Does Observation of Others Affect People's Cooperative Behavior? An Experimental Study on Threshold Public Goods Games*

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Abstract

This paper investigates whether observation of others affects people's behavior in the context of two threshold public goods games, the no rebate rule game and the utilization rebate rule game. In both rebate rules, subjects can get a benefit from the public good if their group can collect enough contributions to the public good. The difference between the two rules is how the excess amounts of contributions are distributed among people: the excess amounts of contributions are not distributed among people in the no rebate rule, while they are distributed among people equally in the utilization rebate rule. In spite of this difference, the two rules have the same Nash predictions. To see the effect of observation of others' actions, we ran three observational treatments under the two rebate rules. In one treatment, subjects could convey their individual contribution to other group members if they would like to reveal it. Although adding such a revelation stage does not change the equilibrium prediction, subjects in the utilization rebate rule cooperated significantly more than when they could not reveal their individual contribution to others. However, such positive effect was not clearly found in the no rebate rule. These experimental results show that the effect of observation of others can be different depending on games, even though the theoretical predictions of the games are similar. Also the results imply that the free-rider problem can be solved when people can use a reputation building strategy effectively.

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1. Introduction

There are a number of experimental and theoretical studies on public goods. The center issue dealt with in those works is the free-rider problem, which means that although people enjoy public goods, such as a publicly funded museum, a local festival, or a small park in the neighborhood, they do not like to share the cost to produce or maintain them. This pessimistic and parsimonious view of people's cooperative behavior has been questioned by many experimental economists. They have shown that people are not always totally selfish and do in fact exhibit some altruistic behaviors.[1] Recently some theorists have been trying to establish a theory to explain such anomalous results (Bolton (1991), Rabin (1993), Falk and Fishbacker (1998), Dufwenberg and Kirchsteiger (1998), Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Sally (2001)).

Most experimental works on public goods games have so far tried to eliminate social interaction among subjects as much as possible. For example, experimentalists usually assemble college students who have been randomly chosen from a campus, and put each of them in an individual booth so as not to allow them to communicate with each other directly. During the experiment, their individual decisions are anonymous, and after the experiment subjects are made to leave the laboratory separately after being paid confidentially for the experiment result. The purpose of such artificial procedures is to observe people's selfish motivation uncontaminated by social norms.

However, such an approach might be misleading if one wants to understand how people successfully produce or maintain a public good in reality. In reality, people can usually observe what others do more or less, and they can sometimes socially punish free-riders. It is impossible and meaningless, however, to create a situation in a laboratory exactly the same as reality. The usefulness of doing experiments is that we can examine separately possible social factors, such as

[1] See the excellent survey by Ledyard (1995).

the observation of others or the ability to punish[2], to understand how those factors affect people's cooperative behavior.

The main focus of this paper is the effect of observation of others. To examine the effect, we ran three observationally different treatments. Since the effect of observation of others must be different depending on (1) what kind of reward structure is given or (2) whether subjects play with the same people repeatedly or not, four different treatments were run sequentially within each observationally different session. For (1), two different threshold public goods games which have different reward structures were used. In both games, the public goods are not produced unless people collect contributions more than or equal to the minimum cost to produce them. In one of the games, people get a fixed payoff from the public good regardless of the amount of contributions collected (as long as they achieve or exceed the threshold level). In the other game, the more contributions are collected, the more people can get from the public good. In short, aggregate group payoff from the public good can be different between the two games depending on how much of a contribution people can collect.

For (2), subjects were allowed to play with the same group members for finitely repeated periods in one treatment and with different group members every period for finitely repeated periods in another treatment. Theoretically it is well known that people have a rational reason to continue to cooperate even in a finitely repeated non-cooperative game (Kreps et al. (1982)). Since the motivation of reputation building is a selfish motivation and is not inconsistent with the basic economics assumption about human behavior—that people are perfectly rational and selfish—it is expected to solve some economic problems. However, the question of what kind of informational environment leads people to a more efficient outcome by using a reputation building strategy has not been well investigated experimentally.

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[2] Fehr and Gächter (2000) found that the ability to punish makes people more cooperative. Most people punish free-riders even though punishing free-riders costs them. Such punishing behavior is not theoretically supported as rational behavior. Eventually people did not need to punish anybody since they became cooperative with each other.

The differences among the three observationally different treatments described in this paper are as follows. One is called the "No Revelation" treatment, which is similar to standard threshold public goods games. In this treatment, subjects can only observe group contribution at the end of every period. However, they are not given the information on individual contributions of others. The second treatment is called the "Voluntary Revelation" treatment. In this treatment, subjects can convey the information of their action to others (within each group) if they want. The third treatment is called the "Forced Revelation" treatment. In this treatment, people's individual actions are revealed to others (within each group) automatically at the end of each period by the experimenter. None of these revelation treatments change the theoretical prediction of the two threshold public goods games.

As Camerer (2003) mentioned, public goods games are blunt tools to detect what kind of social preference subjects have since they can not distinguish between many hypothetical human motivations to cooperate such as altruism, fairness or self-centered reciprocal preference. Therefore, the main purpose of this study is to evaluate how threshold public goods games work when people can observe others' behavior.

The composition of this paper is as follows. Section 2 reviews previous experimental literature on the effect of observation of others and threshold public goods games. Section 3 describes the theoretical background. Section 4 explains the experimental design and procedures. Section 5 provides some hypotheses. Section 6 describes experimental results. Finally, section 7 offers conclusions.

2. Previous literature

2.1 How does the Information of Others' Actions Affect People's Behavior?

As Duffy and Feltovich (1999) point out, economists have recently begun to recognize how the observation of others affects people's behavior, while anthropologists and behavioral psychologists have already hypothesized and discovered that people learn through the

observation of others.[3] Duffy and Feltovich (1999) show that observation of other players' actions and payoffs may affect the evolution of play in the repeated ultimatum game and the repeated best-shot game, which have similar equilibrium predictions. On the other hand, in the context of the linear public goods game, Wilson and Sell (1997) asked subjects to announce in the first stage how many contributions they would make in the following contribution stage. The announcement does not bind their decision making in the contribution stage. They showed that announcements before the contribution stage (cheap talk) and the information of what others have done did not facilitate cooperation.

One question is what kind of cheap talk would motivate people to cooperate with each other. When people are obligated to reveal their past behavior or their future plans, they may not be able to convey their willingness to cooperate effectively. For example, people might think that it is hard to distinguish others' intentions just by observing their actions or their plans. Suppose one subject announced that she planned to contribute half of her endowment to the public good. Such behavior probably indicates her preference to cooperate, but it could be that she just followed her intuition without understanding the game fully. In addition, if subjects have more than binary choices and there are more than two players involved in the game, it is not easy for them to track what other players did in all past periods and detect what intentions they have. Such a task might be psychologically costly for people and they may easily give up trying to cooperate with each other.

In this study, subjects were not asked to tell how much they were going to contribute, but they were asked whether they wanted to reveal their actions to others at the end of contribution stage. Although the content of the announcement is much simpler than that in Wilson and Sell (1997), the decision to reveal or not may convey his or her intention to cooperate with others

[3] For economics research, see Selten and Stoecker (1986) and Selten (1991). See Reichard (1938) for anthropology research and see Baudura and Walters (1963) for behavioral psychology research.

more effectively. Since such voluntary announcement of revelation does not bind subjects' following actions, adding this sort of stage does not change the theoretical prediction. Optimistically thinking, giving subjects this kind of announcement opportunity may help them to cooperate more than when such an opportunity is not given. Pessimistically thinking, people may consider any voluntary message as cheap talk and they may behave as if such a stage does not exist.

From these predictions, three kinds of treatments, "No Revelation," "Voluntary Revelation" and "Forced Revelation" were implemented. Although the experimenter showed individual actions within each group in Voluntary Revelation and Forced Revelation, identities of who made the actions were kept confidential.

2.2 Threshold Public Goods Games Experiments

There are a number of studies on threshold public goods games. Ledyard (1995) summarized experimental works in this field and concluded that adding a threshold level to the linear voluntary contribution mechanism has a positive effect on people's cooperative behavior. Cadsby and Maynes (1999) found that people contribute more when they can contribute any desired proportion of their endowments rather than when they are constrained to binary "all-or-nothing" contributions. They also found that offering a "money-back guarantee," where each person's contributions are returned when they can not achieve the threshold level, encouraged contribution to the public good. Marks and Croson (1998) examined three rebate rules of threshold public goods games and discovered the rate of equilibrium conversion and the variance of contributions differ significantly among the rules. Our threshold public goods games are similar to the "No Rebate" and "Utilization Rebate" rules in Marks and Croson (1998). Although they gave their subjects a money-back guarantee, subjects in this study did not have one. When there is a money-back guarantee, outcomes below the threshold level are all inefficient Nash equilibria. Without the money-back guarantee, there is a unique inefficient

equilibrium only when nobody contributes anything. Therefore, the inefficient equilibrium can be more clearly recognized by subjects. Since the purpose of this study is to see how observation of others can solve the free-riding problem, a more risky condition was intentionally placed on subjects (i.e. they might lose their contributions if others are not cooperative enough).

3. Theoretical Background

3.1 Rebates

3.1.1 No Rebate Rule[4]

Assume there are N individuals in a group. They are asked to contribute to a public good (g_i) from their individually given endowments (E). If they can collect contributions more than or equal to a threshold level (T), each person can receive a benefit from the public good ($r<E$) regardless of how much they contributed. Therefore, the utility function of each person (U_i) is derived as follows:

$$U_i = (E - g_i) + r \quad \text{if } \sum_{i=1}^{N} g_i \geq T,$$

$$U_i = E - g_i \quad \text{otherwise.}$$

Since the benefit from the public good is always the same as long as the threshold level of contributions is achieved, it is socially wasteful to invest more than the threshold level. Therefore the efficient Nash equilibrium is for people to contribute exactly as much as the threshold level. Since any outcome whose sum is equal to the threshold level is a Nash equilibrium, there are an infinite number of Nash equilibria if individuals can make any amount of contributions which is within their initial endowment. Since the benefit from the public good is strictly less than the initial endowment, no individual has an incentive to invest more than (or equal) to the benefit from the public good (r). Therefore, the set of efficient Nash equlibria is any combination of $\{g_1, g_2, \ldots, g_N\}$ such that $\sum_{i=1}^{N} g_i = T$ and $0 \leq g_i \leq r$. On the other hand the inefficient Nash

[4] The name of this rule is taken from Marks and Croson(1998).

equilibrium is that all individuals contribute nothing. The Pareto efficient outcome coincides with the efficient Nash equilibria.

3.1.2 Utilization Rebate Rule[5]

This rule is different from the No Rebate rule only in how the excess amounts of contributions above the threshold are distributed among people. In the No Rebate rule, the excess amounts of contributions are wasted and no benefit is created from them. In the Utilization Rebate rule, the excess amounts of contributions are distributed among people equally. Therefore, the utility function of each individual is derived as follows:

$$U_i = (E - g_i) + r + \frac{\alpha\left(\sum_{i=1}^{N} g_i - T\right)}{N} \qquad \text{if } \sum_{i=1}^{N} g_i \geq T,$$

$$U_i = E - g_i \qquad \text{otherwise.}$$

α is the sum of marginal individual benefit from the public good, which is larger than 1 and smaller than N.[6] The inefficient Nash equilibrium is that all individuals contribute nothing, which is the same as in the No Rebate rule. The set of efficient Nash equllibria is also the same as in the No Rebate rule. However, the Pareto efficient outcome in this rule is such that all individuals contribute all initial endowments, which is different from that in the No Rebate rule. Figure 1 shows how these two rules differ from a social benefits point of view.

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[5] The name of this rule is also taken from Marks and Croson (1998).

[6] Since it is assumed that people contribute to a public "good," α must be larger than 1. The social surplus will be maximized if all people contribute everything if α is larger than 1. In addition, α has to be smaller than N since it is assumed there exists the free-rider problem.

