A Dynamic Analysis of the Evolution of Conventions in a Public Goods Experiment with Intergenerational Advice[*]

Ananish Chaudhuri
Department of Economics
University of Auckland

Pushkar Maitra
Department of Economics
Monash University

Sara Graziano
Charles River Associates

Corresponding Author: Ananish Chaudhuri
Department of Economics
University of Auckland
#124 Commerce A Building
Private Bag 92019
Auckland, New Zealand.
Phone: 64-9-373-7599 Extn. 88307
Fax: 64-9-373-7427
E-mail: a.chaudhuri@auckland.ac.nz

[*] We would like to thank Lata Gangadharan for suggestions and discussions on different aspects of the paper. We are grateful to Catherine Eckel for suggesting this line of research to us in the first place. The usual caveat applies. The experiments were conducted when Ananish Chaudhuri was a visiting Assistant Professor at Wellesley College. We are grateful to Wellesley College for providing the funds for running these experiments through two separate grants – one to Chaudhuri and one to Graziano.

A Dynamic Analysis of the Evolution of Conventions in a Public Goods Experiment with Intergenerational Advice

Abstract

In this paper we analyse contributions to a public good within an inter-generational framework where at the end of each session one generation of subjects leave advice for the succeeding generation via free form messages. Such advice can be private (advice left by one player in generation t is given only to her immediate successor in generation $t+1$) or public (advice left by players of generation t is made available to all members of generation $t+1$). We estimate a panel regression model that enables us to understand the dynamics of the process better and to highlight the learning that occurs over time. Our estimation results show that contributions in any period depend crucially on contributions in the previous period and on the group average in the previous period - more specifically whether a subject's own contribution in the previous period fell above or below the group average. We find that in the public advice treatment when a subject's contribution fell below the group average in the previous period there is a tendency on the part of that subject to increase contributions in the next period.

Keywords: Voluntary contributions mechanism, Public goods, Experiments, Panel Data, Generations

JEL Classification: C92, C33, C34

1. Introduction

There is now a voluminous experimental literature capturing the tension between contributing to a public good or free-riding on others' contribution using a voluntary contributions mechanism. See Isaac, Walker and Thomas (1984), Isaac, McCue and Plott (1985), Isaac and Walker (1988a, 1988b), Kim and Walker (1984) and Andreoni (1988, 1990, 1995a, 1995b) for studies describing this phenomenon. Ledyard (1995) provides a broad review of much of this literature as well as a description of what a typical public goods experiment looks like.[1]

Prior experimental work in the area has documented a number of empirical regularities. First, while groups of subjects do not manage to reach the socially optimal level of contributions, the strong free riding hypothesis of zero contribution is clearly refuted since subjects do contribute to the public good. Second, in a one-shot version of the public goods game, a group of subjects on average contribute between 40% and 60% of the optimal level. There are, however, wide variations in individual contributions with some players contributing 100% with others contributing 0%. But most prior studies find that on average most groups contribute somewhere in the 40%-60% range. Third, contributions decline steadily with repetition, i.e. if the players interact repeatedly over a number of rounds then contributions decline steadily over

[1] Ledyard (1995, p. 112) provides the following description of a generic public goods game. A group of four subjects are gathered in a room. They are each given a sum of money (say $5) and they are told that they can keep any or all of this amount. Or if they want they can contribute some or all of this amount to a public pool. However any amount contributed to a public pool is multiplied by a factor greater than 1 (say 2) by the experimenter. This multiplied amount is then distributed equally between the four group members. The socially optimal outcome in this game is for every player to contribute the entire amount to the public pool. Total contribution to the public pool is $20 which is doubled to $40 by the experimenter and redistributed back to the group members netting each person $10. Each member then gets a 100% return on her initial investment. However individual rationality suggests a different course of action. Think about an individual player trying to decide how much to contribute. If this individual contributes $1 and no one else contributes anything, then the $1 is doubled to $2. Distributed equally between the four players, gives each player $0.50. The player who contributed the $1 is worse off (incurs a 50% loss on the investment) while every other player is better off at the expense of the player who contributed. Thus if a player does not contribute, then she is no worse off if no one else contributes, but she is actually better off if some others contribute, while she herself does not.

time. In repeated plays of the game, contributions often start out at between 40% and 60% but then contributions decline steadily over time as more and more players choose to "free ride."

In this paper we approach the issue of free-riding in public goods games from a different perspective. We use an inter-generational framework where a group of subjects are recruited into the lab and play the public goods game for 10 periods (the exact experimental design and parameters are explained in Section 2). After her participation is over, each player is replaced by another player, her laboratory descendant, who then plays the game for another 10 periods as a member of a fresh group of subjects. The generations are therefore non-overlapping. Advice from a member of one generation to her successor can be passed along via free-form messages that generation t players leave for their generation $t+1$ successors. Finally, payoffs span generations in the sense that the payoff to a generation t player is equal to what she has earned during her lifetime plus 50% of what her laboratory descendant earns. This was done to provide an incentive to the subjects to pass on meaningful advice to their successors.[2]

We incorporate two separate mechanisms for passing advice from one generation to the next. The first is the "private advice" treatment where advice from one subject in generation t is given to her immediate successor in period $t+1$. The second is the "public advice" treatment where advice from one generation of players is made public to the next generation in the sense that all the advice left by the former group is made available to all the members of the latter group. Moreover, this advice is *read aloud by the experimenter for all members of the group to hear.* Behaviour by

[2] This inter-generational framework was pioneered in Schotter and Sopher (2003, 2001a, 2001b) where the authors study the battle of the sexes game, the trust game and the ultimatum game respectively. Chaudhuri, Schotter and Sopher (2001) use this framework to study a coordination problem with multiple Pareto-ranked equilibria. Schotter (2003) provides a broad overview of the findings and insights from this line of inquiry.

the subjects in the two advice treatments is compared to that in a no-advice treatment, which simply replicates a standard public goods experiment a number of times.

The public goods game is an excellent vehicle for understanding the inherent tension between cooperative and competitive behaviour in social dilemmas. A public goods game is really an n-person prisoner's dilemma where free-riding is the dominant strategy Nash equilibrium in a one-shot version of the game and also the subgame perfect outcome in finitely repeated versions of the game. It is also the evolutionarily stable strategy in such situations. See the discussion in Miller and Andreoni (1991) among others. As is well documented in the literature, however, while free-riding does occur in finitely repeated versions of such games still the game theoretic prediction of total free-riding is clearly refuted even after a number of rounds. Most laboratory studies of the public goods game (see the references above) find significantly high levels of cooperation. Andreoni (1995a) finds that there is significant kindness behind the decision to contribute. Andreoni (1995a, p. 899) goes on to say, "Kindness in experiments corresponds to a large body of evidence from privately provided public goods, like charitable giving, which indicates that people contribute more than the theory predicts."

We believe that in real life human beings approach such social dilemmas in a manner that is different from those captured in previous public good experiments. When confronted with such situations we have access to the wisdom of the past in the sense that those who have played before us (or at least immediately before us) are available to give us advice as to how to play. While the conventions passed from one generation of decision makers to the next may not be efficient solutions to the problem at hand, they at least avoid the need to have these problems solved repeatedly each time a new agent or set of agents arrive. Our conjecture is that playing a public goods game using such an inter-generational design, over time, will lead to the evolution of norms of cooperation with later generations not only achieving higher levels of contribution but also managing to mitigate problems of free-riding. Norms or conventions of behaviour – so-called “memes” (a term coined by Dawkins, 1976) – that arise during one generation may be passed on to the successors.[3] We believe that in this context, the concept of an evolutionarily stable strategy does not adequately capture the way in which social evolution, as opposed to biological evolution, might function. This is primarily because the concept of evolutionarily stable strategies is “unthinking” and does not allow for socialization and learning and therefore it may not be able to explain large patterns of human cooperation. To do so we need theories of cultural evolution or gene-culture co-evolution along the lines of Boyd and Richerson (1985) and Cavalli-Sforza and Feldman (1981). We view social conventions as artefacts that can be established in an early generation and passed on from generation to generation in the history of a human society. The inter-generational framework that we use is an attempt to capture the evolution of such social norms.

