Private vs. Public Strategies in a Voluntary Contributions Mechanism

November 10, 2010

Abstract

Can more information on others’ strategies increase voluntary contributions to public goods? This paper addresses this question in a public goods experiment where subjects submit partially-binding strategies for what they will contribute in the next period depending on what others contribute in the current period. In one treatment, these strategies are kept private, while in another they are publicly displayed to the other group members. Results show that when strategies are elicited but kept private, people contribute significantly more in early periods, but contributions decline over time. When strategies are made public, contributions start low but increase over time as subjects learn to strategically contribute when others are reciprocal. This study has policy implications for a wide range of public issues, ranging from climate change mitigation, to fisheries management, to local recycling efforts.

Sean D’Evelyn*
University of Hawai‘i at Mānoa
E-mail: sdevelyn@hawaii.edu

*The author thanks Katerina Sherstyuk, John Lynham, Gary Charness, Timothy Cason, Anthony Kwasnica, Lata Gangadharan, Andreas Leibbrandt, and the members of his dissertation committee for their helpful comments and suggestions. All mistakes are the sole responsibility of the author. The University of Hawai‘i College of Arts and Sciences Student Research Award is gratefully acknowledged for funding the experiment.
1 Introduction

Individuals, companies, and governments voluntarily contribute time and money to philanthropic causes. They give food to starving families around the world, fight for human rights, buy carbon offsets, and recycle. Yet despite this evidence that people are not purely self-interested, the free rider problem persists. Countries struggle to cooperate on climate change mitigation, fisheries around the world are in danger of collapsing, and litter remains on the streets. While agents can reciprocate the pro-social choices of others, efficient production of public goods is not always a Nash Equilibrium outcome (see Polasky et al. 2006, Tarui et al. 2008 for examples). If agents did announce how they intend to respond to the actions of others, would this lead to more efficient outcomes, or would this only exacerbate the free rider problem? This paper uses a controlled laboratory experiment to examine whether voluntary provisions of public goods are higher when individuals keep their strategies private or when they send public signals of their strategies to others.

A growing body of experimental work suggests that many subjects in Voluntary Contribution Mechanism (VCM) experiments are highly reciprocal – they contribute to the public good in proportion to what others contribute (Fischbacher et al. 2001, Croson 2007, Muller et al. 2008). Two studies in particular, Fischbacher et al. (2001) and Muller et al. (2008) directly elicit subjects’ willingness to contribute to a public good depending on what others contribute. Fischbacher et al. (2001) find that half of subjects are willing to contribute more to the public good the more their group members contribute while thirty percent free ride in their one-shot public goods game. Fischbacher and Gächter (2010) show that these one-shot results are also highly predictive of behavior in repeated VCM experiments with random rematching. Muller et al. (2008) use a series of two-stage VCM games in which subjects complete a full strategy detailing how their contribution in the second stage changes with others’ contributions in the first. As with Fischbacher et al. (2001), they find that most subjects are reciprocal, but that degrees of reciprocity are highly heterogeneous across subjects.

Several theoretical papers note that if a large subset of subjects is (or at least is believed to be) reciprocal, then self-interested subjects also contribute to the public good (Kreps et al. 1982, Bochet et al. 2006, Ambrus and Pathak 2010). By contributing, subjects build a reputation as cooperators, causing reciprocal subjects to increase their own contributions. While the theory is sound, there is little evidence that subjects actually make these reputation-building contributions. Andreoni and Croson (2008) review the literature that compares public goods experiments with and without random rematching.
of subjects (partners vs. strangers treatments). Unlike games where groups remain fixed, subjects are unable to build a reputation as cooperators in games with random rematching of subjects. Yet Andreoni and Croson (2008) find little evidence of any difference in contribution rates between the two treatments, especially in the ten-period, finitely repeated game, suggesting that subjects do not attempt to profit from others’ reciprocity in the standard VCM setting with fixed groups.

There are two possible explanations for why we observe so few reputation-building contributions. Either subjects are aware that they can benefit from others’ reciprocity and choose not to, or they simply do not realize that others may reciprocate their actions in the future. The first explanation suggests that people are unwilling to “take advantage” of others’ reciprocity through reputation-building contributions and that an increase in the perceived reciprocity of others would have no impact on contribution rates. Under the second explanation, however, people simply underestimate the degree of others’ reciprocity and would contribute more to the public good if their estimate was more accurate. This paper provides the first direct evidence to discriminate between the two explanations.

In this paper, we compare three treatments. The first treatment is a standard, linear Voluntary Contributions Mechanism with fixed groups and is our control treatment. Our other two treatments use a variation on Selten’s strategy method similar to Fischbacher et al. (2001) and Muller et al. (2008). In these two treatments, subjects report what they will contribute in the following period contingent on what others contribute in the current period. In one treatment, the Private-Strategy treatment, these contingent strategies are kept private and are binding for one of four subjects per group. The Private-Strategy treatment is strategically equivalent to our control treatment and aims to shed new light on subjects’ strategies and how they evolve in a finitely repeated VCM setting without communication. However, past experiments show that outcomes using the strategy method sometimes differ from outcomes using the direct-response method despite strategic equivalence (see Brandts and Charness 2009 for a review). For this reason, our Private-Strategy treatment is also necessary to isolate the effect of subjects thinking more about their period-to-period strategies. Our third treatment, the Public-Strategy treatment, is identical to the Private-Strategy treatment except that these response functions are publicly displayed to the other group members. In this treatment, subjects are able to signal their own level of reciprocity and may respond differently depending on the strategy type of others. This treatment aims to show how subjects’ contributions vary with the reciprocity of others and how the provision of public goods changes with additional information on others’ strategies.
While unstructured, face-to-face communication can dramatically increase contributions to public goods (Isaac and Walker 1988, Sally 1995, Brosig et al. 2003, Bochet et al. 2006), structured, anonymous communication has yet to produce the same results (Brosig et al. 2003, Bochet et al. 2006, Bochet and Putterman 2009). Of particular importance to our experiment, Bochet et al. (2006) find no difference in contribution rates between the standard VCM setting and a treatment where subjects send non-binding, numerical messages about how many tokens they will contribute in the current period. Bochet and Putterman (2009) further examine these non-binding, numerical messages, and find that subjects’ messages are highly correlated with their actual contributions and that subjects frequently adjust their contributions toward the mean stated contribution of other subjects. The implication is that whatever mechanism increases efficiency when subjects engage in free, personal communication is not present when communication is limited to subjects sending meaningful signals of current contributions. Our paper examines whether efficiency improves when communication is limited to subjects sending meaningful, partially binding signals of future contributions.

The novelty of this experiment is fourfold. First, we are able to better understand how subjects’ contributions depend on the contributions of others and how reciprocity changes over time with and without communication. Second, we directly observe whether subjects are willing to make reputation-building contributions when other subjects are reciprocal. Third, we gain a new understanding of how well subjects estimate the reciprocity of others. Fourth, we test the efficiency implications of both planning and communicating strategies in the finitely repeated VCM setting.