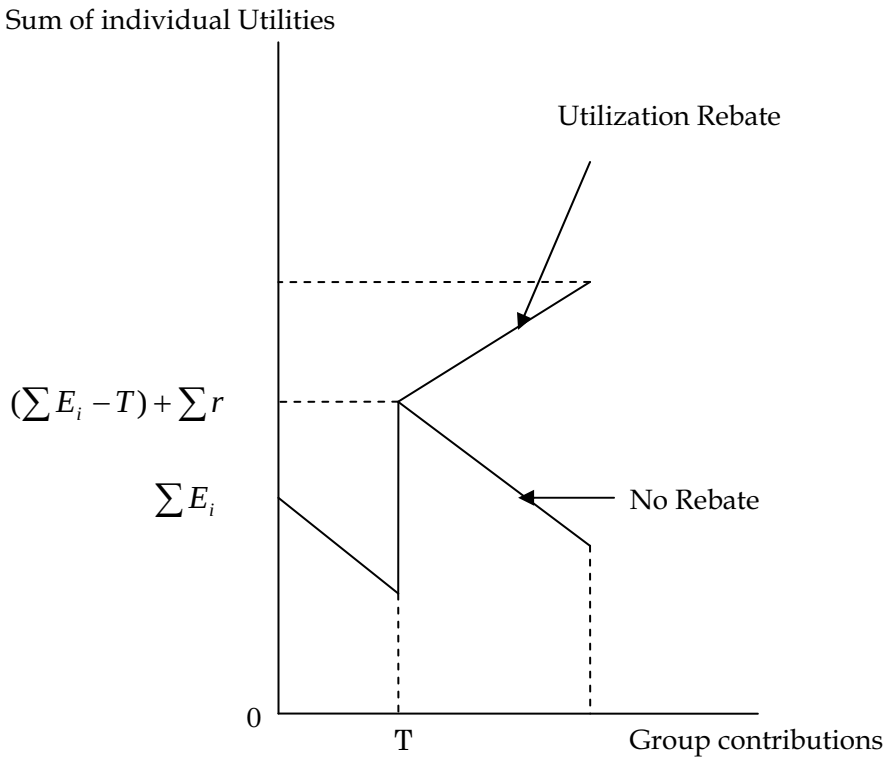


Figure 1. Efficiency Comparison

3.2 Observational Treatments

3.2.1 No Revelation

In the context of the above two public goods games, three kinds of observational treatments were compared. One treatment, considered as the base treatment, is called "No Revelation." In this treatment, each subject is informed only of her own payoff and the sum of individual contributions in her group at the end of every period. This is the standard threshold public goods game. Figure 2 shows the game tree of this treatment for one period.

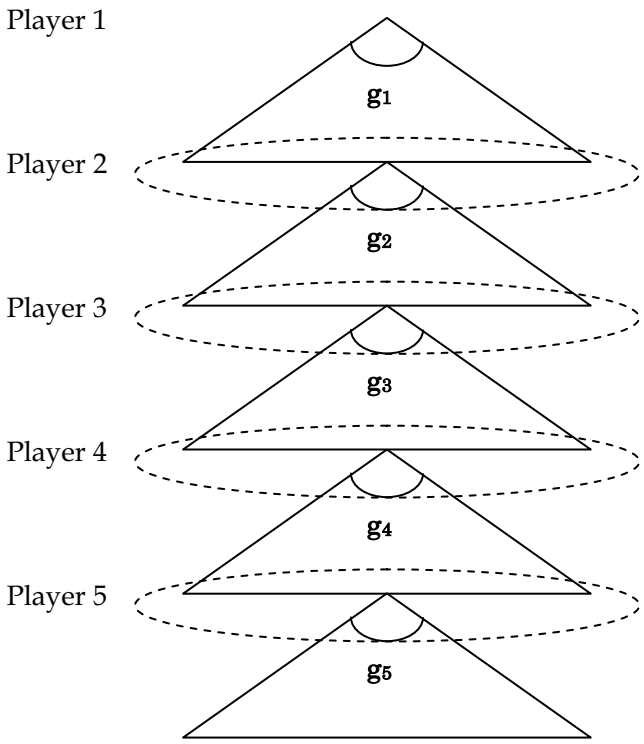


Figure 2. Game Tree for Each Period in No Revelation and Forced Revelation and Stage 2 in Voluntary Revelation

Subjects repeat this game for a finite number of periods without any communication. In addition, they experienced two subject-matching rules: the partners-matching rule and the strangers-matching rule. For the partners-matching rule, subjects play the game with the same group members throughout the experiment; for the strangers-matching rule, group members are shuffled every period. In the partners-matching rule, people can tell how cooperative their group is across periods, but they can not know the individual contributions of other group members and they can not convey the amount of their individual contributions to other group members. In strangers-matching, subjects can not continuously observe how cooperative their group is since their group members are shuffled every period. Therefore, subjects can guess how cooperative people in the session are as the experiment proceeds, but they can not influence other people by their past behavior.

3.2.2 Voluntary Revelation

Although the game in No Revelation consists of only one stage, the game in Voluntary Revelation consists of two stages as follows:

Stage 1 (Announcement Stage): Players announce to other people in their group whether they will show their individual contributions after stage 2.

Stage 2 (Contribution Stage): Upon observing the decision making of others in stage 1, players decide how many tokens to contribute to the public good.

Figure 3 shows the game tree of stage 1.

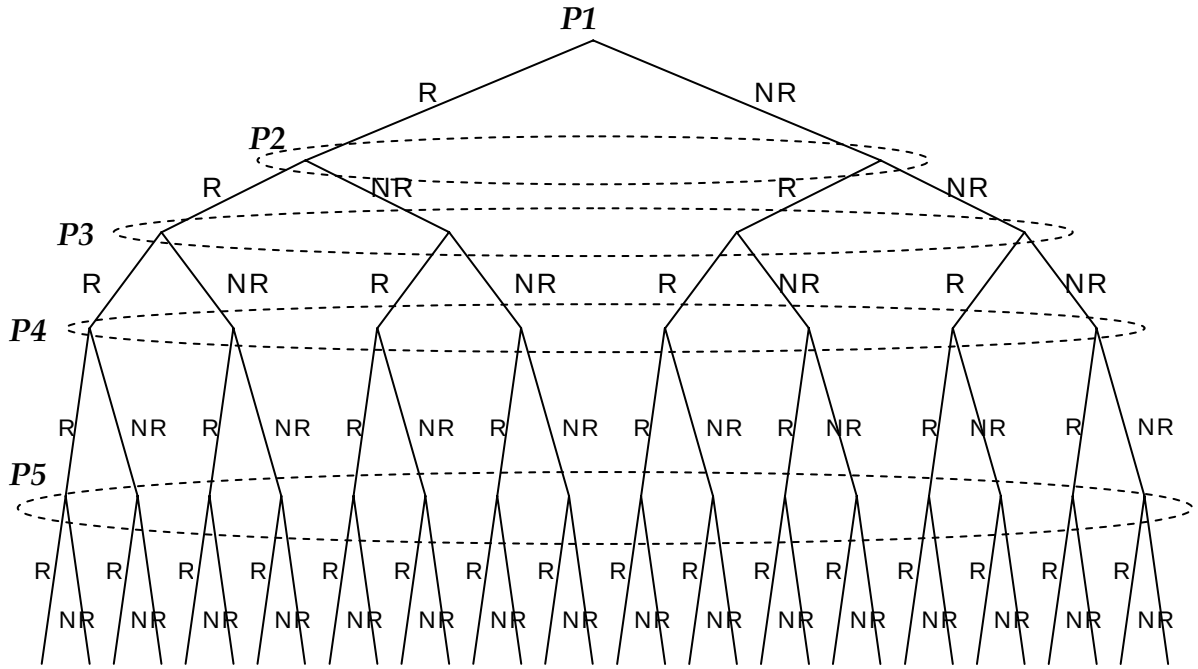


Figure 3. Stage 1 in Voluntary Revelation

(P1="Player 1," P2="Player 2," P3="Player 3," P4="Player 4," P5="Player 5," R="Reveal" NR="Not Reveal")

The game of stage 2 is the same as in the game of the No Revelation treatment. Upon knowing who (indicated by an anonymous ID) wants to reveal their contributions at the end of stage 2, subjects decide how much to contribute to the public good in stage 2. At the end of stage 2, only the decisions of people who decided in stage 1 to reveal their contributions are shown to the other group members. The decisions of people who decided not to reveal their contributions are not shown to anyone else. Theoretically decisions in stage 1 can be just cheap talk since they do not bind the action in stage 2 and do not cost at all in a monetary sense. However, if the decision to reveal successfully conveys the signal "I want to cooperate with you," then adding such an announcement stage might lead people to converge toward an efficient equilibrium. In the partners-matching treatments, people can convey not only their individual contribution of the current period but also their past behavior to other group members if they want. Although they are not sure how much the other group members will actually contribute in the contribution

stage, they can form some expectation of how cooperative their group is by observing how many people would like to reveal their contributions in the announcement stage and how much they contributed in the past periods. In the strangers-matching treatment, subjects can reveal their contributions at the end of the contribution stage of the current period if they want. However, they can not inform others how much they contributed in the past periods since group members are rematched every period.

3.2.3 Forced Revelation

The game tree per period in this treatment is exactly the same as in the No Revelation treatment. The difference from the No Revelation treatment is that individual decisions (g_i) are unconditionally revealed to all the members in the group at the end of every period. In every period, subjects decide how much to contribute simultaneously and independently. In the partners-matching treatments, they can convey their past behavior to other group members, while in the strangers-matching treatments, subjects can not influence others' behavior by their past actions since group members are rematched every period.

4. Experimental Design and Procedures

Subjects were recruited from various majors at Kyoto Sangyo University.[7] Three sessions were run for the three observational treatments. The experiment was programmed and conducted on personal computers with the software z-Tree (Fischbacher (1999)). No subject participated in more than one session. The instructions of each session are provided in Appendix 1. Subjects earned tokens in the experiment. They were told in the instructions that

[7] All subjects but one were undergraduates who applied for the experiments through the Internet homepage of the Experimental Economics Laboratory at Kyoto Sangyo University. Since one subject did not show up in the Voluntary Revelation session, the experimenter (author of this paper) asked one graduate student who has an office near the laboratory. He had never had contact with people involved in the laboratory and his major is management science not economics.

one token would be exchanged for 50 yen (about 45 cents) at the end of the experiment. Subjects were randomly assigned to a booth with partitions in front and on both sides of the desk. It was impossible for them to make direct contact with other subjects during the session. To make subjects understand the instructions clearly, two or four practice periods were run before the real experiment started (two practice periods for No Revelation and Forced Revelation, and four practice periods for Voluntary Revelation since this treatment has two stages).

Each session consists of four sequential treatments. Table 1 summarizes all the treatments. At the beginning of each session, subjects were told that they were going to experience four kinds of treatments, and that only the result of one of the treatments would be paid at the end of session. Therefore, there was no incentive for subjects to sacrifice their profits in one treatment in order to make higher profits in a later treatment.[8]

	Sessions (Observational treatments)		
	No Revelation	Voluntary Revelation	Forced Revelation
Observation of others	No	Conditional	Full
Treatment 1 (10 periods)	No Rebate/Partners		
Treatment 2 (10 periods)	Utilization Rebate/Partners		
Treatment 3 (10 periods)	No Rebate/Strangers		
Treatment 4 (10 periods)	Utilization Rebate/Strangers		
The number of total subjects in each session	25		
The number of subjects per group	5		

Table 1. Treatment details

The experimenter read the instructions for each treatment to subjects at the beginning of each

[8] However, since subjects could learn how cooperative others are in each treatment, the results in the following treatments are not completely independent from the results of the previous treatments.

treatment. Subjects were not aware of the details of each treatment until just before the treatment began. The first treatment was the No Rebate rule with partners-matching. In this treatment, subjects played the No Rebate rule threshold public goods game with the same group members for 10 periods. The number of repetitions was explained in the instructions. Subjects practiced clicking their mouses according to the experimenter's directions to get used to how to manipulate the computers and how to understand the information shown on the screen for their decision making. They were not allowed to make any free decisions until the real period started.

The second treatment is the Utilization Rebate rule with partners-matching. In this treatment, subjects played the Utilization Rebate rule threshold public goods game with the same group members for 10 periods. The experimenter explained the second treatment just before the second treatment started.

After subjects had completed their decision making for the 10^{th} period of the Utilization Rebate with partners-matching treatment, they experienced the third treatment, the No Rebate threshold public goods game with Strangers-matching. The experimenter explained that the experiment was similar to the first treatment, but the group members would be changed every period.