Chaudhuri and Graziano (2003), using the same data-set as in this study, find that when the advice left by one group of subjects is “public” (in the sense that it is made available to all the members of the succeeding group and also read aloud for all to hear), then this advice has a significant positive impact on contributions. Contributions in the public advice treatment average 81% (aggregated over all generations and all rounds), which far exceeds the 51.7% average contribution level

[3] Dawkins (1976, p. 189-192) comments “Cultural transmission is analogous to genetic transmission in that, although basically conservative, it can give rise to a form of evolution. …Examples of memes are tunes, ideas, catch-phrases, clothes, fashions, ways of making pots or building arches. Just as genes propagate themselves in the gene pool by leaping from body to body via sperms or eggs, so memes propagate themselves in the meme pool by leaping from brain to brain via a process which, in the broad sense, can be called imitation.”

attained in the "private" advice (that is, advice from one subject is given only to her immediate successor) treatment. Moreover public advice leads to increasing contributions over generations and mitigates problems of free-riding with later generations achieving contribution levels of 90% or more. One generation even manages to sustain contribution of 100% in 8 out of 10 rounds. No such trend was apparent when this advice was private.

What explains such high levels of contribution in the public advice treatment? Rabin (1998) has pointed out that if one assumes the existence of reciprocal altruism then it is possible to think about the public goods game as a coordination problem where high contributions are efficient equilibria and low contributions are inefficient equilibria. Michael Chwe in his book "Rational Ritual: Culture, Coordination and Common Knowledge" talks at length about the role of common knowledge in fostering coordination. Chwe (2001, p. 3), in talking about a variety of situations where coordinated action is called for, comments

> "Because each individual wants to participate only if others do, each person must also know that others received a message. For that matter, because each person knows that other people need to be confident that others will participate, each person must know that other people know that other people have received a message, and so forth. In other words, knowledge of the message is not enough; what is also required is knowledge of others' knowledge, knowledge of others' knowledge of others' knowledge and so on – that is "common knowledge. To understand how people solve coordination problems, we should thus look at social processes that generate common knowledge."

The argument is that when advice from a previous generation is made public and also read aloud, a common knowledge situation is created where subjects feel emboldened enough to choose higher contribution levels. See Chaudhuri and Graziano (2003) for a detailed exposition on the evolution of contributions, the nature of the advice left by subjects and the impact of advice on contributions over time as well as the implications of these results for current research in the area of voluntary contributions to public goods.[4]

In this paper we revisit the data from Chaudhuri and Graziano (2003) but this time the focus is on the evolution of contributions and the analysis of disaggregated (individual level) data using a dynamic panel regression model. In our experiments a subject is tracked over time as she interacts with her group members and decides on how much to contribute to the public good in each of multiple rounds. Therefore the data generated has a panel structure. Moreover given that contributions are bounded by zero from below and by the token endowment in each period from above (since a subject's contribution to the public good in any period cannot exceed her endowment for that period), the data collected in this setting is doubly censored. Taking the panel structure of the data into consideration allows us to better model the dynamics of the process. It enables us to better understand how contributions in each period respond to various factors such as contributions made by the individual in the previous period, average contributions made by the group in the previous period and whether the individual was above or below the average in the previous period. The other contribution of this paper is to present an innovative way to analyse the data in similar public goods game experiments. To the best our knowledge Ashley, Ball and Eckel (2003) is the only other study, which uses a panel data model to analyse voluntary contributions to a public good.[5] However unlike Ashley, Ball and Eckel (2003) who use a Tobit model with player fixed effects, we use a random effects Tobit model with double censoring, which in our view is the correct model to use in such situations

---

[4] The advice left by each subject is included in Chaudhuri and Graziano (2003). Interested readers can get a copy by writing to Chaudhuri at a.chaudhuri@auckland.ac.nz. The Chaudhuri and Graziano (2003) paper is also available from the website of the Social Science Research Network (SSRN), http://www.ssrn.com at the following address - http://papers.ssrn.com/sol3/papers.cfm?abstract_id=388241.

[5] They use data from the papers by Isaak and Walker (1988b) and Andreoni (1995b) for this purpose.

since a Tobit model with player fixed effects generates biased estimates. However we also compare the estimates from our random effects Tobit model with those produced by an unconditional Tobit model with player fixed effects (as in Ashley, Ball and Eckel (2003)) and a linear random effects model.

There are a number of insights that come out of our dynamic analysis of the data. First and foremost among these is the finding that contributions in any period depend crucially on the group average in the previous period or more specifically whether a subject's own contribution in the previous period fell above or below the group average. We find that those who were above the average in the previous period lowered their contribution in the next period. This is in keeping with prior findings that subjects reduce contribution to avoid being a "sucker". (Orbell and Dawes, 1981). But what is surprising is that in the public advice treatment, when a subject's contribution fell *below* the group average in the previous period there is a tendency on the part of that subject to *increase* contributions in the next period. This behaviour seems to be the primary driving force behind the high rates of contribution in the public advice treatment. While we do not know for sure the reasons behind this behaviour one could conjecture that a subject feels guilt for free-riding and increases contribution in the next period. No such trend is apparent in the private advice treatment. Thus it seems that the public advice treatment manages to generate and sustain a norm of high contribution to the public good and subjects who deviate from this norm in one period seem compelled to rectify that deviation in the succeeding period by increasing their contribution to the public good. In addition our results suggest that subjects of European ethnicity appear to have internalised this norm more than subjects of non-European heritage.

The rest of the paper is organised as follows. In Section 2, we explain the experimental design. This is the same design and data set that is described in Chaudhuri and Graziano (2003). In Section 3 we provide a broad overview of the data. In Section 4 we discuss our econometric methodology. In Section 5 we present our results and finally in Section 6 we discuss the implications of our results and make some concluding remarks.

2. Experimental Design

All the experiments for this project were carried out as non-computerized class room experiments using students at Wellesley College. Students were recruited via postings on an electronic bulletin board. A total of seventeen sessions were held with five students in each session. The composition of the group remains unchanged during the course of a session. Our set-up then corresponds to a "partners" protocol as in Andreoni (1988). Each session constituted one generation and consisted of 5 players playing the public goods game for 10 periods. This group of 5 is then replaced by 5 successors who take their place and play on. When generations change, after 10 periods of repetition, outgoing agents are allowed to pass on advice through free-form written messages to their successors.

In our "private advice" treatment, a message left by one player can be seen only by her immediate successor. In the "public advice" treatment, on the other hand, advice left by one group of players is "made public." All 5 pieces of advice left by this group is given to all 5 players in the next generation, and in addition this advice is read aloud by the experimenter before the start of the actual game.[6] Payoffs to an

---

[6] We would like to point out that all the experiments for this paper were carried out by the third co-author who was an undergraduate student at Wellesley College at that time (Fall 2002). It was she, a peer of the experimental subjects, who read out all the instructions and the advice. Thus any changes in behaviour between the private and public advice sessions cannot be attributed to the fact that a professor, who is in a position of authority, is making the announcements and coming from a authoritative figure such pronouncements have a large impact on behavior.

agent is the sum of the amounts that an agent earns during her lifetime plus 50% of what her successor earns in the next generation so there is partial inter-generational caring. This second payment is designed to act as an incentive for subjects to leave meaningful advice. The subjects are paid their actual earnings from a session immediately upon completion of the session. They are told that they will be contacted via e-mail/phone at a later date and given a second payment (based on the earnings of their successors). This second payment was handed out to the subjects after we had finished running all the sessions. Every player involved in the study received both the first and the second payments.

Also, before the start of the actual rounds and before the advice is made available to the players, we ask them about their expectations regarding the other players. Specifically, we ask them how much they expect the other group members to contribute on average in round 1. This gives us insight into the subject's beliefs about her fellow players. We do not make use of the data on expectations for the purposes of the present study.

Prior to the first period of any generation, subjects are presented with a set of written instructions that are read out loud to them after they are finished reading them privately. After questions are answered subjects are asked the question about their expectations regarding period 1 contributions by fellow group members. Then, depending on the treatment, they are allowed to read the advice offered by their immediate predecessor (in the private advice treatment) or by the immediately preceding group of players (in the public advice). In the latter case the advice is also read aloud by the experimenter.

