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NOTE

Cooperation in an Overlapping Generations Experiment

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Recent theoretical work shows that folk theorems can be developed for infinite overlapping generations games. Cooperation in such games can be sustained as a Nash equilibrium. But, of course, there are other equilibria. This paper investigates experimentally whether cooperation actually occurs in a simple overlapping generations game. Subjects both play the game and formulate strategies. Our main finding is that subjects fail to exploit the intertemporal structure of the game. Even when we provided subjects with a recommendation to play the grim trigger strategy, most of the subjects still employed safe history-independent strategies.

Journal of Economic Literature Classification Numbers: C72, C92, D90.

Key Words: overlapping generations; cooperation; trigger strategies; experiments.

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1. INTRODUCTION

The overlapping generations model, first introduced by Samuelson (1958), has become a standard tool in economics. It has been used to study a wide variety of issues, such as the sustainability of old-age pension schemes (Hammond, 1975), the time consistency of optimal tax policies (Kotlikoff et al., 1988), the relation between growth and environment (John and Pecchenino, 1994), the conditions for the optimal provision of club goods (Sandler, 1982), the relation between regulators and firms (Salant, 1995), and the interaction between old and young members of a political party (Alesina and Spear, 1988). In many of these applications, a central question is whether and under what conditions the efficient or cooperative outcome can be sustained as an equilibrium. Often it is found that, under certain conditions, this is indeed the case. For example, Hammond shows that an efficient pay-as-you-go pension scheme can be supported as an equilibrium outcome if the players employ trigger strategies. The possibility of cooperation in overlapping generations has been studied and established more generally. Folk theorems similar to the ones for “usual” infinitely repeated games may arise (Cremer, 1986; Salant, 1991; Smith, 1992).

In all of these studies cooperation is one of the equilibrium outcomes, but not the only one. As in infinitely repeated games, typically, there are many equilibria in infinite overlapping generations games. Sometimes additional criteria are advanced to argue that a particular equilibrium is more reasonable. For example, Hammond (1975) suggests that the players will realize that the cooperative equilibrium is in their own interest and that it is reasonable to assume that they will coordinate on this equilibrium. Usually, however, authors seem content to establish that a particular cooperative or efficient outcome is an equilibrium. Note, though, that players aiming at a cooperative equilibrium expose themselves to the risk of ending up with the worst-possible outcome: they cooperate while their successor defects. How actual players solve this dilemma is, of course, ultimately an empirical matter (Lucas, 1986).

In the present paper we “put the theory to the test.” We let subjects play a simple overlapping generations game. The structure of the game is identical to the Pension Game studied by Hammond (1975). It is the simplest overlapping generations game that is not strategically trivial. The game is played by an infinite sequence of players, $P_1, P_2, P_3, \ldots$. The first player $P_1$ does not make a choice. Each subsequent player makes one choice only from the set {A, B}. If the second player $P_2$ chooses A (B), $P_1$ receives 50 (30). Each subsequent player $P_t$’s payoff is determined by his own choice and by the choice of the next player $P_{t+1}$, his successor ($t \geq 2$).
Table I shows the payoff scheme. This payoff scheme is common knowledge.

In this game there are two focal outcomes: one noncooperative and one cooperative. If all players choose B, then all will earn 30. This noncooperative outcome is an equilibrium according to the following argument. If player P, believes that the choice by player P_{i+1} will be independent of his own choice, then clearly it is in his best interest to choose B. Whatever the choice of P_{i+1} (A or B), if it does not depend on the choice of P, then it is a best reply for P, to play B. This argument is similar to the dominance argument that leads to noncooperation in the prisoner’s dilemma.

If all players choose to play A, however, then all will receive a payoff of 50. This is the cooperative outcome. It can be supported by a trigger strategy. Suppose that player P, believes that player P_{i+1} will play A if and only if all previous players have played A; then it will be in his interest to play A if and only if all previous players have played A. Therefore, this “grim trigger” strategy is self-enforcing. The cooperative outcome can be supported as a subgame perfect equilibrium if each player is informed about the choices of all previous players, and if there is an infinite sequence of players (cf. Bhaskar, 1998).