The fourth treatment, the Utilization Rebate rule with Strangers, followed the No Rebate rule with strangers-matching treatment. Subjects were told at the beginning of the treatment that this treatment would be the last in the session. The experimenter explained that the experiment was similar to the second treatment, but the group members would be changed every period.

In short, each session proceeds in the following order: (1) No Rebate/Partners, (2) Utilization Rebate/Partners, (3) No Rebate/Strangers, (4) Utilization Rebate/Strangers. Since the experimenter explained the instructions of each treatment just before each treatment started, subjects could not plan how to behave in each treatment at the beginning of session. 25 people participated in each session. We made five groups of five subjects in each session (N=5). In the No Revelation session, individual decisions were totally anonymous among subjects. In the

Voluntary Revelation session and the Forced Revelation session, each subject was given her own ID number in her group. ID numbers ranged from 1 to 5 and they allowed subjects to track the actions of other group members. However, the identities of subjects and where they sat were kept confidential. Although the member ID was the same throughout the partners-matching treatments, it could be a different ID number every period in the strangers-matching treatments since subjects were rematched with different group members every period and the member ID was decided according to the order of seat numbers (each computer terminal has its own ID).

Subjects were given an initial endowment of 5 tokens (E=5) at the beginning of every period. They were asked to divide their tokens into a private account and a public account.[9] The amount of contribution they could make was constrained to only integer numbers. In the No Rebate rule treatment, subjects could receive 4 tokens (r=4) as the benefit from the public good when their group could collect contributions more than or equal to 10 tokens (T=10). In the Utilization Rebate rule treatment, if a group could collect contributions more than or equal to 10 tokens, the total contributions of the group were added together and doubled by the experimenter (α=2) and then equally distributed among the group members. When a group collected exactly as much as 10 tokens, the distributed benefit from the public good was 4 as in the No Rebate rule treatment. However, if a group collected more than 10 tokens, the individual benefit from the public good became more than 4 (up to 10). Therefore, in both rules, the set of efficient Nash equilibria consists of any combination of $\{g_1, g_2, g_3, g_4, g_5\}$ such that $\sum_{i=1}^{5} g_i = 10$ and $0 \le g_i \le 4$, while the inefficient Nash equilibrium is $\{g_i\} = 0$.

All sessions lasted about two hours. The average payment for subjects across the three sessions was 3,348 yen (about 30 US dollars).

5. Hypotheses

[9] We did not use the word "invest," "private," nor "public." Subjects were asked to "divide" their 5 tokens into a personal account and a group account.

5.1 No Rebate Rule versus Utilization Rebate Rule

Croson and Marks (2000) analyzed previous literature on threshold public goods experiments and clarified that the factor called "Step Return" (SR, hereafter) influenced people' cooperative behavior. They formulated SR as follows (p. 242):

$$\mathrm{SR} = \frac{\text{aggregate group payoff from the public good}}{\text{total contribution threshold}}.$$

The SR represents, "the ratio of an individual's value of the public good to his share of the cost " (p. 242). In the No Rebate rule in our experiment, if a group collects contributions exactly as much as the threshold, SR becomes 2. Similarly, in the Utilization Rebate rule, if a group collects contributions exactly as much as the threshold, SR is also 2. Therefore, both SRs are the same when the group contribution equals the threshold level. However, they will not be the same in the case that subjects in the Utilization Rebate rule collect more than the threshold level. The SR in the No Rebate rule is invariant to the sum of individual contribution, while the SR in the Utilization Rebate rule is the increasing function of aggregate group payoff from the public good. Croson and Marks (2000) concluded that the higher the SR, the greater the contributions should be. Therefore, the following hypothesis is derived:

Hypothesis 1: Group contributions in the Utilization Rebate rule will be significantly higher than those in the No Rebate rule regardless of whether subjects play with the same group members or different group members every period, and regardless of whether they can observe others' actions.

5.2 Hypotheses on Observation of Others

As Wilson and Sell (1997) noted, two factors are important for subjects to get onto a cooperative equilibrium path. One is that subjects need information on the past actions of others. The other is that subjects need to be given some method of preplay communication to commit

themselves to carry out a promise. Regarding the first factor, subjects in our experiment can observe their own group contribution of past periods in all revelation treatments. However, subjects can not observe any individual contributions of other group members in the No Revelation treatment. In Voluntary Revelation, subjects can observe individual contributions of other group members if they agreed to show them. In the Forced Revelation treatment, subjects can observe individual contributions of other group members unconditionally. Regarding the second factor, there is no clear promise (how much to contribute) that subjects can make in the announcement stage in the Voluntary Revelation treatment. However, in the partners-matching treatment, people can signal how they commit their intentions by continuing to reveal their contributions to others and contributing a certain amount of tokens. On the other hand, in the partners-matching treatments in Forced Revelation, although subjects can not announce anything before they contribute to the public goods, they can signal their commitments by their past actions. Therefore, we predict that subjects might use a reputation building strategy to achieve a more profitable mutual outcome in the Voluntary Revelation and the Forced Revelation treatments. The following hypothesis is offered:

Hypothesis 2: Group contributions in the partners-matching treatment in Voluntary Revelation and the partners-matching treatment in Forced Revelation will be significantly higher than those in the partners-matching treatment in No Revelation.

It is not easy to predict whether Voluntary Revelation or Forced Revelation leads to more cooperation among subjects since the amount of information about others' contributions might differ between the two treatments. In addition, it is not certain whether or how the content of the announcement stage in Voluntary Revelation affects people's behavior. If the amount of information of others matters more than others' willingness to reveal their contributions, the following hypothesis is derived:

Hypothesis 3: In the partners-matching treatments, group contributions in Forced Revelation will be the highest among the three observational treatments. Group contributions in Voluntary Revelation will be the second highest and those in No Revelation will be the lowest.

On the other hand, one might expect Voluntary Revelation to lead to greater cooperation because subjects in the partners-matching treatments in Voluntary Revelation can convey their intentions to cooperate before the contribution stage. The availability of such a clear message in Voluntary Revelation might facilitate more cooperation than in Forced Revelation. The following counter hypothesis is derived:

Hypothesis 4: Group contributions in the partners-matching treatments in Voluntary Revelation will be significantly higher than in those in the partners-matching treatments in Forced Revelation.

Hypothesis 4 is the optimistic prediction for Voluntary Revelation because the information revealed by subjects in stage 1 could be regarded simply as cheap talk and people might not take the information seriously. Further, it is possible than no one in the group volunteers to reveal their individual actions. Therefore, the following counter hypothesis is presented:

Hypothesis 5: Group contributions in all treatments in Voluntary Revelation will not be significantly different from those in No Revelation.

Since revealing their actions costs nothing in Voluntary Revelation, all subjects may be indifferent between revealing their actions or not. Therefore, they might as well reveal their

actions as in Forced Revelation. In addition, people may not be affected at all by the voluntariness of others to reveal their actions. Therefore, the following hypothesis is derived:

Hypothesis 6: Group contributions in all treatments in Voluntary Revelation will not be significantly different from those in Forced Revelation.

5.3. Nash Prediction and Convergence

Since Forced Revelation gives subjects complete information about what others contributed, subjects in Forced Revelation should be able to figure out which equilibrium their group is heading for most clearly. Therefore we make the following hypothesis:

Hypothesis 7: Convergence toward any equilibrium in the partners-matching treatments in Forced Revelation will be quicker and closer than that in the partners-matching treatments in Voluntary Revelation and No Revelation. Furthermore, convergence toward any equilibrium in the partners-matching treatments in Voluntary Revelation will be quicker and closer than that in the partners-matching treatments in No Revelation.

Marks and Croson (1998) observed more Nash outcomes in the No Rebate rule than in the Utilization Rebate rule. Although subjects in our study are not provided a money back guarantee, such a result might be observed in our experiment. Therefore, the following hypothesis is offered:

Hypothesis 8: Group contributions in the No Rebate rule will be significantly closer to Nash equlibria than the Utilization Rebate rule. And the frequency of Nash outcomes will be greater in the No Rebate rule than in the Utilization Rebate rule in both the partners-matching treatment and the strangers-matching treatment and across all

three observational treatments.

6. Experimental Results

6.1 Comparison of Group Contributions among Observational Treatments

Figure 4 shows the boxplots to compare the results of the three revelation treatments. To analyze the difference of means of group contributions among the three observational sessions, analysis of variance (ANOVA) was used. Table 2 summarize the results. In the No Rebate/Partners treatment, the hypothesis of equal means among the three observational treatments is not rejected ($p=0.76$). This result is also supported in the Scheffé multiple-comparison test. However, the hypothesis for equal variances is highly rejected. Therefore, the result of the ANOVA *F* test is not strongly trustable. In the Utilization Rebate/Partners treatment, the hypothesis of equal means among the three observational treatments is rejected ($p<0.00005$). The Scheffé multiple-comparison finds significant difference between the means in any pair of comparison ($p<0.05$). However, the hypothesis of equal variances is rejected ($p<0.10$). Therefore, the result of the ANOVA *F* test is not strongly trustable. In the No Rebate/Strangers treatment, the hypothesis of equal means among the three observational treatments is rejected ($p=0.01$). The Scheffé multiple-comparison finds significant difference only between the means of No Revelation versus Voluntary Revelation ($p<0.10$). Since the hypothesis of equal variances is accepted, the result of the ANOVA *F* test is valid. In the Utilization Rebate/Strangers treatment, the hypothesis of equal means among the three observational treatments is rejected ($p<0.00005$). The Scheffé multiple-comparison finds significant difference between the means of No Revelation versus Voluntary Revelation and No Revelation versus Forced Revelation. Since the hypothesis of equal variances is accepted, the result of the ANOVA *F* test is valid.

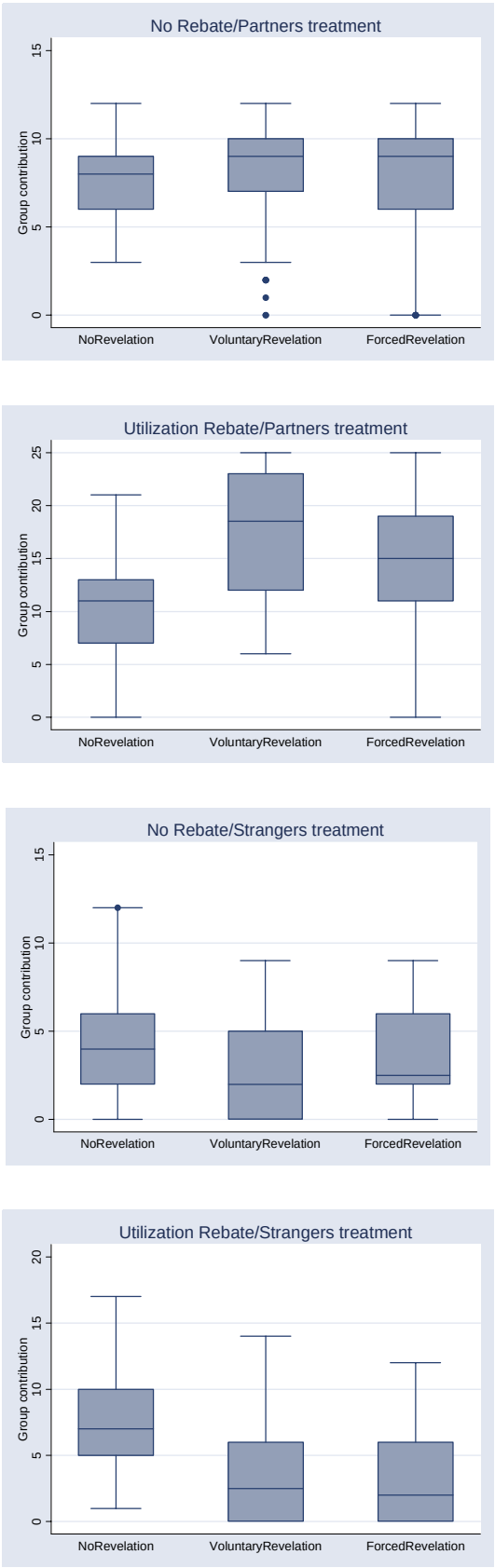


Figure 4. Comparison of Group Contributions

Table 2. Analysis of Variance of Group Contributions

No Rebate/Partners

Summary of Group Contribution					
Observational Treatments	*Mean*		*Std. Dev.*		*Frequency*
No Revelation	7.3		2.29		50
Voluntary Revelation	7.72		3.23		50
Forced Revelation	7.34		3.69		50
Analysis of Variance					
Source	*SS*	*df*	*MS*	*F*	*Prob>F*
Between groups	5.37	2	2.69	0.28	0.76
Within groups	1435.80	147	9.77		
Scheffé multiple comparison test					
Row mean-Column mean	Forced Revelation			No Revelation	
No Revelation	-0.04 (1.00)				
Voluntary Revelation	0.38 (0.83)			0.42 (0.80)	
Bartlett's test for equal variance: $\chi^2(2)=10.87$			Prob> $\chi^2(2)=0.00$		

