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► **To cite this version:**

Laurent Muller, Martin Sefton, Richard Steinberg, Lise Vesterlund. Strategic Behavior and Learning in Repeated Voluntary-Contribution Experiments. *Journal of Economic Behavior and Organization*, 2008, 67 (3-4), pp.782-793. 10.1016/j.jebo.2007.09.001 . hal-00614682

**HAL Id: hal-00614682**

**<https://hal.science/hal-00614682>**

Submitted on 15 Aug 2011

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## Accepted Manuscript

Title: Strategic Behavior and Learning in Repeated  
Voluntary-Contribution Experiments

Authors: Laurent Muller, Martin Sefton, Richard Steinberg,  
Lise Vesterlund



PII: S0167-2681(07)00197-7  
DOI: doi:10.1016/j.jebo.2007.09.001  
Reference: JEBO 2148

To appear in: *Journal of Economic Behavior & Organization*

Received date: 3-7-2006  
Revised date: 10-9-2007  
Accepted date: 11-9-2007

Please cite this article as: Muller, L., Sefton, M., Steinberg, R., Vesterlund, L., Strategic Behavior and Learning in Repeated Voluntary-Contribution Experiments, *Journal of Economic Behavior and Organization* (2007), doi:10.1016/j.jebo.2007.09.001

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# Strategic Behavior and Learning in Repeated Voluntary-Contribution Experiments<sup>1</sup>

by

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## Abstract

Voluntary contribution experiments systematically find that contributions decline over time. We use a two-stage voluntary contribution game to investigate whether this decrease is caused by learning or strategic behavior. Using a strategy method we find a robust pattern of declining contributions: contributions in stage two are 45% lower than in stage one. Repeating the game five times we find that experience generates a smaller decline in contributions: stage one contributions decrease by around 7% per game. Finally we find no significant differences between the strategy and direct response method, which suggests that our results help explain behavior in the latter.

*JEL classification:* C92, C72, H41

*Keywords:* Voluntary Contributions; Cooperation; Experiment.

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<sup>1</sup> We thank Nick Bardsley, Rachel Croson, Simon Gächter, Elke Renner, two anonymous referees and participants at the Economic Science Association International Meetings, Pittsburgh, June 2003, for useful comments. This research was supported by the Center on Philanthropy, Indiana University-Purdue University at Indianapolis. We thank them for their generosity. Vesterlund also thanks the NSF for financial support and CEBR at Copenhagen Business School for their hospitality.

## 1. Introduction

A robust finding in experimental studies of voluntary contribution games is that cooperation decreases with repeated interaction. Two explanations have been provided for this result. One is that strategies initially are well-defined, but that they depend on the history of play and therefore may cause players to change their actions over the course of the game. Another is that participants slowly begin to understand the game and refine their strategies accordingly. While both of these explanations are convincing, limited work has been done to determine directly the role played by each. The purpose of this study is to provide such an examination.

To determine the extent to which decreasing contributions arise as a result of history-dependent strategies we need to identify the participants' strategies in a multi-stage voluntary contribution game. To do so we apply the strategy method (Selten, 1967) to a two-stage voluntary-contribution game. Participants submit a plan of action for the two-stage game, specifying a stage-one contribution and a stage-two response to any possible number of aggregate stage-one contributions by other group members. The plan is then matched with that of two other participants, and actions are taken in accordance with the participant's strategy. An advantage of the strategy method is that participants make a decision for every possible history of aggregate contributions, not just those reached in the course of actual play. Thus participants indicate how they would respond to different contributions, making it straightforward to identify strategies. We find that the pattern of contributions is comparable to the results from previous repeated voluntary-contribution experiments in that the elicited strategies imply declining average

contributions. The elicited strategies cause contributions to decline by an average of 45% within the two-stage games.

To examine the potential role played by learning we have participants play a sequence of five two-stage games, playing against a new set of people in each game. This allows us to examine whether experience with the game leads to a modification of strategies and whether such modifications decrease contributions. We find that individuals do modify their strategies across games, and as a result contributions in stage one sometimes increase and sometimes decrease across successive games. On average stage-one contributions decrease by 7% per game. Stage two contributions decrease more reliably across games, but the average decrease is still only 14% per game. Thus experience with the game leads to an erratic and less pronounced deterioration in contributions, compared with the systematic and substantial deterioration generated by submitted strategies.

We observe, as in previous studies, considerable heterogeneity in submitted strategies. Following Fischbacher et al. (2001), we classify participants on the basis of the strategies they submit, and in any game most participants are classified as “free-riders” (participants who contribute nothing in stage two) or “conditional cooperators” (contributors whose stage-two contribution increases with other group members’ stage-one contribution). Individual strategies are not stable across games, as many participants switch from one class to another across successive games. While a potential interpretation is that these participants do not have stable preferences, we cannot rule out that participants nonetheless have well-defined preferences over the outcome of the two-stage game. The reason is that experience may help participants better understand the

game and may cause them to update their prior on the strategies others use when playing the game. Thus participants may be uncertain of what strategies best serve their interests and revise strategies as they learn how their own strategies influence outcomes. While participants often change their strategies from game to game, the *distribution* of strategies is quite stable: the proportions of different types do not change much from game to game.

A potential disadvantage of the strategy method is that it may elicit strategies that differ from those used by participants in a traditional experiment based on a ‘direct-response’ method. The direct-response method, applied to this game, would have participants make simultaneous stage-one contributions, and then, after observing aggregate stage-one contributions, participants would make simultaneous stage-two contributions. For our strategy-method examination to shed light on the decrease in contributions observed in direct-response games it is necessary that the elicited behavior is similar across the two elicitations. Although the two methods are equivalent from a standard theoretical point of view, there are plausible behavioral reasons why differences may emerge (see the discussions in Roth 1995 or Brandts and Charness 2000). For example, eliciting a strategy for the two-stage game encourages subjects to consider stage two when making their stage-one decision; if the two stages were played out in sequence a subject might make a stage-one decision without giving any consideration to stage two. If behavior under the direct-response method differs from that under the strategy method, then it is unreasonable to argue that we can use elicited strategies to shed light on the decrease in contributions observed in the direct-response interaction.

