A Test for Rational Altruism in a Public-Goods Experiment

John R. Carter

Brian J. Drainville

and

Rob P. Poulin

College of the Holy Cross

Worcester, Massachusetts 01610*

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Abstract

A regularity observed in public-goods experiments is that contributions tend to increase with the marginal per capita return (MPCR) on group contributions. In this paper we exploit the MPCR effect to explore whether contributions are due at least in part to rational choice based on altruistic motives. Specifically, we modify the standard public-goods design in a manner that limits alternative explanations for the MPCR effect and permits the effect to be used as a comparative-static test of altruism. Overall, the empirical results provide substantial evidence consistent with rational altruism.

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I. INTRODUCTION

According to the free-rider hypothesis, when individuals cannot be excluded from group benefits, rational self-interest will prompt them to free ride off the contributions of others. Because all group members have this incentive, voluntary contributions will be zero and collective action will fail in the absence of selective inducements (Olson [1971]). Over the past two decades an extensive literature has grown up involving experimental tests of the free-rider hypothesis. In brief, these tests show that free riding tends not to be complete but varies systematically with environmental features of the experiments. For reviews, see Dawes [1980] and Dawes and Thaler [1988].

Of particular interest to us is the replicable result in public-goods experiments whereby contributions tend to increase with the marginal per capita return (MPCR) on group contributions. Isaac and Walker [1988a] have conjectured that the MPCR effect might be due to a degree of altruism on the part of subjects. To our knowledge the conjecture has not been tested in further multiple-round public-goods experiments. In this paper we exploit the MPCR effect to explore whether observed contributions to a public good are consistent with altruistic motives. Specifically, we modify the standard public-goods design in a manner that limits alternative explanations for the MPCR effect and permits the effect to be used as a comparative-static test of altruism.

II. PUBLIC-GOODS EXPERIMENTS AND ALTRUISM

Public-Goods Experiments

While variations exist, the basic design in most public-goods experiments is attributable to Marwell and Ames [1981]. Subjects are provided with equal endowments of wealth in the form of tokens which they allocate between an individual account and a group account. Each token that a subject places in the individual account yields to that subject a return equal to one cent. In contrast, each token that a subject places in the group account yields collectively to the group a return greater than one cent, which is then divided equally among all group members. The resulting return to each group member, including the contributor, is called the marginal per capita return (MPCR) from the group account and is fixed at a value less than one cent. Communication among subjects is not permitted. In formal terms, the decision environment constitutes a voluntary-contribution public-goods game with a zero-contribution dominant strategy. The group optimum requires that subjects contribute all tokens to the group account; the dominant strategy, however, is to contribute nothing. Therein lies the familiar social dilemma. The game may be played as a single-shot game or a repeated game. In the single-shot game, the game-theoretic solution based on strict dominance is zero contributions by all subjects. In the repeated game with a known end period, the solution based on backward induction is zero contributions across all rounds. Hence, in both forms of the game the standard prediction is for complete free riding.[1]

Against this prediction three regularities have been observed in public-goods experiments (Marwell and Ames [1981], Isaac, Walker, and Thomas [1984], Isaac and Walker [1988a], and Andreoni [1988]). First, contributions are substantial in both the single-shot game and the first round of the repeated game, with subjects allocating on average forty to sixty percent of their tokens to the group account. Second, contributions diminish or decay between the first round and the end round, with the rate of decay tending to be less pronounced the higher is the MPCR. Third, in a given round of the repeated game, contributions are higher when the MPCR is higher. At least three hypotheses have been offered as explanations for these empirical regularities: learning, strategies, and altruism. Our interest is with the altruism hypothesis, but the learning and strategies hypotheses constitute important background.

According to the learning hypothesis, subjects might contribute in the single-shot game and in the first round of a repeated game because they fail to recognize their dominant strategy. In subsequent rounds of the repeated game, accumulated experience allows subjects to learn the dominant strategy, causing contributions to decay. The decay might also reflect subjects' strategic play. The zero-contributions prediction in the repeated game rests on a backward induction argument, which in turn assumes common knowledge of rationality among subjects (Sugden [1991]). Common knowledge in this case requires that subjects know the dominant strategy, know that other subjects know the dominant strategy, know that other subjects know that they know the dominant strategy, and so on and so forth. According to the strategies hypothesis, if subjects lack such common knowledge, then it may be rational for them to contribute in the early and middle rounds in hopes of encouraging others to contribute.[2] As appealing as the learning and strategies hypotheses are, empirical evidence indicates that they do not provide, either singly or jointly, a complete explanation for the regularities observed in subjects' contributions.

To test the learning hypothesis, Andreoni [1988] ran a public-goods experiment wherein, upon completion of a standard ten-round game, he immediately started a previously unannounced second game. Contrary to what would be predicted by the learning hypothesis, Andreoni found that in the restart subjects returned to the same high rates of contribution observed in early rounds of the first game. Similar evidence is implicit in Isaac and Walker's [1988] experiment, which was conducted with experienced subjects who had participated in previous public-goods experiments. Subjects completed two successive ten-round games with only the MPCR changing between games. Despite apparently plentiful opportunities to learn the dominant strategy, subjects' contributions still exhibited all three empirical regularities.

Andreoni [1988] also presented evidence contrary to the strategies hypothesis by contrasting contributions in two environments. In the first environment, groups remained intact throughout the experiment, as is standard in repeated public-goods games. In the second environment, groups were randomly changed at the beginning of each round, thereby eliminating or severely limiting any incentive for strategic play. Subjects in stable groups were called partners; subjects in scrambled groups were called strangers. Surprisingly, Andreoni found that strangers contributed significantly more than partners, precisely the opposite of what would be predicted by the strategies hypothesis.