Utilization Rebate/Partners

Summary of Group Contribution					
Observational Treatments	*Mean*		*Std. Dev.*		*Frequency*
No Revelation	10.16		5.06		50
Voluntary Revelation	17.58		5.89		50
Forced Revelation	14.42		6.95		50
Analysis of Variance					
Source	*SS*	*df*	*MS*	*F*	*Prob>F*
Between groups	1386.49	2	693.25	19.16	0.00
Within groups	5319.08	147	36.18		
Scheffé multiple comparison test					
Row mean-Column mean	Forced Revelation			No Revelation	
No Revelation	-4.26 (0.00)				
Voluntary Revelation	3.16 (0.03)			7.42 (0.00)	
Bartlett's test for equal variance: $\chi^2(2)=4.87$			Prob> $\chi^2(2)=0.09$		

Table 2. Continued

No Rebate/Strangers

Summary of Group Contribution					
Observational Treatments	*Mean*		*Std. Dev.*		*Frequency*
No Revelation	4.42		3.16		50
Voluntary Revelation	2.58		2.74		50
Forced Revelation	3.84		2.96		50
Analysis of Variance					
Source	*SS*	*df*	*MS*	*F*	*Prob>F*
Between groups	88.49	2	44.25	5.06	0.01
Within groups	1285.08	147	8.74		
Scheffé multiple comparison test					
Row mean-Column mean	Forced Revelation			No Revelation	
No Revelation	0.58 (0.62)				
Voluntary Revelation	-1.26 (0.11)			-1.84 (0.01)	
Bartlett's test for equal variance: $\chi^2(2)=0.96$			Prob> $\chi^2(2)=0.62$		

Utilization Rebate/Strangers

Summary of Group Contribution					
Observational Treatments	*Mean*		*Std. Dev.*		*Frequency*
No Revelation	7.4		4		50
Voluntary Revelation	3.78		3.86		50
Forced Revelation	3		3.43		50
Analysis of Variance					
Source	*SS*	*df*	*MS*	*F*	*Prob>F*
Between groups	551.21	2	275.61	19.38	0.00
Within groups	2090.58	147	14.22		
Scheffé multiple comparison test					
Row mean-Column mean	Forced Revelation			No Revelation	
No Revelation	4.40 (0.00)				
Voluntary Revelation	0.78 (0.59)			-3.62 (0.00)	
Bartlett's test for equal variance: $\chi^2(2)=1.23$			Prob> $\chi^2(2)=0.54$		

Further, group contributions among the three observational treatments were compared period by period by using a Mann-Whitney U test.[10] Table 3 and table 4 provide the results. The results basically confirms the conclusion by ANOVA.

In the No Rebate/Partners treatment, there is almost no significant difference in group contributions in any comparison (No Revelation versus Voluntary Revelation, No Revelation versus Forced Revelation and Voluntary Revelation versus Forced Revelation).

In the Utilization Rebate/Partners treatment, the comparison between No Revelation and Voluntary Revelation finds that group contributions in Voluntary Revelation are significantly higher than those in No Revelation in almost all periods, which was not clearly detected by the ANOVA *F* test because of the inequality of variance among the data of the three observational treatments. On the other hand, there is no period that shows statistical difference in the comparison between No Revelation and Forced Revelation or the comparison between Voluntary Revelation and Forced Revelation. In short, observation of others has no impact when people play the No Rebate rule game with the same people for finite periods. However, under the Utilization Rebate rule with partners, the Voluntary Revelation method encourages people to cooperate more.

In No Rebate/Strangers, although the ANOVA *F* test finds significant difference of means

[10] Since the independent data is the session level data which consists of only five samples in each period, we used a Mann-Whitney U test rather than t-test.

Table 3. Comparison of Group Contributions in Partners-matching Treatments between Observational Treatments Using Mann-Whitney U test

(*NoR=No Revelation **VoR=Voluntary Revelation ***FoR=Forced Revelation)

No Revelation vs. Voluntary Revelation							No Revelation vs. Forced Revelation							Forced Revelation vs. Voluntary Revelation						
Matching	Rebate	Period	Group Contribution		Z	p-value (if p≤0.10)	Matching	Rebate	Period	Group Contribution		Z	p-value (if p≤0.10)	Matching	Rebate	Period	Group Contribution		Z	p-value (if p≤0.10)
			NoR	**VoR**						**NoR**	**FoR**						**FoR**	**VoR**		
Partners	**No Rebate**	1	7.60	8.60	-0.96	_	**Partners**	**No Rebate**	1	7.60	8.00	-0.44	_	**Partners**	**No Rebate**	1	8.00	8.60	-0.55	_
		2	7.20	9.40	-1.80	0.07			2	7.20	9.60	-2.22	0.03			2	9.60	9.40	0.22	_
		3	8.00	9.60	-1.75	0.08			3	8.00	8.20	-0.11	_			3	8.20	9.60	-0.87	_
		4	7.80	9.00	-0.76	_			4	7.80	7.80	0.00	_			4	7.80	9.00	-0.65	_
		5	7.40	8.00	-0.43	_			5	7.40	8.80	-0.96	_			5	8.80	8.00	0.45	_
		6	7.40	7.20	-0.11	_			6	7.40	7.80	-0.64	_			6	7.80	7.20	0.56	_
		7	6.80	6.80	-0.53	_			7	6.80	6.80	-0.53	_			7	6.80	6.80	0.00	_
		8	6.80	6.00	0.00	_			8	6.80	6.00	0.00	_			8	6.00	6.00	0.00	_
		9	7.00	6.40	0.32	_			9	7.00	5.00	0.75	_			9	5.00	6.40	-0.53	_
		10	7.00	6.20	0.21	_			10	7.00	5.40	0.64	_			10	5.40	6.20	-0.43	_
	Utilization Rebate	1	12.00	17.20	-1.79	0.07		**Utilization Rebate**	1	12.00	16.20	-1.57	_		**Utilization Rebate**	1	16.20	17.20	-0.21	_
		2	12.20	18.80	-1.79	0.07			2	12.20	17.00	-1.59	_			2	17.00	18.80	-0.42	_
		3	12.40	19.40	-1.80	0.07			3	12.40	17.40	-1.16	_			3	17.40	19.40	-0.73	_
		4	11.00	18.40	-1.99	0.05			4	11.00	16.60	-1.47	_			4	16.60	18.40	-0.74	_
		5	10.20	17.80	-1.79	0.07			5	10.20	15.40	-1.38	_			5	15.40	17.80	-0.53	_
		6	11.20	17.40	-0.95	_			6	11.20	14.20	-0.53	_			6	14.20	17.40	-0.75	_
		7	9.60	19.00	-2.23	0.03			7	9.60	13.80	-1.36	_			7	13.80	19.00	-0.85	_
		8	9.80	18.40	-1.49	_			8	9.80	13.00	-0.52	_			8	13.00	18.40	-0.95	_
		9	8.00	17.20	-1.58	_			9	8.00	12.00	-0.73	_			9	12.00	17.20	-0.94	_
		10	5.20	12.20	-1.80	0.07			10	5.20	8.60	-0.37	_			10	8.60	12.20	-0.73	_

Table 4. Comparison of Group Contributions in Strangers-matching Treatments between Observational Treatments Using Mann-Whitney U test

(*NoR=No Revelation **VoR=Voluntary Revelation ***FoR=Forced Revelation)

No Revelation vs. Voluntary Revelation							**No Revelation vs. Forced Revelation**							**Forced Revelation vs. Voluntary Revelation**						
Matching	Rebate	Period	Group Contribution		Z	p-value (if p≤0.10)	Matching	Rebate	Period	Group Contribution		Z	p-value (if p≤0.10)	Matching	Rebate	Period	Group Contribution		Z	p-value (if p≤0.10)
			NoR	**VoR**						**NoR**	**FoR**						**FoR**	**VoR**		
Strangers	**No Rebate**	1	8.80	6.60	1.62	0.10	**Strangers**	**No Rebate**	1	8.80	7.60	0.87	_	**Strangers**	**No Rebate**	1	7.60	6.60	1.09	_
		2	7.20	5.40	0.74	_			2	7.20	6.80	0.22	_			2	6.80	5.40	0.65	_
		3	5.80	4.20	0.98	_			3	5.80	6.40	-0.53	_			3	6.40	4.20	1.49	_
		4	5.00	3.60	1.06	_			4	5.00	5.60	-0.64	_			4	5.60	3.60	1.48	_
		5	5.80	2.20	2.00	0.05			5	5.80	3.80	1.57	_			5	3.80	2.20	1.28	_
		6	2.40	1.00	1.39	0.07			6	2.40	2.60	0.00	_			6	2.60	1.00	1.28	_
		7	2.60	1.20	1.19	_			7	2.60	1.60	0.75	_			7	1.60	1.20	0.45	_
		8	2.80	1.40	0.79	_			8	2.80	1.60	0.34	_			8	1.60	1.40	0.69	_
		9	2.20	0.20	1.53	_			9	2.20	1.20	0.67	_			9	1.20	0.20	1.55	_
		10	1.60	0.00	2.37	0.02			10	1.60	1.20	0.54	_			10	1.20	0.00	1.96	0.05
	Utilization Rebate	1	10.00	9.00	0.53	_		**Utilization Rebate**	1	10.00	7.00	0.95	_		**Utilization Rebate**	1	7.00	9.00	-0.64	_
		2	10.60	8.60	0.95	_			2	10.60	7.60	1.16	_			2	7.60	8.60	-0.76	_
		3	8.80	5.20	1.37	_			3	8.80	5.20	1.36	_			3	5.20	5.20	0.32	_
		4	9.60	4.20	1.99	0.05			4	9.60	3.60	1.68	0.09			4	3.60	4.20	-0.43	_
		5	6.60	4.60	0.85	_			5	6.60	2.00	2.23	0.03			5	2.00	4.60	-0.98	_
		6	7.60	2.20	2.34	0.02			6	7.60	1.60	2.13	0.03			6	1.60	2.20	-0.75	_
		7	6.80	2.00	1.92	0.06			7	6.80	0.80	2.23	0.03			7	0.80	2.00	-0.47	_
		8	7.20	1.00	2.44	0.01			8	7.20	1.20	2.48	0.01			8	1.20	1.00	-0.83	_
		9	4.20	0.60	2.34	0.02			9	4.20	0.20	2.60	0.01			9	0.20	0.60	-0.78	_
		10	2.60	0.40	2.29	0.02			10	2.60	0.80	1.95	0.05			10	0.80	0.40	-0.39	_

between No Revelation and Voluntary Revelation, such difference is not consistent throughout the treatments in the comparison of the data period by period (period 1, 5, 6, 10 show significant difference). There is almost no significant difference between No Revelation and Forced Revelation and between Forced Revelation and Voluntary Revelation.

In Utilization Rebate/Strangers, the results are consistent with the conclusion of the ANOVA test. One interesting finding is that the average group contribution in No Revelation is higher than in Voluntary Revelation and Forced Revelation in most periods. This result indicates that providing the information about individual contributions does not lead people to a more efficient outcome but facilitates the spread of distrust among people and makes them act less cooperatively.

6.2 The Effect of Utilization Rebate

Figure 5 shows group contributions across periods in No Rebate/Partners and Utilization Rebate/Partners of all observational treatments. The graphs on the left-hand side are for the No Rebate rule and the graphs on the right-hand side are for the Utilization Rebate rule. In the results of the No Rebate rule, exceeding the threshold (=10) is rarely observed.