Previous research delivers mixed evidence on whether people behave differently according to whether the strategy or direct-response method is used. Brandts and

Charness (2000) examined Sequential Prisoners' Dilemma and Sequential Chicken games and found no significant differences between outcomes elicited by direct-response and strategy-method approaches. They carefully conclude (p.234): "in games of low complexity, the strategy method may be a valid technique for collecting a rich data set without affecting participants' decisions significantly." However, other studies provide evidence of significant differences between direct-response and strategy-method approaches. For example, Güth et al. (2001) compare alternative versions of a mini-ultimatum game and find significant differences when the direct-response method is used, but not when a strategy method is used, Brandts and Charness (2003) study a game with pre-play communication in which players can punish opponents who send misleading messages and find that under a direct-response method punishment rates are significantly higher than under a strategy method, and Burton et al. (2005) study coordination games with pre-play communication, finding that under a direct-response method coordination on the efficient equilibrium was significantly lower than under a strategy method.

Thus, since equivalence between the two elicitation procedures cannot be taken for granted, to test the validity of the strategy-method implementation of our game we also run a direct-response version of the game where participants simultaneously choose contributions for the first stage, are informed of stage-one contributions, and then play the second stage. Our results reveal that the contributions elicited by the strategy method are no different from those of our control, direct-response treatment. Thus the behavior that results from the elicited strategies is consistent with that of the direct-response games.

The remainder of the paper is organized as follows. In Section 2 we review the related literature. We then describe our experimental design and procedures in Section 3. The results are presented in Section 4, and we conclude in Section 5.

## **2. Literature Review**

A number of studies have examined contributions in a repeated voluntary contribution game. Among the first is Isaac and Walker (1988) who, in a series of repeated linear public goods games, find that repetition causes free riding to increase and contributions to ‘decay.’ With ten repetitions of the game contributions tend to start out at about half the endowment and decrease to about 15-25% of the endowment in round ten. The two most common arguments for this decrease in contributions are that as the game is repeated, errors diminish as participants learn to free ride and that with repetitions, participants can play history-dependent strategies, potentially causing both conditional cooperators and free riders to decrease their contributions over the course of the game.

A number of studies have used indirect tests to determine whether the ‘learning’ or the ‘strategies’ hypothesis is the likely cause of the decay in contributions. To examine whether participants at the end of the repeated game have learned to free ride, Isaac and Walker invited past participants back to the laboratory to determine if their contribution rates differed from that of inexperienced participants. In contrast to the learning hypothesis and in support of the strategies hypothesis, behavior of experienced and inexperienced participants is practically the same. Initial contributions are high and then decrease as the game is repeated.

Andreoni (1988) uses a different approach to examine the two hypotheses. He argues that if the decay in cooperation is caused by state-contingent strategies then contributions should be larger when participants play in a finitely repeated game (partners) than when they are randomly paired with new participants after every repetition of the game (strangers). To test the learning hypothesis further he also examines the effect of restarting the game after ten periods. If the decay in contributions is caused by participants learning to free-ride, then contributions should be unaffected by the restart. His study provides evidence against both hypotheses. First, in contrast to the learning hypothesis, restarting the experiment causes a significant increase in contributions in the partner treatment. Second, in every round of the game contributions are larger in the stranger than partner treatment, suggesting that the decay in contributions cannot be caused by strategic play alone.<sup>1</sup>

Other researchers have subsequently used Andreoni's design to compare the behavior of partners versus strangers. While these have succeeded in replicating the restart effect, the evidence is mixed when replicating higher contributions in the stranger treatment.<sup>2</sup> Andreoni and Croson (2002) summarize these mixed findings and conclude, along the lines of Palfrey and Prisbrey (1996), that the reason why contributions in the stranger treatments sometimes are larger and sometimes smaller than in the partner treatment is that confusion or errors seem to be larger in the stranger treatment. In addition to errors differing between the two treatments, it has also been noted that both

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<sup>1</sup> Andreoni and Croson (2002) argue that "...with plenty of experience in a number of finitely repeated games, subjects will learn the benefits of reputation building. In a single finitely repeated game, such as these public goods experiments, these results indicate that subjects are unlikely to have learned the sophisticated strategy of reputation building" (footnote 6).

<sup>2</sup> See for example Croson (1996).

the decision-making process and the manner in which one learns across games can differ substantially between the two treatments. Thus it is not clear that by comparing contributions in the partners versus strangers treatments we can determine what causes the decay in contributions in the partner treatments.<sup>3</sup>

This paper uses a different approach to distinguish between the two hypotheses. First our learning hypothesis is broader, as we do not focus solely on learning as a process that teaches participants to free ride over time; rather we examine a repeated public good game multiple times, and we attribute any modification of strategies and resulting changes in contributions between supergames as being due to learning.<sup>4</sup> Second, in determining the role of strategic play we directly elicit the participants' strategies in the repeated public good game. Thus we do not try to infer the strategies by comparing behavior in two different treatments. While others previously have applied the strategy method to public good games, our approach is unique in that we elicit strategies over the entire repeated public good game and use these strategies to determine whether the decrease in contributions is due to learning or state-contingent strategies.

For example, Fischbacher et al. apply a strategy method to a sequential voluntary-contributions game where three group members first make simultaneous contributions, and then after observing their total contribution, a fourth group member makes her contribution. In their experiment the fourth group member indicates a contribution for each possible average contribution by the other three. Thus they can determine whether

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<sup>3</sup> Along the lines of the learning hypothesis a series of other studies have argued that the decay in contributions in the strangers environment primarily is caused by a decrease in errors (see e.g., Andreoni 1995, Palfrey and Prisbrey 1996, and Houser and Kurzban 2002).

<sup>4</sup> Note that this can include learning in terms of developing an improved understanding of the game as well as learning in terms of updating priors on the anticipated strategies of opponents.

behavior in sequential public goods games depends on the contributions made by others and classify the strategies that participants use. 50% of their participants are classified as “conditional cooperators” (participants whose contribution in the role of fourth group member is increasing in others’ contributions) and 30% are classified as free riders (participants whose contribution in the role of fourth group member always is zero). Fischbacher and Gächter (2006) extend this elicitation procedure to a two-part experiment. In one part they classify people in the sequential public good game, and in the second part participants play a 10-period repeated simultaneous-move public good game in a stranger environment with random rematching after every game. The distribution of types is similar to that of Fischbacher et al. (2001), and these type classifications are found to be a good predictor of behavior in the repeated public good experiment. They argue that preference heterogeneity in the sequential public good game can be used to understand better the dynamics of simultaneous-move voluntary contributions games in the strangers environment.