The public goods game was played in the following way. Each group, consisting of 5 subjects, is told that each of them has 10 tokens for each one of 10 rounds.[7] At the beginning of each round (t), each participant i must make a decision on how many of the 10 tokens she wants to contribute to a public account $\left(0 \leq C_{it} \leq 10\right)$ and how many tokens she wants to keep for herself in her private account. Contributions are in whole numbers only and are made simultaneously by all the subjects in a group. After all participants had made their decisions, the total tokens contributed to the public account are added up and then doubled by the experimenter. This doubled amount is then divided equally among all five participants. The participant's personal earning for each round is the sum of the tokens she decided to keep in her private account and the tokens she received back from the public account. Total contributions to the public account and the number of tokens that each participant received from the public pool were announced at the end of each round. Following this the participants made their decisions for the succeeding round. Each successive round proceeded in the same manner. Each token was worth \$0.05. Balances are not carried over from one round to the next.

The payoff for each subject i in any period t then is

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

In our case the marginal per capita return from a contribution to the public good is 0.4 since all contributions are doubled and split 5-ways. The total payoff to a subject is the sum of the per-period payoffs over all 10 rounds $\left(\sum_{t=1}^{10} \Pi_{it}\right)$. It follows that full free riding $\left(C_{it} = 0\right)$ is a dominant strategy in the stage game. This is because

[7] We will use the terms round and period inter-changeably. They refer to the same thing.

$\frac{\partial \Pi_{it}}{\partial C_{it}} = -1 + 0.4 < 0; \forall t$. However, the aggregate payoff $\sum_{i=1}^{5} \Pi_{it}; \forall t$ is maximized if each group member fully cooperates $\left(C_{it} = 10\right)$, because $\frac{\partial \sum_{i=1}^{5} \Pi_{it}}{\partial C_{it}} = -1 + 2 > 0; \forall t$

After the last period, subjects are asked to write advice to their successors and leave. The subjects were also asked separately to indicate a period 1 contribution to their successors by writing a specific number. We provided the next group of subjects with the free-from messages but not the actual number (though a vast majority of subjects included this number in their free-form advice as well). When they wrote advice the subjects knew whether it was to be made public to all five subjects in the next generation or simply be read privately by their successor. They were paid the sum of their payoffs in the 10 period game they played, plus 50% of what their successors earned as a second payment at a later date.

We performed a set of four different experiments that varied according to the information available to subjects. In Experiment 1, the Replicator (No-Advice) Experiment, we simply replicate the standard public goods experiments, five times with 5 subjects in each group, without either generations or advice. In short, we simply ran the public goods game five times with five subjects for 10 periods. This group serves as the control group against which we intend to compare the behaviour of our two experimental groups – one that gets private advice from the immediate predecessor and the other that gets public advice from the immediately preceding group.

In running our inter-generational experiments we started (in Experiment 2) by running a "Progenitor" experiment in which 5 subjects played the public goods game for the first time and hence with no advice. This generation was the progenitor of all the generations in the two advice treatments – private and public – that followed in the sense that the first generations in each treatment used the advice of this progenitor generation.[8] In Experiment 3 we had five generations of subjects play the public goods game with private advice, where each subject could receive advice from her immediate predecessor. So while each agent knew that the others were receiving advice, they did not know the content of any advice other than their own. Finally, Experiment 4 was a public advice experiment, which consisted of six generations. The first generation here received a sheet with all the advice from the progenitor experiment. However, this advice was also read aloud for all the subjects to hear, so the content of the advice on these sheets was common knowledge. Our experimental design is explained in Table 1 and Table 2. Each session lasted about 40 minutes (the advice sessions took a little longer than the ones without advice) and the average payoff to the subjects was $12.30.

3. Overview of the evolution of contributions

We are primarily interested in understanding what happens to contributions over time. Before proceeding to our econometric analysis it is worth taking a broad overview of the data. In Table 3, we present the levels of contribution over 10 rounds aggregated over all generations (or groups) in the three treatments. As can be seen from the table, contributions in the public advice treatment are much higher than that in either the private advice or no advice treatment. Average contribution in the public advice treatment starts at 9.53 tokens out of 10 (95.3%) in round 1, which is close to the efficient contribution level of 10 tokens. While average contributions do decline over the 10 rounds still by the 10th round contribution in the public advice treatment is at a robust 61%. Contributions in the private advice treatment start at 74.4% in round

[8] Since the participants in the "progenitor" sessions did not receive any advice, for the econometric analysis that follows, the progenitor session is regarded as a no advice session, effectively giving us 6 no advice groups.

1 and fall to 27.6% in round 10. In our replicator (no advice) treatment, contributions start at 48.4% and drop to 38% by round 10. Aggregating over generations and advice we find that the public advice groups manage to achieve 81% contribution on average. This is significantly higher than the average of 51.7% achieved by the private advice groups (z = 5.416, p-value < 0.01 using the non-parametric Wilcoxon test). Contributions in the public advice treatment are also significantly higher than those in the no-advice treatment, which averaged 44.88% (z = 5.937, p-value < 0.01). Figure 1 presents the evolution of average contributions by period and treatment. Contributions decline over time in all three treatments. In every period, average contributions in the public advice treatment sessions exceed the average contributions in the private advice and no advice treatment sessions. When comparing the private and no advice sessions we see that the average contributions in the private advice sessions are higher to begin with (periods 1 – 5) but beyond period 5, average contributions in the private advice sessions are actually lower.

Remember that players can contribute any amount between 0 and 10. The maximum contribution is 10 (in this case we will say that contribution is upper censored) and the minimum contribution is 0 (in this case we will say that the contribution is lower censored). In Figure 2, we present the histogram of the contributions by treatment. It follows that a large majority of the players contribute their entire endowment of 10 tokens in the public advice sessions (see Panel C). Figure 3 presents the proportion of individuals who contributed the maximum in each period by treatment. It is clear that the percentage of players who contribute the maximum in each period is the highest in the public advice sessions. However this proportion does decrease over time – falling from 73% in Period 1 to 47% in Period 10. With the exception of Period 1, the proportion that contributes the maximum is higher in the no advice session relative to the private advice session in each period. In Figure 4 we show the proportion of individuals who contribute the minimum in each period, by treatment. Once again we note that the proportion of players who contributed the minimum is the lowest in public advice treatment. The proportion contributing the minimum in the private advice sessions is low in the beginning but beyond period 5 this proportion rises significantly and in the last two periods the proportion contributing the minimum in the private advice sessions is significantly higher than the no advice or public advice treatments.

4. Estimation Methodology

As noted earlier our primary aim is to understand what is happening to contributions over time and the factors that impact contributions. To that end we define C_{it} as the contribution of player i in period t. We have 85 players, each choosing a contribution level for 10 periods, thereby giving us 850 observations. This observed contribution C_{it} equals the desired contribution, C_{it}^{*} (which is a latent variable) if and only if $0 \le C_{it}^{*} \le 10$ and therefore we have:

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

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 $\left(\nu_i\right)$ are IID $N\left(0,\sigma_\nu^2\right)$ and ε_{it} are $N\left(0,\sigma_\varepsilon^2\right)$ independently of ν_i. Here X_{it} denotes a vector of time invariant effects (like treatment effects), time varying variables (like an individual's contribution in the previous period or the deviation of an individual's contribution from the average group contribution in the previous period), and an overall time effect, which is common to all players. We estimate this model as a random effects Tobit.[9] For the sake of comparison we also compute the (linear) random effects regression where, by definition, we do not account for the upper and lower censoring of the dependent variable (contribution by player *i* at time *t*). However note that the linear random effects estimates are inconsistent. Under the assumption of normal distribution for the random effects (ν_i), we can write the joint (unconditional of ν_i) density of the observed data from the i^{th} panel as

$$f\left(C_{i1},\ldots,C_{iT} \mid X_{i1},\ldots,X_{iT}\right)=\int_{-\infty}^{\infty}\frac{e^{-\nu_i^2/2\sigma_\nu^2}}{\sqrt{2\pi}\sigma_\nu}\left\{\prod_{t=1}^{T}F\left(C_{it},X_{it}\beta+\nu_i\right)\right\}d\nu_i$$

where

$$F\left(C_{it},\Delta_{it}\right)=\begin{cases}\left(\sqrt{2\pi}\sigma_\varepsilon\right)^{-1}e^{-\left(C_{it}-\Delta_{it}\right)^2/2\sigma_\varepsilon^2}, \text{ if } C_{it}\in C\\ \Phi\left(\dfrac{C_{it}-\Delta_{it}}{\sigma_\varepsilon}\right), \text{ if } C_{it}\in L\\ 1-\Phi\left(\dfrac{C_{it}-\Delta_{it}}{\sigma_\varepsilon}\right), \text{ if } C_{it}\in R\end{cases}$$

where C is the set of non-censored observations, L is the set of left (lower) censored observations, R is the set of right (upper) censored observations and $\Phi(\ )$ is the cumulative normal distribution. The log Likelihood function can be written as:

$$L=\sum_{i=1}^{n}\log\left\{f\left(C_{i1},\ldots,C_{iT} \mid X_{i1},\ldots,X_{iT}\right)\right\}$$