Altruism

The evidence to date against the learning and strategies hypotheses is not conclusive, but it suggests at least that equal attention should be given to the hypothesis that voluntary contributions in public-goods games are due in part to altruistic motives. In discussing their experimental results, Isaac and Walker [1988a, 198] offered the following conjecture:

> Perhaps individuals retain a residual motive to altruism. This is consistent with positive contributions in the true end-period as well as with the fact that these positive contributions seem to be related to MPCR. That is, by the very nature of MPCR, altruism is relatively more "expensive" when MPCR is [low] than when it is [high].

As will become clear in the next section, Isaac and Walker's insight that the MPCR effect might reflect altruistic motives is central to our test of the altruism hypothesis.

The experimental literature on altruism is vast and will not be reviewed here (see Piliavin and Charng [1990]). However, two experiments that are particularly relevant to our own work can be sketched briefly. In the first of these experiments van de Kragt, Dawes, and Orbell [1988] tested whether subjects who cooperate in social dilemmas are rational altruists, meaning that they base their decisions on the altruistic benefits and opportunity costs of alternative choices. Randomly selected subjects played a single-shot five-person prisoner's dilemma game, while other subjects served as external beneficiaries of cooperative play. The number of beneficiaries and the aggregate dollar amount going to the beneficiaries were varied across treatments. Consistent with altruistic motives, van de Kragt, Dawes, and Orbell found that just over fifty percent of the active subjects chose to cooperate despite the dominant strategy to defect. However, contrary to the rational altruism hypothesis, the rate of cooperation did not vary significantly with either the number of beneficiaries or the total dollar value of the external benefit.

In the second experiment Goetze and Galderisi [1989] tested essentially the same hypothesis in the context of a single-shot public-goods game. The distinctive feature of their design was that the return on a contribution to the group account could be divided unequally between the contributor and other group members. Specifically, when a subject made a contribution, the return to that subject could be either high or low; likewise, on that same contribution, the return to the other members could be either high or low. This allowed four treatments across which contributions were compared. As in other public-goods experiments, Goetze and Galderisi found that subjects in the several treatments contributed on average between forty and sixty percent of their endowments. Consistent with the altruism hypothesis, observed contributions were sensitive to both the external benefits and private opportunity costs; however, most pairwise differences between treatments were not statistically significant.

Two fundamental features of the above experiments are noteworthy relative to our own experiment. First, both experiments involve single-shot games. Ours instead uses a multiple-round game. Second, both experiments involve comparative-static tests of rational altruism. We also undertake a comparative-static test of rational altruism. However, the terms rational and altruism can have different meanings for different speakers and contexts. Hence, several comments are in order concerning how the terms are used in this paper.

The general hypothesis we test states that contributions observed in public-goods games are due at least in part to rational choice based on altruistic motives. By rational choice we mean utility-maximizing choice. By altruistic motives we refer broadly to residual nonmalevolent motives that lie beyond the self-interest assumed by the free-rider hypothesis. Note that we speak of altruistic *motives* and thus allow for the substantial possibility that the ultimate *function* of altruism might be self-gain (Hirshleifer [1985] and Frank [1988]). We also allow that altruistic motives might be satisfied by consequences of actions or by actions per se. In this regard we find useful the distinction between pure and impure altruism suggested by Dawes and Thaler [1988, 192]. Pure altruism, in their words, involves "taking pleasure in others' pleasure"; it is the desire to see "positive payoffs for others." Impure altruism, on the other hand, involves the "satisfaction of conscience, or of noninstrumental ethical mandates"; it is the desire for "[d]oing the right (good, honorable, ...) thing." In the language of ethics, actions motivated by pure altruism have consequential value; actions motivated by impure altruism have deontological value. As we will use the terms, pure and impure altruism are not mutually exclusive: it is possible simultaneously to desire to benefit others and to desire to do the right thing. Still, the two forms of altruism are distinct and, as we indicate in the next section, can generate distinct testable implications.[3]

III. EXPERIMENTAL DESIGN, HYPOTHESES, AND PROCEDURES

Design and Hypotheses

The object of our experiment is to replicate the MPCR effect observed in previous experiments and then use it as a comparative-static test for rational altruism. Hence, in most respects our experiment follows the standard public-goods design. Subjects participate in a ten-round game with group size equal to four and with a known end period. In each round subjects are given a fixed endowment of tokens and are required to allocate the endowment between an individual account and a group account. Each token placed by a subject in the individual account returns one cent to the subject; each token placed in the group account returns more than one cent to the group but less than one cent to the subject. The group optimum requires maximum contributions to the group account, whereas strict dominance recommends zero contributions.

To exploit the MPCR effect we adopt two modifications to the standard design. First, following Andreoni [1988], at the start of each round we randomly assign subjects to new groups such that they never participate in the same group twice. In this way we eliminate or severely limit strategic play as a likely source of the MPCR effect. Second, following Goetze and Galderisi [1989], we allow a token placed in the group account to pay distinct returns to the contributor and other group members. We call the return to the contributor the *internal* MPCR and the return to each other group member the *external* MPCR.

Details of our design are summarized in Table I. The internal and external MPCRs, measured in cents, are set at either 0.3 (low) or 0.8 (high), thus permitting a two-by-two design with four treatments. Listing the internal MPCR first, we refer to these treatments as low-low (LL), high-low (HL), low-high (LH), and high-high (HH). Twenty-four subjects participate in each treatment. Following previous work (Isaac, Walker, and Thomas [1984] and Isaac and Walker [1988a]), token endowments are determined so as to equate group-optimum payoffs across treatments. Specifically, if all subjects contribute all tokens to the group account, they each earn $1.00 per period.

