Department of Economics
Discussion Papers
ISSN 1441-5429

Communication, Advice and Beliefs in an Experimental Public Goods Game

Ananish Chaudhuri, Pushkar Maitra and Susan Skeath

No: 05/06

Communication, Advice and Beliefs in an Experimental Public Goods Game

Ananish Chaudhuri
Department of Economics
University of Auckland
a.chaudhuri@auckland.ac.nz

Pushkar Maitra
Department of Economics
Monash University
Pushkar.Maitra@buseco.monash.edu.au

Susan Skeath
Department of Economics
Wellesley College
sskeath@wellesley.edu

Communication, Advice and Beliefs in an Experimental Public Goods Game

Abstract

We study the efficacy of horizontal versus vertical social learning processes in a public goods game. In one treatment, subjects about to play the game can make non-binding common knowledge announcements about their intentions while, in another, subjects do not communicate directly with group members but receive common knowledge advice from the previous generation of players. A third treatment has subjects play with neither communication nor advice. We find that groups that engage in peer communication achieve much lower levels of contribution to the public good than do groups that receive advice. We attribute this finding in part to the fact that some subjects in the communication treatment opted to make no announcement during the communication phase of play.

Keywords: Voluntary contributions mechanism, Advice, Communication, Beliefs, Experiments

JEL Classification: C92, C33, C34

1. Introduction

Early experimental research looking at private provision of public goods using a voluntary contributions mechanism under anonymity and non-assortative matching established two empirical regularities. First, in one-shot plays of the game, contributions usually average 40%-60% of the social optimum with wide variations in individual contribution ranging from 0% to 100%. Second, if the game is repeated, contributions in round 1 start out in the 40%-60% range but then decline gradually over time as more and more subjects choose to "free ride." This second result suggests that cooperation unravels over time and that experimental play of the public goods game tends toward the Nash equilibrium of 100% free riding, even though the strong free riding hypothesis of zero contribution is seldom borne out. Ledyard (1995) describes a generic public goods experiment and provides a comprehensive review of the literature.

In recent years, a number of researchers have adduced evidence which raises questions about the free-riding hypothesis in public goods games. Fischbacher, Gächter and Fehr (2001), Kocher (2004), Houser and Kurzban (2005), Burlando and Guala (2005), and Keser and van Winden (2000) have documented that a large majority of subjects in experimental public goods games are *conditional cooperators.* Conditional cooperators are subjects whose contributions are positively correlated with the expected contribution of others and whose behaviour, therefore, is not consistent with the free riding hypothesis. These subjects are willing to contribute more if they believe that others in the group will contribute more as well.

Further research has investigated broader behaviour patterns of the conditional cooperators. Fehr and Gächter (2000, 2002), Gächter, Hermann and Thöni (2003), Masclet et al. (2003), and Noussair and Tucker (2005) show that conditional cooperators are often willing to punish non-cooperators even if non-negligible monetary costs are incurred in carrying out such punishment and even if it is a pure one-shot game with no scope for future interactions or reputation building. Thus, conditional cooperation coupled with the opportunity to punish non-cooperators (who violate norms of cooperation among players) results in subjects being able to sustain high levels of cooperation over time. Yamagishi (1986) also finds that the possibility of punishing free-riders leads to substantial increases in the provision of the public good. Fehr and Gächter (2002) and Bowles and Gintis (2002) suggest that such "altruistic punishment" may be the primary driving force behind sustaining cooperation in a variety of social dilemmas.

Beyond the existence of conditional cooperators and the possibility of altruistic punishment, other mechanisms have been shown to reduce the extent of free riding in experimental public good games. Dawes, McTavish and Shaklee (1977) show that cooperation can be enhanced significantly in a one-shot version of a binary-decision public goods game (where subjects have to choose between cooperating or not) if subjects are allowed to engage in communication prior to participating in the game. However such communication has to be "relevant," in that subjects have to be afforded a chance to talk about the game itself, rather than "irrelevant" when subjects are given a chance only to get acquainted and only to talk about an unrelated topic. Results show that rates of defection (free riding) are significantly higher in the no communication (73% defection rate) or irrelevant communication conditions (65% defection rate) as compared to the relevant communication treatment (26% defection rate). Isaac and Walker (1988) also report increased cooperation with communication. In one design, subjects start out playing for 10 rounds with no communication and then are allowed to communicate prior to each round during rounds 11 through 20.

Cooperation levels jump up in the latter rounds and approach 100% at the end of the final round. In another design, subjects communicate during the first 10 rounds but not during the next 10 rounds. This latter treatment results in very high levels of contributions in the first set of 10 rounds and, surprisingly, shows contribution levels remaining high even during the latter 10 rounds when no communication could occur.

All of these results suggest that the strong free-riding hypothesis for public goods games is not borne out in experimental play and that a variety of social learning processes play an important role in enhancing cooperation in such social dilemmas. Given that we are acculturated by both our elders and our peers, such social learning typically involves "vertical" transmissions (from elders) in addition to the more familiar "horizontal" transmissions such as the communication with peers studied in the literature described above.[1] Recent work by Chaudhuri, Graziano and Maitra (2005) studies "vertical" social learning using an inter-generational framework in which subjects in generation *t* can leave free-form advice for their generation *t+1* successors. Their work finds that allowing subjects to leave advice in an inter-generational framework can lead to high contributions to the public good but only when such advice is *public and common knowledge*, that is, when all advice from the previous generation is read aloud to subjects by the experimenter.

Chaudhuri, Graziano and Maitra (2004) identify an additional component of the advice mechanism that influences cooperation in their public goods game. That component is the creation of optimistic beliefs about the cooperation of others that follows the reading of advice to subjects. Data on beliefs about what other group members would contribute during the first round of play was collected before and after the reading of advice. Subjects became convinced that their group members

[1] See Boyd and Richerson (1985) and Richerson and Boyd (2005) for more on methods of social learning.

would contribute more to the public good after hearing the advice read aloud than they had believed prior to hearing that advice.

To our knowledge, no study has been done to compare the efficacy of the vertical and horizontal social transmission mechanisms, nor the extent to which these mechanisms influence player beliefs about others' behaviour. Evidence regarding which type of transmission is more valuable in achieving and sustaining cooperation would be of *significant* import to our understanding of social dilemmas and how they are, or can be, solved. Such evidence would also be significant to the more popular (parental) nurture versus (peer) nurture debate in which, for example, Harris (1999) has argued that peers are vastly more important in social acculturation than are elders.

In the current study, we build on the Chaudhuri, Graziano and Maitra (2005) results by directly addressing the question of which social transmission mechanism, vertical or horizontal, leads to greater cooperation in a public goods game. We use some of the data from that paper[2] and collect additional data on play of the public goods game with common knowledge announcements by players. We explicitly consider both the presence of conditional cooperators and the impact of the social transmission mechanisms on players' beliefs about group behaviour. Specifically, we look at the behaviour of several types of groups: (1) groups in which members communicate with one another in advance of playing the game; (2) groups in which members receive advice from a previous generation; and (3) groups in which members neither communicate nor receive advice.[3] To be more specific, we have four separate treatments in all. The "communication" treatment involves common

[2] This data is part of the supplementary material that accompanies Chaudhuri, Graziano and Maitra (2005) and is available publicly from http://www.restud.com/accepted.htm.
[3] Of course social learning will almost always involve a combination of communication and advice. Our experimental design aims to reduce the confounding effects of studying both effects simultaneously and considers them separately instead.

knowledge announcements of intentions by players about to participate in the game. The "inter-generational advice" treatment entails free-form advice left by players in generation t and read aloud to players in generation $t+1$. The "constant advice" treatment relies on the reading aloud of the same advice to a number of groups; this treatment is included to allow us to hold the quality of advice unchanged for several groups.[4] Behaviour in these communication and advice treatments is compared to a "no advice" treatment in which a control group of subjects plays the public goods game with no opportunities for communicating or receiving advice. This last group serves as a benchmark against which we can compare the behaviour of the advice and communication groups.

We find that the contributions to the public good in the communication treatment are significantly lower than those achieved by the other groups. The subjects in the communication treatment achieve statistically significantly lower levels of cooperation than do subjects in the inter-generational advice, constant advice, and even the no advice treatments. Further, within the communication treatment we find the presence of heterogeneous types of subjects who behave quite differently.