[9] We cannot compute the corresponding fixed effects Tobit model as there does not exist a sufficient statistic allowing the fixed effects to be conditioned out of the likelihood function. We do examine the robustness of the results by computing the unconditional Tobit estimates with player fixed effects. However these unconditional fixed effects Tobit estimates are biased.

5. Results

Table 4: presents the Random Effects Tobit Regression for the variable of interest: contribution of player i in period t $\left(C_{it}\right)$. We estimate and present four different specifications. In specification 1 - the most parsimonious specification – the set of explanatory variables includes only two treatment dummies (Private Advice and Public Advice). Here the no advice treatment is the reference category. In specification 2 we add the variable time. However we incorporate the effect of time as $\left(1/t\right)$. This has two advantages. First it allows us to capture the non-linearity in the effect of time on contributions. Second it allows us to distinguish between the effects of early and later periods on contributions. Specification 3 adds contribution made by each subject in the previous period $\left(C_{i,t-1}\right)$. Finally specification 4 – the complete specification – also includes the deviation of an individual's contribution from the group average in the previous period $\left(\Lambda_{i,t-1}\right)$. $\Lambda_{i,t-1}$ is defined as $\Lambda_{i,t-1} = \frac{1}{5}\sum_{g=1}^{5} C_{g,t-1} - C_{i,t-1}$. A positive $\Lambda_{i,t-1}$ implies that individual i had contributed less than the group average in the previous period and a negative $\Lambda_{i,t-1}$ implies that individual i had contributed more than the group average in the previous period. We should point out that in specifications 3 and 4 we lose one period in each session for each player, since we have introduced lagged variables. So the number of observations in these two specifications comes down to 765.

A look at the Random Effects Tobit results presented in Table 4: reveals the following results. First, relative to the no advice sessions (the reference category) contributions are significantly higher in the public advice sessions. In every specification, the coefficient estimate of the public advice dummy is positive and statistically significant. However the coefficient estimate of the private advice dummy is positive in specifications 1 and 2 and negative in specifications 3 and 4 though it is statistically significant only for specification 3. We conduct a test for the equality of the two treatment dummies. The null hypothesis that the treatment dummies have similar effects on contributions is rejected for every specification. We conclude that contributions are significantly higher in the public advice treatment as compared to the no advice treatment. However there seem to be no significant differences between contributions in the private advice and no advice treatments.

Second, an increase in previous period's contribution increased current period's contribution – the coefficient estimate of $C_{i,t-1}$ is positive and statistically significant. Note that the magnitude of the coefficient estimate of $C_{i,t-1}$ is more than double in specification 4, where we control for deviations from the group average, as compared to specification 3.

Third, the coefficient estimate of $\Lambda_{i,t-1}$ is positive and statistically significant. This essentially implies that an increase in $\Lambda_{i,t-1}$ is associated with an increase in contributions. Another way of looking at it is that a positive $\Lambda_{i,t-1}$ is associated with a higher C_{it} and a negative $\Lambda_{i,t-1}$ is associated with a lower C_{it}. A positive $\Lambda_{i,t-1}$ implies that the individual's contribution in the previous period is actually less than the group average for the previous period. Therefore if an individual contributes less than the group average in one period, she responds by increasing contributions in the next period and the further away the individual is from the group average in period $t-1$, the greater is her contribution in period *t*. This, we believe, is the most important insight of this study. Below we will explore this phenomenon in greater detail. We will show that this tendency is the strongest in the public advice treatment and that it provides a strong vindication of our claim that the public advice treatment leads to the creation of virtuous norms that manage to sustain high levels of cooperation. Those who were above the group average in the previous period however respond by reducing their contribution in the next period.

Finally, and no surprises here, contributions fall over time – as t increases, $1/t$ decreases and this is associated with a reduction in contributions and hence the positive coefficient of $1/t$. This holds for every specification.

We next examine the sensitivity of the results to alternative estimation techniques. We compare the random effects Tobit estimates to estimates derived using two alternative methods: the fixed effects unconditional Tobit and the (linear) random effects. In Table 5 we present the coefficient estimates and the corresponding standard errors from the three models using the complete specification which includes the two treatment dummies for private and public advice, the non-linear specification for time $\left(1/t\right)$, lagged contributions $\left(C_{i,t-1}\right)$ and lagged deviation of previous period's contribution by each subject from the group average $\left(\Lambda_{i,t-1}\right)$. We cannot compute the corresponding fixed effects Tobit model since there is no sufficient statistic allowing the fixed effects to be conditioned out of the likelihood function. We do examine the robustness of the results by computing the unconditional Tobit estimates with player fixed effects.[10] However these unconditional fixed effects Tobit estimates are biased. The player fixed effects turn out to be jointly statistically significant (p-value = 0.00). The (linear) random effects estimates, on the other hand, are inconsistent. A comparison of the three sets of estimates presented in Table 5 reveals the following.

[10] As far as player fixed effects are concerned, we are only able to include in the set of explanatory variables dummies for those players who are not censored in every period.

First, contributions decline over time. It is worth noting however that what $1/t$ captures is essentially the effect of initial periods. The magnitude of the coefficient estimates implies that the contributions in the initial period is the highest in the fixed effect unconditional Tobit and the lowest in the linear random effects regression. Second, there is significant dynamics in contributions – contribution in period t is significantly affected by contribution in period $t-1$ and the coefficient estimate of $C_{i,t-1}$ is always positive. One important point to note is that while the fixed effects unconditional Tobit regression under estimates this effect, the linear random effects regression over estimates this effect relative to the random effects Tobit regression. Third, once again the group effects are statistically significant. The coefficient estimates of $\Lambda_{i,t-1}$ show that irrespective of the estimation methodology used, if an individual contributes less than the group average in period $t-1$, she responds by contributing more in period t and the further away she is from the group average in period $t-1$ the higher is the contribution in period t. However it is worth noting that both the fixed effects conditional Tobit and the linear random effects regressions both under estimate this effect relative to the random effects Tobit regression. Finally, the signs and statistical significance of the two treatment dummies are interesting. Note that in the two Tobit regressions the public advice dummy is positive and statistically significant while in the linear random effects regression the public advice dummy is positive but not statistically significant. So relative to the no advice sessions, contributions are generally higher in the public advice sessions. On the other hand in all three regressions the private advice dummy is negative. It is statistically significant in the fixed effects unconditional Tobit and weakly significant in the linear random effects regression. The actual magnitude of the coefficient is higher in the linear random effects regression compared to the random effects Tobit regression. The coefficient estimates reveal that the fixed effects Tobit regression over estimates contribution in the public advice treatment and under estimates contribution in the private advice treatment (relative to the no advice treatment). Using a standard $\chi^2(1)$ test the null hypothesis of equality of treatment effect is always rejected but it is worth noting that the null hypothesis is only weakly rejected (at 10%) in the linear random effects regression.

Thus far we have captured the difference between the treatments using treatment dummies. In the first three columns of Table 6 we present the random effects Tobit regression of contribution after stratifying the sample by session type – private advice, public advice and no advice. It is worth pointing out that in the public advice session, the proportion of upper censored observations is significantly higher (53.70%) compared to either the private advice (4.89%) and the no advice (15.93%) sessions.