We test for pure and impure altruism by varying independently the internal and external MPCRs. The internal MPCR determines the marginal opportunity cost of altruism. Each additional token contributed to the group account by a subject decreases the subject's monetary payoff by an amount equal to one cent minus the internal MPCR. Hence, the higher is the internal MPCR, the lower is a pure altruist's opportunity cost of contributing a token in order to benefit other group members. Likewise, the lower is an impure altruist's opportunity cost of contributing a token so as to do the right thing. Our first hypothesis is that contributions by a pure or impure altruist will increase with the internal MPCR.

> *Hypothesis 1*: If subjects choose rationally based on pure or impure altruistic motives, then contributions to the group account are expected to vary directly with the internal MPCR.

Under Hypothesis 1, we expect contributions to be greater in treatment high-high than in low-high and greater in treatment high-low than in low-low.

The external MPCR, on the other hand, determines a pure altruist's marginal benefit of contributing a token so as to increase the payoffs to others. Each additional token contributed to the group account by a subject increases the monetary payoff of every other group member by an amount equal to the external MPCR. Our second hypothesis is that contributions by a pure altruist will increase with the external MPCR.

> *Hypothesis 2*: If subjects choose rationally based on pure altruistic motives, then contributions to the group account are expected to vary directly with the external MPCR.

Under Hypothesis 2, we expect contributions to be greater in treatment high-high than in high-low and greater in treatment low-high than in low-low.

Procedures

We recruited subjects from intermediate and upper-level courses in the Economics Department at the College of the Holy Cross. We told students that the experiment involved the economics of group decision making, included no deception, and required about one hour and fifteen minutes of their time. We promised three dollars for showing up and indicated that significantly more could be earned depending on decisions made during the experiment. Using their campus box numbers, volunteers signed up for one of four experimental sessions, which were conducted in a large room at the college library on consecutive evenings in September 1991. Each session was randomly assigned one of the four treatments. To assure us of twenty-four subjects per session, we overbooked by about one-third. If extra volunteers arrived after a session began, they were paid for showing up and were invited to sign up for a subsequent session.

As subjects arrived at a session, they were randomly seated at desks equipped with front and side blinders for privacy. On each desk were a consent form and a packet which included printed instructions, a table of sample earnings, a decision/record sheet, and a brief questionnaire (see Appendix A). After consent forms were collected, one of the experimenters read the instructions aloud as subjects read silently. Parameter values were known to be the same for all subjects. Emphasis was given to how earnings would be computed using the internal and external MPCRs and to how groups would be changed in successive rounds. At appropriate points in the instructions, time was allowed for subjects to familiarize themselves with the sample earnings table and with the decision/record sheet. At the end of the instructions the experimenter answered questions and then began the first decision round.

In each round subjects recorded on the decision/record sheets the number of tokens they wanted to allocate to the group account; remaining tokens were allocated automatically to the individual account. The sheets were collected and carried to an adjacent room, where earnings were calculated by computer. The computer output was recorded by hand on the sheets and included for each subject the number of tokens contributed by other group members, earnings from the group account, earnings from the individual account, and total earnings. The sheets were returned to the subjects, where upon the next round was begun. Rounds required five to six minutes to complete. After the tenth round, subjects filled out a brief questionnaire and then were paid in cash privately in the adjacent room. Including the $3.00 for showing up, cash payments averaged $9.94 and ranged from a low of $7.03 to a high of $12.77.

IV. EMPIRICAL RESULTS

Like Isaac, Walker, and Thomas [1984] and Isaac and Walker [1988a], we present our empirical results using two measures of contributions. For the first, we compute the percentage of endowed tokens contributed to the group account by a subject. Averaging across subjects yields the mean percentage of tokens contributed, which is plotted round-by-round for each treatment in Figure 1. For the second measure, we define a Bernoulli variable equal to one if a subject contributes any tokens to the group account and zero if the subject free rides. Averaging across subjects yields the percentage of subjects contributing one or more tokens, which is plotted similarly in Figure 2. We refer to the two measures respectively as percent of tokens contributed and percent of subjects contributing. With these measures we conduct formal hypothesis tests across relevant pairs of treatments. In each instance we test the null hypothesis that contributions are equal between treatments versus the one-sided alternative that contributions are greater for the second treatment listed in the pair. Standard normal z-values and their associated p-values are shown in Table II. To allow for possible learning during the early rounds, and also to keep the presentation manageable, we limit our test statistics and discussion to the last five rounds, both singly and pooled.[4]

As stated earlier, our objective is to replicate the MPCR effect found in previous public-goods experiments and then to use it as a comparative-static test of rational altruism. Guided by this objective, we organize the discussion of our results around three summary observations. We begin with the MPCR effect.

Observation 1: Jointly increasing the internal and external MPCRs significantly increases contributions.

In moving from treatment LL to HH, the internal and external MPCRs both increase from 0.3 to 0.8. If the MPCR effect is operative, we should observe a corresponding increase in contributions, and indeed we do. As seen in Figures 1 and 2, observed contributions are substantially higher in treatment HH than in LL. Pooling the last five rounds, the p-values for both measures of contributions in Table II are under 0.01; the same is generally true for the individual rounds, including the tenth. Hence, the positive MPCR effect found by Isaac, Walker, and Thomas [1984] and Isaac and Walker [1988a] is replicated here. This is important because our design differs in several respects, most notably in the framing of the MPCRs and in the scrambling of the groups. That the MPCR effect is observed with scrambled groups and persists in the tenth round suggests that it is not due to strategic play. Whether the MPCR effect is consistent with altruism is the question addressed in the remaining observations.

