the strategy space was 'too large' to employ trigger-like strategies. In other words, the more options a player has, the more difficult the support of any implicit social contract might be. For example, people might be confused about the transfer level aimed at by a contract. Alternatively, it might be unclear to people whether transfer levels of, say, 2 should also be punished or that punishment should only occur with levels of 0 and 1. To simplify the development and employment of trigger strategies, we have run four additional sessions with transfer levels restricted to the set {0,4} instead of {0, 1, ..., 7} as in our baseline design. Information about previous transfers in a round was given. The design with restrictions, in a sense, solves the coordination problem for the subjects. If an implicit contract aims at a positive transfer level, it is clear what this level should be. Moreover, defining defection is much easier.

It turns out that the restriction of the strategy space results in lower average transfer levels (1.04) than in treatment I of the basic design (1.83). The difference is significant at $p<0.04$ (two-tailed *U* test with session averages as observations). Moreover, there is no indication of any increased success or even attempt to use trigger-like strategies. Under trigger strategies, the least we should expect is that the average transfer in case there should be a punishment is smaller than the transfer in case there should be no punishment. However, we do not find a significant difference. On the contrary, the weakly positive relation between T_t and T_{t-1} displayed in Figure 4, becomes even weaker. Average transfer in response to $T_{t-1}=0$ (1.04) are almost identical to average transfers in response to $T_{t-1}=4$ (1.02). What the restriction of the transfer set seems to do, is to make it more difficult for subjects to balance the tension between individual and collective rationality. On average, subjects seem to make a balance at a value of about 2, but restricting the choice to 0 or 4, seems to tip the balance downward.

In summary, the additional series of experiments confirms the general picture of our baseline experiment. Although average transfer levels are somewhat lower, they are still clearly bounded away from zero. More importantly, the absence of strong signs of reciprocity or trigger strategies in treatment I of our baseline design, is not due to the ’repetition of OLG games without reincarnation’ or the ’large strategy set’.

6. Concluding remarks

The main observation is that the average level of transfers only slowly decreases with the repetitions of the OLG game. This is in contrast with the general experience that the transfer level strongly declines after about five repetitions. It seems that the underlying OLG structure provides a kind of social cohesion in the experiments. This relation between players may explain our finding that the development and stability of the voluntary transfer system are not much facilitated by the possibility of present generations to monitor the transfers of previous generations. It could also explain why the average transfer is high in treatment N. Furthermore, only weak signs of reciprocity or ’trigger-like’ strategies are discernable in the data. Subjects seem responsive to the tension between the collective efficiency of a transfer scheme and the individual temptation to defect on such a scheme. On average, subjects seem to balance the tension somewhere ’halfway’, with marginal adjustments in response to personal experience.

It is interesting to relate these results to the typical results of both bargaining experiments and public-goods experiments. Although public-goods experiments lack the intertemporal structure that characterizes our experiment, there also, the typical result is that subjects balance the tension between individual and collective rationality somewhere halfway. With repetition and experience, contribution rates fall, but usually they stay clearly bounded away from zero. In bargaining experiments, a general finding is that the power of the receiving side is a prime determinant of the ’generosity’ of the proposing side. In bilateral relationships, the possibility to monitor and reciprocate is an important check on the power of the proposer. Our results, on the other hand, do not show a strong impact of the possibility to monitor and reciprocate. Hence, in our OLG experiment, the public-good feature may be more important than the inter-temporal and informational features.

We started our paper with the observation that, in spite of the financial strain on public pension systems their support does not seem to decrease. This raises the question of the basic motivation and the driving force behind the establishment and maintenance of intergenerational transfer systems. Theoretical explanations have hinted at the presence of a strong positive link between the decisions of past and present generations.

Although our experimental results show a weak positive link between present and past decisions, the aggregate support for the transfer system does not seem to depend strongly on this link. Individuals appear to have, on average, a generic willingness to support transfers to powerless individuals almost irrespective of past behaviour of these recipients. So, notions of confidence, saying to what degree the fruits of one’s own investment can be reaped, hardly play a role. In other words, the (private) insurance element is less important than the (collective) transfer element in transfer systems. It might be noticed here that in actual public pension plans the intertemporal relationship between present private costs and future private benefits is becoming increasingly unfavourable. However, provided that our experimental results have some ’external validity’, they suggest that shrinking support for these plans in European countries is less probable. The reason is that in these countries the emphasis is being placed on the public-good aspect of pay-as-you-go-financed public pension schemes. In the US, on the other hand, the investment character of public pension contributions is stressed, as witnessed by the existence of a trust fund for financing the US public pension system. Taking for granted that this implies that the transfer element is of minor importance in the US-scheme, this suggests that the inclination to support old individuals through the public pension plan may be declining in the US. In that case, the European schemes have a better chance of surviving the current demographic transition than the US-scheme has.

Appendix: Instructions

Introduction (read aloud only)
You are about to participate in an experimental study of decision-making. The experiment will last for about one hour. 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 ƒ 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 ƒ 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 ƒ 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.

Instructions (distributed and read aloud)[17]

Decisions and earnings
The experiment exists of fifteen 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 ƒ 5. Your total earnings will thus be equal to ƒ 5 plus the number of points earned times 5 cent. Now, we describe how the points earned in each round will be determined.

Each round will consist of seven *periods*. In every period two participants are involved, a so-called *Decider* and a *Receiver*. In each round of seven periods, every participant will, in principle, once have the role of Decider and once the role of Receiver. The earnings of a participant in a round are determined by the final assets of a participant in the period in which he or she is a Decider, and by the final assets of the participant in the period in which he or she is a Receiver. We denote the final assets as Receiver by E_O and the final assets as Decider by E_B. The earnings in points of a participant in a round are determined by the product of the final assets as Receiver and the final assets as Decider. The earnings of a participant in a round are thus equal to E_B x E_O points. Next, we describe how the final assets as Decider E_B and the final assets as Receiver E_O are determined.

