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When does the game end? Public goods experiments with non-definite and non-commonly known time horizons

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Abstract

In repeated public goods experiments, ruling out information about an exact, commonly known, and symmetric terminal period does not alter average contributions significantly, although asymmetric information about the time horizon reduces the frequency of end-game effects.

Keywords: Public goods experiment; End-game effect; Asymmetric information

JEL classification: C72; C92; D82; H41

1. Introduction

Applied to supergame experiments with an upper bound for the number of repetitions, common knowledge of rationality requires players to apply backward induction to establish optimal choices. For finitely repeated linear public goods games, backward induction–based on own payoff maximization–would yield free-riding in each period. However, experimental behavior does not conform to this

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theoretical prediction. Many public goods experiments[1] show that individuals, interacting finitely often, start out by contributing substantial amounts, although contributions decline over time and reach their minimum when the interaction terminates (the so-called "end-game effect"; Andreoni, 1988).[2]

The usual experimental practice is to publicly inform all participants (so as to establish common knowledge) about the terminal period, which is the same for all individuals. Yet, in real life, one hardly knows in advance when a social relationship will end, but has at best some subjective expectations about the minimum and the maximum number of interactions. Thus, a commonly known and symmetric time horizon, as typically implemented in the laboratory, appears highly artificial and unrealistic.

To try to avoid the end-game effect, a few experimental studies of the prisoners' dilemma game have examined repeated play with fixed probabilities of termination (see, e.g., Roth and Murnighan, 1978; Axelrod, 1980; Murnighan and Roth, 1983; Van Huyck et al., 2002). However, laboratory implementation of such a procedure is at least problematic because for any finite number of repetitions participants should believe that the probability of playing longer is positive.

We study the robustness of the end-game effect in repeated linear public goods experiments without changing the equilibrium structure, but by simply deviating from the typical practice of setting a fixed, commonly known and symmetric endpoint to the game.[3] We find that asymmetric information about the number of periods to be played induces people to delay free-riding. In contrast, replacing a definite endpoint by a commonly or privately known symmetric interval does not have a significant impact on overall efficiency or contribution dynamics, although initial contributions are slightly increased.

2. Experimental design and procedures

Our experiments are based on the standard linear public goods game as introduced by Isaac et al. (1984). Groups of size 3 interact for several periods in a partners design. In any one period, each participant is endowed with 20 ECU (Experimental Currency Unit), and must privately decide how much to contribute to a public good, keeping the remaining ECU for herself. Let c_i denote individual i's contribution to the public good (with $c_i \in \{0, 1, \ldots, 20\}$), and let $C = \sum_{j=1}^{3} c_j$ be the total amount of public good provided. The monetary payoff of each i is given by

$$u_i(c_i, C) = 20 - c_i + 0.5C.$$

We study four variants (treatments) of this game, which are linked to each other by simple deviations from usual assumptions. Our control treatment is the *standard protocol* (SP), in which all three group members are publicly informed that the interaction will last for exactly 10 periods.

We first give up the assumption of a fixed endpoint. In the *interval protocol* (IP), all three group members are publicly informed that they will interact for at least 8 and at most 12 periods.

To deviate one step further, we remove the common knowledge assumption.

[1] See Ledyard (1995) for a review.

[2] One possibility to explain why a finite number of repetitions may allow cooperation is to assume some kind of (experimentally uncontrolled) incomplete information (cf., Kreps et al., 1982).

[3] As noted by a referee, a more realistic protocol would say nothing regarding the final period. This is explored more thoroughly in a follow-up study in which we elicit expectations about the terminal period after informing participants about either the lower or the upper bound of the horizon (or none of them).

In the *private information protocol* (PIP), each group member is aware that the interaction will last at least 8 and at most 12 periods, but also that other group members may receive different information about the time horizon.