The data yields some surprising results. First, subjects contribute significantly more to the public good in the first period of our Private-Strategy treatment than in either the control treatment or the Public-Strategy treatment. A closer look at individual data suggests that subjects either suffer from projection bias or make evidential decisions. This leads conditional cooperators in the Private-Strategy treatment to believe that others are also conditionally cooperative, enabling them to make reputation-building contributions. Contributions decline over time as the perceived potential for reciprocation declines.

Second, contributions in the Public-Strategy treatment do not decrease over time as is typical of VCM experiments, and even weakly increase. Decreasing contributions is considered one of the primary features of VCM experiments (Fischbacher and Gächter 2010), and increasing contributions usually happen only with face-to-face communication (Isaac and Walker 1988) or when subjects are able to punish low contributors (Fehr and Gächter 2000, Bolton and Ockenfels 2000). Like experiments with a punishment mecha-
nism, contributions increase over time in the Public-Strategy treatment because subjects slowly learn what actions are punished or rewarded by others and adapt their strategies accordingly.

The rest of the paper is organized as follows. Section 2 provides a theoretical model for subject behavior. Section 3 details the experimental methodology. Section 4 provides the experimental results. Section 5 concludes.

2 Model

The basic experimental design follows a standard, linear Voluntary Contributions Mechanism. Each session lasts for ten periods, and the length is announced in the instructions. We divide subjects into groups of four that remain fixed through all paid periods. Each period, subjects receive an endowment of three tokens and choose how many tokens to keep in a private fund and how many to contribute to a public good. Any tokens kept in the private fund yield one experimental dollar for the participant, while tokens contributed to the public good yield 0.5 experimental dollars for each group member. Per-period payoff functions in experimental dollars can be summarized as:

$$\Pi_{i,t} = 3 - x_{i,t} + 0.5 \sum_{j=1}^{4} x_{j,t}$$

The number of tokens that subject $i$ contributes to the public good is denoted $x_{i,t}$ and $3 - x_{i,t}$ represents the number of tokens subject $i$ keeps in the private fund. The sum of all group members’ contributions, $\sum x_{j,t}$, is multiplied by the marginal per capita return, 0.5, to yield $i$’s return from the public good. We assume that subjects do not observe the individual contributions made by other players, but at the end of period $t$ observe the total contribution made by others to the public good:

$$x_{\Sigma,-i,t} \equiv \sum_{j \neq i} x_{j,t}.$$

Under common knowledge of rationality, the sole subgame perfect Nash Equilibrium of this finitely repeated game is for all subjects to contribute zero tokens to the public good each period. However, past experiments have shown that subjects do contribute to public goods. This is true for one-shot games and for repeated games with and without random rematching. Some past experiments classify this motivation as resulting from a genuine concern for the well-being of others (Ledyard 1995), a “warm glow” from contributing (Andreoni 1989), or simply confusion (Ferraro et al. 2003). This paper does
not differentiate between these hypotheses. Instead, all motivations that are not narrowly self-interested are referred to as *intrinsic* motivations and are taken as given.

If subjects’ intrinsic motivation to contribute depends on the history of others’ actions, this could also generate strategic, reputation-building contributions as we describe below. First, let the history of the total contributions made by other agents in the same group be denoted:

\[ H_{x_{-i},t} \equiv \{x_{-i,1}, \ldots, x_{-i,t-1} \} \]

Let \( s_i \) be subject \( i \)'s complete (pure) strategy such that \( s_i(H_{x_{-i},t}) = x_{i,t} \). We further say that strategy \( s_i \) is (weakly) reciprocal if:

\[ s_i(H_{x_{-i},t}) \geq s_i(\tilde{H}_{x_{-i},t}) \forall H_{x_{-i},t} \geq \tilde{H}_{x_{-i},t}. \]

Because complete strategies for a ten-period game can be exceedingly complex, this paper focuses on subjects’ reaction functions. A reaction function is formulated by subject \( i \) in time \( t \) and therefore takes \( H_{x_{-i},t} \) as given. It states what a subject will contribute in the following period depending on the contributions of others in the current period. Formally, a reaction function

\[ f_{i,t}(x_{-i,t}) = x_{i,t+1}. \]

Note that any strategy \( s_i \) can be written as a series of reaction functions such that:

\[ \forall H_{x_{-i},t} \exists f_{i,t} : x_{i,t+1} = s_i(\{H_{x_{-i},t}, x_{-i,t}\}) = f_{i,t}(x_{-i,t} x_{-i,t}) \forall x_{-i,t}. \]

For reciprocal strategy profiles, this also implies that reaction functions are increasing in \( x_{-i,t} \):

\[ x_{-i,t} \geq \tilde{x}_{-i,t} \implies f_{i,t}(x_{-i,t}) \geq f_{i,t}(\tilde{x}_{-i,t}). \]

While reciprocal strategy profiles need not reward or punish the actions of others immediately, it makes intuitive sense that the strongest responses come in the very next period. As a result, response functions may be highly indicative of a subject’s propensity for reciprocation. Furthermore, even if subjects’ strategies only depend on the most recent actions of others (ie \( f_{i,t}|H_{x_{-i},t} = f_{i,t}|\tilde{H}_{x_{-i},t} \forall H_{x_{-i},t}, \tilde{H}_{x_{-i},t} \)), a change in one period’s contribution could impact contributions for several periods whenever more than one group member is reciprocal. This is because after the reciprocal group members respond to \( i \)'s increased contribution, they can also respond to each other in subsequent periods.

For example, suppose subject \( i \) is a pure free rider and that the other three subjects \( j \neq i \) prefer to match the average contribution of others from the previous period. Subject

---

1This implicitly assumes that strategies do not directly depend on the history of one’s own actions.
i’s reaction function is then \( f_{i,t}(x_{-i,t}) = 0 \) and, ignoring the constraint that contributions must take integer values, \( f_{j,t}(x_{-j,t}) = \frac{x_{-j,t}}{3} \forall j \neq i. \) Now suppose subject i’s contribution increases by one in period t but reverts to zero in all other periods. The contributions of the other three subjects would increase by \( \frac{1}{3} \) in period \( t + 1. \) This increases \( x_{-j,t+1} \) by \( \frac{2}{9} \) for all \( j \neq i, \) and so j’s contribution in period \( t + 2 \) also increases by \( \frac{2}{9}. \) If \( x_{i,t} \) increases by one in period 1, other group members’ contributions to the public good increase by almost 3 over the course of the entire experiment.\(^2\)

In this way, subjects may strategically cooperate in order to elicit reciprocity from others, especially in early periods. It is even possible that some contributions are payoff-maximizing, as in the example above. This is similar to a finding in the centipede game of McKelvey and Palfrey (1992). In their game, when subjects are uncertain about whether or not they are paired with an altruist, even self-interested subjects can mimic the actions of an altruist in the hope that they are reciprocated. Even if contributions are not payoff-maximizing, the possibility of reciprocation from other group members should increase contributions, assuming that subjects are both aware of this possibility and that these profit-oriented motives do not “crowd out” the intrinsic motives for contributing (see Bénabou and Tirole 2003 for a discussion). Any contribution that aims to increase the contributions of others in future periods is termed a reputation-building contribution.