Observation 2: Holding the external MPCR fixed, increasing the internal MPCR significantly increases contributions.

According to Hypothesis 1, if subjects are pure or impure altruists, we expect to observe a positive internal MPCR effect. We can test the hypothesis by comparing contributions in treatment pairs LH versus HH and LL versus HL. In the first comparison, the internal MPCR is increased from 0.3 to 0.8, with the external MPCR fixed at 0.8. Consistent with the hypothesis, observed contributions are higher in treatment HH than in LH. Pooling the last five rounds, p-values for both measures of contributions are under 0.01. The same is generally true for the last several rounds individually, including the tenth. In the second comparison, the internal MPCR is likewise increased from 0.3 to 0.8, but with the external MPCR constant at 0.3. Again consistent with the hypothesis, observed contributions are higher in treatment HL than in LL. For both the pooled rounds and the individual rounds, the p-values for both measures of contributions are very low. Thus we find strong empirical support for Hypothesis 1.

Observation 3: Holding the internal MPCR fixed, increasing the external MPCR increases contributions, but it is not evident that the effect persists through the tenth round.

According to Hypothesis 2, if subjects are pure altruists, we expect to observe a positive external MPCR effect. We can test the hypothesis by comparing contributions in treatment pairs HL versus HH and LL versus LH. In the first comparison, the external MPCR is increased from 0.3 to 0.8, with the internal MPCR fixed at 0.8. Consistent with the hypothesis, observed contributions are higher in treatment HH than in HL. Pooling the last five rounds, the p-values are 0.03 for percent of tokens contributed and 0.11 for percent of subjects contributing. However, in the individual rounds the p-values are considerably higher, and in round ten are 0.13 and 0.19 respectively. In the second comparison, the external MPCR is likewise increased from 0.3 to 0.8, but with the internal MPCR constant at 0.3. Again consistent with the hypothesis, observed contributions tend to be higher in treatment LH than in LL. Pooling the last five rounds, the p-values for both measures of contributions are under 0.01. In the middle rounds the p-values are also quite low. In round ten, however, the p-values for the two measures jump to 0.67 and 0.76. Hence, while we find some empirical support for hypothesis 2, we cannot say with confidence that a positive external MPCR effect persists through the final round.

V. DISCUSSION

To recap briefly, Hypothesis 1 predicts a positive internal MPCR effect if subjects are pure or impure altruists. We find strong support for Hypothesis 1 in our data. Hypothesis 2 predicts a positive external MPCR effect if subjects are pure altruists. We also find support for Hypothesis 2, but the evidence is weaker, particularly in the last round of the experiment. Overall we believe that the empirical results provide substantial evidence consistent with rational altruism. Whether such altruism is pure, impure, or both remains for us an open question. In concluding we discuss a number of possible improvements that we hope to incorporate in future experiments.

1. Hypothesis 1 tests for pure or impure altruism; Hypothesis 2 tests for pure altruism. Here we show that a third hypothesis is possible that tests for impure altruism. Central to the hypothesis is the observation that the relative prices of impure and pure altruism are distinct. Impure altruists value the act of contributing tokens to the group account. Hence, the relative price of impure altruism is the opportunity cost of contributing an additional token, equal to (1 - internal MPCR). Pure altruists value payoffs to other subjects. Hence, the relative price of pure altruism is the opportunity cost of increasing other group members' payoffs by one cent, equal to (1-internal MPCR)/(external MPCR). Note that assuming altruism is a normal good, Hypotheses 1 and 2 follow immediately from these relative price expressions. Moreover, note that with suitable selection of MPCRs, it is possible

When the internal or external MPCR is increased, the relative price of altruism is decreased. As noted above, this holds for both pure and impure altruism. Assuming normality, altruistic contributions should increase if other things are held equal. However, if token endowments are simultaneously decreased, there can be an income effect on contributions in the opposite direction. Hence, the adjustment of token endowments in our experiment could have inadvertently limited the scope within which altruism could operate. In future tests of altruism, token endowments clearly should be held constant across treatments.[5]

4. In addition to eliminating any inadvertent income effects, holding token endowments constant would simplify and enrich the available measures of contributions. In particular, percent of tokens contributed could be simplified to number of tokens contributed, since there would be no need to scale by token endowments. Moreover, contributions could be measured in terms of payoffs returned to others, computed as the external MPCR times the number of tokens contributed. The result would be three measures, each with its own advantage. Impure altruists value the act of contributing, which suggests that the number of tokens contributed should be used to test for impure altruism. Pure altruists value the consequence of contributing, which suggests that payoffs returned to others should be used to test for pure altruism. And egoists value neither, which leaves percent of subjects contributing useful primarily to test for free riding.

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NOTES

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[1]Described in the text is what we term the standard public-goods design. Alternative designs have allowed for diminishing MPCRs, group optimums requiring less than full contributions, provision points, asymmetric endowments, and communication among subjects. See Bagnoli and McGee [1991]; Isaac, McCue, and Plott [1985]; Isaac, Schmidtz, and Walker [1988]; and Isaac and Walker [1988b; 1991a; 1991b].

[2]See Kreps, Milgrom, Roberts, and Wilson [1982] for a formal treatment demonstrating equilibrium cooperation when common knowledge is lacking in a finite repeated prisoner's dilemma.