The coefficient estimate of $1/t$ is positive and statistically significant in every treatment showing that contributions decline over time. However ceteris paribus in every period, contributions are the highest in the public advice sessions and the lowest in the private advice sessions with contributions in the no advice sessions lying in between.

In every treatment contributions in the previous period $\left(C_{i,t-1}\right)$ have a positive and statistically significant effect on contributions in the current period. However the cross period effects are the strongest in the no advice sessions and the weakest in the public advice sessions. We believe this is because in the public advice treatment group contributions matter more than one's individual contribution while in the other two treatments group contributions have much less impact on individual contributions.

The coefficient estimates of $\Lambda_{i,t-1}$ are always positive and statistically significant. If an individual contributes less than the group average in period $t-1$, she responds by increasing his contributions in period t. But this group effect is the strongest in the public advice sessions and the lowest in the no advice sessions with the private advice sessions lying in between.

While stratifying the sample by treatment allows us to compare the three cases directly, it has the disadvantage of not enabling us to use the full set of observations. An alternative to estimation on sub-samples is to use treatment dummies and interact the dummies with all explanatory variables. This is what we do next. We restrict ourselves to the private and public advice sessions (i.e., ignore the no advice sessions). Apart from $1/t, C_{i,t-1}, \Lambda_{i,t-1}$, we include in the set of explanatory variables a public advice treatment dummy and also interact this dummy with each of the explanatory variables $\left(1/t, C_{i,t-1}, \Lambda_{i,t-1}\right)$. In this case the non-interacted coefficients give the effect for the private advice treatment and the interacted coefficients give the public advice – private advice difference. The coefficient estimates are presented in Column 4 of Table 6. Note that the only interaction effect that is statistically significant is $\Lambda_{i,t-1} \times \text{Public Advice}$. In addition the non-interacted coefficient $\Lambda_{i,t-1}$ is also positive and marginally significant. Essentially there is a group effect and the coefficient estimates show that this group effect is significantly stronger for the public advice sessions compared to the private advice sessions.

What exactly is the source of the differential group effects in the public and private advice treatments? To understand this we sub-divide the subjects in these two treatments into two groups – those who contributed equal to or more than the average in a given period i.e. subjects for whom $\Lambda_{i,t-1} \leq 0$ and those who contributed less than the group average for a given period, i.e. subjects for whom $\Lambda_{i,t-1} > 0$. Once we know whether a subject was above or below the average in period *t-1*, we then look at how she responded in period *t*, i.e. what happened to her contribution in the succeeding period. In Table 7 we present the average change in contribution from period $t-1$ to period t depending on whether $\Lambda_{i,t-1} \leq 0$ or $\Lambda_{i,t-1} > 0$, for the public and private advice treatments. $\Delta C_{it} > 0$ implies that an individual increases her contribution while $\Delta C_{it} < 0$ implies that an individual reduces her contribution between periods *t-1* and *t*. We find that in the private advice treatment $\Delta C_{it} < 0$ always, i.e., contributions in period *t* decline regardless of whether a subject contributed equal to or more than the average in period *t-1* (i.e. $\Lambda_{i,t-1} \leq 0$) or contributed less than the group average is period *t-1* (i.e., $\Lambda_{i,t-1} > 0$). However the responses of those above and below the average are different. Those at or above the average in period *t-1* (i.e. $\Lambda_{i,t-1} \leq 0$) reduced their contribution by approximately 9.4 percentage points in period *t* while those below the average (i.e., $\Lambda_{i,t-1} > 0$) reduced their contributions by roughly 4 percentage points. In the public advice treatment, on the other hand when the subject contributed less than the group average i.e. $\Lambda_{i,t-1} > 0$, that subject actually *increased* her contribution in the succeeding period by roughly 9.75 percentage points (ΔC_{it} = 0.975 > 0). Those at or above the average (i.e., $\Lambda_{i,t-1} > 0$) however reduce their contribution (by 9.5 percentage points) in the succeeding period even in the public advice treatment. However the magnitudes of these two responses are close to one another with the reduction in contributions by one group being virtually offset by the increase in contributions by the other group. The very different response of those who were below the average in the two treatments gives us this strong asymmetric group effect.

At the end of each session the participants were asked to fill up a small questionnaire. In particular they were asked what their ethnicity was. We created the variable WHITE (= 1 if the participant was White, 0 if Asian or Hispanic). We used this variable to examine whether there are any ethnic differences in contributions. A positive and statistically significant coefficient associated with this ethnicity variable would imply that White participants contribute more, relative to those who identify themselves as Asian or Hispanic. However in this case there was some missing data and we have information for 82 of the 85 players. We conduct four sets of estimations: all sessions, with treatment dummies (private and public advice session dummies), and separately for the private advice, public advice and no advice sessions. The coefficient estimates are presented in Table 8. Overall ethnicity does not matter – the coefficient of WHITE is not statistically significant. See Column 1 of Table 8. However when we examine each session separately, we find that the ethnicity dummy is positive and statistically significant in the public advice sessions only. What the results imply is that in public advice sessions, participants who identify themselves as White contribute more than those who identify themselves as Asian or Hispanic but no such difference exists in the private advice and no advice sessions.

Finally to examine the robustness of our results we run regressions where the dependent variable is the change in contribution from period $t-1$ to period t $\left(\Delta C_{it}\right)$. The estimating equation now is

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

Note that there is no censoring in the dependent variable any more. We estimate this equation as a linear random effects model. The explanatory variables are the same (private and public advice treatment dummies, $1/t$, $C_{i,t-1}$ and $\Lambda_{i,t-1}$. The coefficient estimates are presented in Table 9 (Column (1)). The private advice session dummy is negative and weakly statistically significant but the public advice dummy is positive but not statistically significant. The coefficient estimate of $C_{i,t-1}$ is negative and statistically significant – the higher is the contribution in period $t-1$ the lower is the change in contribution from period $t-1$ to t. Finally the group effect $\left(\Lambda_{i,t-1}\right)$ continues to be positive and statistically significant – if an individual contributes less than the group average in period $t-1$ $\left(\Lambda_{i,t-1} > 0\right)$ the change in contribution is positive and the further away the individual is from the group average in period $t-1$, the higher is the change in contribution from period $t-1$ to t. We re-estimated this specification separately for each treatment. The results presented in Table 9 (columns 2, 3 and 4) show that, while the coefficient of $\Lambda_{i,t-1}$ is always positive it is not statistically significant for the private advice sessions. The coefficient estimates also imply that the response (in terms of change in contribution from period $t-1$ to period t) to the deviation from the group average is the highest in the public advice sessions. This once again strengthens our case about the positive group effects in the public advice treatment.

6. Discussion of our remarks and some concluding comments

There are a number of insights that come out of our dynamic analysis of the data. First and foremost among these is the finding that contributions in any period depend crucially on the group average in the previous period or more specifically whether a subject's own contribution in the previous period fell above or below the group average. We find that when a subject's contribution fell below the group average in the previous period there is a tendency on the part of that subject to increase contributions in the next period possibly due to guilt for free-riding. This in turn leads to increased contribution in the next period. However as we show in Table 6, this effect is really strong in the public advice treatment. Thus it is clear that it is only the public advice treatment, which manages to generate and sustain a norm of high contribution to the public goods, and subjects who deviate from this norm in one period seem compelled to rectify that deviation in the succeeding period by increasing their contribution to the public good. In addition, the coefficient estimates presented in Table 8 suggest that subjects of European ethnicity seem to have internalized this norm more than subjects of non-European heritage.

Based on our results we would like to draw a connection between our findings and two other broad research themes in the literature – first, the idea of altruistic punishment in humans and second, the general issue of sustaining cooperation is social dilemmas. There is now a voluminous body of evidence showing that a large number of subjects in experimental games are conditional co-operators, i.e. they will cooperate as long as they expect other subjects to cooperate.[11] See for instance the papers by Fishbacher, Gächter and Fehr (2001) and Gächter, Hermann and Thöni (2003), Keser and van Winden (2000) for public goods games and Chaudhuri and

[11] Ernst Fehr and his associates have done extensive research along these lines. See for instance Fehr and Gächter (2000, 2002), Fehr and Fischbacher (2002) and Fisbacher, Gächter and Fehr (2001).