While reputation-building contributions can exist in any VCM experiment with fixed groups, there is little evidence of it as discussed above. This study examines whether or not there is a potential for reputation-building contributions in the partners treatment, and whether communicating strategies increases or decreases this potential.

### 3 Experimental Design

The experiment is divided into three treatments: the Control treatment, the Private-Strategy treatment, and the Public-Strategy treatment. Each subject experiences only one treatment. The treatments are described in detail below, with instructions and screenshots provided in the appendix.

#### 3.1 Control Treatment

The first treatment is the Control treatment and is used for comparisons. During this treatment, the subjects play the game described in section 2 exactly, choosing their

---

\(^2\)Each subject \( j \neq i \) contributes \( \frac{1}{3} \ast (\frac{2}{9})^{t-2} \) in period \( t > 1, \) totaling 0.97 tokens each.
allocation of tokens each period. This treatment is necessary to provide a benchmark for comparisons with other treatments and with the wider VCM literature.

3.2 Private-Strategy Treatment

The second treatment, the Private-Strategy (PriStrat) treatment, is identical to the Control treatment except that subjects’ decisions are partially made using the strategy method rather than exclusively using the direct-response method. In each period $t$, subjects fill out a response function ($f_{i,t}$) that asks for each contribution level of others in the current period ($\sum_{-i,t}$), what their contribution level will be next period ($x_{i,t+1}$). We reveal the total contribution to the public good made by others ($\sum_{-i,t}$) at the end of period $t$. In period $t+1$, one subject chosen randomly is forced to contribute according to their previous response function, $f_{i,t}(\sum_{-i,t})$, and all other subjects are able to choose $x_{i,t+1}$ freely. In this way, the truthful revelation of response functions is incentive compatible, and is similar to the mechanism used in Fischbacher et al. (2001).

In the PriStrat treatment, subjects have exactly the same information and strategies available to them as in the Control treatment. The only difference is that strategies are formalized ahead of time similar to Selten’s strategy method rather than purely reactively. As such, we treat the PriStrat treatment as strategically equivalent to our Control treatment. Using it we will see not only what response functions subjects use, but also how these response functions evolve over time.

3.3 Public-Strategy Treatment

The final treatment, the Public-Strategy (PubStrat) treatment, is the primary treatment of interest. While the other treatments can yield insights into subjects’ response functions and the extent that subjects think about their strategies, this treatment is designed to test how subjects react to the strategies of others. Furthermore, this treatment is useful from a policy perspective, as it can help determine whether agents in a public goods setting could benefit from stating contingent strategies to the other agents.

In the PubStrat treatment, the subjects play the same game as in the PriStrat treatment, except strategies are made public rather than kept private. As in the PriStrat treatment, each subject begins each period by writing down a response function detailing how they will act in the following period depending on the contribution levels of others in the

---

3Technically, each allocation decision is made twice in the Private-Strategy treatment, but only once in the Control treatment. As subjects gain no new information between the two decisions and are free to keep the same decision made earlier, the two treatments are effectively equivalent.
current period, \( f_{i,t}(x_{\sum_{-i,t}}) \), and this response function is binding for only one of the four group members chosen at random. Unlike previous treatments, these functions are then shown to the other participants of the group. After they see the strategies set by their group members, three out of four subjects update their contribution decisions and the remaining subject contributes according to the previous period’s response function.

### 3.4 Procedures

The experiment was conducted on the University of Hawai‘i, Manoa campus. Participants were either undergraduate students or were graduate students in programs other than economics. We ran the experiment in the social science computer laboratory using the experimental software z-Tree (Fischbacher 2007).

Before the experiment began, subjects completed three tasks in order to ensure that they understood the experiment. First, subjects went through an on-screen tutorial alongside printed instructions. All instructions were read aloud to ensure common information among participants. Second, subjects took a written quiz and were not allowed to proceed until they answered all questions correctly. Finally, subjects also participated in several practice periods with different groupings than in the paid periods.\(^4\) During the paid periods, subjects had access to several tools to aid their decision making. They could access the entire tutorial, use a scenario calculator that would calculate payoffs from each option, and were free to ask questions to the experimenter. Each session consisted of ten paid periods, and the length was announced in the instructions.

After each period, subjects saw the total tokens contributed by others, but did not see individual contributions. At the end of the experiment, subjects were paid the sum of their per-period payoffs and experimental dollars were converted to US$ at the rate of $0.25:1 experimental dollar. Each session lasted between 45 minutes and an hour and a half, and subjects earned an average of $15.40 (including a $5 show-up fee).

### 4 Results

The results section is organized as follows. In section 4.1, we look at the average contributions per period in each treatment. We find two major results. First, contributions in first few periods of the PriStrat treatment are significantly higher than in either the

\(^4\)The majority of sessions had two practice periods. One session under the PriStrat treatment had only one practice period. One session under the Control treatment also had only one practice period and the other Control session had three. The number of practice periods did not affect contribution rates and so we include all sessions in our analysis regardless of the number of practice periods.
CONTROL treatment or the PubStrat treatment; however they decrease over time just as in the Control treatment. Second, contributions in the PubStrat treatment start low but are weakly increasing over time in the PubStrat treatment. In sections 4.2 and 4.3, we explain these findings in detail using individual level data. Table 1 reports a breakdown in the number of sessions, subjects, and groups in each treatment along with demographic statistics on % female, % graduate students, and average number of economics courses taken. Unless otherwise noted, we use group averages as the unit of observation.

[Table 1 about here]

4.1 Contribution Dynamics

Figure 1 reports the average contributions to the public good by treatment and by period. Differences in contribution rates across treatments determine the relative efficiency of each treatment. A cursory glance at Figure 1 shows that average contributions in the PriStrat treatment are larger on average than either the Control treatment or the PubStrat treatment. However, these differences are only significant in the early periods. In the very first period, contributions to the public good in the PriStrat treatment are significantly higher than the contributions in either the Control treatment or the PubStrat treatment (Wilcoxon-Mann-Whitney test (WMW), \( p < .001 \), two-tailed). Even though contributions remain on average higher than either of the other two treatments through the eighth period, they are not significantly higher than the Control treatment by the second period (WMW, \( p > .20 \), two-tailed) nor higher than the PubStrat treatment by the fourth period (WMW, \( p > .20 \), two-tailed).

[Figure 1 about here]

These results are also supported by the regressions found in Table 2. The table reports random-effects regressions of individual contributions on period by treatment. Random effects are used to control for individual and group-specific heterogeneity. In the regression, period one is the intercept and its coefficient corresponds to the average contributions to the public good in the first period of each treatment. The coefficient on extra period describes how contributions change over time. By comparing the intercepts in each treatment, we find that contributions in period one is significantly higher in the PriStrat treatment than either the Control or the PubStrat treatment (t-test, \( p < .01 \), two-tailed).

[Table 2 about here]
Conclusion 1 In the first period, subjects contribute more to the public good in the PriStrat treatment than in either the Control treatment or the PubStrat treatment. This difference declines in subsequent periods and vanishes by the final period.