[3]The terminology of pure and impure altruism set out in the text differs somewhat from that originally introduced by Andreoni [1989, 1448-1449, footnote omitted] as follows:

> In this model, individuals are assumed to contribute to a public good for two reasons. First, people simply demand more of the public good. This motive has become known in the literature as "altruism." Second, people get some private goods benefit from their gift per se, like a warm glow. Because of this second and seemingly selfish motive, this is called a model of "impure altruism."

Thus, what we call the simultaneous occurrence of pure and impure altruism, Andreoni would call impure altruism. See also Andreoni [1990].

[4]Note in our design that the percent of tokens contributed is equivalent to the number of tokens contributed as a percent of the group optimum. It also measures dollar payoffs returned to other group members from the group account as a percent of the maximum possible payoffs returned to others from that account.

For tests based on the percent of tokens contributed, we use the nonparametric Mann-Whitney U test (see Hogg and Craig [1978]). Observations from two treatments are rank ordered from the smallest to largest value. The U statistic counts the number of times an observation from the first treatment precedes an observation from the second treatment. Hence, a large U statistic suggests that the first distribution is centered to the left of the second. Under the null hypothesis that the distributions are equal, U is approximately normal for moderately large samples.

For tests based on whether a subject contributes any tokens, we use a two-by-two chi-square test of homogeneity (see Daniel and Terrell [1989]). The minimum expected frequency is at least five in all cells for all tests reported except for high-low versus high-high in round six. Because there is only one degree of freedom, the chi-square is equivalent to a z-test between two population proportions. For convenience of interpretation, z-values are reported in Table II.

All tests are performed using SPSS Release 4.1. Original data are available in Appendix B.

[5]In an experiment using a diminishing MPCR, Isaac and Walker [1991a] observed a small endowment effect with inexperienced subjects but no effect with experienced subjects.

TABLE I

Summary of Experimental Design

Treatment	Number of Subjects	Internal MPCR (cents)	External MPCR (cents)	Number of Tokens	All Cooperate Payoff (dollars)	All Defect Payoff (dollars)
Low-Low	24	0.3	0.3	83	1.00	0.83
High-Low	24	0.8	0.3	59	1.00	0.59
Low-High	24	0.3	0.8	37	1.00	0.37
High-High	24	0.8	0.8	31	1.00	0.31

TABLE II

Hypothesis Tests Based on Percent of Tokens Contributed and Percent of Subjects Contributing*

	Round 6	Round 7	Round 8	Round 9	Round 10	Rounds 6-10
A. Percent of Tokens						
Low-Low v. High-High	3.77 (0.00)	3.24 (0.00)	3.55 (0.00)	4.12 (0.00)	2.95 (0.00)	7.99 (0.00)
Low-High v. High-High	1.21 (0.11)	1.55 (0.06)	2.25 (0.01)	3.37 (0.00)	3.02 (0.00)	5.11 (0.00)
Low-Low v. High-Low	4.37 (0.00)	2.61 (0.00)	2.89 (0.00)	3.05 (0.00)	2.05 (0.02)	6.75 (0.00)
High-Low v. High-High	-1.14 (0.87)	0.91 (0.18)	0.97 (0.17)	2.02 (0.02)	1.12 (0.13)	1.85 (0.03)
Low-Low v. Low-High	2.74 (0.00)	2.65 (0.00)	(1.98) (0.02)	1.45 (0.07)	-0.44 (0.67)	3.91 (0.00)
B. Percent of Subjects						
Low-Low v. High-High	3.20 (0.00)	2.31 (0.01)	2.60 0.00)	3.18 (0.00)	2.34 (0.01)	6.07 (0.00)
Low-High v. High-High	1.27 (0.10)	0.60 (0.28)	1.48 (0.07)	2.34 (0.01)	2.98 (0.00)	3.79 (0.00)
Low-Low v. High-Low	3.51 (0.00)	1.74 (0.04)	2.02 (0.02)	2.32 (0.01)	1.51 (0.07)	4.92 (0.00)
High-Low v. High-High	-0.37 (0.64)	0.60 (0.28)	0.61 (0.27)	0.93 (0.18)	0.87 (0.19)	1.23 (0.11)
Low-Low v. Low-High	2.02 (0.02)	1.74 (0.04)	1.17 (0.12)	0.91 (0.18)	-0.71 (0.76)	2.39 (0.01)

*Reported are z-values based on Mann-Whitney U and chi-square tests, respectively. One-tailed p-values are shown in parentheses. See note 4 and accompanying text for details.