In Section 2 we outline the design of our public good experiments. Then in Section 3 we present an overview and analysis of behaviour in our "communication" treatment. We move to a comparison of this horizontal transmission mechanism to the vertical "advice" mechanism in Section 4 and analyze the effects of communication on player beliefs in Section 5. Our final section provides a discussion of the

[4] In the "inter-generational advice" treatment, the quality of the advice changes and tends to become more exhortative regarding cooperation over time. We include the "constant advice" treatment to test whether any increased cooperation in the advice treatments results from the simple reading of the advice or from the change in the quality of the advice across generations. Note that there is no temporal sequence in the "constant advice" treatment.

implications of our results for the free-riding hypothesis and for the solution of social dilemmas.

2. Experimental Design

All of the experiments in this paper were implemented as non-computerized pen-and-paper sessions. All of them were run at Wellesley College in Wellesley, Massachusetts using female undergraduate students.[5] We first describe the general structure of the public goods game in which all subjects participated and then explain the four different treatments that we implemented.

The public goods game was played using groups of five subjects who played the game for 10 rounds. In each round, each subject had an endowment of 10 tokens. At the beginning of each round (t), each participant (i) had to simultaneously decide on how many of the 10 tokens (in integers) she wanted to contribute to a public account and how many tokens she wanted to keep in a private account. Total tokens contributed to the public account were doubled by the experimenter and then divided equally among all five participants. Total contributions to the public account, the doubled amount and the returns to each participant from the public account were announced at the end of each round. Each participant's personal earning for each round was the sum of the tokens she decided to keep in her private account and the tokens she got back from the public account. Each successive round proceeded in the same manner with each subject starting each round with an initial endowment of 10 tokens.

In every treatment, tokens were worth US $0.05. The payoff for each subject i in any round t was then

[5] Wellesley is one of the few remaining all women liberal arts colleges in the US and hence the gender bias in the subject pool.

$$\Pi_{it} = 10 - C_{it} + 0.4\sum_{i=1}^{5} C_{it}; \quad t = 1,\ldots,10$$

The total payoff to a subject was the sum of the per-round payoffs over all 10 rounds $\left(\sum_{t=1}^{10}\Pi_{it}\right)$. Because $\frac{\partial\Pi_{it}}{\partial C_{it}} = -1 + 0.4 < 0, \ \forall t$, it follows that if players care only about monetary payoffs then full free riding $(C_{it} = 0)$ is a dominant strategy in the stage game. However, the aggregate payoff $\sum_{i=1}^{5}\Pi_{it}, \ \forall t$, is maximized if each group member fully cooperates $(C_{it} = 10)$ because $\frac{\partial\sum_{i=1}^{5}\Pi_{it}}{\partial C_{it}} = -1 + 2 > 0, \ \forall t$.

For our first treatment, the *communication* treatment, we have 10 groups of five subjects each for a total of 50 subjects. Here, prior to the beginning of the actual public goods game, subjects are given the opportunity to communicate their intentions. They can choose whether to make one of two specific types of announcements or to stay silent. Announcements can be of the form "I wish to contribute ___ tokens to the public account in round 1" or "Everyone should contribute ___ tokens to the public account in round 1" with the subject filling in the blank space with a number between 0 and 10. All announcements are made in writing by the subjects, collected by the experimenter, and then read out loud to the entire group prior to the beginning of the actual experiment. In this way, the anonymity of the subjects making each announcement is preserved.

We also collect data on the beliefs held by subjects regarding the (future) play of the group in the communication treatment. In each session, immediately after the instructions are read aloud, we ask each subject what she thinks each member of the group (including her) will contribute to the public account in round 1. After each subject finishes making this prediction, we proceed to the communication phase. Immediately following the communication phase, we once more ask each subject her beliefs about contributions to the public account in round 1. Only after this second set of beliefs data is collected do subjects start playing the public goods game itself. Subjects get paid for both predictions depending on their accuracy, using a quadratic scoring rule.[6] This payment is separate from what is earned from playing the game and is a small fraction of what can be earned in the game itself.

In our *inter-generational advice* treatment we start by running a "progenitor" experiment in which five first-generation subjects play the public goods game with no advice. This group leaves advice that is given to the second generation, whose advice is passed on to the third generation, etc.[7] All advice is given to each member of a generation in writing and also read aloud by the experimenter so that we have common knowledge of advice in each generation.

In this particular case we have data for three distinct "families." Family 1 starts with a progenitor group and is followed by four generations. Here, including the progenitor we have five groups of five subjects for a total of 25 subjects. Family 2 starts with a different progenitor group that is followed by six generations for a total of seven groups and 35 subjects. Finally, family 3 starts with a new progenitor generation followed by five additional generations for a total of 30 subjects. Each family represents one independent observation and a distinct time series of data. Payoff to a subject in this treatment is the sum of the amounts that she earns during her lifetime plus 50% of what her successor earns in the next generation; thus we have partial inter-generational caring.[8] Subjects are paid their actual earnings from a

[6] See the instructions for the experiment in Appendix A for the scoring rule used.

[7] Note that data generated by multiple generations is not made up of independent observations but constitutes one time series. To allow time for behaviour to evolve, we must run our experiments for several generations. This process tends to be both time- and money-intensive and we face a trade-off between running more generations versus running more replications.

[8] The data for families 1 and 2 of the inter-generational advice treatment is taken from the Chaudhuri, Graziano and Maitra (2005) study. This data includes contribution levels for all subjects in all groups in

session immediately upon completion of the session. The second payment, which acts as an incentive for subjects to leave meaningful advice, is distributed at a later date after all sessions are complete.

Our third treatment is the *constant advice* treatment in which we have five groups with five subjects in each for a total of 25 subjects. Each subject in each group here receives a sheet of paper with the exact same advice written on it; this advice is also read aloud by the experimenter.[9] In comparison to the *inter-generational* treatment, we have no temporal sequence to the groups here and we keep the quality of the advice constant. There is also no second payment in the *constant advice* treatment so the incentive structure and the payoff scheme here is identical to that in the *communication* treatment.

Finally, in the *no advice* treatment subjects play the public goods game without any opportunity to communicate with fellow group members or receive advice. We have five groups in this treatment, each with five subjects, for a total of 25 subjects. This data comes from the Chaudhuri, Graziano and Maitra (2005) study as well. In our econometric analysis below we also include the three progenitor groups (that did not receive advice) from Families 1, 2 and 3 of the inter-generational advice treatment among the no advice groups. This gives us data for eight no advice groups for a total of 40 subjects in this treatment. See Table 1 for an overview of the design of the experiments and the various treatments implemented.

---

all rounds as well as information on beliefs before and after the receipt of advice; the belief data was examined extensively in the earlier study. Note that the experimental design for family 3 differed slightly from the other two. Each subject in family 3, but none in families 1 or 2, played five practice rounds prior to playing 10 rounds for money. Each subject in family 3 also received a second payment which was 20% of what her successor received rather than the 50% received by those in families 1 and 2. We do not find any significant differences in the contribution levels in family 3 as compared to families 1 and 2, so we have chosen to include this family in order to gain data from an additional independent replication.

[9] Appendix B includes a copy of the advice used in this treatment.

<<Table 1 about here>>

3. Results for the *communication* treatment

We start with some descriptive statistics relating to the behaviour of three distinct sets of subjects within the *communication* treatment. Recall that subjects here are given the choice of remaining silent or making one of two announcements, either "I will contribute ___ tokens in round 1" or "Everyone should contribute ___ tokens in round 1." We assign each subject a "communication code" (CC) determined by her choice. Subjects who choose silence are assigned the code CC0. Those who choose to make a statement of the "I will contribute…" type are assigned CC1 while those who choose to say "Everyone should contribute…" are coded as CC2. There are seven subjects in the CC0 category, 13 in CC1 and 30 in CC2.

Figure 1 illustrates the average contribution for each communication code group in each round. Not surprisingly, we see that average contributions for each group fall over time. Average contributions are the lowest in every round for CC0, the subjects who choose silence, and interestingly, average contributions are highest in every round for the CC1 subjects (those who make an "I will contribute…" statement).

<<Figure 1 about here>>

The data indicate that average contributions for the group as a whole start at 57.2% in round 1 (approximately six out of ten tokens) and decay over time to 18.4% in round 10 (slightly less than two out of ten tokens). Each communication group shows a similar pattern, although the levels differ for each group. Average contributions start at 70% for CC1 subjects compared to around 59% for subjects in CC2 and only 24% for those in CC0. Average contributions actually drop to 0% by the last period for the CC0 group.