Unlike contributions in either the Control or the PriStrat treatments, contributions in the PubStrat treatment actually increase over time. On an aggregate level, the increase appears very slight, but it is marginally significant. From regression (3) in Table 2, the coefficient on period in the PubStrat treatment is significantly positive at the $p < .10$ level. In the other two treatments, period has a negative coefficient that is significant at the $p < .01$ level for the Control treatment and at the $p < .001$ level for the PriStrat treatment. The increase in average contributions per round in the PubStrat treatment is also marginally significant using the Spearman rank test (SRT, $p < .10$, two-tailed). By comparison, the Spearman rank for the Control and the PriStrat treatment are significantly negative at the $p < .001$ level. Further, we find that of the ten different four-person groups in the PubStrat treatment, eight have contributions that increase over time and only two decrease. Under the assumption that increasing and decreasing contributions are equally likely, this is significant at the $p < .10$ level (one-tailed). Contributions decrease over time in eight of the nine groups in the PriStrat treatment, and this is highly significant with $p < .05$ (one-tailed).

Conclusion 2 In the PubStrat treatment, subjects contribute more tokens to the public good over time, while subjects in the Control and PriStrat treatments contribute fewer.

Although contributions decline in the PriStrat treatment and increase over time in the PubStrat treatment, contributions are not significantly higher in the PubStrat treatment even in the final period (WMW, $p > .20$, two-tailed). At the same time, total earnings in the PriStrat treatment are also not significantly higher than the other two treatments (WMW, $p > .20$, two-tailed).

4.2 Differences in Early Contributions

We now seek to explain the first of our major conclusions by using individual level data: why contributions in the first period of the PriStrat treatment are significantly higher than in either the Control or the PubStrat treatments. The difference between the Control treatment and the PriStrat treatment is especially puzzling because the two treatments differ only in the method that subjects make decisions. The strategic equivalence between the two treatments immediately rules out several explanations including
most models of other-regarding preferences (Bénabou and Tirole 2006), learning models (Crawford 1995, Van Huyck et al. 2007), and several popular models of bounded rationality (Nagel 1995, McKelvey and Palfrey 1995).

The differences cannot be explained by differences in the subject pool either. Subjects in all three treatments had almost equivalent gender ratios, average number of economics classes, and graduate student status as reported in Table 1. In addition, we divide subjects’ response functions into five different strategy types and find that the distribution of types in the first period of the PriStrat treatment is almost identical to the distribution in the PubStrat treatment. The five strategy types parallel the types in Fischbacher et al. (2001): Free Rider, Unconditional Cooperator, Conditional Cooperator, Triangle Cooperator, and Other, and are defined as follows. Any response function that will not contribute any tokens to the public good regardless of the contributions of others is defined as a free rider strategy \( f_{i,t} = 0 \). Any function that will contribute all three tokens to the public good regardless of the contributions of others is defined as an unconditional cooperator strategy \( f_{i,t} = 3 \). Conditional cooperator strategies are those that either promise monotonically increasing contributions depending on the contributions of others \( f_{i,t} \text{ increasing in } x_{-i,t} \), or those whose Spearman rank coefficient is positively significant at the \( p < .01 \) level. Triangle cooperator strategies are either strategies that monotonically increase up to a point and then monotonically decrease afterward \( (f_{i,t} \text{ concave in } x_{-i,t}) \), or whose Spearman rank coefficient is positive for contributions less than half the total possible and negative for contributions greater than half the total and are jointly significant at the \( p < .01 \) level. All other strategy types are grouped into an “other” category. In order to better visualize these strategy types, Figure 2 graphs the average response function for each strategy type by treatment. The percent of strategies of each type are listed in the legend.

[Figure 2 about here]

Figure 3 displays the percent of subjects with each strategy type in the very first period of the PriStrat and PubStrat treatments. As mentioned above, the distribution of types between the two treatments is almost identical. The similarity in strategy types and demographic statistics as found in Table 1 suggest that subjects in PriStrat treatment are not fundamentally different than the subjects in the other two treatments.

[Figure 3 about here]
The difference in contribution rates could be due to confusion. Even though the Control and the PriStrat treatments are strategically equivalent, the direct-response method used in the Control treatment is much simpler than the strategy method used in the PriStrat treatment. Confusion has been associated with increased contributions in past experiments (Ferraro et al. 2003, Ferraro and Vossler 2010), and could potentially explain the different contribution rates in the Control and the PriStrat treatments. At the same time, the PubStrat treatment is arguably even more complicated than the PriStrat treatment. If confusion is responsible for the increase in early contributions in the PriStrat treatment, then we would expect even higher contribution rates in the PubStrat treatment. Yet first period contribution rates in the PubStrat treatment are significantly lower than in the PriStrat treatment (WMW, p < .01, two-tailed), and are roughly equal to those in the first period of the Control treatment (WMW, p > .90, two-tailed). Without a significant offsetting effect, confusion is therefore unable to explain the difference in contribution rates between the three treatments.

A more reasonable explanation for the difference in early contribution rates between the PriStrat and the other two treatments is that subjects in the PriStrat treatment have higher expectations about how others would respond to their contributions in future periods. Because the Control treatment does not ask specifically about response functions, subjects may not realize that others’ contributions depend on their own and therefore have little external motivation to contribute. In the PriStrat treatment, subjects are directly asked about their own response functions and therefore subjects also think more about the response functions of other group members. If subjects believe that others are reciprocal, this can lead to more reputation-building contributions. Even though subjects also write down their own response functions in the PubStrat treatment, they need not think as much about the response functions of other group members because these response functions will be shown publicly.

If subjects either suffer from projection bias or make evidential decisions we would further expect that subjects’ beliefs of others’ strategies closely mirror their own strategies in the PriStrat treatment. Projection bias occurs when someone uses their own feelings or beliefs to predict an unknown entity’s feelings or beliefs. Projection bias is used in the economics literature to explain an individual’s inability to separate current preferences from future preferences (Conlin et al. 2007), and in the psychology literature to explain differences in interpersonal perceptions (Maner et al. 2005). In evidential decision theory, subjects mistakenly let their own actions influence their beliefs about others’ actions. The classic example is Newcomb’s Problem (Nozick 1969) where a decision maker may choose
a dominated strategy if they believed another player could have predicted their action. Evidential decision theory has been used to explain voter turnout (Grafstein 1991) and is a subset of the beliefs considered in Mandler (2007).

In either case, this would predict that subjects in the PriStrat treatment that use conditionally cooperative strategies are more likely to initially think that other subjects are reciprocal as well. All else equal, this means that conditional cooperators in the PriStrat treatment are more likely to make reputation-building contributions in the first period than conditional cooperators in the other two treatments. Even if conditional cooperators in the PubStrat treatment suffer some initial projection bias, they see the reaction functions of the other subjects before making actual contributions and realize that not all subjects are as reciprocal.