In our baseline treatment we investigate whether subjects spontaneously devise trigger strategies supporting the cooperative outcome. Subjects both make a choice and formulate a strategy to play the game. The latter information gives us more direct and detailed evidence on the strategies considered by our subjects.

The game has many equilibria. In fact, with mixed strategies any outcome and any payoff between the cooperative and the noncooperative payoff can be supported as an equilibrium (Bhaskar, 1998, Theorem 2). Let p^A (p^B) be the probability that player t + 1 plays A when player t plays A (B). It is straightforward to check that, with p^A = 3/7 + 8/7p^B, whatever the history of play, each player will be indifferent between playing A and B, thus justifying the mixed strategies. Hence, each outcome can be an equilibrium outcome and by varying p^A and p^B each average payoff in the interval (30, 50) can be supported. Restricting attention to pure strategy equilibria, only outcomes of the form (B, ..., B, A, A, A, ...) can be supported; that is, there can never be a B choice after an A choice. Otherwise, each player choosing A followed by B would have an incentive to deviate.
In our recommendation treatment we provide subjects with a recommendation supporting the cooperative equilibrium. This recommendation was suggested by one of the subjects in the baseline treatment. It amounts to the grim trigger strategy. This strategy has the advantage of being both subgame perfect (contrary to, e.g., “tit-for-tat”) and simple. Previous experimental work shows that recommendations affect subjects’ play in some circumstances. Van Huyck et al. (1992) find the result that subjects tend to follow equilibrium recommendations as long as they do not conflict with payoff dominance or symmetry. The results of Brandts and McLeod (1995) are mixed in this respect. In some of their games recommendations matter for subjects’ play. However, they also construct a game where the perfect equilibrium does not coincide with the Pareto-efficient equilibrium. In that game subjects do not tend to follow the recommended efficient equilibrium strategy.

Ours is not the first experimental study of overlapping generations. For example, Cadsby and Frank (1990) study Ricardian equivalence in an overlapping generations experiment. Lim et al. (1994) and Marimon and Sunder (1993) study expectation formation in an experiment in which the value of money savings depends on future prices as determined by the behavior of future generations. These experiments have a focus different from ours and are much more complex than our simple “pension game.” A paper with a focus similar to the present one is Van der Heijden et al. (1998). They study the occurrence of cooperative transfers in a finite overlapping generations game. Strictly speaking, however, in their experiment cooperation is not an equilibrium. Furthermore, in all of these studies the subjects play the game repeatedly, which may introduce other reasons for cooperation than those purely based on the dynamic structure of the overlapping generations game. It is the latter that is the focus of the present study. Moreover, by using the strategy method (alongside simple play of the game) we can see whether subjects actually use trigger strategies to exploit the dynamic structure of the game.

2. EXPERIMENTAL DESIGN AND PROCEDURE

The experiment was conducted with the help of the computer. The instructions were distributed to subjects and read aloud (a transcription of the instructions and the data is available from the authors upon request). We used neutral terminology when explaining the game. For example, we avoided words like “cooperating.” First we explain the baseline treatment. Then we focus on the recommendation treatment.

In the experiment, of course, it is impossible to have an infinite sequence of players, and we used the following stopping rule. The first
player, $P_1$, did not make a choice, but his or her payoffs were affected by
the choice of the next player. After player $P_2$ had made a choice, there was
a 90% probability that an additional player, $P_3$, was added to the sequence
and a 10% probability that player $P_2$ was the last player. In the latter case
the game ended, and the final player ($P_3$ in this case) earned a fixed payoff
of 40 guilders. In the former case the game continued and player $P_2$ made
a choice. Table I shows the payoffs in guilders for subjects that are not at
the beginning or the end of the sequence. Then a 90–10 lottery again
determined whether an additional player, $P_4$, was added to the sequence.
This procedure was repeated until the game ended. As we explained to our
subjects, we carried out this lottery procedure before the start of the
experiment. Subjects were randomly allocated to a position in the se-
quenue. A subject made at most one choice in the sequence. We invited
more people to the experiment (at least 19) than the number determined
by the lottery (the maximum length of a sequence was 12). Therefore,
some subjects did not make a choice at all, because the sequence was
closed before it was their turn to choose. Subjects who did not make a
choice earned a fixed payoff of 40 guilders.