To the best of our knowledge Keser (2000) is the only study that elicits strategies for a repeated simultaneous-move public good game and allows participants to experience their submitted strategies. Using a rather special participant pool she asked 50 academic economists to submit strategies for playing a 25-period repetition of a voluntary-contributions game. Strategies were then translated into computer code and matched with one another in every possible group of four, generating average payoffs for each strategy. Participants received feedback about payoffs and could then revise and resubmit strategies for a second, then a third simulation. She found that in all three simulations, strategies typically consisted of high initial contributions, followed by a

phase of reciprocation, and then by a very pronounced endgame effect. With each simulation initial contributions increased, and endgame effects occurred earlier. The endgame effects she observes are stronger than in most standard experiments, perhaps because her participants are more sophisticated or more familiar with public goods experiments or because their motivations differed from those of typical experimental participants.<sup>5</sup>

As we focus specifically on the role played by learning and state-contingent strategies in decreasing contributions in the standard repeated simultaneous-move public good game, our approach differs from that of the earlier studies. Using a standard experimental subject pool we eliminate all possibilities of repeated interaction across games as participants are matched with another participant for one supergame only. Furthermore, to check that the elicited strategies match those of actual play of a repeated simultaneous-move game we conducted some control sessions with new subjects where subjects played an identical direct-response version of the game.

### **3. Experimental Procedures**

The experiment was conducted at the University of Nottingham in Spring 2001. We ran four sessions of a direct-response treatment and four sessions of a strategy-method treatment, with 15 participants in each session, for a total of 120 participants. Participants were recruited from a pool of undergraduate students at the University and randomly

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<sup>5</sup> Her participants were informed that after each simulation one participant would be randomly selected to be paid their earnings, and also that after the experiment participants' strategies, payoffs, and identities would be publicly revealed. In this latter respect her study is somewhat similar to the repeated prisoner's dilemma tournaments reported in Axelrod (1984).

assigned to a treatment. No participant participated in more than one session of the experiment.

All sessions used an identical protocol. Upon arrival, participants were seated at computer terminals and given a set of instructions, which the experimenter then read aloud (Appendix A on the JEBO website contains the instructions). As part of the instructional phase of the session participants completed a computerized quiz that tested their understanding of the game. When all participants had completed the quiz correctly, the experimenter continued with the instructions.

The decision-making phase of the session then began with a practice two-stage game in which the participants played against the computer for hypothetical payoffs. The practice game was used simply to familiarize participants with their task and the computer screens. Participants then played five two-stage games in which they were randomly and anonymously matched into groups of three, no participant ever being matched with another participant more than once. Participants were endowed with 2 tokens in each stage of the game and had to decide how many tokens to place in a group account and how many to place in a private account. For each token an individual placed in the private account, that individual received £1.50, and for each token placed in the group account by any individual all three group members received £1. Denoting subject  $j$ 's contribution in stage  $t$  by  $x_{jt}$ , participant  $i$ 's payoff from a two-stage game is

$$\pi_i = 1.5(2 - x_{i1}) + \sum x_{j1} + 1.5(2 - x_{i2}) + \sum x_{j2} \quad i=1, \dots, 3$$

where summations are taken over all three group members. At the end of the experiment one of the five two-stage games was selected at random, and the participant was paid her earnings from that game. These monetary incentives imply that to maximize group

earnings participants should contribute all tokens to the group account in all stages, whereas the standard game-theoretic prediction, based on own-earnings maximization, dictates zero contributions.

In our direct-response treatment each participant was informed of the total contributions to the group account in stage one by the other two group members and then made a stage-two decision. In our strategy-method treatment, each participant completed a strategy for the two-stage game, which consisted of a stage-one contribution and a stage-two contribution conditional on total stage-one contributions by the other group members.<sup>6</sup> At the end of the game participants were informed of own-contributions, total contributions by other group members, and their payoff, and they recorded this information on a record sheet.

Throughout the session participants were only allowed to ask questions by raising their hands and speaking to the experimenter in private. Participants were not allowed to communicate with one another throughout the session, except via the decisions they entered on their terminal. At the end of the experiment participants were paid their earnings in private. All sessions lasted less than an hour and participants earned an average of £7.83 (with a minimum of £4.50 and a maximum of £12.00).

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<sup>6</sup> With this strategy-method design the strategy space grows geometrically as stages are added. For example, a three-stage game would require participants to complete a matrix of conditional contributions. Thus, submitting strategies becomes much more cognitively demanding and time-consuming as the number of stages grows, and we therefore focus on the simple two-stage games. Likewise, we restricted the endowment space to 0, 1 or 2 tokens, and group sizes to three, to reproduce a voluntary-contributions game structure while maintaining a relatively simple task.

## 4. Results

Our analysis of the data starts with a brief overview. We first confirm that the behavior derived from the strategy method is consistent with that of the direct-response game, and we determine the extent to which these strategies cause contributions to decrease over the repeated public good game. Second, we then classify the elicited strategies, examine the distribution of types, and how these change with repetition. These changes in strategies help us assess the extent to which learning is responsible for decreasing contributions in the repeated public good game.

### 4.1 Overview

Average contributions are similar in our direct-response and strategy-method treatments: averaging over all games of the direct-response (strategy-method) treatment, participants contribute 37% (41%) of their endowment in stage one and 23% (23%) in stage two. Moreover, the time-series of contributions in the two treatments track each other closely, as shown in Figure 1.<sup>7</sup>

--- Figure 1 here ---

Formal statistical tests fail to reject the hypothesis that the two elicitation methods induce the same contribution behavior. Using two-sided Wilcoxon rank-sum tests applied to session-level data from all games we find no significant difference across treatments for either stage-one contributions ( $p = 0.561$ ) or stage-two contributions ( $p = 0.773$ ). We also find no significant differences in contributions if we focus on any given game.<sup>8</sup> All of these tests compare two sets of four observations, a rather conservative use of the data.

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<sup>7</sup> Average contributions for each stage of each session are tabulated in Appendix B on the JEBO website.

<sup>8</sup> The p-values are 0.363 (game 1), 0.883 (game 2), 0.307 (game 3), 0.549 (game 4) and 0.309 (game 5) for comparisons using stage-one contributions. The corresponding p-values using stage-two contributions are 0.243, 0.468, 0.773, 0.554, and 0.885.