FIGURE 1

PERCENT OF TOKENS CONTRIBUTED

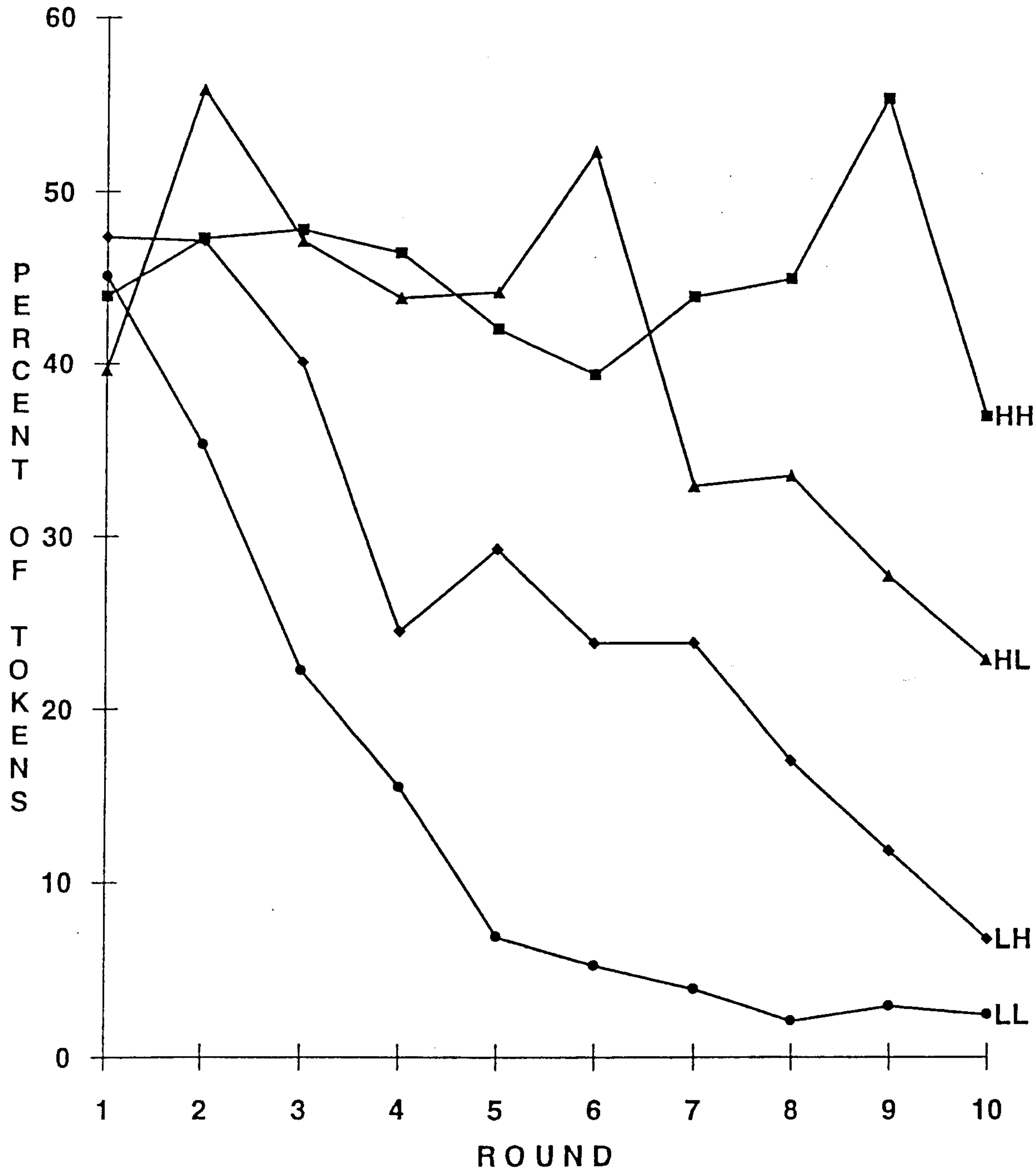

FIGURE 2

PERCENT OF SUBJECTS CONTRIBUTING

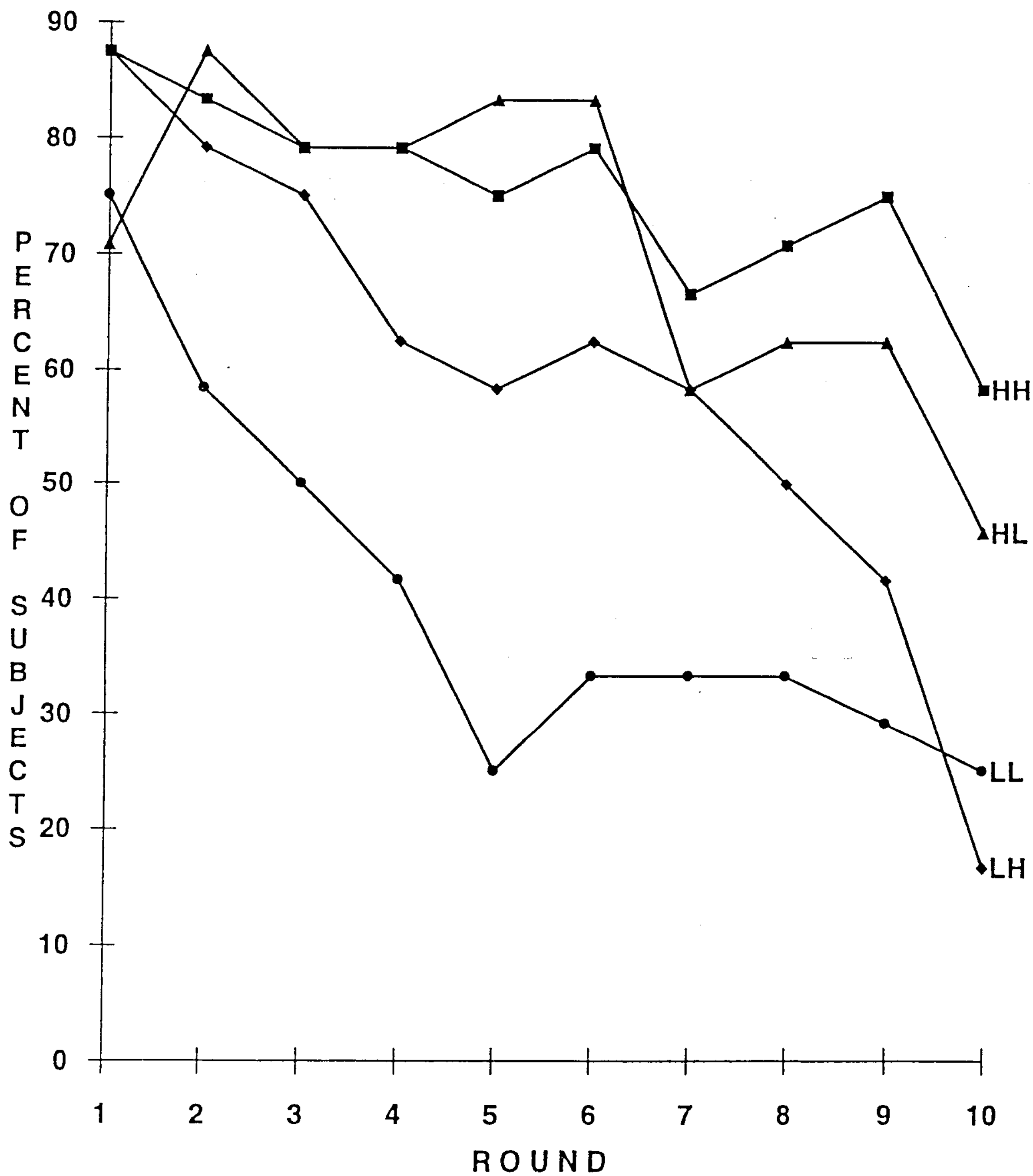

TABLE OF SAMPLE EARNINGS

Your endowment in each round is 59 tokens. Your earnings in each decision round are determined as follows:

your earnings = 0.8 cent times the number of tokens you place in the GROUP ACCOUNT (1)

\+ 0.3 cent times the number of tokens the other 3 group members place in the GROUP ACCOUNT (2)

\+ 1 cent times the number of tokens you place in your INDIVIDUAL ACCOUNT. (3)

Each other group member's earnings are determined similarly. Shown below are examples of possible earnings in a given round, where *=minimum and **=maximum.

(1) Examples of Your and Each Other Group Member's Possible Earnings from Your Tokens in the GROUP ACCOUNT

Your Tokens in Group Account	Your Earnings from Your Tokens in GROUP ACCOUNT	Each Other's Earnings from Your Tokens in GROUP ACCOUNT
0*	0.0 cents	0.0 cents
4	3.2 cents	1.2 cents
8	6.4 cents	2.4 cents
12	9.6 cents	3.6 cents
16	12.8 cents	4.8 cents
20	16.0 cents	6.0 cents
24	19.2 cents	7.2 cents
28	22.4 cents	8.4 cents
32	25.6 cents	9.6 cents
36	28.8 cents	10.8 cents
40	32.0 cents	12.0 cents
44	35.2 cents	13.2 cents
48	38.4 cents	14.4 cents
52	41.6 cents	15.6 cents
56	44.8 cents	16.8 cents
59**	47.2 cents**	17.7 cents**

(2) Examples of Your Possible Earnings from All Other Group Members' Tokens in the GROUP ACCOUNT

Others' Tokens in GROUP ACCOUNT	Your Earnings from Others' Tokens in GROUP ACCOUNT