Our solution for inducing an infinite horizon is not a perfect one. In
particular, subjects could infer an upperbound on the length of the
sequence from the number of subjects present in the lab. However, a
perfect solution probably does not exist. There is no generally accepted
method of inducing an infinite horizon in experimental games (see, e.g.,
Ochs, 1995, especially notes 8 and 9). Often some variant of the stopping
rule is used. Strictly speaking, any (constant) random stopping probability
is incredible, because subjects know that the experiment will have to be
stopped at some point (e.g., because the building closes). Some exper-
imenters have therefore chosen not to tell the subjects anything about how
often the game will be repeated. This procedure has its own problems,
however. For one thing, subjects will form their own subjective beliefs
about the number of repetitions without the experimenter having any
control about these beliefs.

It is particularly difficult to induce an infinite horizon in overlapping
generations games, because the random device determines how many
subjects are needed and not merely how long they will be in the lab. Some
experimenters have tried to solve this problem by “reincarnating” subjects
and using them repeatedly at different places in the same sequence (e.g.,
Lim et al., 1994). We did not use this procedure because it may introduce
(undesirable) repeated play considerations and because it gives subjects

\footnote{The structure of the payoffs is similar to a prisoner’s dilemma. Our payoff matrix is comparable to the payoff matrices used in other experimental studies on the prisoner’s dilemma.}
the opportunity to learn about the strategies of others (while we are particularly interested in applications in which people only play once). Another possibility would have been to implement a game with a fixed length, where the last player actually knows she is last. Since the last player's payoff does not depend on her choice, she could choose to play cooperatively if and only if the previous player played cooperatively. Thus, cooperation can be supported as an equilibrium in a game with a known fixed end. Note, however, that in the cooperative equilibrium the last player in the finite game does not strictly increase her expected payoff by adhering to a (grim) trigger strategy, in contrast to all the players in the infinitely repeated game. Hence, the cooperative equilibrium in a game with finite length relies on weaker incentives than the one in an infinitely repeated game.

When the basic structure of the game had been explained to the subjects, they had ample time to practice. In the practice phase subjects made choices for all participants in the row (to avoid any guidance about what to choose). When a subject entered a choice, the computer determined the corresponding payoffs and whether an additional player was added to the sequence, for which the subject then again entered a choice. In some practice rounds the subjects were requested to enter the corresponding payoffs themselves, and then the computer reported whether the answer was correct.4

After the practice phase subjects were informed that they also had to write a strategy for this game. A strategy for this game requires a player to make a decision for any possible position in the sequence and for any possible history of choices by earlier players in the row. To help subjects formulate a strategy, we provided them with some forms containing “strategy classes,” differing in the degree to which choices are dependent on history. The experiment was carried out twice, once with the formulated strategies and once in the natural time order. When making a choice in the natural time order, a subject had information about all choices of her predecessors.

Subjects formulated a strategy before they chose in the natural time order. The results of the strategy experiment were communicated only after the end of the natural time experiment to avoid the subjects’ learning about other subjects’ strategies. In playing out the strategy experiment, subjects were put in the same row and place as in the baseline experiment. Before subjects were paid they filled out a post-experimental question-

4 The post-experimental questionnaire suggests that this procedure gave them a good understanding of the game. For example, 98% of the subjects indicated that the instructions were clear, 90% did not find it too difficult to formulate a strategy, and only 5% indicated that they were provided with too little information about the decisions of other participants.
naire. Finally, each subject threw a six-sided die. When the throw yielded 1, 2, or 3, his or her payoffs in the strategy experiment were used for actual payment, and when it yielded 4, 5, or 6, the payoffs of the natural time order experiment were used.

The only difference between the baseline and recommendation treatment was that in the latter treatment subjects received the following recommendation at the end of the instructions: “A participant of a previous experiment made a recommendation to participants of future experiments which amounted to: ‘I recommend the second participant to choose A; I recommend each subsequent participant to choose A if all previous participants in the row choose A and to choose B otherwise. If everybody sticks to this recommendation, then it is in each participant’s interest to stick to it. A choice for A is then rewarded with a choice for A by the subsequent participant, while a choice for B is punished with a choice for B by the subsequent participant in the sequence. In this way the aggregate payoff of all participants will be as large as possible.’”