Using a standard t-test (t = 1.99, p-value = 0.0619), we find that average contributions are statistically significantly lower for those subjects who remain silent (CC0) compared to those who make an announcement of some type (CC1 and CC2). However, if we consider only those subjects who choose to make an announcement (CC1 and CC2), we find that average contributions are not significantly different across the two treatments (t = 0.5251, p-value = 0.6077). The results are the same using a rank sum test. Contributions are statistically significantly lower for the CC0 subjects compared to the CC1 and CC2 subjects (z = 1.939, p-value = 0.0526) and if we restrict ourselves to the CC1 and CC2 subjects we find that contributions are not significantly different across the two treatments (z = 0.583, p-value = 0.5602).

The descriptive statistics presented in Figure 1 give us a broad overview of subject behaviour, but it is worthwhile to carry out some econometric analysis of individual level contributions across the three communication types as well. Such an analysis provides insight into the temporal pattern of contributions.

Let C_{it} be the contribution of player i in period t. This observed contribution C_{it} equals the desired contribution, C_{it}^* (which is a latent variable), if and only if $0 \leq C_{it}^* \leq 10$. Therefore we have:

$$C_{it} = \begin{cases} 0 & \text{if } C_{it}^* < 0 \\ C_{it}^* & \text{if } 0 \leq C_{it}^* \leq 10 \\ 10 & \text{if } C_{it}^* > 10 \end{cases}$$

and C_{it}^* is determined by the following equation:

$$C_{it}^* = X_{it}\beta + \nu_i + \varepsilon_{it}$$

for $i = 1,\ldots,n$ and $t = 1,\ldots,T$. The random effects (ν_i) are IID $N(0,\sigma_\nu^2)$ and the errors (ε_{it}) are $N(0,\sigma_\varepsilon^2)$ independent of ν_i. Here X_{it} denotes a vector of time invariant effects (like treatment effects), time varying variables (like an individual's earnings in the previous round), and an overall time effect, which is common to all players. Each subject's contribution is bounded by zero from below and by ten (the token endowment) from above and thus we estimate this model as a random effects Tobit.[10]

We include four explanatory variables. There are two communication category dummies, one each for CC1 and CC2 with CC0 serving as the reference category. There is the inverse of time $\left(\frac{1}{t}\right)$ which allows us to capture the non-linearity in the effect of time on contributions and also allows us to distinguish between the effects of early and later rounds on contributions. Also included is the deviation from the average group contribution in the previous period, which we define as $\Psi_{it-1} = \frac{1}{n}\sum_{i=1}^{n} C_{it-1} - C_{it-1}$, where n is the size of the group. This last variable is included to test whether players, in deciding on their contribution levels in period t, take into account how their individual contributions compared to the group average in period $t-1$. Note that $\Psi_{it-1} > 0$ implies that player i contributed less than the group average in period $t-1$ while $\Psi_{it-1} \leq 0$ implies that player i contributed at least the group average or more in period $t-1$.

Table 2 presents the regression results for individual contributions by player i in period t for each of the three different models. The Random Effects Tobit regressions are presented in column (2). We note, first, that individual contributions

[10] For the sake of comparison we also compute the random effects GLS regression, although this method does not account for the upper and lower censoring of the dependent variable and the estimates are inconsistent. We cannot compute the corresponding fixed effects Tobit model because there does not exist a sufficient statistic that allows the fixed effects to be conditioned out of the likelihood function. We do check the robustness of our results by obtaining the unconditional Tobit estimates with player fixed effects, conceding that these latter estimates are biased.

are negatively and significantly affected by time. Contributions fall as t increases and as $1/t$ decreases, hence the positive coefficient associated with $1/t$.

Second, both of the communication category dummies (CC1 and CC2) are positive and statistically significant. This result implies that those who choose to communicate contribute more to the pool. However, using a $\chi^2(1)$ test, we cannot reject the null hypothesis that the two communication categories (CC1 and CC2) have the same effect. This result suggests that the contributions of CC2 subjects are not statistically significantly less than those of CC1 subjects.

Third, the deviation from the group average in period $t-1$ $\left(\Psi_{i,t-1}\right)$ does not have a statistically significant effect on contributions in period t. As a result, we must conclude that our hypothesis that there is a dynamic component to individual contributions is not borne out in the data.

The Random Effects GLS regression results and the Tobit regression results with player fixed effects are presented in columns (3) and (4) in Table 2. The results from these models are qualitatively very similar to the Random Effects Tobit regression results presented in column (2). We find positive and significant time variables and communication category dummies in both models and the χ^2 test in our unconditional Tobit with player fixed effects shows that the player dummies are jointly statistically significant at the 1% level.

<<Table 2 about here>>

It is clear from our discussion above that the CC0 subjects are especially uncooperative. In fact, when examining the behaviour of each actual playing group in this treatment, we find that groups with more CC0 subjects are less cooperative than groups with fewer CC0 subjects. Furthermore, we find that the presence of CC0 subjects reduces the level of cooperation of the CC1 and CC2 types as well; these other subjects contribute significantly less to the public good with an increase in the number of CC0 types in the group. In Figure 2 we focus exclusively on the behaviour of CC1 and CC2 subjects, dividing them into four sub-groups as follows: (1) when there are no CC0 types in the group; (2) where there is only 1 CC0 type in the group; (3) when there are 2 CC0 types in the group and finally (4) when there are 3 CC0 types in the group. There are no groups with more than three CC0 types in the group. The figure presents the average contribution by the CC1 and CC2 types in each round when the sample is subdivided using the number of CC0 types in each group. It is clear from Figure 2 that the CC1 and CC2 subjects are far less cooperative when there are more CC0 types in the group. In fact there is a dramatic reduction in contributions of the CC1 and CC2 types when the number of CC0 types in the group increases from 0 to 1. Contribution levels are not as low when there are 2 CC0 types in the group as opposed to 1 or 3 CC0 types, but given the small number of observations we are working with, it is not surprising that we do not find a clearly delineated pattern of behaviour. What is striking is that the presence of CC0 subjects does seem to have a dampening effect on the contributions of the CC1 and CC2 types. We return to this issue in Section 6 where we discuss the implications of our results.

<<Figure 2 about here>>

4. Comparing vertical and horizontal transmission

In this section we compare behaviour in the communication treatment with that in the two (inter-generational and constant) advice treatments and in the no advice treatment. As described above, subjects in the communication treatment are limited to a set of two specific announcements or silence while subjects in the inter-generational advice treatment can leave free-form advice for later players. Most of the advice left in this treatment specified some kind of a dynamic rule, but in two out of the three families, and especially in the later generations, the advice left was very strong with literally every subject exhorting her successors to contribute all ten tokens to the public account "...all the time!" We control for the quality of advice in the constant advice treatment by providing the same set of advice, chosen for its representative nature, to each of the five groups. Subjects in the no advice treatment received neither communications nor advice from other members of their group (or previous groups).

Figure 3 illustrates average contributions aggregated over all groups, or generations, for each of our four treatments over the 10 rounds of play. Contributions in both advice treatments are higher in every round than those in the communication or no advice treatments; contributions are actually highest in the constant advice treatment. For those subjects playing with constant advice, average contributions start near 85% in round 1, and in the inter-generational advice treatment average contributions start at about 73%. In contrast, in the communication and no advice treatments, average contributions start at only about 57% and 59%, respectively. By round 10, average contributions in all treatments have fallen considerably but those in the constant and inter-generational advice treatments are at 51% and 42%, respectively. These ending levels are not far below the starting levels in the other two treatments. Those treatments end even lower, with average contributions in the no advice treatment falling to 37% and average contributions in the communication treatment falling considerably lower to 18% in round 10. Overall, we also see in Figure 3 that average contributions in the communication treatment decay appreciably faster over the 10 rounds than do contributions in any of the other three treatments.

Finally it is worth noting that a Kruskal-Wallis test rejects the null hypothesis that the samples (for the different treatments) are drawn from the same population (p-value = 0.0270).

<<Figure 3 about here>>

In addition to analysing the descriptive statistics associated with each the four treatment groups, we provide some additional parametric evidence that the communication groups attain lower levels of contribution to the public good than do the advice, and even the no advice, groups. We regress contributions against a set of dependent variables identical to those employed in Section 3, except for the category dummies. Here we include three treatment dummies, one for the no advice treatment, one for the inter-generational advice treatment and the third for the constant advice treatment. We use the communication treatment as the reference category. As in Section 3, we test our data with three separate models, a random effects Tobit model, a random effects GLS model and also a Tobit model with player fixed effects.