In the first period of a particular round, two participants are randomly assigned by the computer to be Receiver and Decider. The Receiver starts with an endowment of 1, whereas the Decider starts with and 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 about the transfer T to the Receiver, the final assets of the Receiver are E_O=1+T, and those of the Decider are E_B=9-T. After the Decider has decided about her or his

[17] The text between square brackets ([]) is added in information condition two ("full information"). The text between brackets ({}) is added when more than 8 participants show up.

transfer to the Receiver, the next period of the round will be started. The participant who was the Receiver in the previous round is finished for this round.

In the next period, the Decider of the previous period will now be the Receiver. The new Decider is selected by the computer from the participants who have not yet taken turns in this round. The determination of the final assets 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 about the part of her or his endowment that will be transferred to the Receiver. This transfer T determines the final assets of both participants: E_O=1+T for the Receiver and E_B=9-T for the Decider.

Subsequently, a new period will be started in which the old Decider becomes the new Receiver and the new Decider is selected from the participants who have not yet taken turns. In this way, we continue up to and including the seventh period. After that, the next round of seven periods will be started.

Note that the participant who is Receiver in the first period of a round will not take a turn as Decider in that particular round. For this participant the final assets as Decider are determined to be E_B=2. Further, the participant who is Decider in the seventh period of a round will not take a turn as Receiver in that round. For this participant the final assets as Receiver E_O will be equal to the average final assets of all seven Receivers in that particular round, so including the current Receiver.

As said, your earnings in a round are determined by the product of your final assets E_B in your role of Decider and the final assets E_O in your role of Receiver. Your assets E_B are dependent on your transfer to the Receiver in the period you are Decider and your assets E_O are dependent 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 x E_O, for the round. The earnings in money are determined by multiplying the stated amount 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 you are the *first* Receiver in a round, your final assets as Decider are determined to be E_B=2. In that case, your earnings in points, E_B x E_O, only depend on the transfer from the Decider *to you* E_O=1+T. You can read these earnings from the table by looking for the column with the concerning transfer *to you* in the bottommost row (with transfer *from you* is 7).

When you are the *last* (seventh) Decider in a round, your final assets as Receiver E_O are determined as the average final assets of all seven Receivers in that round (rounded up). Your earnings in points, E_B x E_O, are determined via the table by the row with the transfer *from you* and the column of which the number equals the average transfer to all Receivers in that round.

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 that determine your earnings for this experiment will be run.

In every round the computer, in a completely random manner, determines who will get the roles of Receiver and Decider in the first period. On the screen the Receiver will see the message " You are the first Receiver". The Decider will see the number of the current period on the upper left part of the screen. [Next to it, you will see "INFORMATION until now". In the first period this information will only consist of the message "There have been no previous periods in this round"]. 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 current Receiver.

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 the Decider has taken her or his Decision, the current Receiver will see the number of the present period on the screen and underneath how much she or he receives and her or his earnings for the round. After the Receiver has taken note of this, he or she has to press Return to close the current period and to start the new one.

The Decider of the previous period becomes Receiver in the new period and the computer will select a new Decider from the participants who have not yet taken turns in this round. This new Decider sees the number of the current period on the upper left part of the screen [and next to it "INFORMATION until now". Underneath, it is reported for every decider of the previous periods how much he or she has transferred, how much he or she has received as Receiver and what her or his earnings are for the round. For the Decider of the previous period it is only shown how much he or she has transferred because this Decider is Receiver in the current period] and underneath the question "How much of your endowment do you transfer (0-7)?" After this decision has been typed and passed on to the current Receiver a new period will be started in which the Decider of the previous period will be the new Receiver. This procedure will be repeated up to and including period 7.

In all periods, a new Decider is randomly selected by the computer from all participants who have not yet taken turns in that round. After all seven periods in a round have been completed, the first period of the next round is started. Then, a new Receiver and Decider are again randomly selected by the computer for the first period and time after time a new Decider for the next periods is selected. Therefore, the order in which the participants take turns in every round is not fixed but is determined time after time by the computer in a random way. You cannot know when it will be your turn in a round. Moreover, you cannot know to whom you will be paired in a certain period.

Summary

The experiment consists of 15 rounds, and every round consists of 7 periods. In every period, two participants are involved, a Receiver and a Decider. The endowment of the Receiver is 1 and the endowment of the Decider is 9. The final assets of Receiver and Decider are dependent on the transfer T of the Decider to the Receiver: E_O=1+T, E_B=9-T. In every round, in principle, you are the Decider in one period, and the Receiver in the next period. Your earnings in points in a round are determined by the product of your final assets in the period you are Decider and your final assets in the period you are Receiver: E_B x E_O.

The participant who is Receiver in the first period will not act as a Decider in that round. His or her final assets as Decider are determined to be E_B=2. The participant who is Decider in the last period will not act as a Receiver in that round. His or her finals assets as Receiver E_O are determined as the average final assets of all seven Receivers of that round, so including the current Receiver.

In every period, only the current Decider and Receiver are informed about the size of the transfer given from the Decider to the Receiver. [instead of the previous line in information condition 2: In every period the current Decider receives information about the transfer of the deciders in the previous periods.]

The order in which the participants participate in the periods of a certain round is determined by the computer in a completely random way time after time. You will never be able to know when it will be your turn in a round or to whom you will be paired in a certain period.

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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