As supporting evidence, we look at the first-period actual contributions of subjects by strategy type in the PriStrat and the PubStrat treatments as displayed in Figure 4. While contributions to the public good are lower in the PubStrat treatment than the PriStrat treatment for every strategy type, it is for the subjects with conditional cooperator strategies where we see the largest difference. In both treatments, conditional cooperator strategies are the most common response function in the first period. In the PriStrat treatment, 6 of the 11 conditional cooperators contribute fully to the public good in the first period and only two contribute nothing. In the PubStrat treatment, none of the 14 conditional cooperators contribute fully in the first period, and half contribute nothing to the public good. The difference in contributions between the conditional cooperators in the two treatments is statistically significant (WMW, $p < .01$, two-tailed), and the difference in contributions between the two treatments is not statistically significant for any other strategy type (WMW, $p > .60$ for each type, two-tailed).

[Figure 4 about here.]

**Conclusion 3** The high contribution rates in the first period of the PriStrat treatment can be explained by the difference in contribution rates made by conditional cooperators across treatments. Projection bias and evidential decision theory provide two explanations for why conditional cooperators behave differently in the three treatments.

---

5Several experimental VCM studies also look at the relationship between contributions and the believed contributions of others (Croson 2007, Fischbacher and Gächter 2010). These studies typically characterize a positive relationship between the two variables as evidence that subjects are reciprocal; contributing more to the public good the more they believe others will contribute. Projection bias and evidential decision theory suggest that the reverse could be true as well; subjects that contribute more are likely to believe that others also contribute more.

6Evidential decision theory would also explain why (slightly) more subjects in the PriStrat treatment initially chose unconditional cooperator strategies in the first period than in the PubStrat treatment.
4.3 Increasing Contributions in the Public-Strategy Treatment

We now turn our attention to understanding our other major aggregate result. Specifically, we want to understand why contributions are weakly increasing in the PubStrat treatment when they are decreasing in the Control and the PriStrat treatments (and the vast majority of other VCM experiments).

If subjects are willing to make reputation-building contributions to the public good, they will contribute more the more likely their contributions are reciprocated. Table 3 reports a set of regressions that investigates how subjects’ contributions vary by the strategy types of the other members in their group. As our five strategy types (free rider, unconditional cooperator, conditional cooperator, triangle cooperator, and other) are mutually exclusive, the table uses conditional cooperator strategies as the baseline. Coefficients then represent the expected difference in contributions if one member in the group uses one strategy type rather than a conditional cooperator strategy.

[Table 3 about here]

Regressions (1) and (2) have contributions as the dependent variable, and allow contributions to vary by period and by the strategy types of the other group members. Regressions (3) and (4) use contribution adjustments as the dependent variable, the difference between actual contributions and the contributions promised by the previous period’s response function. Regressions (1) and (3) are standard OLS regressions, and (2) and (4) include random effects dummies for subjects and groups. As we see in Table 3, all coefficients on the different types are negative in all four regressions. In other words, subjects’ contributions increase with the number of conditional cooperators in their group. Similarly, they are also more likely to revise their contribution upward (or down less) the more conditional cooperators in their group.

While it is unsurprising that subjects contribute more when others are conditionally cooperative than when they are matched with free riders, it is somewhat surprising that conditional cooperators on average receive more contributions from others than unconditional cooperators. Subjects care not only about the level of contributions from others, as has been shown in previous studies, but also about the perceived possibility of reciprocation. This suggests that some contributions in the PubStrat treatment are profit-

7We also repeat this analysis for the PriStrat treatment to test if subjects are able to correctly anticipate the strategy types of others. Unlike the regression in the PubStrat treatment, almost all the coefficients are completely insignificant. The only exception is the “other” category, which predicts significantly lower contributions from other group members in the standard OLS regression.
motivated and is direct evidence that some subjects are willing to make reputation-building contributions.

**Conclusion 4** Subjects’ contributions are sensitive to the strategy types of their group members. They contribute more tokens to the public good the more group members use conditional cooperator strategies in the PubStrat treatment.

If subjects contribute more of their tokens when in a group with conditional cooperators, then subjects also have an incentive to switch to conditional cooperator strategies. Table 4 displays the transition matrix for subjects’ strategy types. The left hand side represents the strategy type used in a given period, and the columns represent the strategy type used in the following period. In the PubStrat treatment for example, 14.3% of the subjects who use a free rider strategy and 22.9% of the subjects who use an unconditional cooperator strategy one period use a conditional cooperator strategy in the following period. The diagonals represent the frequency that subjects maintain the same strategy from one period to the next.

According to Table 4, subjects frequently keep the same strategy type from one period to the next. Of all strategy types in both treatments, all but one are more often kept in the following period than changed. This supports the claim by Muller et al. (2008) that strategies are largely stable across periods. At the same time, strategies are much more stable in the PriStrat treatment than in the PubStrat treatment. Subjects in the PriStrat treatment change strategy types less often than in the PubStrat treatment (17% of the time vs. 31%), and every diagonal element is larger in the PriStrat treatment’s transition matrix than the corresponding element in the transition matrix for the PubStrat treatment. This makes sense because subjects in the PubStrat treatment receive more information on what strategies others are using and can use that to inform their own decisions. What is less clear from this table is whether or not conditional cooperator strategies are more popular in one treatment versus the other. While more subjects with conditional cooperator strategies keep them from one period to the next in the PriStrat treatment, subjects switch to conditional cooperator strategies from free rider, unconditional cooperator, and other strategies more frequently in the PubStrat treatment.

Table 5 reports the different strategy types subjects use in periods 2-9 by what strategy type they use in the first period for both the PriStrat and the PubStrat treat-
ment. As expected, subjects are more likely to use conditional cooperator strategies in the PubStrat treatment than in the PriStrat treatment for any initial strategy type. This is especially true for subjects that use unconditional cooperator strategies in the first period. In the PriStrat treatment, fewer than 10% of their subsequent strategies are conditional cooperator strategies, while subjects that are initially unconditional cooperators in the PubStrat treatment later use conditional cooperator strategies 44% of the time. Subjects also rarely used triangle or “other” strategies in the PubStrat treatment unless they initially use one of those strategies, but dabble in both in the PriStrat treatment. Initial conditional cooperators, triangle cooperators, and “other” cooperators also adopt unconditional cooperator strategies more often in the PubStrat treatment than in the PriStrat treatment as we discuss later.

Conclusion 5 Subjects frequently keep the same strategy type from one period to the next in both the PriStrat and the PubStrat treatments. To the extent they do change strategy types, subjects in the PubStrat treatment switch to conditional cooperator strategies more often than do subjects in the PriStrat treatment, regardless of the subjects’ initial strategy type.