Gangadharan (2003) for the trust game. This is analogous to Axelrod's (1984) concept of Tit-for-Tat cooperation in games. Moreover Fehr and Gächter (2000, 2002), Gächter, Hermann and Thöni (2003) and Masclet et al. (2003) show that conditional cooperators are willing to punish non-cooperators for their non-cooperation even if such punishment has pecuniary costs for the cooperators. 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. In fact Fehr and Gächter (2002) and Bowles and Gintis (2002) suggest that such "altruistic punishment" by *homo reciprocans* – humans who are willing to punish free-riders even when such punishment is costly to the punishers – may be the primary driving force behind sustaining cooperation in a variety of social dilemmas. Our results suggest that given the presence of a large number of conditional cooperators, communities may be able to create inherent social norms that lead to efficient levels of contribution to the public good even in the absence of punishment mechanisms. All that is needed is the creation of conducive conditions that lead to the generation of optimistic beliefs about other subjects. Once subjects have adequately optimistic beliefs about one another then a group of conditional cooperators may be able to sustain cooperation even without the explicit threat of punishments. Punishments (or the threat thereof) will keep contributions high but we have shown that while punishments may be sufficient to sustain cooperation they may not be necessary.[12] It is possible to argue that the groups in our experiment who receive public advice are more "socially connected" – in the sense of Putnam (2000) – than the groups who receive private or no advice. What we find is that such socially

[12] See Fehr and Gächter (2000) for examples of social situations where cooperators can actually punish non-cooperators for their lack of cooperation.

connected groups may be able to generate social norms that serve the same purpose of sustaining cooperation.

Our results have implications for the general evolution of cooperation in social dilemmas as well. Axelrod and Hamilton (1981) argue that in the presence of Tit-for-Tat (TFT) players, as in Axelrod (1984), it is possible to think of a public goods game as a coordination problem and the problem of sustaining cooperation in this context as really a problem of equilibrium selection. In the presence of TFT players a prisoner's dilemma game (and the public goods game is in essence an n-person prisoner's dilemma) can be converted into a coordination problem with two Pareto-ranked equilibria – one payoff dominant and the other risk-dominant. Axelrod and Hamilton further show that the evolutionary dynamics in such games are able to sustain stable polymorphic equilibria where the population converges to one of the two equilibria. If the population consists of a majority of defectors then the evolutionary dynamics lead to a convergence to the risk dominant outcome where everyone defects (or free-rides in the case of public goods). However they demonstrate that once the number of TFT players exceeds a certain threshold the population is able to sustain the cooperative outcome as an evolutionarily stable outcome. So in the presence of a large number of conditional cooperators it is possible to sustain cooperative behaviour. However how could TFT players succeed in sustaining cooperation if the majority of players are using a strategy of defection? One answer, Axelrod and Hamilton suggest, is clustering, i.e. TFT players need to arrive in clusters. We believe that our results suggest another mechanism. We posit that at least in human interactions players are not constrained to play genetically hard-wired strategies such as cooperate or defect. Players choose a strategy based on their beliefs about the distribution of types within the population. If players place a large enough probability on the presence of TFT players then they might be predisposed to cooperate to begin with. The answer we believe lies with the creation of strong enough beliefs about the presence of other conditional cooperators. Our public advice treatment manages to elevate beliefs above the minimum threshold, which allows the sustenance of cooperative behaviour.

Our results then have interpretable implications for all areas of research which focus on the resolution of social dilemmas and the creation of cooperative norms. These would include a gamut of subjects such as charitable giving, environmental protection, dispute resolution and others.

References:

Andreoni, J. (1995a), "Cooperation in Public-Goods Experiments: Kindness or Confusion?, *American Economic Review*, 85 (4), p. 891-904.

Andreoni, J. (1995b), "Warm-Glow versus Cold-Prickle: The Effects of Positive and Negative Framing on Cooperation in Experiments, *Quarterly Journal of Economics*, 110 (1), p. 1-21

Andreoni, J., (1990), "Impure Altruism and Donations to Public Goods: A Theory of Warm-Glow Giving?, Economic Journal,100 (401),p. 464-477.

Andreoni, J., (1988), "Why Free Ride? Strategies and Learning in Public Goods Experiments", *Journal of Public Economics*, 37, p. 291-304.

Ashley, R., S. Ball and C. Eckel (2003), "Analysis of Public Goods Experiments using Dynamic Panel Regression Models:, Working Paper, Department of Economics, Virginia Tech, Blacksburg, VA.

Axelrod, R. and W. D. Hamilton (1981), "The evolution of cooperation, *Science*, 211, p. 1390-1398.

Axelrod, R. (1984), "The Evolution of Cooperation", Basic Books.

Bowles, S. and H. Gintis (2002), "Homo Reciprocans", *Nature*, 415, January 10, 2002, p. 125-128.

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

Cavalli-Sforza, L.L. and M.W. Feldman (1981), Cultural Transmission and Evolution: A Quantitative Approach, Princeton, Princeton University Press

Chaudhuri, A. and L. Gangadharan (2003), "Sending Money in the Trust Game: Trust or Other-Regarding Preferences", Working Paper, Department of Economics, University of Auckland, Auckland, New Zealand, August 2003.

Chaudhuri, A. and S. Graziano (2003), "The Evolution of Conventions in an Experimental Public Goods Game with Private and Public Knowledge of Advice", Working Paper, Department of Economics, University of Auckland, Auckland, New Zealand, March 2003.

Chaudhuri, A., A. Schotter and B. Sopher (2001), "Talking Ourselves to Efficiency: Coordination Conventions in an Inter-Generational Minimum Game with Private, Almost Common and Common Knowledge of Advice, Economics Research Report #2001-11, C.V. Starr Center for Applied Economics, New York University, December 2001.

Chwe, M.S. (2001), Rational Ritual: Culture, Coordination and Common Knowledge, Princeton University Press, Princeton, NJ, 2001.

Dawkins, R. (1976), The Selfish Gene, First Edition, Oxford University Press.

Fehr, E. and S. Gächter (2002), "Altruistic Punishment in Humans" *Nature*, 415, January 10, 2002, p. 137-140.

Fehr, E. and S. Gächter (2000), "Cooperation and Punishment in Public Goods Experiments", *American Economic Review*, 90(4) p. 980-94.

Fehr, E. and U. Fischbacher (2002), "Why Social Preferences Matter--The Impact of Non-selfish Motives on Competition, Cooperation and Incentives", *Economic Journal*, 112 (478), p. C1-33

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

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

Issac, R. M. and J. Walker, (1988a), "Communication and Free Riding Behavior: The Voluntary Contributions Mechanism", *Economic Inquiry*, 26(2), p. 585-608.

Issac, R. M. and J. Walker, (1988b), "Group Size Effects in Public Goods Provision: The Voluntary Contributions Mechanism", *Quarterly Journal of Economics*, 103, p. 179-99.

Isaac, R. M., K. F. McCue, and C. Plott (1985), "Public Goods Provision in an Experimental Environment," *Journal of Public Economics*, 26, p. 51-74.

Isaac, R. M., J. M. Walker, and S.H. Thomas (1984), "Divergent Evidence on Free Riding: An Experimental Examination of Possible Explanations," *Public Choice*, 43, p. 113-149.

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

Kim, O. and J. Walker, (1984), "The Free Rider Problem: Experimental Evidence", *Public Choice*, 43, p. 2- 34.

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

Marwell, G. and R. Ames (1981), "Economists Free Ride, Does Anyone Else?" *Journal of Public Economics*, 15, p. 295-310.

Masclet, David, et al.(2003), "Monetary and Nonmonetary Punishment in the Voluntary Contributions Mechanism**,** *American Economic Review*, 93 (1), p.

366-380

Miller, J.H. and J. Andreoni, (1991), "Can Evolutionary Dynamics Explain Free Riding in Experiments?, *Economics Letters,* 36(1),p. 9-15

Orbell, J. and R. Dawes, (1981), "Social Dilemmas", in *Progress in Applied Social Psychology*, Vol. 1., G. Stephenson and J.H. Davis (eds.), Wiley Publishers, Chichester, England.