0*	0.0 cents*
12	3.6 cents
24	7.2 cents
36	10.8 cents
48	14.4 cents
60	18.0 cents
72	21.6 cents
84	25.2 cents
96	28.8 cents
108	32.4 cents
120	36.0 cents
132	39.6 cents
144	43.2 cents
156	46.8 cents
168	50.4 cents
177**	53.1 cents**

(3) Examples of Your Possible Earnings from Your Tokens in Your INDIVIDUAL ACCOUNT

Your Tokens in INDIVIDUAL ACCOUNT	Your Earnings from Your Tokens in INDIVIDUAL ACCOUNT
0*	0.0 cents*
4	4.0 cents
8	8.0 cents
12	12.0 cents
16	16.0 cents
20	20.0 cents
24	24.0 cents
28	28.0 cents
32	32.0 cents
36	36.0 cents
40	40.0 cents
44	44.0 cents
48	48.0 cents
52	52.0 cents
56	56.0 cents
59**	59.0 cents**

DECISION/RECORD SHEET

Participant No. ______

Your endowment in each round is 59 tokens. Each token you place in your INDIVIDUAL ACCOUNT earns 1 cent for you. Each token you place in the GROUP ACCOUNT earns 0.8 cent for you and 0.3 cent for each other group member. Likewise, each token another group member places in the GROUP ACCOUNT earns 0.8 cent for that person and 0.3 cent for you. In column (1) write the number of your tokens that you wish to place in the GROUP ACCOUNT. This may be any integer number from 0 up to 59. Any remaining tokens from your endowment are placed automatically in your INDIVIDUAL ACCOUNT.

	(1)	(2)	(3)	(4)	(5)
Round	**Your Tokens Placed in GROUP ACCOUNT**	**Others' Tokens Placed in GROUP ACCOUNT**	**Your Earnings from GROUP ACCOUNT**	**Your Earnings from INDIVIDUAL ACCOUNT**	**Your Total Earnings**
1			$	$	$
2					
3					
4					
5					
6					
7					
8					
9					
10					