The results for our three regressions are presented in columns 2 through 4 of Table 3. Not surprisingly, these results are similar in all three models. Relative to the reference category of the communication treatment, Table 3 indicates that contributions are statistically significantly higher in all three of the other treatments, with the constant advice treatment highest and the no advice treatment lowest of the three. The null hypotheses of overall and pair wise equality of treatment effects are always rejected. Unlike in our regressions for the communication treatment alone, we find evidence of dynamics in individual behaviour in this model; the coefficient estimate for $\Psi_{i,t-1}$ is negative and statistically significant. This result implies that subjects in the different treatments do take into account how their contributions in the previous round deviated from the group average and whether they were above or below that average. Finally the time variable $\left(\frac{1}{t}\right)$ is significant at the 1% level in all three models.

<<Table 3 about here>>

Our results imply that the groups in which there is direct communication among fellow group members are significantly less cooperative (achieve significantly lower average contributions to the public good) than their peers who engage in vertical social learning via advice from predecessors. Most studies to date have suggested that direct communication is generally beneficial to players. Thus our data appears surprising in light of this literature on the positive impact of player communication, even cheap talk, on both coordination and cooperation in experimental games.[11]

Perhaps the observed low levels of contributions in our communication treatment are not surprising after all. Bear in mind that a non-negligible number of subjects (seven out of 50, or 14%) chose to remain silent in this treatment. Such silence is very possibly (and *ex post* correctly) perceived by other group members as a signal of non-cooperation and might have contributed to the generally low levels of contributions in the communication treatment.

Interestingly, there is one earlier paper that is more in keeping with the results we obtain. Orbell, Dawes and van de Kragt (1990) study the role of communication in a different version of the public goods game in which subjects are free to make non-binding promises to others about their contributions. They find that such non-binding promises can lead to significantly higher levels of cooperation but only when *all* members of a particular group promise to contribute. There is no evidence that

[11] See Cooper, et. al. (1989, 1992) for evidence on the effect of player communication in coordination and battle-of-the-sexes games and Dawes, McTavish and Shaklee (1977) as well as Isaac and Walker (1988) for evidence in public goods games.

promises enhance cooperation when such promise-making is less than universal. We may be observing another example of the same type of phenomenon in which cooperation is enhanced only when an entire group signals cooperative intent. In our case, we can go even further and claim that perhaps only one particular *type* of announcement ("promise"), when made universally, can improve cooperation in the game.

The obvious heterogeneity among the subjects in our communication treatment leaves us with lingering questions regarding their differential performance relative to the subjects in the three other treatment groups. To address these issues, we perform a simulation designed to ensure that the small number of subjects in each communication code group does not mask larger tendencies in the population. We consider what would happen if we could separate the three communication code groups (CC0, CC1, and CC2) into three distinct groups. What behaviour could we expect to see in these three separate populations, each consisting of subjects of all one type?

We know that behaviour in the public goods game is extremely path dependent, with contributions typically falling off over time. Thus, what happens in the first round of a ten round interaction is crucial. If a group of subjects is going to succeed in achieving high levels of contributions to the public good, then they must start at a reasonably high level. Within our simulation, we focus on this specific issue, looking at first-round behaviour of all possible five-member groups that can be formed for each of our three sub-populations. Given that we have seven subjects in the CC0 group, 13 in CC1 and 30 in CC2, we have $\frac{7!}{2!5!} = 21$ possible groups among the CC0-types, $\frac{13!}{8!5!} = 1287$ possible groups among the CC1-types and finally $\frac{30!}{25!5!} =$

142,506 possible groups among the CC2-types. We determine the average first-round contribution for each possible group in each sub-population and then compare the averages across communication code groups.

In Figure 4, we present the results of our simulation exercise. Each histogram in Figure 4 is associated with a particular communication code group and shows the percentage of our simulated groups (within the given CC population) yielding a particular average first-round contribution. Among the sub-population that chooses silence (CC0), illustrated in the top panel of Figure 4, we find that almost 30% of the simulated groups have average first-round contributions of 0 and almost 85% of them have average first-round contributions at or below 40% (4 out of 10 tokens). With these low levels of cooperation in the very first round, the vast majority of the simulated CC0 groups would end up achieving very low levels of contribution to the public good overall.

At the other extreme, the middle panel of Figure 4 shows that almost 90% of the simulated CC1 groups would contribute 50% or more on average in round 1, with almost a third of the groups starting their 10 round interaction with an average first-round contribution above 90%. Such high initial contribution levels should mean that a sub-population of CC1-types could keep contributions quite high over the 10 rounds.

The data for the CC2 group presented in the bottom panel of Figure 4 is more diffused. These groups would clearly end up being more cooperative than the CC0-types but probably less cooperative than the CC1-types. A substantial number of potential CC2 groups would start at relatively low levels of contributions in the first round. About 50% of the simulated groups would have an average round 1 contribution of 60% or more. However, in almost 35% of these groups the average round 1 contribution would be 40% or less.

<<Figure 4 about here>>

There is too much interdependence among the observations in the simulation exercise to carry out any formal statistical tests. We also do not try to simulate play for 10 full rounds; it is not clear whether that exercise would convey much meaningful information given that subjects do learn from each others' choices over rounds. Based on the limited simulation results presented above, however, we can conjecture that even the *communication* treatment would witness high levels of contribution if the majority of announcements were of the CC1, "I will contribute…," type. Thus our evidence is indeed consistent with Orbell, Dawes, and van de Kragt (1990) but only for one of our communication code groups.

5. Beliefs in the communication treatment

Chaudhuri, Graziano and Maitra (2004) report that one way in which common knowledge of advice influences contributions to the public good is via the creation of optimistic beliefs about the cooperation of others. They find that with common knowledge of advice, subjects become convinced that their group members will contribute more to the public good after hearing the advice read aloud than they had believed prior to hearing that advice.

As explained in Section 2, subjects in our communication treatment were asked about their beliefs regarding their fellow group members' contributions to the public good. Specifically each subject was asked to indicate how many subjects she believed would choose to contribute 0 or 1 or 2 or …10 tokens to the public good in round 1. This was done once before the communication round and once after each subject had been given the opportunity to make an announcement about her intentions and had heard the entire groups' announcements read aloud. (See the instructions in Appendix A for the belief elicitation mechanism and the scoring rule used.) This prediction about the contributions of fellow group members is payoff relevant since subjects are paid according to the accuracy of their predictions using a quadratic scoring rule. Their responses to the belief question give us insight about what they expect other members of the group to contribute to the public account in round 1 of their 10 round interaction. Each subject makes this prediction for everyone in the group including her.

As we mentioned in the introduction, there is now evidence that a large majority of subjects in experimental public goods games are *conditional cooperators.* Such players make contributions that are positively correlated with the expected contribution of others. We can categorize our subjects into different groups depending on how their beliefs about contributions to the public good in round 1 are related to their actual contributions in that round. We define a subject as a conditional cooperator if that subject's contribution to the public account in round 1 falls within one token more or less than her stated beliefs about average group contributions in round 1. (See Burlando and Guala (2005) and Chaudhuri, Graziano and Maitra (2005) for a similar metric) For example, if a subject expects the average contribution to the public account to be x tokens in round 1, then we classify this subject as a conditional cooperator if she contributes x-1, x or x+1 tokens in round 1. Any subject who contributes more than the expected average contribution plus one token (i.e. x+2 tokens or more) is a *cooperator* and anyone who contributes less than the expected average minus one token (i.e. x-2 tokens or less) is a *non-cooperator*.

Using this method of classifying subjects we find that among the CC0 types, 57% are non-cooperators while the remaining 43% are conditional cooperators and there are no cooperators. Among the CC1 types the majority (54%) are conditional cooperators while the remaining 46% are divided equally into non-cooperators and cooperators. The CC2 group (which consists of 30 out of 50 subjects in this treatment) is interesting. Here 50% of the subjects are conditional cooperators, 33% are non-cooperators and 17% are cooperators. This might explain why the round 1 contributions of this sub-population are so diffuse in the simulation exercise presented in Figure 4.