We also made the same comparisons using individual-level data (i.e., comparing two sets of 60 observations). Although this procedure is biased toward finding significant differences because it overstates the number of independent observations, we still fail to find any significant differences between elicitation methods.<sup>9</sup>

We also examined changes in contributions between stages one and two. Using a two-sided Wilcoxon rank-sum test applied to session-level data from all games, we again find no significant difference between the two elicitation methods ( $p = 0.468$ ), and with one exception for the case of game 1, we also find no significant differences between treatments if we focus on a given game or use individual-level data.<sup>10</sup> The overall message is that our strategy-method treatment generates behavior remarkably consistent with that of a direct-response method.

The time-series displayed in Figure 1 are similar to those from previous experiments in that contributions decline within each two-stage game. In game one for example, contributions decrease from 45% to 32% of token endowments (averaging over all eight sessions). Moreover, the same pattern is seen in subsequent games: contributions fall from 36% to 26%, 39% to 23%, 41% to 18%, and 31% to 14% in games two through five respectively.<sup>11</sup> Thus, on average, contribution rates decrease by 16 percentage points between stage one and two, corresponding to an average decrease of contributions of 41%. Similar decreases are observed in each treatment (38% and 45% for the direct-

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<sup>9</sup> The p-values are 0.280 (game 1), 0.966 (game 2), 0.575 (game 3), 0.915 (game 4), 0.335 (game 5) and 0.448 (overall) for comparisons using stage-one contributions. The corresponding p-values using stage-two contributions are 0.281, 0.441, 1.000, 0.628, and 0.837.

<sup>10</sup> Based on session-level data the p-values are 0.020 (game 1), 0.307 (game 2), 0.884 (game 3), 0.559 (game 4), 0.147 (game 5) and 0.468 (overall). The corresponding p-values using individual-level data are 0.056, 0.577, 0.567, 0.661, 0.245, and 0.433.

<sup>11</sup> While end-game contributions are quite similar to those of Isaac and Walker, stage-one contributions of the two-stage game tend to lie below stage-one contributions of the ten-stage game.

response and strategy-method treatments respectively). Indeed, average stage-two contributions are lower than average stage-one contributions in all eight sessions; thus stage-two contributions are significantly lower than stage-one contributions (using a one-sided Wilcoxon matched-pairs signed-ranks test,  $n = 8$ ,  $p < 0.005$ ).

Figure 1 exhibits substantial ‘restart effects;’ that is, after contributions decline within a game, they restart at a higher level at the beginning of the next game. While this effect is not significant for the first restart, that is, the transition from game one to game two, ( $p$ -value = 0.6367 using a one-sided Binomial test), it is significant for the other three ( $p$ -value = 0.035 for the second restart, and  $p$ -value = 0.0039 for the third and fourth restarts).<sup>12</sup>

#### 4.2 Individual Strategies: Game One

Next we analyze the strategies elicited in the first game. With four strategy-method sessions, each involving fifteen participants, we elicited strategies from sixty participants. Figure 2 presents the strategies initially submitted for the second stage by each participant, demonstrating substantial heterogeneity across participants. Following Fischbacher et al., we classify participants into four categories. Of our sixty participants, 35% contribute nothing independent of the contributions of others (free-riders), while the contribution schedules are increasing for 38% (conditional cooperators), hump-shaped for 15%, and cannot easily be classified for the remaining 12%.<sup>13</sup>

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<sup>12</sup> The restart observed here differs slightly from that of Andreoni (1988) and Croson (1996), as they focus on the cases where participants either continue with the same group of people after the restart or are rematched with new people after every decision in the game.

<sup>13</sup> The corresponding percentages in the four person sequential public good game reported by Fischbacher et al. were 30%, 50%, 14%, and 7%, and by Fischbacher and Gächter were 23%, 55%, 12%, and 10%. They use a slightly different classification procedure based on Spearman rank correlation statistics (see their paper for details).

--- Figure 2 here ---

In examining how our participants' stage-one decisions relate to their stage-two decisions, we observe some consistency. There is a systematic relation between stage-one decision and the type-classification based on stage-two decisions, and we reject the hypothesis that the distribution of types is independent of stage-one decisions ( $\chi^2(6) = 13.46$ ,  $p\text{-value} = 0.036$ ). As can be seen in Table 1, of the eighteen participants who contributed zero in stage one, 67% were classified as free-riders and 22% were classified as conditional cooperators, while of the seventeen participants who contributed two tokens, 24% were classified as free-riders and 53% as conditional cooperators. Thus, our results mirror Brandts and Charness (2000) and Fischbacher et al. (2001) in that stage-one contributors are more likely to be conditional cooperators.<sup>14</sup> Table 1 also reveals that almost half of the participants who free ride in the second stage nonetheless made a positive contribution in the first stage. Thus free riders too cause contributions to decrease with repetition.

--- Table 1 here ---

The notion of consistency addressed by these data is quite limited. An own-earnings maximizer may either contribute or not in stage one, depending on his beliefs about how other participants respond in stage two. Similarly, a participant who prefers to contribute, but only so long as others also contribute, may either contribute or not in stage

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<sup>14</sup> Brandts and Charness had participants play two games, once as first-mover and once as second-mover (against different opponents and with no feedback between the two games). This allowed them to note a certain consistency in participants' decision rules in the two roles: when making decisions as second-movers, first-mover cooperators were much more likely to respond positively to a cooperative move than first-mover defectors. Fischbacher et al. also require participants to make decisions in two roles and note a similar consistency across decisions in each role. Conditional cooperators contribute 42% of their endowment, on average, when they make an unconditional decision, while free riders contribute 10%.

one, depending on his beliefs about other participants' contributions in stage one. In the next sub-section we will examine a different notion of consistency by studying how participants' strategies develop across games.

The predominance of free-riding and conditionally cooperative strategies implies that on average stage-two contributions increase with stage-one contributions, albeit with a rather shallow slope. At best contributing one additional token in stage one increases the stage-two contribution by each of the other group members by 0.25 tokens.<sup>15</sup> Fitting a line through the conditional contribution schedules using a simple OLS approximation for game one yields

$$\text{stage-two contribution} = 0.29 + 0.16 * (\text{aggregate stage-one contributions by other group members}).$$

Thus, by contributing an additional token in stage one a participant induces other group members to increase their stage-two contributions by 0.16 tokens each, on average; as this is not sufficient to cover the £0.50 contribution cost, it is not worthwhile for an own-earnings maximizer to contribute in stage one.<sup>16</sup> Given this relationship between stage-two contributions and stage-one contributions, stage-two contributions will be below stage-one contributions as long as stage-one contributions exceed 21% of endowments; in fact stage-one contribution rates were 45%, easily exceeding this threshold.<sup>17</sup>

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<sup>15</sup> The marginal increase in tokens by other group members depends on the initial contribution level. Increasing the contribution by other group members from 0 to 1 or from 1 to 2 tokens will on average increase the contribution by each of the other group members by 0.25, going from 2 to 3 it increases 0.1, and from 3 to 4 it increases 0.03. The average stage-one contribution by other members is 1.97 in game one.