Next, the hypothesis that the mean group contribution is equal to 10 was checked statistically in No Rebate/Partners of all observational treatments. The counter hypothesis that the mean group contribution is lower than 10 was supported by a t-test in all revelation

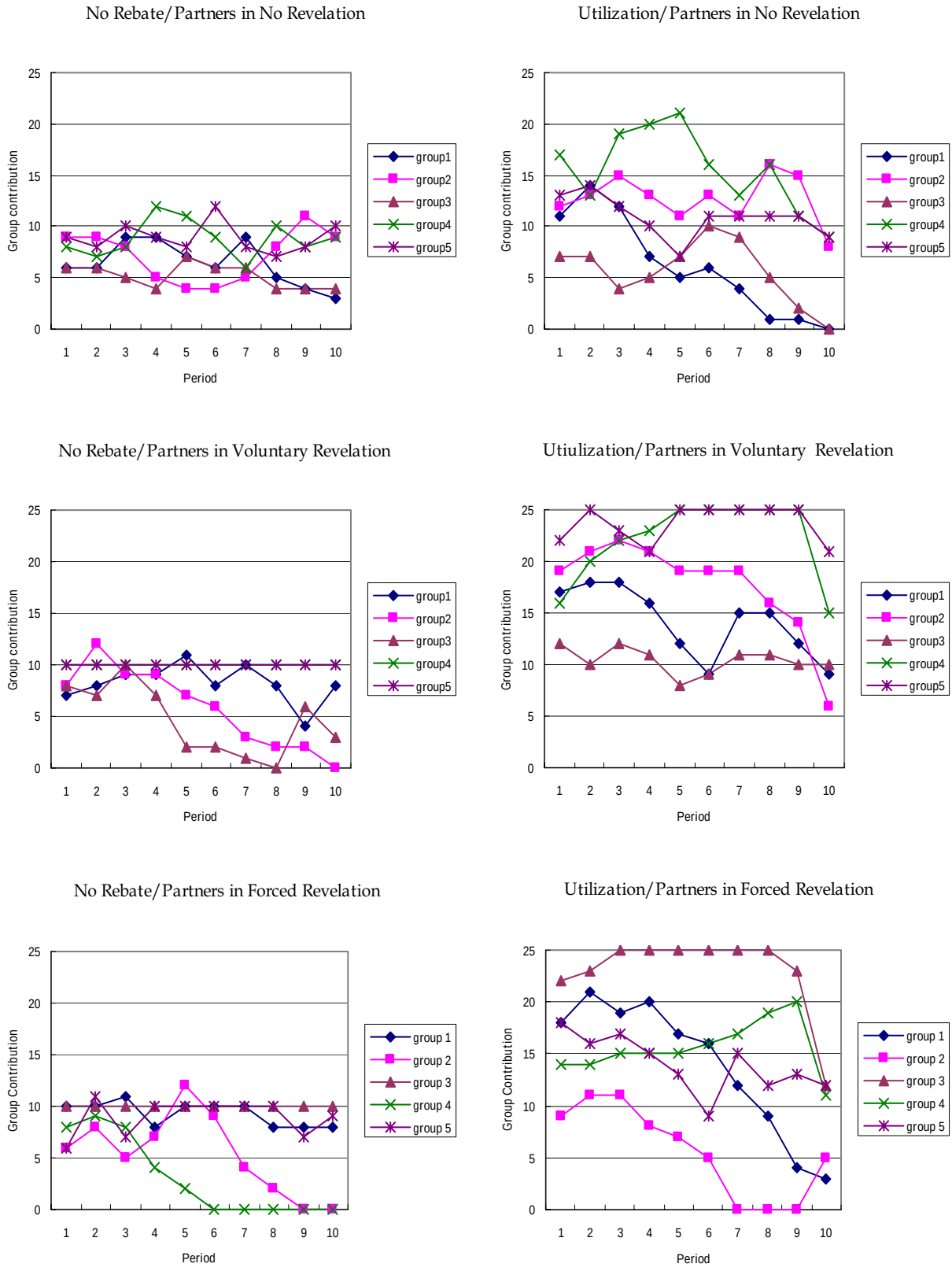


Figure 5. Comparison between the No Rebate Rule and the Utilization Rebate Rule in the Partners-matching Treatments

treatments (one-tailed $p<0.00005$).

Looking at the data closely, although all groups in No Rebate/Partners with No Revelation failed to achieve the threshold level in most periods, no group fell to the inefficient equilibrium in any period. In contrast, two groups in No Rebate/Partners with Voluntary Revelation successfully chose the efficient (Pareto) Nash equilibrium from period 1 to the end period consistently, while the other groups failed to achieve the threshold in almost all periods. In No Rebate/Partners with Forced Revelation, one group successfully chose the efficient Nash equilibrium from period 1 to the end period consistently and two other groups contributed around the threshold across periods, and the rest of the remaining two groups fell to the inefficient equilibrium (one of the groups fell to the inefficient equilibrium from period 6 till the end period). These results support hypothesis 7 and are summarized in observation 1.

Observation 1: People can better coordinate toward a Nash equilibrium in the No Rebate rule with partners-matching in both Voluntary Revelation and Forced Revelation. However, the observation of others does not always facilitate the convergence toward an efficient equilibrium. Group contributions rarely exceed the threshold.

Let us focus on the results of Utilization Rebate/Partners (right-hand side of figure 4). The variance of results of Utilization Rebate with partners-matching looks much higher than the results in No Rebate with partners (left hand side of figure 4). The hypothesis of equal variance between No Rebate/Partners and Utilization/Partners was rejected by a Bartlett's χ^2 test in all revelation treatments ($p<0.00005$). A similar difference of variance was observed in Marks and Croson (1998).

Looking at the results closely, the results of Utilization Rebate/Partner with No Revelation show that no group could achieve the Pareto optimum outcome. Two groups converged toward the inefficient equilibrium at the end period and the other three groups succeeded to achieve or

exceed the threshold level in almost all periods before the end period. The hypothesis that the mean group contribution in the Utilization/Partners with No Revelation treatment is equal to 10 was checked and it was not rejected statistically (two-tailed p=0.82).

In Utilization Rebate/Partners with Voluntary Revelation, groups achieved the Pareto optimal outcome 11 times. The groups in this treatment contributed above the threshold level in nearly every period. The hypothesis that the mean group contribution is higher than 10 was statistically supported by a t-test (one-tailed p<0.00005).

In Utilization Rebate/Partners with Forced Revelation, one group achieved the Pareto optimum outcome 6 times from period 3 to period 8. There were more failures to achieve the threshold in this treatment than in Utilization/Partners with Voluntary Revelation. The hypothesis that the mean group contribution is higher than 10 was statistically supported by a t-test (one-tailed p<0.00005). These results are summarized in observation 2 and observation 3.

Observation 2: In the Utilization Rebate rule with partners matching, the mean group contribution is equal to or higher than the threshold regardless of observational treatments. This observation and observation 1 support hypothesis 1.

Observation 3: When people can observe others' actions, some groups focus on the Pareto optimum outcome rather than an efficient Nash equilibrium. Figure 6 compares average group contributions between the No Rebate rule and the Utilization Rebate rule in Strangers-matching treatments of all observational treatments period by period.[11] The common aspect among all observational treatments is that the average group

[11] It is useful to see how group contributions change across periods in both the No Rebate partners-matching treatment and the Utilization Rebate partners matching treatment.. Therefore, we show all independent data (group contributions) in figure 4. However, since we rematched all subjects to put them in a new group every period in the strangers-matching treatments, it is not of interest to see how each group evolves as the experiment proceeded. Therefore, we averaged all the group contribution data per period in the strangers-matching treatments in figure 5.

contributions almost monotonically decreased toward the inefficient equilibrium as the experiments proceeded.

Table 4 reports the statistical results comparing the No Rebate rule and the Utilization Rebate rule in each observational treatment period by period. Hypothesis 1 is clearly supported in the partners-matching treatments of Voluntary Revelation. In Forced Revelation, group contributions in the Utilization Rebate rule are significantly higher than those in the No Rebate rule mainly in the first-half periods. In the partners-matching treatments in No Revelation, although the average group contributions of Utilization Rebate are higher than those of No Rebate in each period, such difference is not significant in all periods except for period 1 and period 2.

Contrary to the partners-matching treatments results, in the strangers-matching treatments, hypothesis 1 is more supported in No Revelation than Voluntary Revelation and Forced Revelation. There is almost no period that has a significant difference both in Voluntary Revelation and Forced Revelation. However, 4 middle periods show significant difference in No Revelation. This result is consistent with the observation of the previous section that observation of others spreads distrust among subjects and hinders them from acting cooperatively in Voluntary Revelation and Forced Revelation.[12] Figure 6 indicates that the speed of the spread of distrust among people is faster in Voluntary Revelation and Forced Revelation than in No Revelation.[13]

This could be because somebody's individual incorporative behavior can influence not only one group's members but also other people in the experiment session in the strangers-matching

[12] Making it common knowledge that people can reveal their actions or they are forced to reveal their actions itself may harm the effect of the Utilization Rebate rule.

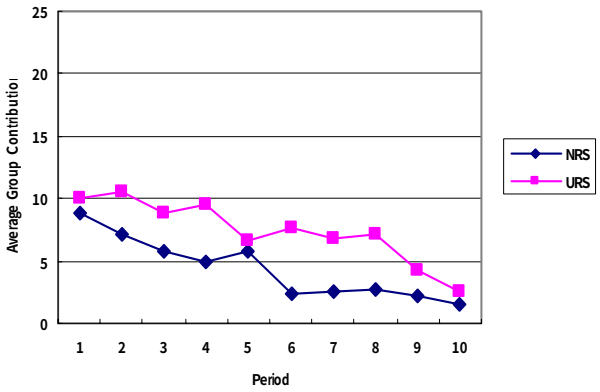
[13] We must admit that the data for each period is not independent from the data for other periods. In partners-matching treatments, each individual interacts with other group members across periods. In the strangers-matching treatments, each individual interacts with other subjects in the experiment room. However, we compared data from all periods to see whether the Utilization Rebate rule has a different impact on people's cooperative behavior.

treatments, while such non cooperative behavior is limitedly observed within groups in partners-matching treatment. In addition, free-riding behavior is distinctively observed in Voluntary Revelation and Forced Revelation. Therefore, the potential cooperative incentive of Utilization Rebate was diminished by distrust among people in those treatments.

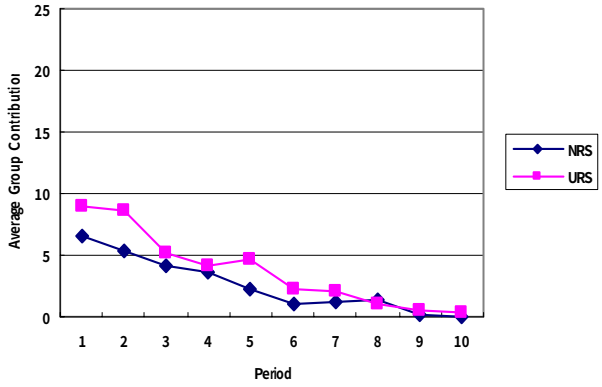
Figure 6 confirms and extends this interpretation. It shows that although the average group contribution of the Utilization Rebate rule is almost always higher than that of the No Rebate rule with No Revelation and Voluntary Revelation across periods (this supports hypothesis 1), the relationship between Utilization Rebate and No Rebate is reversed in almost all periods in Forced Revelation. In Forced Revelation, the average group contribution of the No Rebate rule is almost always higher than that of the Utilization Rebate rule (except for period 2). Although this reversed relationship is statistically supported only for period 5 (see table 5), these results indicate that when people play with strangers, giving them full information about others' actions unconditionally weakens their motivation to cooperate.

On the other hand, in No Revelation, since non-cooperators can hide their behavior behind others' cooperation, the impact of the Utilization Rebate rule was not lost. Moreover, table 5 shows that subjects became more responsive to the rebate under the strangers-matching rule than in the partners-matching rule.