In order to look more closely at the beliefs held by subjects in the communication treatment, we take recourse once again to a simulation exercise similar to the one described in Section 4. Instead of looking only at the actual data on beliefs as stated by the subjects we create a hypothetical data set of beliefs. This hypothetical belief data set consists of beliefs expressed both before and after the common knowledge announcements made during the communication round. We could simply look at the actual beliefs expressed by the subjects but as argued before if we did so then the small number of observations might easily mask population wide tendencies. Thus, as before, we divide up the subjects into the three communication categories – CC0, CC1 and CC2 – and then form all possible five member groups among those sub-populations. Then we look at the beliefs expressed by each of those possible five-member groups and aggregate over all groups to come up with the data contained in Table 4. The belief data contained in Table 4 then provides us with an overview of the beliefs that these three sub-populations might possess about their fellow group members. We use this hypothetical data on beliefs as a benchmark of the beliefs held by the different groups.

We find that the CC0 subjects have very pessimistic initial beliefs, expecting only 28% (2.83 tokens) contribution to the public good on average in round 1. The communication round does make these subjects more optimistic but not much more so. After the communication rounds, CC0 types believe that average contributions to the public good will be 37% in round 1. The beliefs of the CC1 and CC2 subjects prior to the communication round are roughly similar; each group expects average contributions of around 50-55% in round 1. After the communication round the CC1 players experience the greatest increase in optimism in that they now expect average contributions of around 73% as opposed to 51% before. This result is somewhat surprising because in some cases the communication round would include subjects choosing to stay silent and would have confirmed the presence of subjects who chose an announcement other than the CC1 type (and, in retrospect, only an announcement of the CC1 type is an indication of strong cooperation). We would have expected this to lead to greater pessimism rather than to increased optimism. For the CC2 subjects, there is also slightly greater optimism following the communication round. These subjects believe post-communication that contributions in round 1 will average 63%, versus a belief of 55% before.

What is interesting is that prior studies find that, under anonymity, non-assortative matching, and in the absence of explicit communication between group members, or punishments or rewards, contributions usually start around 60% and decline steadily from then onwards. Thus for the majority of subjects in the communication treatment (37 subjects out of 50 or 74% in the two categories CC0 and CC2 combined) their beliefs are not any more optimistic than one would have expected to see in most prior studies. The common knowledge announcements of intentions did not succeed in elevating the beliefs of the majority of our subjects beyond the historical 60% benchmark. Again bear in mind that we are not talking about the actual beliefs held by members of groups that actually formed but about beliefs that could have been held by all possible five-member groups.

From rows 3 through 5 of Table 4 we find that first round contributions in the communication treatment are indeed strongly correlated with the actual post-communication beliefs held prior to that first round. Given that those beliefs are not terribly optimistic it is not surprising that contributions in this treatment started out at about 57% which is well within the benchmark established in prior studies.

<<Table 4 about here>>

6. Discussion of our results and concluding remarks

The most striking finding in our study is the relative lack of success of the communication groups, those with horizontal information transmission, vis-à-vis the advice groups, in which information moved vertically. Importantly, we cannot attribute this difference in behaviour to differences in payment schemes. Although each inter-generational advice subject receives a second payment, equal to 50% her successor's earnings, subjects in the constant advice and communication treatments each receive only one payment. If the payment scheme is driving the result, then we should observe little difference between the constant advice and communication treatments. But we get very strong differences there as well.

Nor can the differences in contribution levels between the communication and advice groups be attributed to the fact that the communication in our experiment is a one-time announcement prior to the start of the public goods game itself. Our method of communication is certainly a factor behind the low contributions; other papers that find that communication enhances efficiency, such as Isaac and Walker (1988) or Cooper et al., (1989, 1992), all have subjects engaging in multiple rounds of communication. But, in our set-up, advice is also read out loud only once prior to starting the game. If any cooperation-enhancing effect of the one-round, pre-play communication is short-lived, we would not expect a single, pre-play round of advice to fare any better. Thus we are left with the conclusion that the presence of advice seems to have a salutary effect on contributions which the availability of communication does not. The finding that advice makes a difference is not surprising. Schotter (2003) provides an overview of a series of experiments where subjects can receive advice and where this advice has a salutary impact on decision making. The puzzle here is why announcements made at the beginning of the game by the participants themselves have such little impact on contributions to the public good.

Rabin (1993) shows that in the presence of reciprocal preferences it is possible to think of the public goods game as a coordination problem with multiple Pareto-ranked equilibria. The problem of sustaining high contributions in the public goods game is then primarily a problem of equilibrium selection. Cooper et al. (1992) shows that two-way communication facilitates coordination to the payoff dominant outcome in a simpler coordination game with two Pareto-ranked equilibria. Cooper et al. (1992, p. 750) comment, "The role of communication then is to provide a basis for the strong beliefs needed to overcome coordination failure." This insight might explain why our communication treatment does not perform well. It is possible that sufficiently optimistic beliefs can be created only if announcements possess some degree of unanimity; Orbell, Dawes and van de Kragt (1990) make this point in their work. Also, as Farrell and Rabin (1996) have observed, the availability of communication creates a meta-coordination game where players must coordinate their beliefs on what various messages *mean* before they can coordinate on what actions to choose. It is conceivable that all non-announcements are perceived as signals of subsequent non-cooperative behaviour. This hypothesis is consistent with the observed behavior illustrated in Figure 2, above, with subjects contributing less when there are more "silent" players in their group. Moreover it is possible that all announcements of the type "Everyone should..." also created a degree of ambiguity regarding future behaviour; *ex post,* we do find that a substantial number of subjects who chose an announcement of this type ended up behaving as free-riders.

Another possible explanation for the poor performance of the communication treatment is that there is a greater sense of disillusionment in that treatment. Subjects hear statements made by others about what each will do or what each thinks the group should do. Then the subjects play the first round of the game and observe that the outcomes are not commensurate with the announcements. This discovery may lead to a greater sense of "betrayal" and subsequent loss of trust in the communication treatment. There is no similar issue of trust in the advice treatments because there is no direct communication among subjects in those treatments. In the communication groups, subjects "know" more about each other, via the communication, than in any of the other treatments. For this reason, they may be more sensitive to low contributions.

To the extent that the previous literature is correct in declaring public goods games to be ones primarily of coordination, our results suggest that communication is not as efficient in achieving coordination as is the passing of advice. Of course, any pre-play announcement, whether direct communication or advice, is essentially a correlating device in the sense of Aumann (1974). Thus our results further suggest that advice from a previous generation of players is a better correlating device, facilitating coordination at higher contribution levels, than is direct communication among players. Vertical social learning processes appear to be more useful to subjects in the public goods game than do similarly structured horizontal learning processes.

In her book "The Nurture Assumption" Harris (1999) makes a powerful argument that it is peers rather than parents who matter more in the social acculturation of new generations. We find virtually the opposite to be true in the public goods game. When considering cooperative behaviour in the presence of a conflict between cooperation and self-interest and in the presence of heterogeneous types, social learning via peer communication does not always enhance cooperation while received wisdom from elders generally does.

Appendix A: Public Goods Game Instructions

Instructions

This is an experiment in economic decision-making. Wellesley College has provided the funds to conduct this research. The instructions are simple. If you follow them closely and make appropriate decisions, you may make an appreciable amount of money. This money will be paid to you in cash at the end of the experiment.

You are in a market with 4 other people. The experiment will consist of 10 decision rounds. You will keep track of your decisions and your earnings using the Record Sheet on page 6. Please take a look at the Record Sheet now to see how you will keep your track of your earnings.

At the beginning of each round (say Round 1) each participant will have an endowment of 10 tokens. In that round, each participant will choose how many tokens (ranging from 0 to 10) to allocate to a private account (Column 2) and how many tokens (ranging from 0 to 10) to allocate to a public account (Column 3). For each round, these two numbers should add to 10, the total number of tokens you have for that round. In each round you will also write the number of tokens you wish to contribute to the public account on the Decision Form (see page 7) and hand it to the experimenter. The experimenter will then add up the total contributions to the public account and announce it publicly. The total number of tokens invested in the public account will **be doubled** and divided equally among all 5 participants. This is your return from the public account and will be announced publicly. Please write this number in Column 4. Your personal earnings for this round will equal the number of tokens you invested in your private account (Column 2) plus the number of tokens you get back from the public account in Column 4 (the latter may be a fractional amount). The round then comes to an end and the next round begins.

Each new round will proceed in the same way. Tokens invested in the private account in any round do not carry over to the next round. Every round you start with a fresh endowment of 10 tokens. At the end of the experiment your total earnings from the 10 decision rounds will be added up and converted into cash at the rate of 5 cents per token. Earnings in this experiment are denoted in experimental dollars and cents which will be converted into cash using the exchange rate of 1 experimental dollar equal to $1.50.