<sup>16</sup> This result is based on the submitted strategies; if we instead look at the actual responses that were carried out, the corresponding coefficient on contributing would be 0.18.

<sup>17</sup> That is, denoting average stage-two contribution by  $s_2$  and average stage-one contribution by  $s_1$ , since aggregate stage 1 contribution by others equals  $2s_1$  we can see that  $s_2 < s_1 \Leftrightarrow 0.29 + 0.16 * 2 * s_1 < s_1 \Leftrightarrow s_1 > 0.42$ . 0.42 tokens represent 21% of the endowment.

### 4.3 Individual Strategies: Development across Games

Next we examine how learning influences behavior, that is how behavior changes across the five repetitions of the games. Twenty-two of our sixty participants submitted the same type of strategy in every game. Thirteen of these submitted free-riding strategies in every game, and seven submitted conditionally cooperative strategies in every game. This leaves almost two-thirds of our participants whose strategy classification varies from game to game.

With sixty participants, each having four opportunities to revise their strategies, there are 240 opportunities for participant-strategies to change. Table 2 presents a transition matrix indicating the transition rates from each class. For example, 92 free-riding strategies were observed in the first four games, and in 80% of these cases the participant remained in the free-riding category in the next game. Similarly, participants classified as conditional cooperators usually remain in the same category in the next game. In contrast, participants in the “hump-shaped” or “other” category usually transit to a different category.

--- Table 2 here ---

Transitions from one category to another occur throughout the experiment: of sixty subjects 20 are classified differently in games one and two, 24 in games two and three, 21 in games three and four, and 15 in games four and five. A variety of explanations may account for these transitions. One possibility is that a participant whose behavior changes across games does not have stable preferences, while another possibility is that the participant is aware of what outcomes they prefer, but is learning how best to achieve them. This suggests caution should be used when interpreting

strategies and their development over time. First, if participants do have stable preferences, elicited strategies do not map onto these in a direct way. Second, if we interpret changes in behavior from game to game as ‘learning,’ many of the observed transitions suggest that this goes beyond simply learning how best to maximize own-payoff.

Figure 3 shows the distribution of strategy-types across the five games. Depending on the game, free-riding strategies account for between 33% (game two) and 45% (game five) of submitted strategies, while conditionally-cooperative strategies account for between 35% (game four) and 42% (game two) of submitted strategies. What is clear from Figure 3 is that together, free riding and conditionally cooperative strategies predominate throughout the experiment. We also see that the distribution of types is fairly stable over the course of the experiment.

--- Figure 3 here ---

The implication of these distributions of strategy-types for the average response function is shown in Figure 4. The presence of conditionally cooperative strategies generates a response function that increases in others’ stage-one contributions, but the rate of increase is dampened by the presence of free-riding strategies.

--- Figure 4 here ---

From Figure 4 it appears that the average response function is approximately an increasing linear function that shifts downward from game to game. Table 3 reports the results of using simple OLS to approximate the average response function by a straight line. The slopes vary from 0.14 to 0.17, indicating that if a participant were to increase his stage-one contribution by a token, the aggregate contributions made by others in stage

two would, on average, increase by between 0.28 and 0.34 tokens. Thus, the degree of responsiveness, as measured by this slope, does not vary much and is never strong enough to make it worthwhile for an own-earnings maximizer to contribute in stage one.<sup>18</sup> The more apparent change over time is with respect to the intercept of the response function, which falls steadily across games. Thus participants respond less generously to a given level of others' stage-one contributions as games progress. A linear average response function with a slope less than  $\frac{1}{2}$  implies that if stage-one contribution rates exceed some critical percentage of endowments, then average stage-two contributions will be lower than average stage-one contributions. Given the estimated response functions presented in Table 3, these critical percentages are 21%, 20%, 17%, 12%, and 10% for games one through five respectively. Note that these critical percentages all are far below the contribution rates observed in the experiment.

--- Table 3 here ---

The actual dynamic pattern of contributions depends on how stage-one decisions and the stage-two response function develop over games. As seen in Figure 1, contributions in stages one and two appear to be lower in game five than game one. Wilcoxon matched-pairs signed-ranks tests support this impression: stage-one contributions are significantly lower in game five than game one ( $n = 8$ , one-sided  $p < 0.01$ ), as are stage-two contributions ( $n = 8$ , one-sided  $p < 0.005$ ). However, the effect of experience is quite erratic from one game to the next; for example, stage-one

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<sup>18</sup> If we instead use actual behavior to estimate the marginal increase in contributions, we find coefficients ranging from 0.14 - 0.22. Alternatively we may look at the average marginal response at each contribution level in each of the five games; doing so we find marginal responses ranging from 0.03-0.25. Thus, at best a contributor will break even by contributing in stage 1, and with the exception of the first game her monetary return is always strictly lower from contributing in stage 1 (the largest marginal return is 0.19 in the remaining games).

contributions actually increase between games 2 and 3 and between games 3 and 4. Stage-two contributions show a more regular pattern of decrease across games. Overall, both stage-one and stage-two contribution rates vary quite widely around an average decrease of 4 percentage points per game. This corresponds to an average decrease in contributions in stage one of 7% and in stage two of 16%. Thus the decay in contributions from one game to the next (for a given stage) is lower and less stable than the decay from the first to the second stage (for a given game).

## 5. Conclusion

Previous experiments where participants repeatedly make voluntary contributions to public goods consistently result in a declining pattern of contributions. The existing literature has discussed two mechanisms that can lead to this pattern. One is that declining contributions are the result of reduced confusion as participants gain experience with the game. The other is that declining contributions result from the interaction of agents with heterogeneous motivations and reflect deliberate strategies whereby some participants condition their contributions on past contributions by others. In one of our experimental treatments participants submit strategies for playing a two-stage game and repeat this process five times, against new opponents each time. This treatment allows for a direct comparison of the effects of strategic responses and learning.