Finally, we withdraw the symmetry assumption, and provide different group members with different interval information. In the *private asymmetric protocol* (PAP), each participant is informed that the experiment will consist of at least t_1 and at most t_2 periods, and that t_1 and t_2 differ across group members. We set $t_1=8$ and $t_2=10$ for one group member, $t_1=9$ and $t_2=11$ for another group member, and $t_1=10$ and $t_2=12$ for the third group member, with the actual horizon being 10 periods (i.e., the intersection of the three information sets). As participants are warned that the biggest t_1 and the smallest t_2 determine the number of periods to be played, if they could pool the individual information they would discover that the final period is the 10th.

Regardless of the type of information provided (common or private, symmetric or asymmetric), all treatments are bounded with respect to the number of repetitions. Hence, classical folk theorems do not apply and the unique subgame perfect equilibrium of the four games is for all players to free-ride in all periods (if one disregards uncontrolled incomplete information).[4]

The computerized experiment was conducted at the laboratory of the Max Planck Institute in Jena, using the z-Tree software (Fischbacher, 1999). Participants were undergraduate students from different disciplines at the University of Jena. After being seated at a computer terminal, participants received written instructions. Understanding of the rules was ensured by a control questionnaire that subjects had to answer before the experiment started.[5] Each session took about an hour. We implemented an exchange rate of 100 ECU = € 4.00, and the average earning per subject was € 12.92 (including a show-up fee of € 2.50).

In total, we ran six sessions. Four sessions involved 27 participants, and each employed one of the four protocols. The other two sessions involved 18 participants, and employed either IP or PIP. Therefore, there are 9 independent observations for SP and PAP, and 15 for IP and PIP. The additional sessions with IP and PIP were conducted in order to gather data for each possible final period in the interval $t\in$ [8]. In particular, we have 3 observations for each of the five possible terminal periods.

3. Experimental results

3.1. Contribution levels

Fig. 1 displays the evolution of average contributions in each of the four protocols.

Due to the different termination of IP and PIP as compared to the other two protocols (both terminating in period 10), an unequal number of repetitions results across individuals and games. Average contribution levels in the first 8 periods can, however, be compared across various sessions. The average contribution over the first 8 repetitions and over all (9 or 15) groups is 10.1 in SP, 12.8 in IP,

[4] As Fudenberg and Maskin (1986) show, many other outcomes can be sustained as reputation equilibria of a finitely repeated game if players expect others to be non-opportunistic with small probability (depending on the number of repetitions). The analysis of these equilibria goes, however, beyond the scope of this paper.

[5] Instructions and control questionnaire are available from the authors upon request.

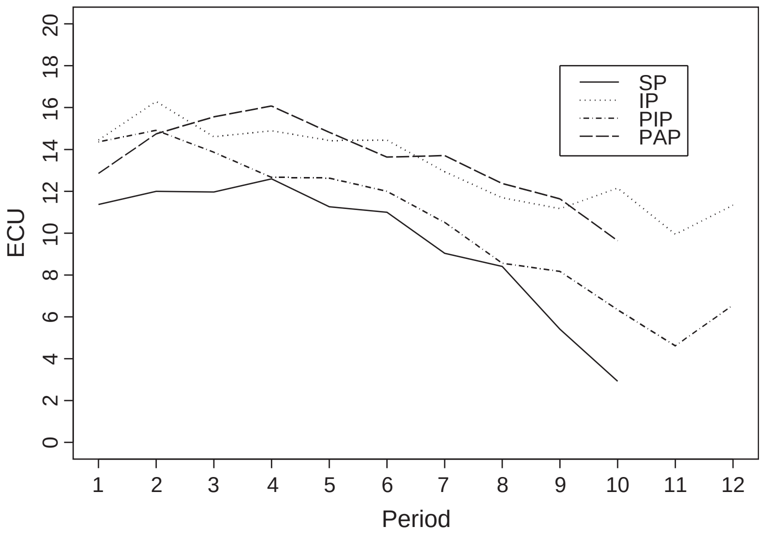

Fig. 1. Time paths of average contributions in each protocol.