[Prior to the beginning of Round 1 of the actual experiment you will have an opportunity to communicate with other members of your groups about the proposed course of action in Round 1, should you choose to do so. Such communication will occur using the form on page 4. Notice that you have two options – either to communicate with your groups members or to remain silent. When asked to do so please circle one of the two options – **Communicate** or **Remain Silent**. If you choose to communicate then you have to choose one of two statements – (1) I will contribute __ tokens to the public account in round 1 (and you need to fill in the blank with a whole number between 0 and 10 or (2) Everyone should contribute __ tokens to the public account in Round 1 (where once again in you need to fill in the blank with a whole number between 0 and 10). When done please tear out this sheet and hand it to the experimenter. The experiment will then publicly announce whether each individual has chosen to communicate or remain silent and if the former then the actual statement that the individual has chosen to make.]

In the common knowledge of advice treatment the previous paragraph was replaced by the following paragraph.

[**Unless you are in the first group** to participate in this experiment, when you start the experiment you will receive written advice on how to make your decisions from a group of subjects who participated in the experiment prior to you. Each of you will get to see the advice left by all the players in this group prior to you. So each of you is looking at the exact same set of advice as every body else. Besides providing you with this advice the experimenter will also read the advice out loud prior to starting the experiment. At the end of your 10 decision rounds you will be asked to leave advice to the next group of players in the experiment. Each of you is paired with another player, who you do not know and who will participate in the experiment immediately after you. On top of what you make in this session of the experiment, you will receive an additional payment equal to 50% of the earnings of this. You will be told how to collect this second payment after the instructions have been read. Please write your advice on the sheet (page 6) provided, and write or print legibly. In writing advice we encourage you to specify a contribution amount or range to the next player. You will be notified by email or telephone when your second payment is ready.]

If you have any questions, please ask them now.

Subject ID ________________

Additional Instructions

Before the beginning of the actual experiment, i.e. before Round 1, you will be asked to predict how many tokens every participant will choose to contribute to the public account in Round 1. When you are asked to do so, please write down your prediction of how many people will contribute 0, or1, or 2, or 3, or 4, or 5, or 6, or 7, or 8, or 9 or 10 tokens in the space provided on page 4. Please take a look now. When you add your predictions of the number of people that will contribute 0, or 1, or 2, or 3, or 4, or 5, or 6, or 7, or 8, or 9 or 10 tokens they should add to 5. You are predicting for everyone in the group including you.

You will be paid for each of your correct predictions as follows. Your earnings will equal 50 cents less the sum of squared differences between your predictions and the actual choices.

EXAMPLES: Suppose that 5 people each had a red ball and a blue ball, and that they were all asked to put one and only one of the balls into an urn. At the same time they each were asked to predict the number of red balls and the number of blue balls that would end up in the urn. With a payment rule like that above they would find their earnings as follows:

	Predict	Actual	Sq.Diff		Predict	Actual	Sq. Diff
Blue	5	0	25	Blue	5	5	0
Red	0	5	25	Red	0	0	0
Total	50			Total	0		
50 – 50 =	0			50 - 0 =	50		

	Predict	Actual	Sq. Diff		Predict	Actual	Sq. Diff
Blue	3	3	0	Blue	4	1	9
Red	2	2	0	Red	1	4	9
Total	0			Total	18		
50 – 0 =	50			50 – 18 =	32		

You will make this prediction once before you get to read the advice and once after you get to read the advice.

You will be told the actual choices made for the periods you were asked to make predictions at the end of the experiment.

IF YOU HAVE ANY QUESTIONS PLEASE ASK THEM NOW!!!

Subject ID_______________

Prediction BEFORE the Communication Round

Tokens	Predict	Actual	Sq. Diff
0			
1			
2			
3			
4			
5			
6			
7			
8			
9			
10			
TOTAL			

Earnings = 50 - __ =

Subject ID ____________

The Communication Round

Please pick one of the following two options: (Circle One)

Communicate **Remain Silent**

If you chose "Communicate" then please pick one of the two statements below (by checking the box on the left) and fill in the blank in that statement with a whole number between 0 and 10.

☐ 1. I will contribute _____ tokens to the public account in round 1.

☐ 2. Everyone should contribute ____ tokens to the public account in round 1.

Tear out this page when you are done and hand it to the experimenter.

Subject ID________________

Prediction AFTER the Communication Round

Tokens	Predict	Actual	Sq. Diff
0			
1			
2			
3			
4			
5			
6			
7			
8			
9			
10			
TOTAL			

Earnings = 50 - ____ =

Record Sheet

Round	Tokens in Private Acct (Column 2)	Tokens in Public Acct (Column 3)	Returns from Public Account (Column 4)	Total Tokens (Add Cols. 2 and 4)
1				
2				
3				
4				
5				
6				
7				
8				
9				
10				
			TOTAL	

<u>TOTAL EARNINGS:</u>

Earnings from the game: ______________

Earnings from first prediction: ______________

Earnings from second prediction: ______________

TOTAL: ______________

Decision Form

Subject ID: ____________________

Round	Contribution to the Public Account
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	

Appendix B: Advice Used in Constant Advice Treatment

It would be good to give generously in the first two or three rounds as others do as well and so the returns are high. As you go on, maybe keeping at least 5 tokens in the private account is also good as it is yours to keep anyway.
Suggested first round contribution: 7

Please contribute more but not too much at private account.
Suggested first round contribution: 5

Do not listen to anyone else's advice! Here is the maths. Public 10 X 5 players = 50 (which gets doubled) = 100/5 = 20 each. Private 10 = 10 ea. You must trust each other. Always give 10. If you get greedy and only start paying in a little to the public then the group (you are in the group!) loses. For everyone to win (and get the highest possible amount) give 10 always to the public account. Trust me, trust the other players. Always give 10! Be smart – group max = your pay max. Nobody likes a greedy person either! If everyone listens to me then you should predict everyone to choose 10! Good luck.
Suggested first round contribution: 10

By having more invested return will be more unless your group is very conservative. Take the first few rounds to see whether your group is aggressive. If your group is, then throw all you have at it. If not then invest 1 or 2.
Suggested first round contribution: 10

The contribution amounts of the others in the group is unexpected, so just contribute average money in different rounds.
Suggested first round contribution: 6

References:

Aumann, R., 1974. Subjectivity and Correlation in Randomized Strategies. *Journal of Mathematical Economics* 1, 67-96.

Bowles, S., Gintis, H., 2002. Homo Reciprocans. *Nature* 415, January 10, 2002, 125-128.

Boyd, R., Richerson, P.J., 1985. *Culture and the Evolutionary Process*. Chicago, University of Chicago Press.

Burlando, R., Guala, F., 2005. Heterogeneous Agents in Public Goods Experiments. *Experimental Economics* 8(1), 35 – 54.

Chaudhuri, A., Graziano, S., Maitra, P., 2005. Social Learning and Norms in a Public Goods Experiment with Inter-generational Advice. *Review of Economic Studies*, forthcoming.

Chaudhuri, A., Graziano, S., Maitra, P., 2004. Social Learning and Norms in a Public Goods Experiment with Inter-Generational Advice. August 2004, http://ssrn.com/abstract=580481 .

Cooper, R., DeJong, D., Forsythe, R., Ross, T.W., 1989. Communication in the Battle of the Sexes Game: Some Experimental Results. *Rand Journal of Economics* 20, 568-587.

Cooper, R., DeJong, D., Forsythe, R., Ross, T.W., 1992. Communication in Coordination Games. *Quarterly Journal of Economics* 107, 739-771.

Dawes, R., McTavish, J., Shaklee, H., 1977. Behavior, Communication, and Assumptions About others People's Behavior in a Commons Dilemma Situation. *Journal of Personality and Social Psychology* 35, 1-11.

Farrell, J., Rabin, M., 1996. Cheap Talk. *Journal of Economic Perspectives* 10, 103-118.

Fehr, E., Gächter, S., 2002. Altruistic Punishment in Humans. *Nature* 415, January 10, 2002, 137-140.

Fehr, E., Gächter, S., 2000. Cooperation and Punishment in Public Goods Experiments. *American Economic Review* 90(4), 980-994.

Fischbacher, U., Gächter, S., Fehr, E., 2001. Are People Conditionally Cooperative? Evidence from a Public Goods Experiment. *Economics Letters* 71(3), 397-404.

Gächter, S., Hermann, B., Thöni, C., 2003. Understanding Determinants of Social Capital: Trust, Cooperation, and Informal Sanctions in a Cross-Societal Perspective. Working Paper, Universität St. Gallen and CESifo*.