As in previous studies that elicit strategies in social dilemma experiments, most strategies can be classified as either free-riding (contributing nothing in the second stage of our game, regardless of others' contributions in the first stage), or conditionally cooperative (where stage-two contribution is increasing in others' stage-one

contributions). The observed distribution of submitted strategies is robust across the five repetitions and results in declining contributions within our two-stage voluntary contribution games: on average, stage-two contributions are 45% lower than stage-one contributions. Thus, deliberate strategic choices made by heterogeneous agents generate a substantial decrease in contributions.

However, for almost two-thirds of our participants the elicited strategy type falls in different classes in different games. This suggests caution be exercised in interpreting someone who uses a ‘conditionally cooperative strategy’ as a ‘conditionally cooperative person.’ We suggest that, as in many other experiments, these changes in behavior over time reflect some type of learning. Participants in our experiment are not able to dictate the outcome of a game, so even if they have well-defined preferences over the outcome of the two-stage game, they may not necessarily know what strategy best serves these interests. The changes in strategy may then reflect the attempts of participants to find the strategies that best work for them. Under this interpretation it is important to point out that transitions from one class of strategy to another are not always in the direction that serves the interests of selfish participants. Thus, participants are not simply learning how to maximize own payoff.

Indeed, although individuals switch between strategy-classes throughout the experiment, the *distribution* of strategy-types is quite stable, so experience has a relatively small effect on average contributions. On average, stage-one contributions fall by about 7% each time the two-stage game is repeated.

This direct approach to testing the ‘strategy’ and ‘learning’ hypotheses leads us to conclude that strategic responses have a more pronounced and systematic effect than

learning. The decline in contributions is much greater and more reliable across the two stages of a given game than across a given stage of successive games. In extrapolating this conclusion to the causes of declining contributions in other experiments, two further comments are in order.

First, we focus on a game that is simpler in many respects than that used in more conventional public goods experiments. Our game consists of three players (compared with four players in the “small” group treatments of Isaac and Walker); each player decides how to allocate a two-token endowment (compared with at least a ten-token endowment in Isaac and Walker) in each of two stages (compared with ten stages in Isaac and Walker). These differences between our game and the more conventional one were dictated by our implementation of the strategy method; whether the dynamics in contribution behavior differs according to these variables is an open question.

Second, a possible way to account for our results is that our strategy-method approach encourages participants to think about the two-stage game in a way that they would not if they were playing the two stages in sequence. If this were the case, then our strategy-method results may have little to say about the declining pattern of contributions in other experiments where subjects are not required to submit strategies for playing the entire game. However, our results from a control treatment using a direct-response method do not square with this account. In our direct-response treatment subjects contribute 37% of their endowments in stage one (compared with 41% in the strategy-method treatment) and 23% in stage two (compared with 23% in the strategy-method treatment). The similarity between contributions under the two elicitation methods makes us confident that our results are not an artifact of the strategy method.

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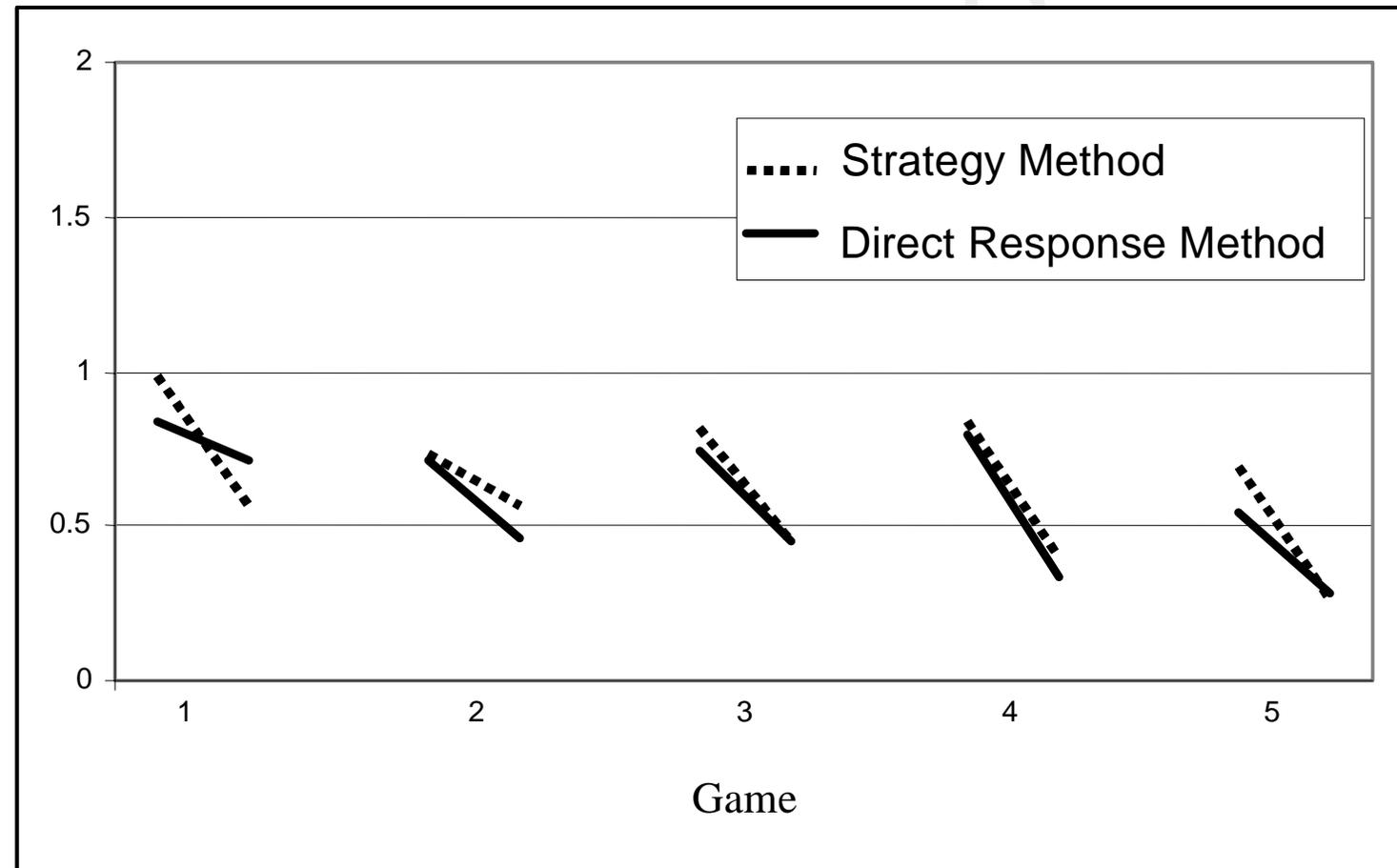
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Accepted Manuscript

## Figures

**Figure 1.** Average Contributions by Game (Note: The left end of each line indicates the mean contribution in stage one, and the right end indicates the mean in stage two).