No Revelation



Voluntary Revelation



Forcecd Revelation

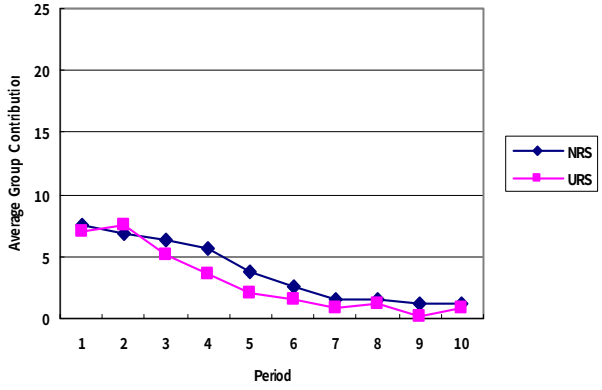


Figure 6 Comparison between the No Rebate Rule and the Utilization rebate rule in the Strangers-matching Treatments

Table 5. Comparison between the No Rebate Rule and the Utilization Rebate Rule in Each Session Using Mann-Whitney U test

(*NR=No Rebate **UR=Utilization Rebate)

No Revelation						Voluntary Revelation						Forced Revelation					
Matching	Period	Mean Group Contribution		Z	p-value (if p≤0.1)	Matching	Period	Mean Group Contribution		Z	p-value (if p≤0.1)	Matching	Period	Mean Group Contribution		Z	p-value (if p≤0.1)
		NR*	**UR****					**NR**	**UR**					**NR**	**UR**		
Partners	1	7.60	12.00	-2.00	0.05	**Partners**	1	8.60	17.20	-2.63	0.01	**Partners**	1	8.00	16.20	-2.21	0.03
	2	7.20	12.20	-2.12	0.03		2	9.40	18.80	-2.22	0.03		2	9.60	17.00	-2.52	0.01
	3	8.00	12.40	-1.58	_		3	9.60	19.40	-2.66	0.01		3	8.20	17.40	-2.51	0.01
	4	7.80	11.00	-1.05	_		4	9.00	18.40	-2.64	0.01		4	7.80	16.60	-2.11	0.04
	5	7.40	10.20	-0.43	_		5	8.00	17.80	-2.00	0.05		5	8.80	15.40	-1.80	0.07
	6	7.40	11.20	-1.59	_		6	7.20	17.40	-1.79	0.07		6	7.80	14.20	-1.06	_
	7	6.80	9.60	-1.48	_		7	6.80	19.00	-2.65	0.01		7	6.80	13.80	-1.70	0.09
	8	6.80	9.80	-0.84	_		8	6.00	18.40	-2.63	0.01		8	6.00	13.00	-1.26	_
	9	7.00	8.00	-0.32	_		9	6.40	17.20	-2.44	0.01		9	5.00	12.00	-1.16	_
	10	7.00	5.20	0.97	_		10	6.20	12.20	-1.38	_		10	5.40	8.60	-1.37	_
Strangers	1	8.80	10.00	-0.11	_	**Strangers**	1	6.60	9.00	-0.85	_	**Strangers**	1	7.60	7.00	0.21	_
	2	7.20	10.60	-1.26	_		2	5.40	8.60	-1.92	0.06		2	6.80	7.60	-0.32	_
	3	5.80	8.80	-1.05	_		3	4.20	5.20	-0.11	_		3	6.40	5.20	0.84	_
	4	5.00	9.60	-2.21	0.03		4	3.60	4.20	-0.11	_		4	5.60	3.60	0.96	_
	5	5.80	6.60	-0.53	_		5	2.20	4.60	-1.18	_		5	3.80	2.00	1.74	0.08
	6	2.40	7.60	-2.02	0.43		6	1.00	2.20	-1.61	_		6	2.60	1.60	0.76	_
	7	2.60	6.80	-1.89	0.06		7	1.20	2.00	-0.24	_		7	1.60	0.80	0.57	_
	8	2.80	7.20	-2.00	0.05		8	1.40	1.00	-0.11	_		8	1.60	1.20	1.27	_
	9	2.20	4.20	-1.49	_		9	0.20	0.60	-0.78	_		9	1.20	0.20	1.55	_
	10	1.60	2.60	-0.97	_		10	0.00	0.40	-1.50	_		10	1.20	0.80	0.84	_

6.3 Equilibrium Coordination

To see how close group contributions approached any equilibrium in each observational treatment, the difference between group contributions and the nearest equilibrium in each period was measured.[14] The way of determining the closest equilibrium is as follows. If a group contribution was larger than 5 tokens, the group was categorized as focusing on an efficient Nash equilibrium ($\sum_{i=1}^{5} g_i = 10$), while a group contribution of 5 tokens or less was categorized as focusing on the inefficient equilibrium ($\sum_{i=1}^{5} g_i = 0$) (the absolute distance from the nearest equilibrium for each period was calculated). Table 6 shows the results. Contrary to hypothesis 1, any significant difference in the rebate rules was observed in the partners-matching treatments in No Revelation. However, the results for the partners-matching treatments in Voluntary Revelation and Forced Revelation show that there is such a significant difference in most periods. That is, hypothesis 8 is highly supported in these treatments. The significant difference of closeness to a nearest equilibrium at each period level is slightly clearer in Forced Revelation than in Voluntary Revelation. This is natural since subjects could observe others' actions perfectly in Forced Revelation. In the strangers-matching treatments in all sessions, a significant difference was rarely observed (only period 4 and period 5 in Forced Revelation show such a difference), and the outcomes of both the No Rebate rule treatment and the Utilization Rebate rule are nearer to the inefficient equilibrium.

[14] This method was developed by Cadsby and Maynes (1998).

Table 6. Comparison of Closeness to the Nearest Equilibrium Between the No Rebate Rule and the Utilization Rebate Rule in Each Session Using Mann-Whitney U test

(*NR=No Rebate **UR=Utilization Rebate)

No Revelation						Voluntary Revelation						Forced Revelation					
Matching	Round	Mean Distance from a nearest equilibrium		Z	p-value	Matching	Round	Mean Distance from a nearest equilibrium		Z	p-value	Matching	Round	Mean Distance from a nearest equilibrium		Z	p-value
		NR	**UR**					**NR**	**UR**					**NR**	**UR**		
Partners	1	2.40	3.20	-0.43	_	**Partners**	1	1.40	7.20	-2.23	0.03	**Partners**	1	2.00	6.60	-1.81	0.07
	2	2.80	3.40	-0.67	_		2	1.40	8.80	-1.80	0.07		2	0.80	7.00	-2.23	0.03
	3	2.00	4.40	-1.51	_		3	0.40	9.40	-2.66	0.01		3	2.20	7.40	-1.79	0.07
	4	2.60	4.20	-0.63	_		4	1.00	8.40	-2.23	0.03		4	1.80	7.40	-2.11	0.04
	5	2.60	4.60	-0.87	_		5	1.20	8.60	-2.02	0.04		5	0.80	6.60	-2.66	0.01
	6	3.00	2.80	0.32	_		6	1.60	8.20	-1.38	_		6	0.20	6.60	-2.60	0.01
	7	3.20	2.00	1.31	_		7	0.80	9.00	-2.34	0.02		7	0.80	5.80	-1.89	0.06
	8	2.80	3.80	-0.84	_		8	0.80	8.40	-2.23	0.03		8	0.80	5.40	-1.31	_
	9	2.60	2.00	0.88	_		9	2.00	7.20	-1.08	_		9	1.00	6.00	-1.84	0.07
	10	1.80	0.80	0.98	_		10	1.00	4.20	-1.51	_		10	0.60	2.60	-2.15	0.03
Strangers	1	2.00	3.60	-1.34	_	**Strangers**	1	3.40	3.40	0.00	_	**Strangers**	1	2.40	2.20	0.11	_
	2	2.00	3.40	-1.41	_		2	3.00	1.40	1.49	_		2	2.80	3.20	-0.32	_
	3	3.40	2.80	1.00	_		3	3.80	2.80	1.34	_		3	2.40	3.20	-0.97	_
	4	3.80	3.20	0.86	_		4	2.80	2.20	0.53	_		4	2.80	0.80	2.04	0.04
	5	3.40	2.60	0.86	_		5	1.80	1.80	0.00	_		5	3.80	2.00	1.74	0.08
	6	2.40	1.60	0.75	_		6	1.00	2.20	-1.61	_		6	2.20	1.60	0.54	_
	7	2.60	1.60	0.85	_		7	1.20	1.20	0.00	_		7	1.60	0.80	0.57	_
	8	2.00	1.60	0.22	_		8	1.40	1.00	-0.11	_		8	1.60	0.80	1.27	_
	9	2.20	3.80	-1.18	_		9	0.20	0.60	-0.78	_		9	1.20	0.20	1.55	_
	10	1.60	2.60	-0.97	_		10	0.00	0.40	-1.50	_		10	1.20	0.80	0.84	_

Table 7 presents the statistical analysis on the effect of the announcement stage in Voluntary Revelation using an OLS regression.

Dependent variable: Group Contribution of each period

Variable	No Rebate/ Partners	Utilization/ Partners	No Rebate/ Strangers	Utilization/ Strangers
Constant	3.95*** (1.17)	3.70 (3.21)	4.12*** (0.97)	6.69*** (1.24)
The number of people who want to reveal their contribution	1.61*** (0.25)	3.57*** (0.65)	0.58*** (0.20)	0.75** (0.29)
Period	-0.44*** (0.11)	-0.38 (0.23)	-0.66*** (0.08)	-0.96*** (0.12)

Numbers in parentheses are standard errors. i's announcement=1 if subject i decided to revel her contribution, 0 otherwise. *p<0.10;**p<0.05;***p<0.01.

Table 7. Ordinary Least Squares Estimates of Group Contribution by Period in Voluntary Revelation

It clearly shows that when more people revealed their contributions, groups were able to collect larger contributions. This shows that the act of revelation was not merely cheap talk in any treatment. Figure 7 shows the rate of revelation in Voluntary Revelation. In the strangers-matching treatments, the rate of revelation is lower than in the partners-matching treatments, but in all treatments the rate of revelation is higher than the rate of non revelation. Subjects were particularly motivated to reveal in partners-matching treatments (individual contributions were revealed 90% of the time in Utilization Rebate/Partners). In short, the information on the numbers of people who want to reveal their contributions encouraged people to cooperate most in the Utilization Rebate with partners-matching treatment. The effect in the No Rebate with partners-matching treatment is second. Even in the strangers-matching treatments, the effect of one more person's decision to reveal her contribution was positive. However, such an impact was much weaker than when they played with the same partners.

6.4 Comparison on Individual Contribution among Three Observational Treatments

Figure 8 shows the distributions of individual contributions in all treatments for all observational sessions. It clearly shows that the focal point of contribution was 2 in the No Rebate/Partners treatment regardless of the degree of observation of others. In the Utilization Rebate/Partners treatment, the most frequent contribution was 0 in No Revelation. However, the most frequent contribution in both Voluntary Revelation and Forced Revelation was 5. In the No Rebate/Strangers treatment, the focal point was clearly a contribution of 0. The frequency of a contribution of 0 was the highest in Voluntary Revelation, the second was Forced Revelation, and No Revelation was the lowest. In Utilization Rebate/Strangers, the focal point was obviously a contribution of 0. The frequency of contribution of 0 was the highest in Forced Revelation, Voluntary Revelation was second, and No Revelation was the lowest. These findings confirm the discussion about group contributions in the previous section that information of individual contributions facilitated the spread of distrust and made people focus on the inefficient equilibrium.

Figure 9 compares individual contributions between revealers and non revealers. In No Rebate/Partners, the range of contributions by revealers was from 0 to 4, while the range of contribution by non revealers was from 0 to 3. The focal point of contribution among revealers was 2. The most frequent contribution among non revealers was 0, but 31 out of 58 non revealers made a positive contribution. None of the subjects contributed 5 tokens throughout the No Rebate/Partners treatment. In the Utilization Rebate/Partners treatment, the focal point of revealers was 5. Although the focal point among non revealers is not so clear, the most frequent contribution was 1 or 2 (8 samples for each). In the No Rebate/Strangers treatment, the focal point of contribution was 0 both among revealers and among non revealers. One interesting observation is that the frequency of a contribution of zero was much higher among revealers than among non revealers in this treatment. On the other hand, a contribution of 2 was the second most frequent contribution among revealers and the frequency of a contribution of 2 was

much more than among non revealers. In the Utilization Rebate/Strangers treatment, the focal point of contribution was 0 both among revealers and among non revealers. Similar to the result of No Rebate/Strangers, the frequency of contribution of zero was higher among revealers than among non revealers, but the difference is not as big as in the No Rebate/Strangers treatment. On the other hand, the frequency of a contribution of 2 was much more among revealers than among non revealers, but the frequency was not as high as in No Rebate/Strangers.