Even if a subject sees that the other group members report conditional cooperator strategies in the PubStrat treatment, that subject cannot necessarily trust that their contributions will be reciprocated. Publicly displayed response functions are only binding for one of the four group members each period, and thus three of the group members are free to revise their contributions however they wish. Figure 5 shows the degree to which subjects deviate from their stated strategy (when they have the option) in both the PriStrat and the PubStrat treatment. Surprisingly, subjects do not deviate from their stated strategies significantly more frequently in the PubStrat treatment than in the PriStrat treatment (WMW, $p > .20$, two-tailed), but the direction they deviate is significantly different (WMW, $p < .05$, two-tailed). In the PriStrat treatment, subjects tended to revise their contributions either up or down with almost equal probability. In the PubStrat treatment, however, subjects overwhelmingly revise their contributions down. This means that publicly stated strategies generally represent an upper bound on the potential for reciprocation.
Indeed, subjects seem to be cautious in their contributions, slowly learning the level to which others actually reciprocate. As we mention earlier, many subjects do switch to conditional cooperator strategies in the PUBSTRAT treatment, but if deviations from stated strategies increase over time, then the additional conditional cooperators may not increase final contributions. Figure 6 reports the average stated contributions, actual contributions, and deviations by period in both the PRISTRAT and the PUBSTRAT treatment. Statistical tests confirm that while deviations from stated strategies get moderately larger over time in the PRISTRAT treatment (SRT, $p < .10$, one-tailed), they do not change over time in the PUBSTRAT treatment (SRT, $p > .30$, one-tailed).

![Figure 6 about here]

**Conclusion 6** Subjects in the PUBSTRAT treatment frequently contribute fewer tokens to the public good than their stated strategies would indicate, and this deviation is stable across periods. In the PRISTRAT treatment, subjects contribute either more or less than their stated strategies at almost equal rates, but deviate from their stated strategies more over time.

We have shown that in the PUBSTRAT treatment subjects reward contingent cooperator strategies with higher contributions. Subjects likely learn this, and adopt contingent cooperator strategies more frequently in the PUBSTRAT treatment than in the PRISTRAT treatment for each initial strategy type. Subjects also follow their response functions about equally across periods, neither learning to lie more nor less. All this points to a strategic reason for why contributions may increase over time in the PUBSTRAT treatment. Similar to past experiments that have a direct punishment mechanism, subjects slowly learn that certain actions are rewarded or punished by others, and adapt their strategies accordingly. Unlike treatments with a punishment mechanism, all contributions remain completely anonymous, and any response from group members cannot be targeted. Instead, subjects simply learn to make reputation-building contributions, and learn to be more reciprocal to encourage reputation-building contributions from others.

Strategic considerations may not be the only reason why contributions increase over time in the PUBSTRAT treatment. Looking back to Table 3, the coefficient on period is still positive (though only significantly so in regression (2)). Furthermore, the number of subjects in the PUBSTRAT treatment with unconditional cooperator strategies rises steadily from 10% of subjects in the first period to 20% of subjects in period 9 (the final period strategies are recorded). This is in sharp contrast to the PRISTRAT treatment.
where unconditional cooperator strategies fall from 17% of strategies in the first period to fewer than 6% of strategies in the final period.

This suggests that communicating strategies also enhances subjects’ intrinsic motivations as well. It is important to remember, though, that despite increasing contribution rates and frequencies of unconditional cooperator strategies, the differences between contributions in the final periods of the PUBSTRAT treatment are still not significantly higher than the contributions in the PRISTRAT treatment ($WMW, p > .20$, two-tailed). More experiments will be necessary to test the relative merits of the two treatments in longer settings, but we are cautiously optimistic that communicating strategies could yield long-run benefits.

5 Summary and Further Research

This experiment shows that when subjects are forced to write down private contingent plans of action, they initially contribute more to the public good, but that these contributions steadily decrease over time. Our evidence suggests that subjects who make conditionally cooperative private contingent strategies expect their actions to have a higher impact on others’ future contributions and therefore make reputation-building contributions more often than in other treatments. By contrast, when subjects’ written contingent strategies are made public, contributions begin at the same level as when strategies are not elicited, but steadily increase over time. The slow increase in contributions over time when strategies are made public is because subjects slowly learn the actual degree to which others are reciprocal and the potential to build a reputation as contributors.

From a policy perspective, our results suggest that people generally underestimate the reciprocity of others unless they are forced to think about their contingent strategies and, by extension, the strategies of others. If true on an aggregate level, fishermen, factories, and even countries make fewer pro-social choices than they would if they knew the full impact of their actions. To help alleviate this bias, institutions should encourage people to think more about how their actions depend on the actions of others. In situations where early cooperation is especially important such as fishing communities with a fragile resource stock, it may be best to keep these response functions private information. In situations where the path of cooperation is more important such as climate change mitigation efforts, it may be best to signal strategies publicly. This could be accomplished if countries set emissions targets that depend on the emissions of others rather than a fixed target.
There are several important follow-ups to this work. The first is to apply this same analysis to infinitely-repeated public goods games, similar to those found in Ostrom et al. (1992) and Dal Bó (2005). Such a setting would enable us to test whether the PubStrat treatment is better able to perpetuate cooperation in the long run, and would be highly relevant for many real world situations such as climate change mitigation efforts. The second extension is to run the exact treatment described here, but with random rematching of groups between each period. If the differences between treatments is due to the relative potential for reputation-building contributions as we hypothesize, then we should see a clear difference between partners and strangers treatments. The third extension is to run the same experiment with a higher or lower chance that previously-stated contingent strategies determine current contribution levels. In the current experiment, subjects could only trust the revealed contribution schedules of others one fourth of the time. As such, they still had to slowly learn the extent to which subjects were actually willing to contribute. If instead subjects’ stated response functions were binding 90% of the time, subjects would be forced to take their schedules and the schedules of others more seriously. Raising or lowering the rate that the schedules are binding could change the game significantly, and would determine the roll that commitment plays in determining contribution rates.

Even without further followup, this paper makes significant contributions to our understanding of voluntary contributions to the public good. It analyzes a setting that stops the typical decline of contributions in a Voluntary Contributions Mechanism with only a minimal amount of communication. It reinforces previous findings that most subjects are reciprocal and that this reciprocity is stable over time. The paper also provides direct evidence that subjects are willing to make reputation-building contributions, but that they underestimate the potential for said contributions in a standard public goods game. While the paper does not solve the free rider problem, it shows that some progress can be made when people realize the full extent that their actions influence others.

References


<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>PriStrat</th>
<th>PubStrat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sessions</td>
<td>2</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Groups</td>
<td>5</td>
<td>9</td>
<td>10</td>
</tr>
<tr>
<td>Subjects</td>
<td>20</td>
<td>36</td>
<td>40</td>
</tr>
<tr>
<td>% Female</td>
<td>50%</td>
<td>47%</td>
<td>48%</td>
</tr>
<tr>
<td>% Graduate</td>
<td>40%</td>
<td>36%</td>
<td>38%</td>
</tr>
<tr>
<td># Econ Courses</td>
<td>1.55</td>
<td>1.08</td>
<td>1.2</td>
</tr>
<tr>
<td>Period</td>
<td>Control</td>
<td>PriStrat</td>
<td>PubStrat</td>
</tr>
<tr>
<td>--------</td>
<td>---------</td>
<td>----------</td>
<td>----------</td>
</tr>
<tr>
<td>1</td>
<td>7.0</td>
<td>6.4</td>
<td>5.2</td>
</tr>
<tr>
<td>2</td>
<td>6.8</td>
<td>6.2</td>
<td>4.8</td>
</tr>
<tr>
<td>3</td>
<td>6.5</td>
<td>6.0</td>
<td>4.6</td>
</tr>
<tr>
<td>4</td>
<td>6.3</td>
<td>5.8</td>
<td>4.4</td>
</tr>
<tr>
<td>5</td>
<td>6.1</td>
<td>5.6</td>
<td>4.2</td>
</tr>
<tr>
<td>6</td>
<td>5.9</td>
<td>5.4</td>
<td>4.0</td>
</tr>
<tr>
<td>7</td>
<td>5.7</td>
<td>5.2</td>
<td>3.8</td>
</tr>
<tr>
<td>8</td>
<td>5.5</td>
<td>5.0</td>
<td>3.6</td>
</tr>
<tr>
<td>9</td>
<td>5.3</td>
<td>4.8</td>
<td>3.4</td>
</tr>
<tr>
<td>10</td>
<td>5.1</td>
<td>4.6</td>
<td>3.2</td>
</tr>
</tbody>
</table>