Gächter, S., Thöni, C., 2005. Social Learning and Voluntary Cooperation Among Like-Minded People. *Journal of the European Economic Association* 3(2-3), 303-314.

Harris, J., 1999. *The Nurture Assumption: Why Children Turn Out the Way They Do*. New York, The Free Press.

Houser, D., Kurzban, R., 2005. An Experimental Investigation of Cooperative Types in Human Groups: A Complement to Evolutionary Theory and Simulations. *Proceedings of the National Academy of Sciences* 102(5), 1803-1807.

Isaac, M., Walker, J., 1988. Communication and Free-Riding Behavior: The Voluntary Contribution Mechanism. *Economic Inquiry* 26(4), 585-608.

Keser, C., van Winden, F., 2000. Conditional Cooperation and Voluntary Contributions to Public Goods. *Scandinavian Journal of Economics* 102(1), 23-39.

Kocher, M., 2004. Conditional Cooperation in the Public Goods Experiments and its Behavioral Foundations. Paper presented at the Annual Meetings of the Economic Science Association, University of Amsterdam, Amsterdam, June.

Ledyard, O., 1995. Public Goods: Some Experimental Results. Ch. 2 in J. Kagel and A. Roth (eds.) *Handbook of Experimental Economics*. Princeton, NJ, Princeton University Press.

Masclet, D., Noussair, C., Tucker, S., Villeval, M., 2003. Monetary and Nonmonetary Punishment in the Voluntary Contributions Mechanism. *American Economic Review* 93(1), 366-380.

Noussair, C., Tucker, S., 2005. Combining Monetary and Social Sanctions to Promote Cooperation. *Economic Inquiry* 43(3), 649 - 660.

Orbell, J., Dawes, R., van de Kragt, A., 1990. The Limits of Multilateral Promising. *Ethics* 100, 616-627.

Rabin, M., 1993. Incorporating Fairness into Game Theory and Economics. *American Economic Review* 83(5), 1281-1302.

Richerson, P., Boyd, R., 2005. *Not by Genes Alone: How Culture Transformed Human Evolution*. Chicago, University of Chicago Press.

Schotter, A., 2003. Decision Making with Naïve Advice. *American Economic Review* 93(2), 196-201.

Yamagishi, T., 1986. The Provision of Sanctioning as a Public Good. *Journal of Personality and Social Psychology* 51(1), 110-116.

Table 1: Design of the Experiment

Treatments	Families	Groups/ Generations	Subjects in each group	Total Number of Subjects	Total number of data points
Treatment 1: Communication	N/A	10	5	50	500
Treatment 2: Inter-generational games with evolving advice	1	4	5	20	200
	2	6	5[12]	29	290
	3	5	5	25	250
Treatment 3: Games with advice held constant	N/A	5	5	25	250
Treatment 4: No advice or communication	N/A	8 (5 plus the 3 progenitor groups from Families 1, 2 and 3 of Treatment 2)	5	40	400
				189	1890

[12] Generation 2 here had four players instead of the usual 5.

Table 2: Tobit Model of Contributions in the Communications Treatment

	Random Effects Tobit	Random Effects GLS	Tobit with Player Dummies
1/t	10.6698***	7.4903***	10.7855***
	(1.1929)	(0.8301)	(1.2882)
Communication Code = 1 (CC1)	4.1862***	3.3399***	9.5193***
	(1.0456)	(1.1217)	(2.0218)
Communication Code = 2 (CC2)	3.0519***	2.3066**	8.9416***
	(0.9736)	(1.0041)	(1.8887)
Lag Deviation from Average Contribution	0.0364	0.0292	-0.0644
	(0.0803)	(0.0583)	(0.0885)
Constant	-1.7065*	-0.2044	-6.2437***
	(0.9031)	(0.9216)	(1.7418)
Log Likelihood	-905.03		-872.28
Wald χ^2 (4)	95.45***	90.48***	
LR χ^2 (50)			347.41***
Test for equality of Treatment Effects ($\chi^2(1)$)#	2.26	1.69	0.18
Joint Significance of Player Dummies ($\chi^2(46)$)			353.61***
τ			3.1259***
			(0.1397)
σ_u	3.3983***	2.2637	
	(0.3908)		
σ_e	2.8945***	2.1521	
	(0.1336)		
ρ	0.5795	0.5253	
Number of Observations	450	450	450
Number Uncensored	292		292
Number Lower Censored	108		108
Number Upper Censored	50		50
Number of players	50	50	

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

ρ: Fraction of Variance due to u_i

#: F-test for Tobit regression with player fixed effects

Table 3: Contributions by Treatment

	Random Effects Tobit	Random Effects GLS	Tobit with Player Fixed Effects
1/t	10.5962***	7.0790***	10.6563***
	(0.6416)	(0.4318)	(0.7182)
No Advice Treatment	1.1180**	1.0133**	2.1338*
	(0.5355)	(0.4774)	(1.1798)
Inter-generational Advice Treatment	3.0045***	2.4413***	5.8191***
	(0.4689)	(0.4120)	(0.9297)
Constant Advice Treatment	4.4628***	3.6356***	9.6395***
	(0.5998)	(0.5512)	(1.1922)
Lag Deviation from Average Contribution	-0.0828*	-0.1047***	-0.2725***
	(0.0440)	(0.0312)	(0.0491)
Constant	1.3028***	2.1361***	2.7454***
	(0.3879)	(0.3314)	(0.8553)
Log Likelihood	-3301.41		-3213.20
Wald χ^2 (5)	345.36***	338.74***	
LR χ^2 (171)			1535.63
Test for equality of Treatment Effects ($\chi^2(2)$)[#]	30.41***	22.09***	20.35***
Joint Significance of Player Dummies ($\chi^2(167)$)			1440.69***
τ			3.2691*** (0.0772)
σ_u	4.1318*** (0.2414)	2.0757	
σ_e	2.8966*** (0.0689)	2.1374	
ρ	0.6705	0.4853	
Number of Observations	1701	1701	1701
Number Uncensored	1056		1056
Number Lower Censored	258		258
Number Upper Censored	387		387
Number of players	189	189	

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

ρ: Fraction of Variance due to u_i

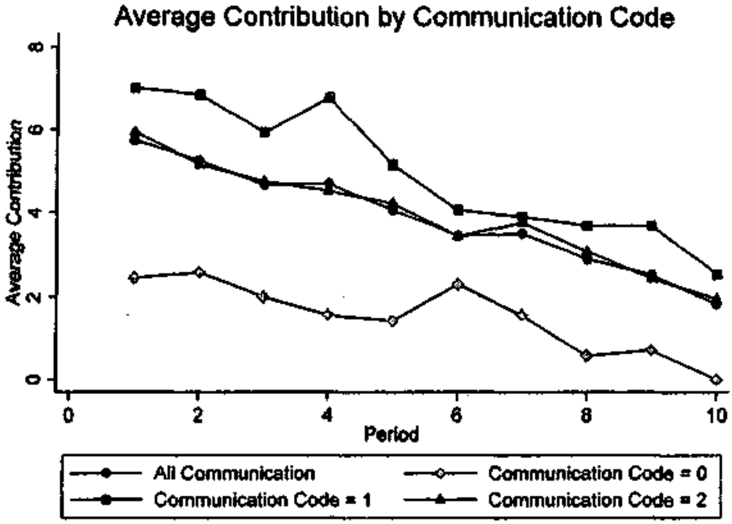
[#]: F-test for Tobit regression with player fixed effects

Table 4: Average Pre and Post Communication Beliefs and First Period Contributions in the Communication Treatment

	Communication Code = 0 (CC0)	Communication Code = 1 (CC1)	Communication Code = 2 (CC2)
Average Pre Communication Beliefs	2.8286 (0.3218)	5.0462 (2.0520)	5.5067 (2.5096)
Average Post Communication Beliefs	3.7143 (0.0092)	7.2462 (1.5694)	6.2867 (2.2323)
First Period Contribution	2.4286 (0.0702)	7.0000 (2.4497)	5.9333 (3.1510)
Correlation between Post Communication Beliefs and First Period Contribution	0.7787	0.5764	0.7571