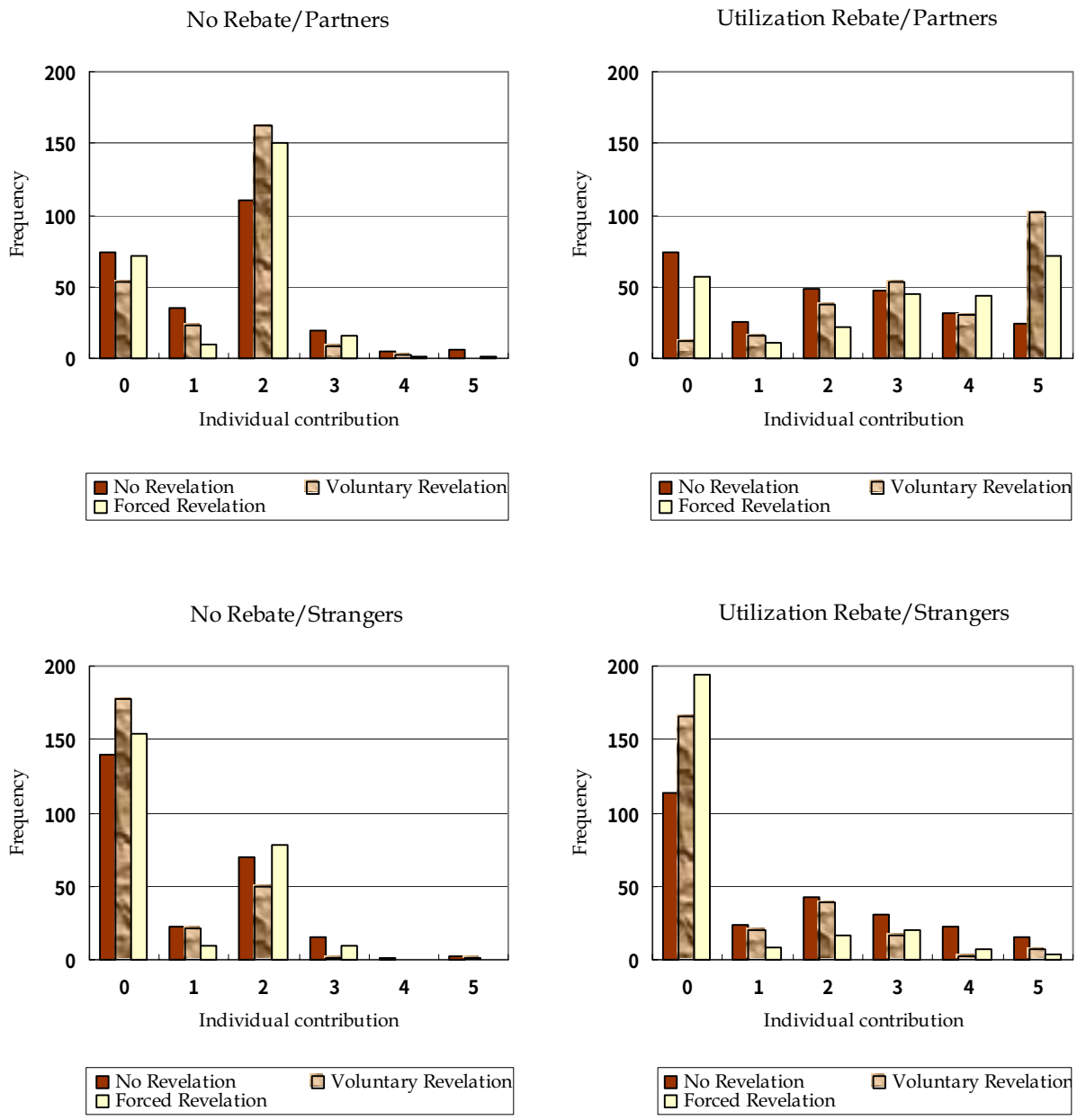


Figure 7. Distributions of Individual Contributions in All Treatments

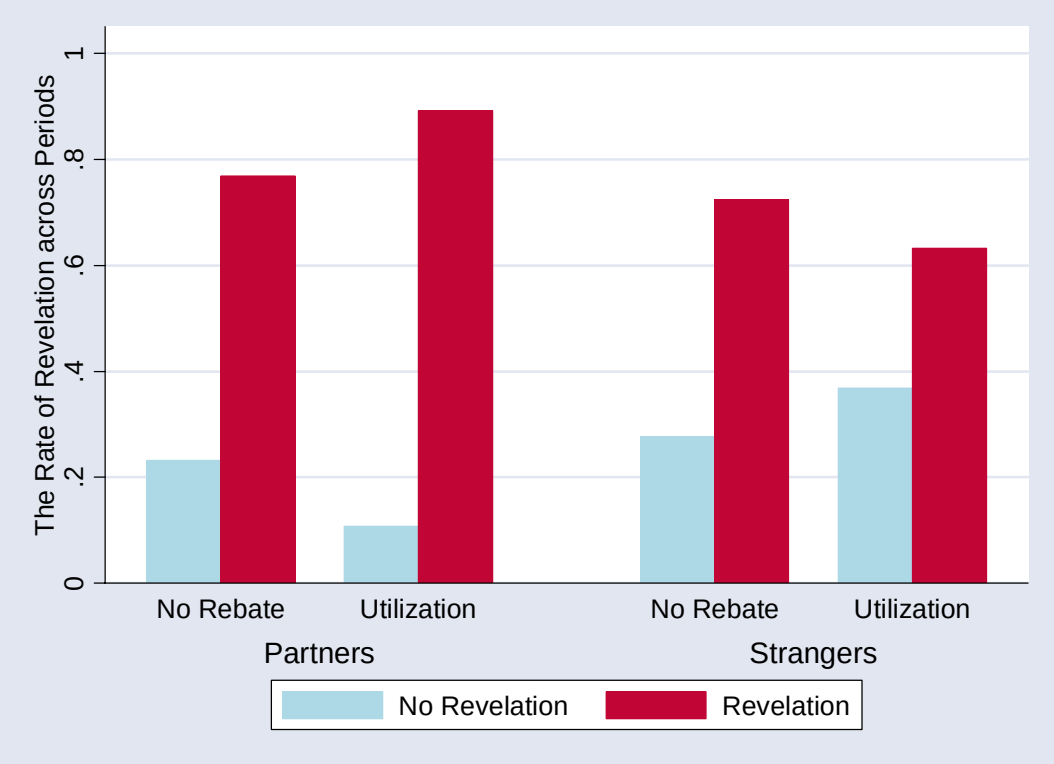


Figure 8. The Rate of Revelation across Periods in Voluntary Revelation

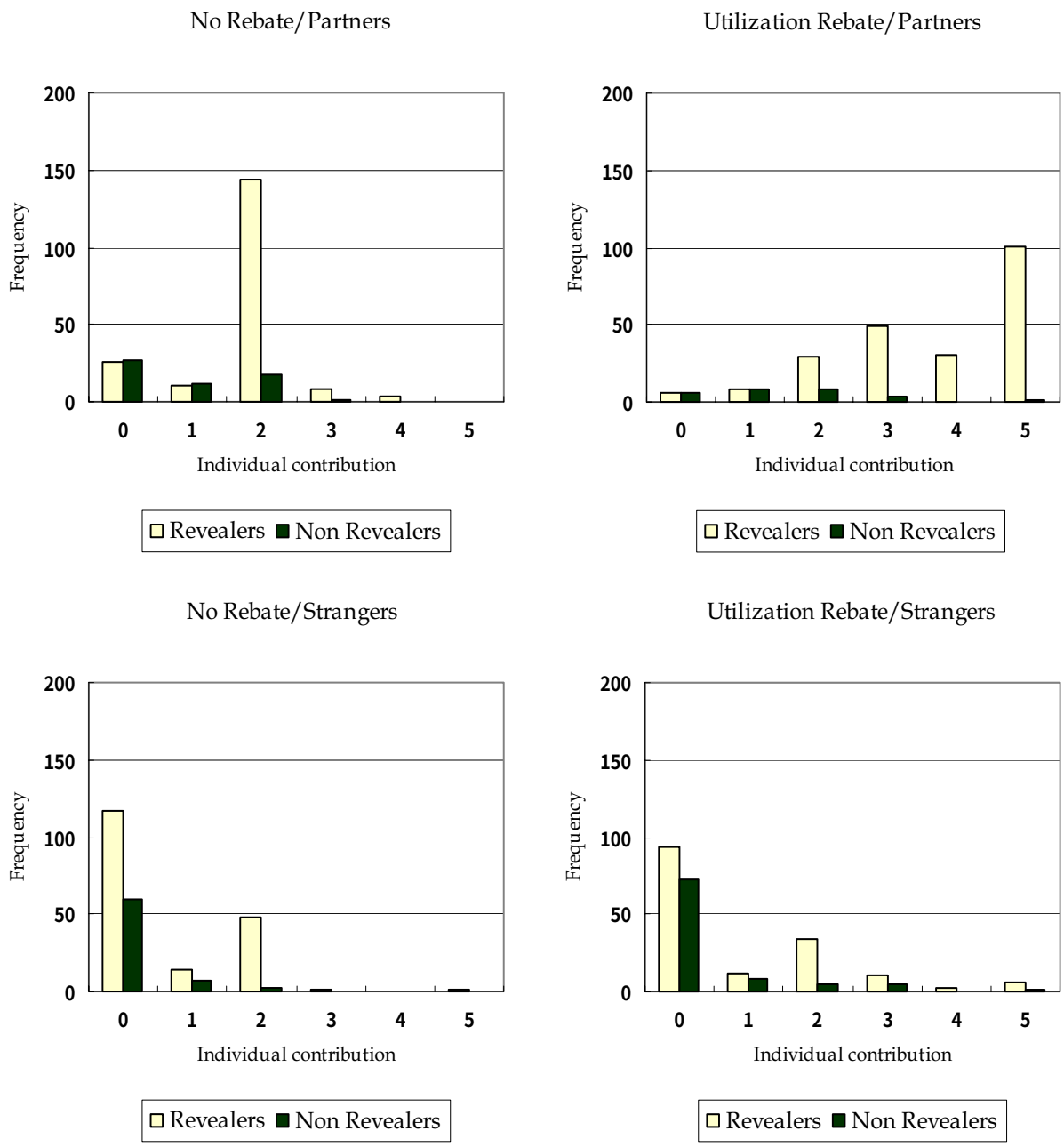


Figure 9. Distributions of Individual Contributions in Voluntary Revelation: Revealers versus No Revealers

To detect any difference in individual behavior among the three observational treatments, an OLS regression model for individual contributions was estimated for each period.[15] Table 8 presents the results of the partners-matching treatments and Table 9 presents the results of the strangers-matching treatments.

No Rebate/Partners

Dependent variable	Individual contribution in each period		
Variable	*No Revelation*	*Voluntary Revelation*	*Forced Revelation*
Constant	0.58* (0.31)	0.19 (0.22)	0.30 (0.21)
i's contribution in the previous period	0.52*** (0.06)	0.24*** (0.07)	0.35*** (0.06)
The sum of others' contributions in the previous period	0.03 (0.03)	0.09*** (0.03)	0.13*** (0.02)
i's revelation announcement	_	0.50*** (0.13)	_
The number of people who want to reveal their contribution	_	0.08 (0.05)	_
Period	-0.01(0.03)	-0.05** (0.02)	-0.03 (0.02)

Utilization Rebate/Partners

Dependent variable	Individual contribution in each period		
Variable	*No Revelation*	*Voluntary Revelation*	*Forced Revelation*
Constant	0.53 (0.35)	0.50 (0.35)	0.61** (0.30)
i's contribution in the previous period	0.51*** (0.06)	0.38*** (0.07)	0.68*** (0.05)
The sum of others' contributions in the previous period	0.09*** (0.02)	0.14*** (0.02)	0.05*** (0.02)
i's revelation announcement	_	0.81*** (0.27)	_
The number of people who want to reveal their contribution	_	-0.13 (0.11)	_
Period	-0.07* (0.04)	-0.10*** (0.03)	-0.08** (0.03)

Numbers in parentheses are standard errors. i's announcement=1 if subject i decided to revel her contribution, 0 otherwise. *p<0.10;**p<0.05;***p<0.01.