13.3 in PIP, and 14.6 in PAP. Although the three experimental protocols (IP, PIP, and PAP) trigger higher average contributions than the standard protocol in any period, this difference is not statistically significant using the two-tailed Wilcoxon rank-sum test.[6]

Analysis of initial contribution levels is performed to check whether information about the number of periods to be played affects "intrinsic" attitudes of people toward cooperation. The average contribution in the first period is 11.4 in SP, 14.5 in IP, 14.4 in PIP, and 12.9 in PAP. These figures suggest that participants in IP and PIP start out with a higher contribution level than participants in SP. Applying a series of Wilcoxon rank-rank tests (two-tailed), we can reject the null hypothesis that initial contributions in SP and in IP or PIP are equal at the 5% level. In contrast, there is no statistically significant difference in initial contributions between SP and PAP.

3.2. Defecting behavior and end-game effect

For an easy terminology, we refer to a reduction in the individual level of contribution as a "defection". We investigate such a defecting behavior and the robustness of the end-game effect by means of a *defection index*, DI, relying on the idea that "defectors" make most of their contributions only in early periods whereas "late contributors" gradually increase their contributions over time. In particular, DI measures whether an individual's cumulative contributions are, on average, concave or convex functions of time (see the appendix for a formal definition). DI takes on higher positive values the earlier the player defects, and lower negative values the later she contributes. If the level of contributions is held nearly constant in all periods, DI is close to zero. Thus, DI can be interpreted as a measure of the end-game effect.

[6] We use the independent group observations as the unit of analysis throughout the paper. Notice that, due to linear payoffs, these results apply also to efficiency comparisons across treatments.

Although our experimental protocols reduce the variation of the defection index as compared to the standard protocol, there are no statistically significant differences in average defection when considering only the first 8 periods (Wilcoxon rank-sum test, two-tailed). However, if we consider all 10 periods of play, *the value of the defection index is significantly lower in PAP than in SP* ($p=0.031$). Interestingly, there is no difference at the 5% significance level among subjects with different information intervals. This suggests that the decrease in the defection index observed in PAP is not due to individual information, but to common knowledge of asymmetric information. On the other hand, comparing the distribution of *DI* over all rounds of play in IP and PIP yields no significant difference. Thus, *removing common knowledge alone does not seem to affect the frequency of early defection* infinitely repeated public goods games. In terms of efficiency, this means that in PAP the level of group welfare decreases later than in the other treatments (controlling for overall levels of efficiency across treatments).

4. Conclusion

At least for linear public goods games, the standard (and unrealistic) laboratory practice of publicly announcing the same definite time horizon does not appear to alter results significantly. Only the most generic protocol, with private and asymmetric interval information, triggers statistically significant deviations from the contribution dynamics of the standard protocol: When it is commonly known that group members receive asymmetric information about the number of periods to be played, participants tend to reduce the variation of cooperation levels across periods.

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Appendix A. Definition of the defection index

Let $c_{i,t}$ be participant i's contribution in period t. We define the normalized cumulative contribution curve, NCC(x) on $x\in$ [0, 1], as the straight segments joining the points

$$\mathrm{NCC}(t|T) = \begin{cases} 0 & \text{if } t=0 \\ \dfrac{\sum_{s=1}^{t} c_{i,s}}{\sum_{s=1}^{T} c_{i,s}} & \text{otherwise} \end{cases}.$$

This is a non-decreasing function whose shape depends on the individual pattern of defection. If "on average" the NCC function is concave, we say that the individual is a defector, and if it is convex we classify her as a late contributor. This observation leads to our definition of the defection index (DI) as the area between the NCC curve and the 45° line, or

$$\mathrm{DI} = \int_0^1 (\mathrm{NCC}(x) - x)\mathrm{d}x.$$

Notice that the NCC curve may cross the 45° line more than once. In this case, DI results from adding up several positive and negative areas.

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