Subjects were recruited at Tilburg University. A total of 156 subjects participated in the experiment, 78 in the baseline treatment and 78 in the recommendation treatment. In each treatment 9 independent sequences were formed with lengths varying between 4 and 12 players. In each session we ran 2 or 3 sequences (rows) at the same time. This allowed us to have many more subjects in the lab (at least 19) than the expected length of a sequence (which is 10 with a stopping probability of 10%). This procedure prevented subjects from anticipating the length of a sequence from the number of subjects in the room. Redundant subjects did not play in the natural time order experiment, but they did formulate a strategy.

3. RESULTS

Table II presents the aggregate number of choices in the baseline experiment in each of the two treatments. In both treatments subjects did not succeed very well in exploiting the intertemporal structure of the game.

<table>
<thead>
<tr>
<th>Choice</th>
<th>Baseline</th>
<th>Recommendation</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>13.8% (8/58)</td>
<td>29.3% (17/58)</td>
<td>21.6% (25/116)</td>
</tr>
<tr>
<td>B</td>
<td>86.2% (50/58)</td>
<td>70.7% (41/58)</td>
<td>78.4% (91/116)</td>
</tr>
</tbody>
</table>
to enforce the cooperative outcome. In the baseline treatment, only 13.8% of the choices were cooperative (A). The recommendation offered in the recommendation treatment did stimulate cooperative play to some extent. In this treatment subjects made 29.3% cooperative choices. The difference in cooperative play between the baseline treatment and the recommendation treatment misses conventional levels of significance, though. A two-tailed Mann–Whitney test with the percentages of A choices in each of the 18 sequences as observations gives a significance level of $p = 0.11$.

There is some evidence that cooperation in the recommendation treatment is to some extent history dependent. Cooperative choices were followed by a cooperative choice in 41.2% of the cases, whereas a noncooperative choice was followed by a cooperative choice in only 18.2% of the cases. In the baseline treatment, on the other hand, no cooperative choice was followed by another cooperative choice (but here, of course, we have few cooperative choices in all).

One may also wonder whether cooperative choices were typically made earlier or later in the sequence. To investigate this we compare the average rank numbers (place in the row) of subjects choosing cooperatively with those choosing noncooperatively. Averaged over all sequences with at least one cooperative choice, the average rank number is 4.71 for a cooperative choice and 4.80 for a noncooperative choice. Hence, there is no difference of cooperative play between the earlier and later players. The same holds if we look at the baseline and recommendation treatments separately. Furthermore, the rate of cooperation does not seem to depend much on the actual length of a sequence. For example, the average fraction of cooperative choices was 0.21 in sequences smaller than 7 and 0.24 in sequences larger than 10. Moreover, almost all of the subjects formulated stationary strategies, that is, strategies that are independent of the place of the subject in the sequence. None of the strategies tried to forecast the actual length of the sequence or to make provision for the (expected) end of the sequence. This supports the view that subjects played the game as if it were infinitely repeated.

The strategies formulated by the subjects help us to understand the failure to arrive at cooperative play. Table III summarizes the strategies formulated by the subjects. In our baseline treatment a large majority of the subjects, 76.9%, developed strategies that did not make use in any way of the intertemporal structure of the game. Most of these subjects unconditionally chose B. Only 12 subjects (15.4%) spontaneously developed a trigger strategy.

A recommendation to play a grim trigger strategy clearly inspired subjects to submit conditional strategies. A substantial proportion of the population followed the recommendation, and some others developed their own trigger strategy. In total, 46.1% of the subjects in the recommen-
dation treatment submitted a trigger strategy, compared to only 15.4% in the baseline treatment. This difference is highly significant ($\chi^2 = 17.3$ at $p < .001$; note that for this test we can use each strategy submitted by a subject as an independent observation). Yet, even in the recommendation treatment, a large fraction (47.4%) of the subjects chose to play noncooperatively unconditionally (always B).

The availability of actual choice data and strategies allows us to investigate the consistency between choices made in the natural time order experiment and the strategies submitted. In the baseline treatment 4 of 58 choices (6.9%) are inconsistent with what would have been prescribed by the strategy submitted. The level of inconsistency is considerably higher in the recommendation treatment. Here, 15 of 58 choices (25.9%) are inconsistent with the strategy. There is a remarkable pattern in the inconsistencies in the recommendation treatment. Nine times a subject selected the recommended strategy but played cooperatively in the natural time order experiment when the strategy would have chosen to defect.