Putnam, R.D. (2000), "Bowling Alone: The Collapse and Revival of American Community", Touchstone, New York, NY, 2000.

Rabin, M. (1998), "Psychology and Economics", *Journal of Economic Literature*, XXXVI, p. 11 – 46.

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

Schotter, A. and B. Sopher (2003), "Social Learning and Coordination Conventions in Intergenerational Games: An Experimental Study", *Journal of Political Economy,* 111 (3), p. 498-529.

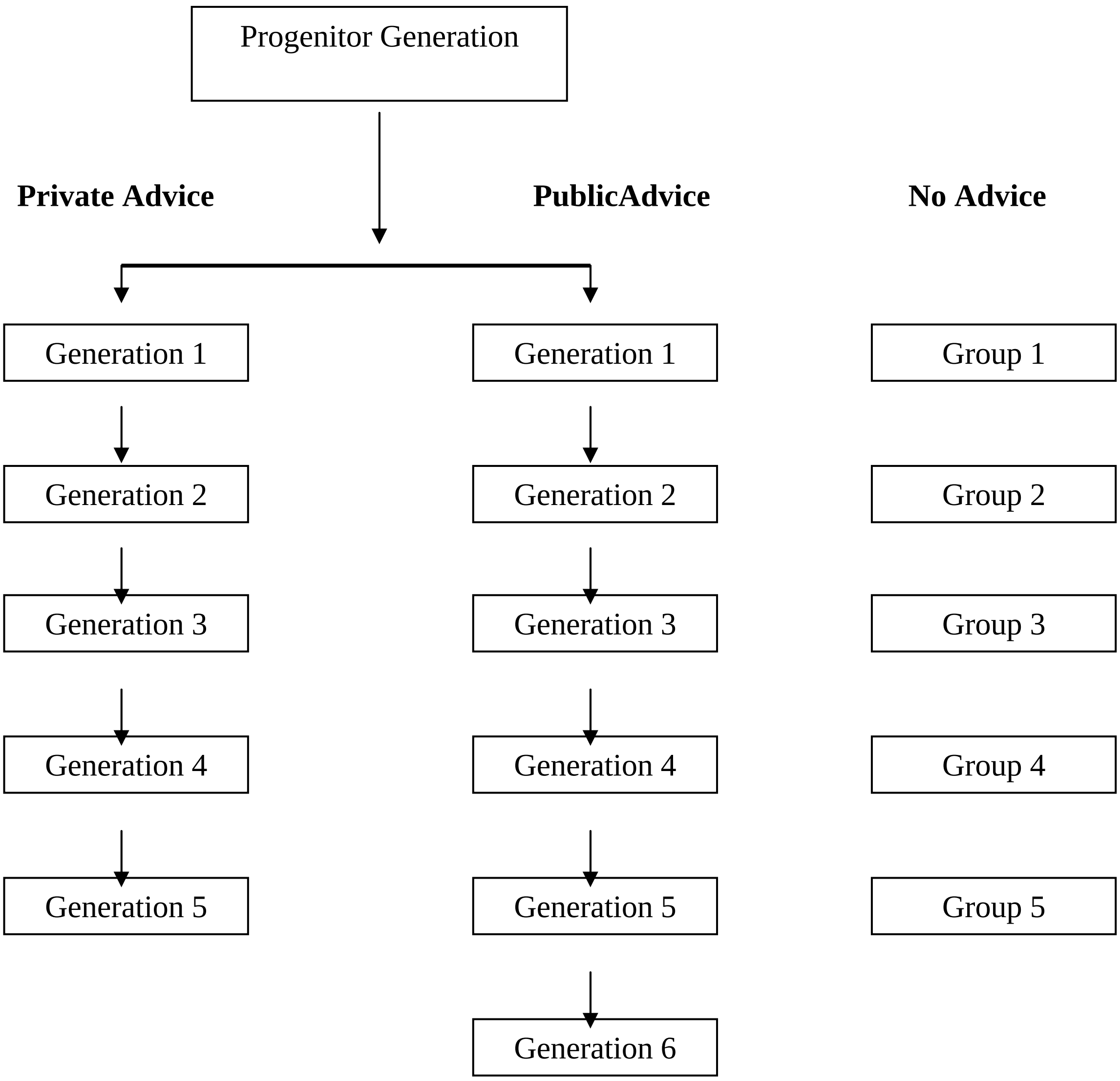
Schotter, A. and B. Sopher, (2001a), "Advice and Behavior in an Inter-generational Trust Game", Mimeo, Center for Experimental Social Science, New York University, New York, September 2001.

Schotter, A. and B. Sopher, (2001b), "Advice and Behavior in Intergenerational Ultimatum Games: An Experimental Approach", Mimeo, Center for Experimental Social Science, New York University, New York , September 2001.

Table 1: Experimental Design

No.	Design of Experiment	Number of Generations	Periods Per Generation	Subjects Per Generation	Treatment	Number of Subjects
1	Replicator No-Advice	5	10	5	No History or Advice	25
2	Progenitor	1	10	5	No History or Advice but Advice Left	5
3	Private Advice	5	10	5	Private Advice only	25
4	Public Advice	6	10	5	Public Advice Read Aloud	30
	TOTAL					85

Table 2: The Structure of the Experiments



5 Players in each generation or group

Table 3: Round by round contributions in the three treatments

Treatment	Rounds									
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Progenitor	8.8	9.2	9.4	8.8	8.8	8.6	8.8	8.2	7	7.8
Public Advice										
Generation 1	8.8	8.2	7.8	7.6	6.2	5.4	2.8	2	2.6	0.2
Generation 2	9.4	9.2	9	8.8	7.2	6.2	3.8	7.6	7	2.4
Generation 3	9.2	9.6	9.2	9.4	9.2	9.2	8.6	8.2	6.4	6.2
Generation 4	9.8	9.8	10	10	10	10	10	10	10	10
Generation 5	10	10	10	8	10	8	9	8	6.8	10
Generation 6	10	9.8	9.2	8.6	8.2	9.4	9.4	9	8	7.8
AVERAGE	**9.53**	**9.43**	**9.2**	**8.73**	**8.47**	**8.03**	**7.27**	**7.47**	**6.8**	**6.1**
Private Advice										
Generation 1	7.6	6.8	5.6	5.2	4.6	5	4.6	4.2	3.8	5
Generation 2	7.8	7.6	7	5.8	4.2	2.8	2.6	3	1.8	0.8
Generation 3	7	7.2	7.6	6.4	6	4.8	5.8	6.4	5	2.2
Generation 4	8	8.6	8	7.8	7.2	4.8	4.2	2.8	3.6	4.2
Generation 5	6.8	6.8	5.2	5.2	4.8	5	3.2	3.6	3	1.6
AVERAGE	**7.44**	**7.4**	**6.68**	**6.08**	**5.36**	**4.48**	**4.08**	**4**	**3.44**	**2.76**
No Advice										
Group 1	3.2	4.2	4	1.8	2	2.8	3	2.2	2.6	2
Group 2	5.8	4.8	4.4	5.4	3.2	4.6	4.8	3.6	4	3.4
Group 3	4.2	7.2	4.4	3.4	2.8	4.2	3	2.8	2.2	1.8
Group 4	6.4	8.6	7.4	8	6.8	7	7.2	7	4.6	7
Group 5	4.6	5.8	5.4	5.6	4.6	4.2	3.6	1.4	2.6	4.6
AVERAGE	**4.84**	**6.1**	**5.1**	**4.8**	**3.9**	**4.6**	**4.3**	**3.4**	**3.2**	**3.8**