Table 8. Ordinary Least Squares Estimates of Individual Contributions to the Public Good by Period in Partners-matching Treatments

[15] This statistical analysis is similar to the analysis in Wilson and Sell (1997).

No Rebate/Strangers

Dependent variable	Individual contribution in each period		
Variable	No Revelation	Voluntary Revelation	Forced Revelation
Constant	0.36 (0.29)	-0.05 (0.26)	0.84*** (0.27)
i's contribution in the previous period	0.45*** (0.06)	0.33*** (0.06)	0.49*** (0.06)
the sum of others' contributions in the previous period	0.05* (0.03)	0.06** (0.03)	-0.02 (0.03)
i's revelation announcement	_	0.24** (0.10)	_
the number of people who want to reveal their contribution	_	0.06 (0.04)	_
Period	-0.03 (0.03)	-0.03 (0.03)	-0.09*** (0.03)

Utilization Rebate/Strangers

Dependent variable	Individual contribution in each period		
Variable	No Revelation	Voluntary Revelation	Forced Revelation
Constant	0.47 (0.35)	-0.18 (0.32)	0.60** (0.30)
i's contribution in the previous period	0.59*** (0.05)	0.48*** (0.05)	0.40*** (0.06)
the sum of others' contributions in the previous period	0.06** (0.03)	0.06** (0.02)	0.02 (0.03)
i's revelation announcement	_	0.29** (0.12)	_
the number of people who want to reveal their contribution	_	0.08 (0.06)	_
Period	-0.06* (0.04)	-0.03 (0.03)	-0.07* (0.04)

Numbers in parentheses are standard errors. i's announcement=1 if subject i decided to revel her contribution, 0 otherwise. *p<0.10;**p<0.05;***p<0.01.

Table 9. Ordinary Least Squares Estimates of Individual Contributions to the Public Good by Period in Strangers-matching Treatments

The dependent variable is subject *i*'s contribution in t^{th} period. Independent variables are subject *i*'s contribution in $(t-1)^{th}$ period, the sum of other group members' contributions in $(t-1)^{th}$ period, subject *i*'s revelation announcement (only in the Voluntary Revelation treatment), the number of people who wanted to reveal their contribution, and period.

First look at the result for the No Rebate/Partners treatment (the top panel of table 8). In the results for No Revelation, the coefficients of *constant* and of *i's contribution in the previous period* are significant (p<0.10, p>0.01, respectively). The coefficient of *Constant* indicates that if other variables are constant, subjects contribute only 0.58 tokens. Although the coefficient of *i's*

contribution in the previous period is positive and significant, subjects contribute only about half of what they contributed in the previous period. Although the coefficient of *the sum of others' contributions in the previous period* is not significant, the value is positive. This indicates that people did not simply focus on the inefficient equilibrium, which was also shown in figure 5. The coefficient of period is insignificantly negative, which is also implied in figure 5.

In the results for No Rebate/Partners with Voluntary Revelation, the coefficients of *i's contribution in the previous period*, *the sum of others' contributions in the previous period*, *i's revelation announcement* and *period* are significant ($p<0.05$). The coefficient of *i's contribution in the previous period* is much smaller than in No Revelation. The value of the coefficient of *the sum of others' contribution in the previous period* is significantly positive, but has a very weak impact on individual contribution decisions. In addition, the coefficient of *the number of people who want to reveal their contribution* is insignificant. Although the OLS estimates of group contributions in table 7 shows that *the number of people who want to reveal their contribution* has a significantly positive impact on group contribution, the results should not be simply interpreted as showing that people contributed more because more others revealed their contributions. Table 8 indicates that subjects were motivated to commit themselves to making a positive contribution when they decided to reveal their contributions. Therefore, the revelation of individual contributions had a significantly positive impact on group contributions. The value of the coefficient of *period* is significantly negative. This confirms the findings in figure 5; that is, less cooperative groups quickly converge toward the inefficient equilibrium.

In the results for No Rebate/Partners with Forced Revelation, both the coefficients of *i's contribution in the previous period* and of *the sum of others' contribution in the previous period* are significantly positive. The magnitude of the coefficient of *i*'s contribution in the previous period is less than in No Revelation but greater than in Voluntary Revelation. When there is only one stage in the game (No Revelation and Forced Revelation), subjects have to make decisions without knowing others' intentions. In Forced Revelation, the magnitude of the coefficient of *the*

sum of others' contributions in the previous period is larger than in No Revelation and in Voluntary Revelation. This means that when people can observe individual information about what others did perfectly, they react more sensitively to what others (in sum) do. That is, one person's free-riding or cooperative behavior has the strongest impact on others' behavior in Forced Revelation.

Let us focus on the results of Utilization Rebate with partners-matching (the bottom panel of table 8). In the result of No Revelation, all of the coefficients are significant except for the coefficient for *constant*. The magnitude of the coefficient of *i's contribution in the previous period* in this revelation is almost the same as in No Rebate/Partners with No Revelation. That is, one person's commitment to her own previous behavior did not change according to the two different rebates. The magnitude of coefficient of *the sum of others' contributions in the previous period* is significant, but has a weak impact on individual decision making. The value of coefficient of *period* indicates that the power of Utilization Rebate is not strong enough for all people to keep cooperating repeatedly, which is shown in figure 5.

The results for Utilization Rebate/Partners with Voluntary Revelation are similar to those in No Rebate/Partners with Voluntary Revelation. The coefficients of *i's contribution in the previous period*, *the sum of others' contributions in the previous period*, *i's revelation announcement* and *period* are significant ($p \leq 0.01$). The magnitude of all the significant variables is larger than in No Rebate/Partners with Voluntary Revelation. Although the coefficient of *the number of people who wanted to reveal their contribution* is insignificant, it is negative. This coefficient and the coefficient of period indicate that although subjects achieved high cooperation in this treatment (figure 5), the gravity of free-riding between the threshold level and the full contribution level is too strong for subjects to achieve or keep the full contribution level repeatedly.

In Utilization Rebate/Partners with Forced Revelation, significant variables are *i's contribution in the previous period* and *the sum of others' contribution in the previous period*, similar to the results of No Rebate/Partners with Forced Revelation. However, the strength of commitment to *i's*

contribution in the previous period is the highest and the impact of *the sum of others' contribution in the previous period* is the lowest compared to other revelation treatments. This indicates that subjects are willing to commit themselves to their own actions in the previous period but without much regard to what others contributed. However, as figure 5 shows, the impact of the utilization rebate was not strong enough for all subjects to keep or achieve a high level of cooperation and they decreased their contributions period by period on average.

Let us focus on the results of No Rebate with strangers-matching (the top panel of table 9). Figure 6 shows that results of all the revelations declined toward the inefficient equilibrium. First look at the results of No Rebate/Strangers with No Revelation. The significant coefficients are *i's contribution in the previous period* and *the sum of others' contribution in the previous period*, similar to the results in No Rebate/Partners with No Revelation. The coefficient of *constant* indicates that if other variables are constant, each subject contributes less than in No Rebate/Partners. The strength of commitment for *i's contribution in the previous period* is lower than in No rebate/Partners. On the other hand, the impact of *the sum of others' contribution in the previous period* is significantly positive, but weak.

In No Rebate/Strangers with Voluntary Revelation, the coefficient of *constant* is much smaller than in No Rebate/Partners with Voluntary Revelation (but it is not significant). The significant coefficients are *i's contribution in the previous period*, *the sum of others' contribution in the previous period* and *i' revelation announcement*. The strength of commitment for *i's contribution in the previous period* is higher than in No Rebate/Partners. However, the strength of commitment for *i's revelation announcement* is about half of that in No Rebate/Partners. The impact of *the sum of others' contribution in the previous period* is also weaker than in No Rebate/Partners. These results indicate that subjects were still willing to cooperate with others, but the impact of the revelation announcement was too weak to build a highly cooperative social norm.

In No Rebate/Strangers with Forced Revelation, significant coefficients are *constant*, *i's contribution in the previous period* and *period*. The magnitude of the coefficient of *constant* is the

highest compared to other treatments. The strength of commitment for *i's contribution in the previous period* is higher than in No Rebate/Partners. However, the impact of *the sum of others' contributions in the previous period* is negative (but insignificantly). In addition, the coefficient of *period* is significantly negative. These estimates indicate that subjects tried to build trust in the beginning, but they were not sufficiently motivated to cooperate with each other in this treatment.

Finally, let us focus on the results for Utilization Rebate/Strangers. First look at the results of No Revelation. The magnitude of the coefficient of *constant* is slightly lower than in Utilization Rebate/Partners with No Revelation (but insignificantly). The strength of commitment for *i's contribution in the previous period* is slightly higher than in Utilization Rebate/Partners. The impact of *the sum of others' contributions in the previous period* is significantly positive, but weak. As figure 6 indicates, the coefficient of *period* is significantly negative.

In the results for Utilization Rebate/Strangers with Voluntary Revelation, the magnitude of the coefficient of *constant* is the lowest compared to other treatments (but insignificantly). The strength of commitment for *i's contribution in the previous period* is higher than in Utilization Rebate/Partners. The impacts of *the sum of others' contributions in the previous period* and *i's revelation announcement* are significant, but weaker than in Utilization Rebate/Partners (especially *i's revelation announcement*). The effect of *the number of people who want to reveal their contribution* is positive (insignificantly), while it is negative in Utilization Rebate/Partners. These findings also indicate that subjects were still willing to cooperate with strangers, but their commitment to contribute was weaker than when they played with partners.

In the results of Utilization Rebate/Strangers with Forced Revelation, the magnitude of the coefficient of *constant* is about the same as in Utilization Rebate/Partners with Forced Revelation (significantly). The strength of commitment for *i's contribution in the previous period* is weaker than in Utilization Rebate/Partners. The impact of *the sum of others' contributions in the previous period* is insignificant, but positive. As figure 6 indicates, the coefficient of *period* is significantly

negative. These results also show that subjects were less motivated to use a reputation building strategy when they played with strangers.

7. Conclusion

This study examined the effect of information of others' actions on people's cooperative behavior under three observationally different treatments in the context of two threshold public goods games. In the No Revelation treatment, subjects could observe only the sum of contributions of their groups in each period. In the Voluntary Revelation treatment, subjects were allowed to signal whether they wanted to reveal their contribution to other group members after the contribution stage. In the Forced Revelation treatment, subjects had to show their individual contributions every period. To see whether people achieve a mutually better outcome by using a reputation building strategy, they were allowed to play with the same partners every period in one treatment and to play with strangers every period in another. The two threshold public goods games have the same theoretical predictions, but different Pareto outcomes.

One interesting finding of this study is that the content of cheap talk matters. Under the Utilization Rebate rule and when subjects play with the same partners, group contributions in Voluntary Revelation were significantly more than those in No Revelation in most periods. Group contributions in Forced Revelation were also higher than those in No Revelation on average, but such a difference was not significant in any period. The reason for these different results is due to whether people could convey *voluntary* intentions to cooperate before the contribution stage. In the Forced Revelation, people could have read others' intentions by observing their individual past actions. However, numbers did not convey enough information on people's intentions to cooperate.

On the other hand, this sort of effect of observation of others was rarely observed under the No Rebate rule. This finding suggests that observation of others has some impact on people's behavior, but such impact is controlled by what outcome people recognize as socially optimal. In

the Utilization Rebate rule, the Pareto efficient outcome is full contribution, while the Pareto efficient outcome coincides with an efficient Nash equilibrium in the No Rebate rule.

Another finding is that offering people the chance to observe others' actions is not socially desirable when people play with strangers. This study showed that people were more cooperative when they did not have information on others' actions than when they did have such information. Moreover, such a tendency was significantly observed more under the Utilization Rebate rule. One reason for these results lies in how quickly distrust spread in the strangers-matching treatments. In addition, the distrust created in the previous treatment may have spread to the following treatment. Since we ran the No Rebate/Strangers treatment before the Utilization Rebate/Strangers treatment, the distrust created in the No Rebate treatment made subjects more risk averse in the Utilization Rebate treatment, thus canceling out the impact of the rebate.

It is not easy to predict in what reward structure environment people would effectively use a reputation strategy to achieve a socially better outcome. However, the results of this study suggest that an authority or an organization which considers using a threshold public goods game for fund-raising should carefully consider the reward structures and investigate whether the targeted people have opportunities to cooperate with each other repeatedly or not.

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