Total Earnings + $3 for Participation $________

APPENDIX B

Number of Tokens Contributed by Individual Subjects

Round:	1	2	3	4	5	6	7	8	9	10
LL 1	0	83	0	1	0	0	1	0	0	0
LL 2	33	30	17	15	16	11	15	3	5	1
LL 3	0	40	0	0	0	0	0	0	0	0
LL 4	83	0	0	0	0	0	0	0	0	0
LL 5	83	83	0	0	0	0	0	0	0	0
LL 6	83	0	0	0	0	0	0	0	0	0
LL 7	30	36	40	38	38	25	21	9	9	0
LL 8	0	0	65	83	0	3	3	3	0	8
LL 9	0	0	0	0	0	0	0	0	0	0
LL 10	32	17	20	12	15	11	12	9	8	7
LL 11	3	48	30	18	0	5	3	5	0	0
LL 12	83	50	40	0	0	0	0	0	0	0
LL 13	83	83	83	83	0	0	0	0	18	0
LL 14	0	0	0	0	0	0	1	0	0	0
LL 15	33	43	24	10	35	35	21	0	15	21
LL 16	83	0	0	0	0	0	0	0	0	0
LL 17	45	20	10	30	0	0	0	0	0	0
LL 18	83	0	0	0	0	0	0	0	0	0
LL 19	42	0	0	18	0	0	0	6	0	0
LL 20	6	18	21	0	3	4	0	5	2	1
LL 21	30	83	10	0	30	10	0	1	1	10
LL 22	0	0	0	0	0	0	0	0	0	0
LL 23	6	68	83	0	0	0	0	0	0	0
LL 24	58	0	0	0	0	0	0	0	0	0
HL 25	0	20	10	12	5	3	2	2	2	0
HL 26	36	20	50	32	25	59	42	46	14	0
HL 27	20	25	30	27	24	33	35	26	18	12
HL 28	32	40	45	40	25	32	40	40	32	37
HL 29	12	59	0	12	10	40	0	40	12	0
HL 30	30	20	15	17	5	10	15	0	0	0
HL 31	59	59	59	0	29	0	0	0	0	0
HL 32	30	59	40	35	50	30	25	30	30	30
HL 33	0	59	59	59	59	59	59	59	59	59
HL 34	0	0	0	50	40	50	0	0	0	0
HL 35	59	59	59	59	59	59	59	59	59	59
HL 36	20	10	5	15	32	40	0	30	10	5
HL 37	28	59	0	59	28	0	0	0	28	0
HL 38	9	50	29	9	15	15	0	10	0	0
HL 39	0	0	10	0	0	59	0	0	0	0
HL 40	50	20	20	25	25	30	30	25	25	20
HL 41	59	45	59	59	59	59	45	59	59	59

HL 42	59	59	59	59	59	59	59	0	0	0
HL 43	0	59	59	0	0	0	0	0	0	0
HL 44	0	0	0	0	0	0	0	0	0	0
HL 45	25	25	40	25	25	25	33	30	25	25
HL 46	0	35	0	0	0	59	0	0	0	0
HL 47	3	3	5	2	2	2	2	2	3	3
HL 48	30	5	15	25	50	17	20	16	17	15
LH 49	37	37	37	0	0	0	0	0	0	0
LH 50	20	35	10	30	30	30	20	15	10	0
LH 51	12	15	12	15	15	15	20	18	0	37
LH 52	20	25	20	5	10	20	30	0	0	0
LH 53	17	37	2	7	0	0	0	20	0	0
LH 54	0	0	0	0	0	0	0	0	0	0
LH 55	16	20	20	18	37	0	10	0	3	0
LH 56	5	4	37	0	37	0	0	0	15	0
LH 57	22	2	15	12	10	17	0	2	5	7
LH 58	17	25	32	23	18	15	17	17	15	12
LH 59	18	12	10	11	7	12	10	7	15	0
LH 60	0	0	0	0	0	0	0	0	0	0
LH 61	25	30	0	0	5	5	5	5	0	0
LH 62	25	0	0	0	0	10	10	0	0	0
LH 63	12	13	8	9	0	2	3	2	0	0
LH 64	14	10	10	10	7	10	0	0	10	0
LH 65	30	33	20	20	0	10	15	5	0	0
LH 66	17	0	0	0	0	0	0	0	0	0
LH 67	10	15	22	9	30	12	18	15	20	0
LH 68	37	37	37	0	0	0	0	0	0	0
LH 69	20	12	22	10	12	12	10	8	10	4
LH 70	0	0	0	0	0	0	0	0	0	0
LH 71	10	20	5	2	5	5	7	0	2	0
LH 72	37	37	37	37	37	37	37	37	0	0
HH 73	31	31	31	31	31	31	31	31	31	31
HH 74	10	20	15	15	20	20	20	20	31	25
HH 75	16	22	0	10	11	5	0	0	0	0
HH 76	15	10	10	15	20	5	5	20	5	0
HH 77	4	20	14	0	2	0	0	0	0	0
HH 78	5	20	26	31	31	31	31	31	31	31
HH 79	18	10	26	22	5	8	25	1	28	3
HH 80	31	31	31	0	0	0	0	0	0	0
HH 81	31	20	25	31	31	10	20	25	25	15
HH 82	20	17	0	5	0	2	0	0	31	0
HH 83	1	1	1	1	1	1	1	1	1	1
HH 84	0	0	0	10	0	0	0	0	0	0
HH 85	15	0	31	31	31	31	31	31	31	31
HH 86	10	10	10	8	10	8	0	0	10	0
HH 87	10	23	12	26	28	15	31	5	18	2
HH 88	12	8	8	10	9	5	3	6	8	10
HH 89	0	0	0	0	0	0	0	0	0	0
HH 90	18	22	15	27	23	20	21	19	22	17
HH 91	16	16	16	16	16	10	16	16	16	16

HH 92	31	31	31	31	0	31	31	31	31	31
HH 93	0	0	0	0	0	0	10	31	31	0
HH 94	15	20	20	25	28	28	20	31	31	31
HH 95	10	15	31	0	8	31	31	31	31	31
HH 96	8	5	3	1	8	1	0	5	0	0