In summary, Table III indicates that subjects’ choices of strategy were significantly affected by the recommendation. At the same time, Table II indicates that their actual choices in the natural time order experiment were affected by the recommendation to a lesser and insignificant extent. The reason for this is that the employment of trigger strategies by one part of the population (i.e., 46.1%) does not lead to cooperation if another part of the population (i.e., 47.4%) plays noncooperatively unconditionally. Punishment is often triggered indeed.

4. CONCLUSION

This paper provides an experimental investigation of Hammond’s Pension Game, the most basic type of overlapping generations game. Our
results show that subjects did not easily arrive at a cooperative outcome and did not spontaneously employ trigger strategies. When we provided subjects with a recommendation to play the grim trigger strategy they became aware of the potential for cooperation offered by the intertemporal structure of the game. A substantial fraction of the subjects (46.1%) then submitted trigger strategies, whereas only a few (15.4%) did so without a recommendation. However, a substantial part of the population (47.7%) still chose unconditionally the safe option of not cooperating. As a result, cooperation was not greatly facilitated by the increased use of trigger strategies because punishment was often triggered.

Theoretically, cooperation can be self-enforcing in overlapping generations games like the one we have implemented. Nevertheless, our results suggest that cooperation may not always be the most likely outcome of such games. Many players preferred the safe unconditional defecting strategy above a risky trigger strategy, even when a recommendation made them aware of the potential benefits of the grim trigger strategy. Therefore, merely establishing that a cooperative outcome is an equilibrium, as is done in many applications of overlapping generations models, is not likely to be a sufficient condition for the actual occurrence of cooperation. It would seem that other facilitating factors are necessary to actually achieve cooperation. We have investigated one such factor—a general recommendation to play the grim trigger strategy—and found that this alone was not sufficient.

Of course, there are other factors that may facilitate cooperation in overlapping generations games. First, people may learn to play cooperatively when they play a game repeatedly. This argument draws some support from the experimental study by Selten and Stoecker (1986). They find that subjects learn to play a trigger-like strategy in a repeated game after they have played many (finitely) repeated games against the same opponent. The experimental study of Van der Heijden et al. (1998), however, does not indicate that cooperation rates increase with repeated play of overlapping generations games. Furthermore, for many applications the suggestion that people play overlapping generations games repeatedly does not make much sense. For example, once you have learned whether or not your children will support you when you are old—possibly in response to how you have treated your parents when you were young—there is no second chance which allows you to revise your strategy. Hence, for many applications of overlapping generations games, any learning should take place before or during the play of the game and not across repeated plays of the game. One possibility is that direct communication between successive generations of players generates trust, which may facilitate cooperation (see also Kotlikoff et al., 1988).
Second, it may be the case that some of our subjects did not choose the recommended grim trigger strategy because they felt that the strategy is too unforgiving. The lack of a systematic decline in the cooperation rates in the natural time order experiment suggests that very few subjects actually played in accordance with the grim trigger strategy. Possibly, more subjects would have been attracted to the recommended strategy if it had been more forgiving. Consider, for example, the “resilient” strategy (Bhaskar, 1998). This strategy punishes a defector, but it does not punish someone who punished a defector. It prescribes cooperation unless an odd number of the direct predecessors defected (after the previous cooperative choice). More formally, let $\omega_t$ be a state variable that takes one of two values, C or D (with $\omega_1 = C$), and let the strategy of player $t$ ($t \geq 2$) be described by $\sigma_t(\omega_{t-1})$, with $\sigma_t(\omega_{t-1}) = A$ if $\omega_{t-1} = C$ and $\sigma_t(\omega_{t-1}) = B$ if $\omega_{t-1} = D$. The state variable takes the value $\omega_t = D$ if and only if the choice by player $t$ was B when the state variable $\omega_{t-1}$ was C. A recommendation to play this (or a similar) subgame perfect strategy may lead to higher levels of cooperation. We leave it to future work to investigate these possibilities.

REFERENCES