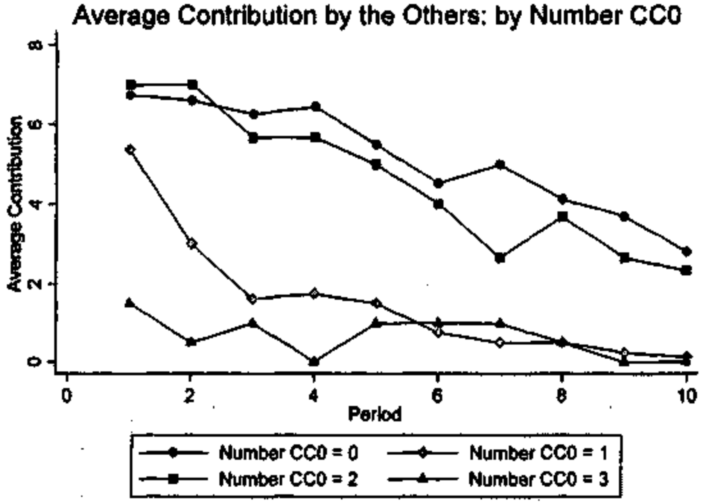
Standard deviations in parentheses

Figure 1:



Average Contribution by Communication Code
Average Contribution
0
2
4
6
8
0
2
4
6
8
10
Period
All Communication
Communication Code = 0
Communication Code = 1
Communication Code = 2

Figure 2:



Average Contribution by the Others: by Number CC0
Average Contribution
0
2
4
6
8
Period
10
Number CC0 = 0
Number CC0 = 1
Number CC0 = 2
Number CC0 = 3

Figure 3:

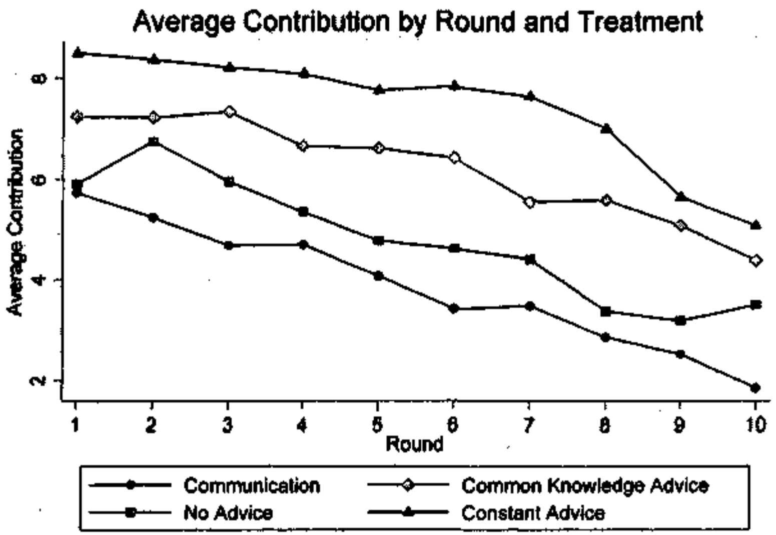
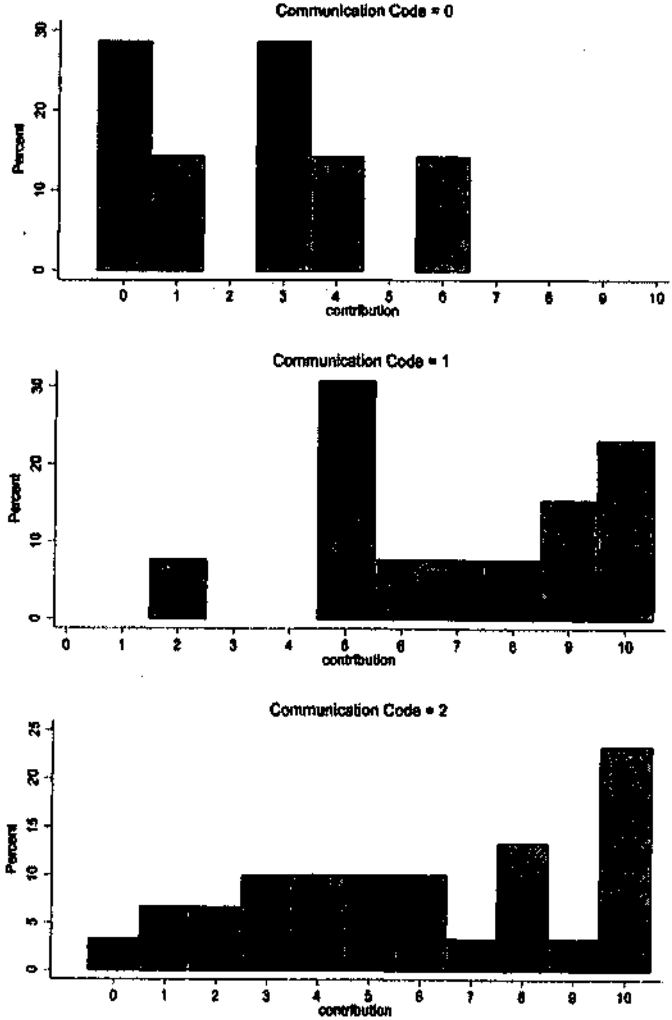

Average Contribution by Round and Treatment
Average Contribution
2
4
6
8
1
2
3
4
5
6
7
8
9
10
Round
Communication
Common Knowledge Advice
No Advice
Constant Advice

Figure 4:

Histogram of First Period Contributions: Simulated Data



Titles in the Department of Economics Discussion Papers 2005-2006

01-05
A Review of Selected Literature in the Economics of Division of Labor from 5th Century to WWII: Part I
Guang-Zhen Sun

02-05
Comparing Competitive Balance in Australian Sports Leagues, the AFL, NBL and NRL: Does the AFL's Team Salary Cap and Player Draft Measure Up?
Ross Booth

03-05
The Economic Development of the Australian Football League
Ross Booth

04-05
Using Turn Taking to Mitigate Conflict and Coordination Problems in the Repeated Battle of the Sexes Game
Sau-Him Paul Lau and Vai-Lam Mui

05-05
Domestic and Global Sourcing
Wenli Cheng and Dingsheng Zhang

06-05
Are Shocks to Energy Consumption Permanent or Temporary? Evidence from 182 Countries
Paresh Kumar Narayan and Russell Smyth

07-05
Elasticity of Risk Aversion and International Trade
Udo Broll, Jack E. Wahl and Wing-Keung Wong

08-05
Prospect and Markowitz Stochastic Dominance
Wing-Keung Wong and Raymond H. Chan

09-05
Robust Estimation of Multiple Regression Model with Non-normal Error: Symmetric Distribution
Wing-Keung Wong and Guorui Bian

10-05
Bivariate Causality between Exchange Rates and Stock Prices on Major Asian Countries
Hooi-Hooi Lean and Marwan Halim

11-05
Growth Accounting for the Chinese Provinces 1990-2000: Incorporating Human Capital Accumulation
Xiaolei Qian and Russell Smyth

12-05
Can Productivity Progress in China hurt the US? Professor Samuelson's Example Extended
Wenli Cheng and Dingsheng Zhang

13-05
Firm Compliance with Social Insurance Obligations where there is a Weak Surveillance and Enforcement Mechanism: Empirical Evidence from Shanghai
Pushkar Maitra, Ingrid Nielsen, Chris Nyland, Russell Smyth and Cherrie Zhu

14-05
Exploiting Gossen's second law: A simple proof of the Euler equation and the maximum principle
Guang-Zhen Sun

15-05
Why might a county want to develop its comparative disadvantage industries? A general equilibrium analysis
Wenli Cheng and Dingsheng Zhang

16-05
Revisiting Calendar Anomalies in Asian Stock Markets Using a Stochastic Dominance Approach
Lean Hooi Hooi, Wong Wing Keung and Russell Smyth

17-05
International Demonstration Effect and Domestic Division of Labour: A Simple Model
Po-Ting Liu and Guang-Zhen Sun

18-05
Is there a Natural Rate of Crime?
Paresh Kumar Narayan, Ingrid Nielsen and Russell Smyth

19-05
Welfare, Population Growth and Dynamic Inefficiency in an OLG Framework
Daniel Mulino

20-05
Would Outsourcing Increase or Decrease Wage Inequality? Two Models, Two Answers
Wenli Cheng and Dingsheng Zhang

21-05
Some Economic Effects of Changes to Gate-Sharing Arrangements in the Australian Football League
Ross Booth

22-05
Household Composition and Schooling of Rural South African Children: Sibling Synergy and Migrant Effects
Katy Cornwell, Brett Inder, Pushkar Maitra and Anu Rammohan