Nash Equilibrium: 4.0
Pareto Optimum: 5.0

Figure 1: Average Contributions per Period, by Treatment
Table 2: Random Effects Regression of Contributions on Period, by Treatment

<table>
<thead>
<tr>
<th></th>
<th>(1) Control</th>
<th>(2) PriStrat</th>
<th>(3) PubStrat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Period One</td>
<td>1.16***</td>
<td>1.87***</td>
<td>0.93***</td>
</tr>
<tr>
<td></td>
<td>(0.178)</td>
<td>(0.166)</td>
<td>(0.147)</td>
</tr>
<tr>
<td>Additional Periods</td>
<td>-0.052***</td>
<td>-0.102***</td>
<td>0.027*</td>
</tr>
<tr>
<td></td>
<td>(0.0201)</td>
<td>(0.0145)</td>
<td>(0.0147)</td>
</tr>
<tr>
<td>Radom Effect Dummies</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>for Subject and Group</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N obs.</td>
<td>200</td>
<td>360</td>
<td>400</td>
</tr>
<tr>
<td>R²</td>
<td>0.230</td>
<td>0.441</td>
<td>0.317</td>
</tr>
</tbody>
</table>

* * p<.10, ** * p<.05, *** * p<.01
Figure 2: Average Reported Response Function by Strategy Type, by Treatment

**Average Response Function by Type, PriStrat Treatment**

- Free Rider (8.6%)
- Unconditional (13%)
- Conditional (33%)
- Triangle (13%)
- Other (32%)
- Average (N=324)

**Average Response Function by Type, PubStrat Treatment**

- Free Rider (12%)
- Unconditional (12%)
- Conditional (44%)
- Triangle (5.6%)
- Other (27%)
- Average (N=360)
Figure 3: Percent of Subjects with Each Strategy Type in Period 1, by Treatment

Strategy Types, Period One

- Free Rider
- Unconditional
- Conditional
- Triangle
- Other

- PriStrat
- PubStrat
Figure 4: Average Period One Actual Contributions by Strategy Type

The bar chart shows the average contributions by strategy type for Period 1. The strategies are categorized into Free Rider, Unconditional, Conditional, Triangle, and Other. The chart compares two types of strategies: PriStrat (blue) and PubStrat (red). The contributions range from 0 to 3. The Unconditional strategy has the highest contribution, followed by the Conditional and Triangle strategies. The Other category has the lowest contributions.
Table 3: Contributions by Number of Group Members of Different Strategy Types, Public-Strategy Treatment

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>(1) Contribution</th>
<th>(2) Contribution</th>
<th>(3) Contribution</th>
<th>(4) Contribution</th>
</tr>
</thead>
<tbody>
<tr>
<td>Free Riders</td>
<td>-0.587***</td>
<td>-0.355***</td>
<td>-0.235**</td>
<td>-0.235**</td>
</tr>
<tr>
<td>Unconditional Cooperators</td>
<td>-0.377***</td>
<td>-0.174*</td>
<td>-0.141</td>
<td>-0.139</td>
</tr>
<tr>
<td>Triangle Cooperators</td>
<td>-0.433**</td>
<td>-0.271*</td>
<td>-0.112</td>
<td>-0.112</td>
</tr>
<tr>
<td>Other</td>
<td>-0.251***</td>
<td>-0.162</td>
<td>-0.174**</td>
<td>-0.172**</td>
</tr>
<tr>
<td>Period</td>
<td>0.034</td>
<td>0.032*</td>
<td>0.0061</td>
<td>0.0062</td>
</tr>
<tr>
<td>Intercept</td>
<td>1.53***</td>
<td>1.28***</td>
<td>0.080</td>
<td>0.078</td>
</tr>
</tbody>
</table>

Random Effect Dummies for Subjects and Groups

<table>
<thead>
<tr>
<th>N obs.</th>
<th>360</th>
<th>360</th>
<th>360</th>
<th>360</th>
</tr>
</thead>
<tbody>
<tr>
<td>R²</td>
<td>0.090</td>
<td>0.302</td>
<td>0.038</td>
<td>0.181</td>
</tr>
</tbody>
</table>

* p<.10, ** p<.05, *** p<.01
The baseline group composition is three Conditional Cooperators.
Table 4: Strategy Type Transition Matrix, by Treatment

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Free Rider</td>
<td>Unconditional</td>
<td>Conditional</td>
<td>Triangle</td>
</tr>
<tr>
<td>Free Rider</td>
<td>0.696</td>
<td>0.000</td>
<td>0.087</td>
<td>0.043</td>
</tr>
<tr>
<td>Unconditional</td>
<td>0.026</td>
<td>0.795</td>
<td>0.077</td>
<td>0.026</td>
</tr>
<tr>
<td>Conditional</td>
<td>0.021</td>
<td>0.011</td>
<td>0.872</td>
<td>0.043</td>
</tr>
<tr>
<td>Triangle</td>
<td>0.103</td>
<td>0.026</td>
<td>0.077</td>
<td>0.744</td>
</tr>
<tr>
<td>Other</td>
<td>0.032</td>
<td>0.022</td>
<td>0.075</td>
<td>0.011</td>
</tr>
</tbody>
</table>