**Figure 2: Stage-Two Strategies in Game One (Note: x-axis measures the aggregate stage-one contribution by other group members, y-axis measures the participant's conditional stage-two response. A panel represents one participant).**

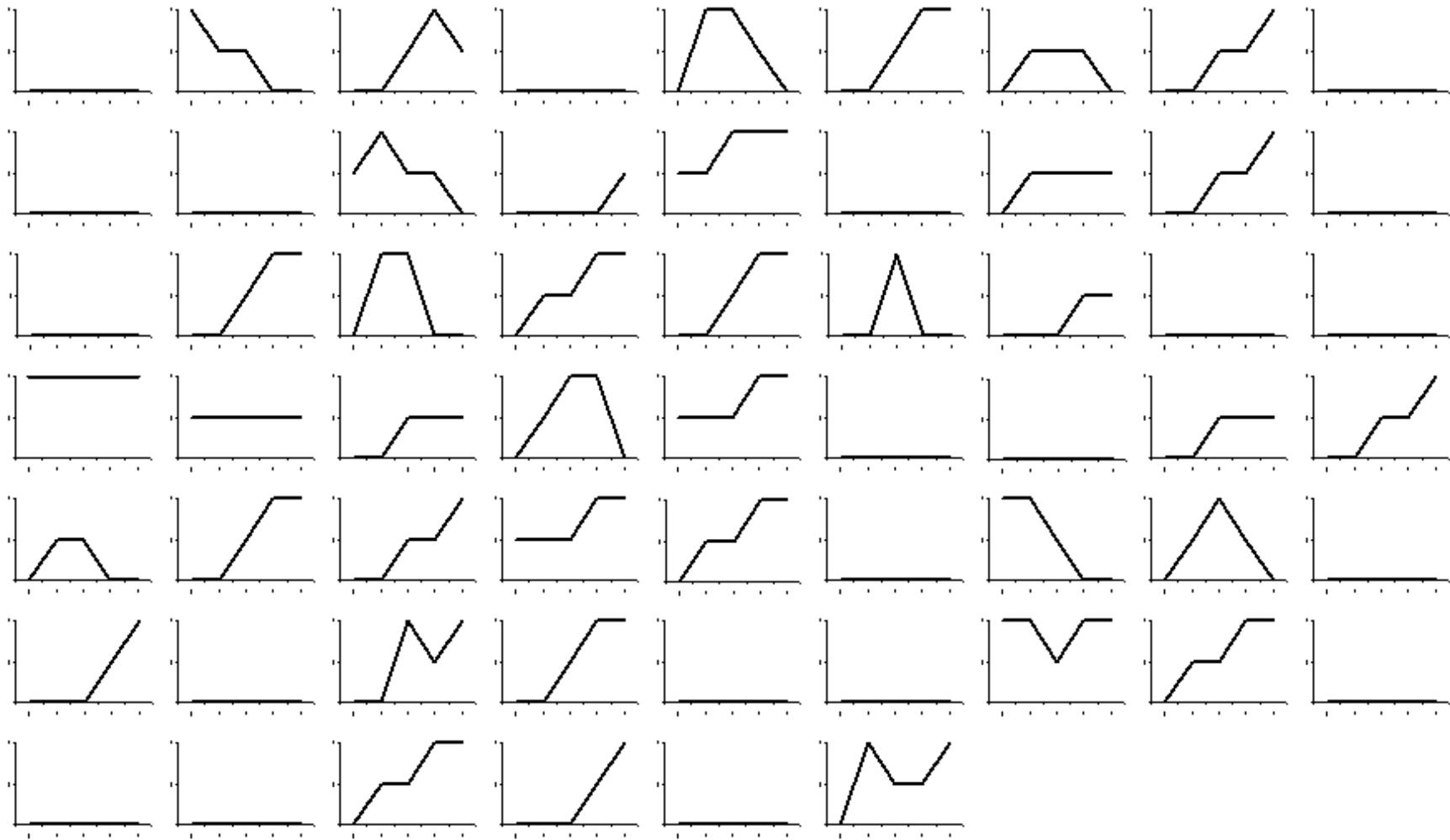


Figure 3. Distribution of Strategy Types across Games

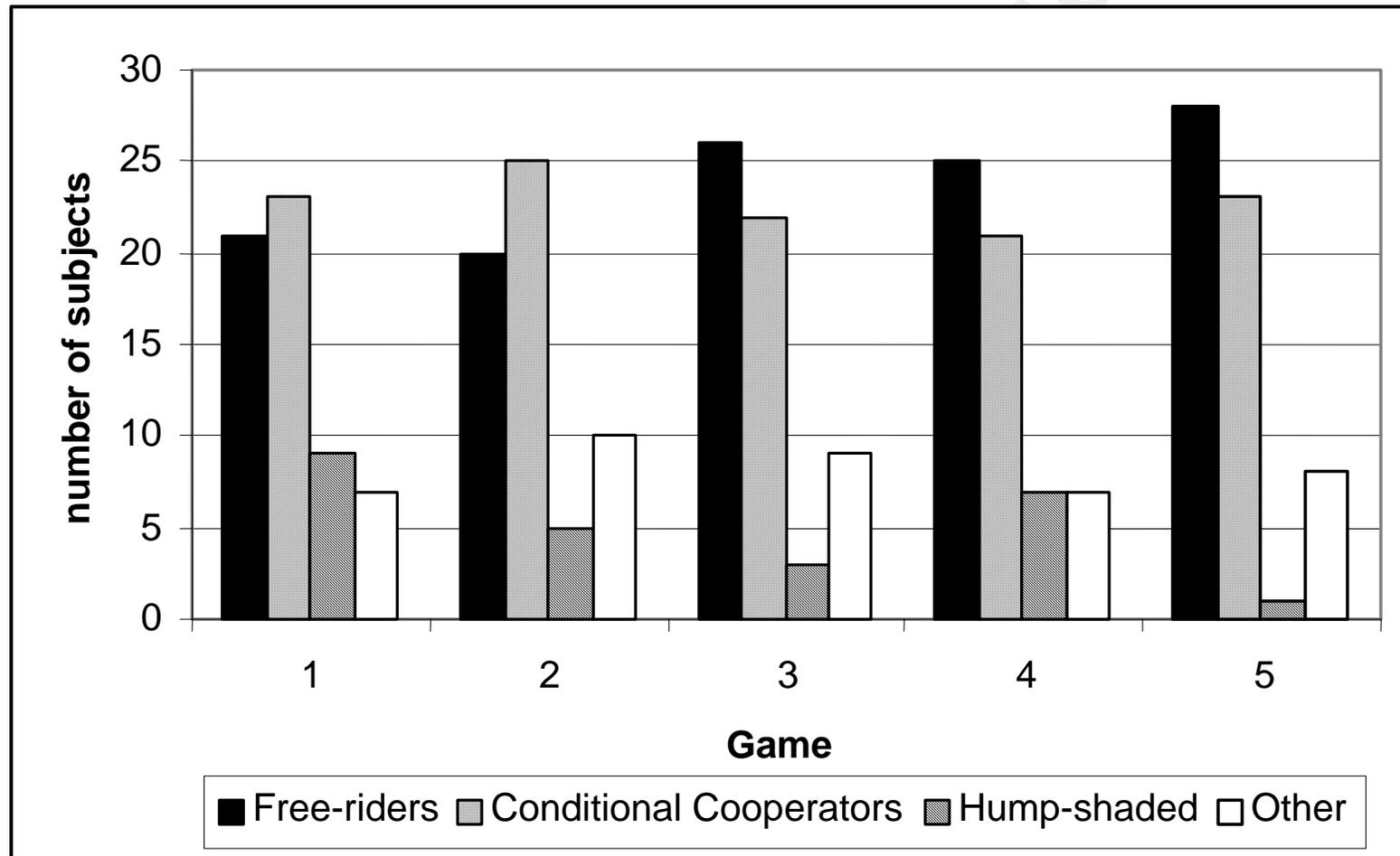
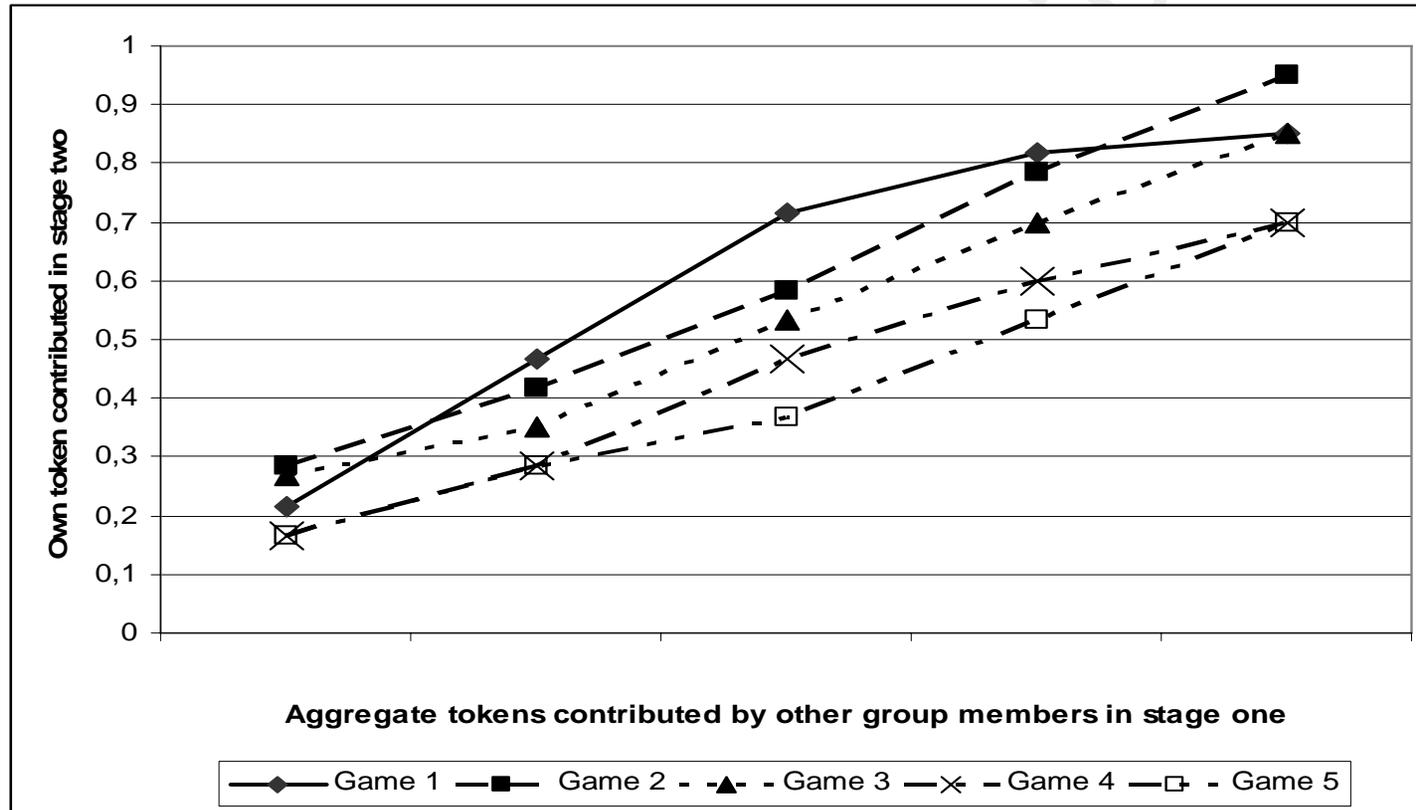


Figure 4. Strategy Method: Stage-Two Average Contribution Schedule



## Tables

	Fraction (percentage) of			
contribution in stage one	Conditional Cooperators	Free-riders	Hump-shaped	Other
0	3/18 (17%)	12/18 (67%)	2/18 (11%)	1/18 (5%)
1	11/25 (44%)	5/25 (20%)	4/25 (16%)	5/25 (20%)
2	9/17 (53%)	4/17 (24%)	3/17 (18%)	1/17 (6%)

category	number observed in any of the first four games	percentage categorized in the following game as:			
		Free Rider	Conditional Cooperator	Hump-shaped	Other
Free Rider	92	80	12	2	6
Conditional Cooperator	91	17	69	2	12
Hump-shaped	24	12	25	42	21
Other	33	21	33	6	40

	game				
	1	2	3	4	5
intercept	0.29	0.26	0.24	0.17	0.15
slope	0.16	0.17	0.15	0.14	0.13