<p>|                   | Free Rider | Unconditional | Conditional | Triangle | Other |
| Free Rider        | 0.600      | 0.029         | 0.143       | 0.029     | 0.200 |
| Unconditional     | 0.086      | 0.657         | 0.229       | 0.000     | 0.029 |
| Conditional       | 0.042      | 0.077         | 0.803       | 0.007     | 0.070 |
| Triangle          | 0.050      | 0.000         | 0.050       | 0.350     | 0.550 |
| Other             | 0.091      | 0.045         | 0.170       | 0.068     | 0.625 |</p>
<table>
<thead>
<tr>
<th>Period One Type</th>
<th>Number</th>
<th>Free Rider</th>
<th>Unconditional</th>
<th>Conditional</th>
<th>Triangle</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td>Free Rider</td>
<td>2</td>
<td>50.0%</td>
<td>0.0%</td>
<td>18.8%</td>
<td>0.0%</td>
<td>31.3%</td>
</tr>
<tr>
<td>Unconditional</td>
<td>6</td>
<td>12.5%</td>
<td>68.8%</td>
<td>8.3%</td>
<td>0.0%</td>
<td>10.4%</td>
</tr>
<tr>
<td>Conditional</td>
<td>11</td>
<td>6.8%</td>
<td>0.0%</td>
<td>79.5%</td>
<td>5.7%</td>
<td>8.0%</td>
</tr>
<tr>
<td>Triangle</td>
<td>7</td>
<td>10.7%</td>
<td>1.8%</td>
<td>10.7%</td>
<td>48.2%</td>
<td>28.6%</td>
</tr>
<tr>
<td>Other</td>
<td>10</td>
<td>0.0%</td>
<td>1.3%</td>
<td>17.5%</td>
<td>5.0%</td>
<td>76.3%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Period One Type</th>
<th>Number</th>
<th>Free Rider</th>
<th>Unconditional</th>
<th>Conditional</th>
<th>Triangle</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td>Free Rider</td>
<td>3</td>
<td>70.8%</td>
<td>0.0%</td>
<td>25.0%</td>
<td>0.0%</td>
<td>4.2%</td>
</tr>
<tr>
<td>Unconditional</td>
<td>4</td>
<td>0.0%</td>
<td>56.3%</td>
<td>43.8%</td>
<td>0.0%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Conditional</td>
<td>14</td>
<td>5.4%</td>
<td>7.1%</td>
<td>83.9%</td>
<td>0.9%</td>
<td>2.7%</td>
</tr>
<tr>
<td>Triangle</td>
<td>5</td>
<td>7.5%</td>
<td>12.5%</td>
<td>15.0%</td>
<td>12.5%</td>
<td>52.5%</td>
</tr>
<tr>
<td>Other</td>
<td>14</td>
<td>11.6%</td>
<td>7.1%</td>
<td>20.5%</td>
<td>8.0%</td>
<td>52.7%</td>
</tr>
</tbody>
</table>
Figure 5: Individual Contribution Adjustments, by Treatment

Contribution Adjustments by Treatment

-3  -2  -1  0  1  2  3

Percent

PriStrat  PubStrat
Figure 6: Contribution and Adjustment Patterns, by Treatment

Contributions and Adjustments, Private Treatment

Contributions and Adjustments, Public Treatment
Welcome

This is an experiment on the economics of decision making. The instructions are simple, and if you pay attention, you can make significant money. During the experiment, the units of account will be in experimental dollars (ED). These ED will be converted to real dollars at the end of the experiment at the rate of $1 per _____ ED. We ask that you not communicate with one another throughout the experiment. If you ever have any questions, feel free to raise your hand to ask.

The experiment is divided into a series of periods – two practice periods followed by ten paying periods. For the experiment, you will be divided into groups of four. Your group will remain the same during all ten paying periods. What happens in your group has no impact on the other groups and vice versa. The tutorial will guide you through each step.

In each period, you are given three tokens. It is your task to decide how many to allocate to a group account, and how many tokens to allocate to a private account. Each token you place in the group account yields 0.50 ED for each member in the group (including you). Each token you place in the private account yields 1 ED for you only. Your total profit for any period is then the sum of your profits from the group and private accounts, and your final profit is the sum of your per-period profits.

Suppose for example you put one token in the group account and two tokens in the private account. Suppose also that the other three group members put a total of 5 tokens in the group account. This means there are a total of 6 tokens in the group account. You would receive 3 ED from the group account (6 tokens x 0.5 ED per token) and 2 ED from the private account (2 tokens x 1 ED per token), for a grand total of 5 ED.

In this experiment, you will make many allocation decisions ahead of time. In previous experiments, some participants preferred to make allocations for the NEXT period dependent on what other group members do THIS period. For that reason, you are asked to complete an allocation chart each period. The allocation chart will indicate what you intend to do NEXT period depending on the number of tokens other group members place in the group account THIS period. ONE of the group members will be locked-in to this decision, and the other THREE will have a chance to revise it later. In the first period, you will also decide your intended allocation for that period at the same time.

After all participants have entered their allocation chart for the NEXT period, you are asked to confirm or revise your allocation for THIS period. After period 1, THREE group members will use this allocation for the period, and ONE member will use the allocation as determined by their previous period’s allocation chart.

To assist you in this, each group member’s allocation chart is displayed. Bolded chart entries indicate what will happen if all players confirm their intended allocations. For example, if under Your Allocation Chart, the entry labeled “If 5, Then:” is bolded, that means that the other three group members intend to allocate 5 tokens to the group
account THIS period. Furthermore, it shows what you intend to do NEXT period if the other group members do put 5 tokens in the group account THIS period. The same is true of bolded entries in the other group members’ charts.

Finally, the results screen is displayed, detailing your allocation and the total tokens allocated by your group members to the group account. When everyone is ready, the next period begins.

QUIZ (Worth $5)

1. Suppose you put 1 token in the group account and 2 tokens in the private account, and the other three group members put a total of 3 tokens in the group account. What is:

   The total number of tokens in the group account? [Yours + Others]__________
   Your profit from the group account? [Total*0.5]________________________
   Your profit from the private account?[Tokens in Private * 1.0] ____________
   Your total profit for this round? [Profit Private + Profit Group] __________

2. Suppose that you entered the allocation chart represented to the right THIS period. What is:

   Your intended allocation THIS period?
   Group Account       Private Account
   __________          __________

   Next Period’s Allocation to the Group Account
   
   If 0, Then: 0 0 0 0
   If 1, Then: 0 0 0 0
   If 2, Then: 0 0 0 0
   If 3, Then: 0 0 0 0
   If 4, Then: 0 0 0 0
   If 5, Then: 0 0 0 0
   If 6, Then: 0 0 0 0
   If 7, Then: 0 0 0 0
   If 8, Then: 0 0 0 0
   If 9, Then: 0 0 0 0

3. Suppose that you entered the allocation chart represented to the right LAST period. Suppose further that the other three group members put a total of 6 tokens into the group account LAST period. If you are chosen to use your allocation from the previous period’s chart to determine your actual allocation THIS period, what is:

   Your actual allocation THIS period?
   Group Account       Private Account
   __________          __________
These screenshots are for the Public-Strategy treatment. In the first round of play, subjects make both an unconditional (and non-binding) allocation of their three tokens THIS period as well as a chart for NEXT period. The chart (found on the far right of the screen) asks subjects how many tokens they will allocate to the group account NEXT period for each possible number of tokens the other three group members allocate to the group account THIS period.
In the Public-Strategy treatment, both the unconditional allocation and the contribution chart of all players are shown (at the top) while subjects decide their revised allocation for THIS period (at the bottom). In the Private-Strategy treatment, each subject only sees their own contribution decisions on this screen.
On the results screen, subjects are told the total number of tokens allocated to the group account. They are not told how many tokens each group member placed. On the right, they are also reminded of their own chart for NEXT period.
Starting in the second period, three of the four subjects will get a chance to revise their allocation decision for that round. The other subject, chosen randomly, is required to make the allocation decision as determined by their allocation chart.