Table 4: Random Effects Tobit Regression of Contribution Made

Dependent Variable: C_{it}

	Specification 1	**Specification 2**	**Specification 3**	**Specification 4**
Private Advice	0.1407	0.0488	-1.2864*	-0.4516
	(0.6016)	(0.4630)	(0.7711)	(0.9540)
Public Advice	3.7831***	3.5011***	3.3894***	2.2837**
	(0.7354)	(0.6253)	(0.8364)	(0.9191)
1/t		4.5798***	8.9905***	5.8916***
		(0.4377)	(1.0467)	(1.1454)
$C_{i,t-1}$			0.3620***	0.7614***
			(0.0616)	(0.0952)
$\Lambda_{i,t-1}$				0.5724***
				(0.1084)
Constant	6.1874***	4.9647***	1.3729**	-0.0115
	(0.4570)	(0.3641)	(0.6994)	(0.7889)
Log Likelihood	-1689.7898	-1646.0099	-1448.4998	-1435.2628
Wald χ^2	33.46***	145.59***	289.35***	275.24***
σ_ε	3.1968***	2.9164***	2.7577***	2.7101***
	(0.1082)	(0.0977)	(0.0989)	(0.0981)
χ^2 for $\sigma_u = 0$	328.86***	379.33***	82.99***	76.48***
χ^2 Test for Equality of Treatment Effects	38.44***	33.22***	34.29***	14.70***
Number of Observations	850	850	765	765
Number Uncensored	542	542	491	491
Number Lower Censored	78	78	75	75
Number Upper Censored	230	230	199	199
Number of players	85	85	85	85

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Determinants of Contribution Made: Sensitivity of Results to Estimation Method Used

Dependent Variable: C_{it}

	(1) Random Effects Tobit	**(2) Fixed Effects Unconditional Tobit$**	**(3) Random Effects (Linear)**
Private Advice	-0.4516	-5.0920***	-0.3706*
	(0.9540)	(1.1842)	(0.2049)
Public Advice	2.2837**	3.8462***	0.0936
	(0.9191)	(0.9075)	(0.2412)
1/t	5.8916***	6.7987***	2.0231***
	(1.1454)	(1.1311)	(0.7319)
$C_{i,t-1}$	0.7614***	0.6373***	0.8871***
	(0.0952)	(0.0943)	(0.0449)
$\Lambda_{i,t-1}$	0.5724***	0.4756***	0.3743***
	(0.1084)	(0.1148)	(0.0584)
Constant	-0.0115	2.0604***	0.0310
	(0.7889)	(0.7847)	(0.2592)
Log Likelihood	-1435.2628	-1350.7848	
Wald# χ^2	275.24***	844.27***	1021.74***
σ_ε	2.7101***		2.0078
	(0.0981)		
χ^2 for $\sigma_u = 0$	76.48***		8.23***
Joint Significance of the Player Fixed Effects		245.436***	
χ^2 Test for Equality of Treatment Effects	14.70***	63.42***	3.68*
Number of Observations	765	765	765
Number Uncensored	491	491	
Number Lower Censored	75	75	
Number Upper Censored	199	199	
Number of players	85		85

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
# :For Fixed Effects Conditional Tobit Regression this is a Likelihood Ratio Test
$: Player Fixed Effects Included in the Set of Explanatory Variables

Table 6: Random Effects Tobit Regression by Session Type

Dependent Variable: C_{it}

	(1) Private Advice Session	**(2) Public Advice Session**	**(3) No Advice Session**	**(4) Including Interaction Terms[$]**
1/t	3.2499	9.5895***	4.0972***	3.9114
	(2.0135)	(3.3139)	(1.4669)	(2.5370)
$C_{i,t-1}$	0.9124***	0.8178***	0.9793***	0.9082***
	(0.1520)	(0.2234)	(0.0865)	(0.1943)
$\Lambda_{i,t-1}$	0.2936*	1.0477***	0.2309**	0.4368*
	(0.1692)	(0.2633)	(0.1134)	(0.2250)
Public Advice				1.8911
				(1.9928)
1/t $\times$ Public Advice				2.3418
				(3.6280)
$C_{i,t-1} \times$ Public Advice				0.0669
				(0.2697)
$\Lambda_{i,t-1} \times$ Public Advice				0.6520**
				(0.3074)
Constant	-0.8900	2.9082*	-0.7755	-1.2690
	(0.6158)	(1.6798)	(0.5210)	(1.0111)
Log Likelihood	-464.0496	-391.6307	-553.0673	-882.7910
Wald χ^2	139.98***	56.75***	230.16***	229.69
σ_ε	2.3660***	3.7618***	2.7491***	2.9056***
	(0.1443)	(0.2860)	(01496)	(0.1356)
χ^2 for $\sigma_u = 0$	1.02	46.42***	2.20*	41.15***
Number of Observations	225	270	270	495
Number Uncensored	183	108	200	291
Number Lower Censored	31	17	27	48
Number Upper Censored	11	145	43	156
Number of players	25	30	30	55

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%;
[$]: Only for Private and Public Advice Sessions

Table 7: Effect of Deviation from Group Average on Change in Contribution (ΔC_{it})

	$\Lambda_{i,t-1} > 0$	$\Lambda_{i,t-1} \leq 0$	**Wilcoxon z-statistic (p-value)**	**T-test t-statistic (p-value)**
ΔC_{it} in Private Advice Treatment	-0.04	-0.94	2.55 (0.01)	3.02 (0.03)
ΔC_{it} in Public Advice Treatment	0.975	-0.95	4.97 (0.00)	5.47 (0.00)

Notes:

$\Lambda_{i,t-1}$ denotes the deviation from group average in period $t-1$

$\Lambda_{i,t-1} \geq 0$ implies that contribution was less than group average in period $t-1$

$\Lambda_{i,t-1} < 0$ implies that contribution was greater than group average in period $t-1$

Table 8: Does Ethnicity Matter? Random Effects Tobit Regression

Dependent Variable: C_{it}

	(1) Does Ethnicity Matter - All Sessions	(2) Does Ethnicity Matter - Private Advice	(3) Does Ethnicity Matter – Public Advice	(4) Does Ethnicity Matter - No Advice
Private Advice	-0.1763			
	(0.8033)			
Public Advice	2.3118***			
	(0.8286)			
1/t	5.9451***	3.0842	1.5776	4.2518***
	(1.1995)	(2.0355)	(2.7287)	(1.5309)
$C_{i,t-1}$	0.7692***	0.9192***	1.5845***	0.9892***
	(0.0967)	(0.1518)	(0.1769)	(0.0908)
$\Lambda_{i,t-1}$	0.5724***	0.3206*	1.3737***	0.2467**
	(0.1060)	(0.1688)	(0.2289)	(0.1219)
WHITE	0.1131	0.2184	0.9783***	0.0255
	(0.1003)	(0.1786)	(0.3076)	(0.0721)
Constant	-0.6326	-1.5470*	-6.5426***	-0.9870
	(0.9088)	(0.8566)	(1.5042)	(0.7931)
Log Likelihood	-1392.4344	-448.6751	-384.6432	-553.0096
Wald χ^2	269.67***	134.70***	119.97***	230.66***
σ_ε	2.6449***	2.2933***	3.9286***	2.7478***
	(0.0964)	(0.1471)	(0.3119)	(0.1496)
χ^2 for $\sigma_u = 0$	67.73***	0.43	5.52**	2.20*
Number of Observations	738	216	252	270
Number Uncensored	483	177	106	200
Number Lower Censored	71	28	16	27
Number Upper Censored	184	11	130	43
Number of players	82	24	28	30

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Random Effects Regression of Change in Contribution
Dependent Variable: ΔC_{it}

	(1) All Sessions	**(2) No Advice**	**(3) Private Advice**	**(4) Public Advice**
Private Advice	-0.3706*			
	(0.2049)			
Public Advice	0.0936			
	(0.2412)			
1/t	2.0231***	2.5200**	3.2147*	1.0908
	(0.7319)	(1.1350)	(1.5647)	(1.2681)
$C_{i,t-1}$	-0.1129**	-0.1450**	-0.2362**	-0.0131
	(0.0449)	(0.0620)	(0.1168)	(0.0801)
$\Lambda_{i,t-1}$	0.3743***	0.2151**	0.1634	0.7685***
	(0.0584)	(0.0837)	(0.1321)	(0.1101)
Constant	0.0310	0.0905	0.0759	-0.5063
	(0.2592)	(0.3807)	(0.4686)	(0.6260)
Wald χ^2	183.92***	49.49***	46.21***	112.54***
χ^2 for $\sigma_u = 0$	8.23***	1.54	0.75	2.49
χ^2 Test for Equality of Treatment Effects	3.68*			
Number of Observations	765	270	225	270
Number of players	85	30	25	30

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1: Average Contribution, by Period and Treatment

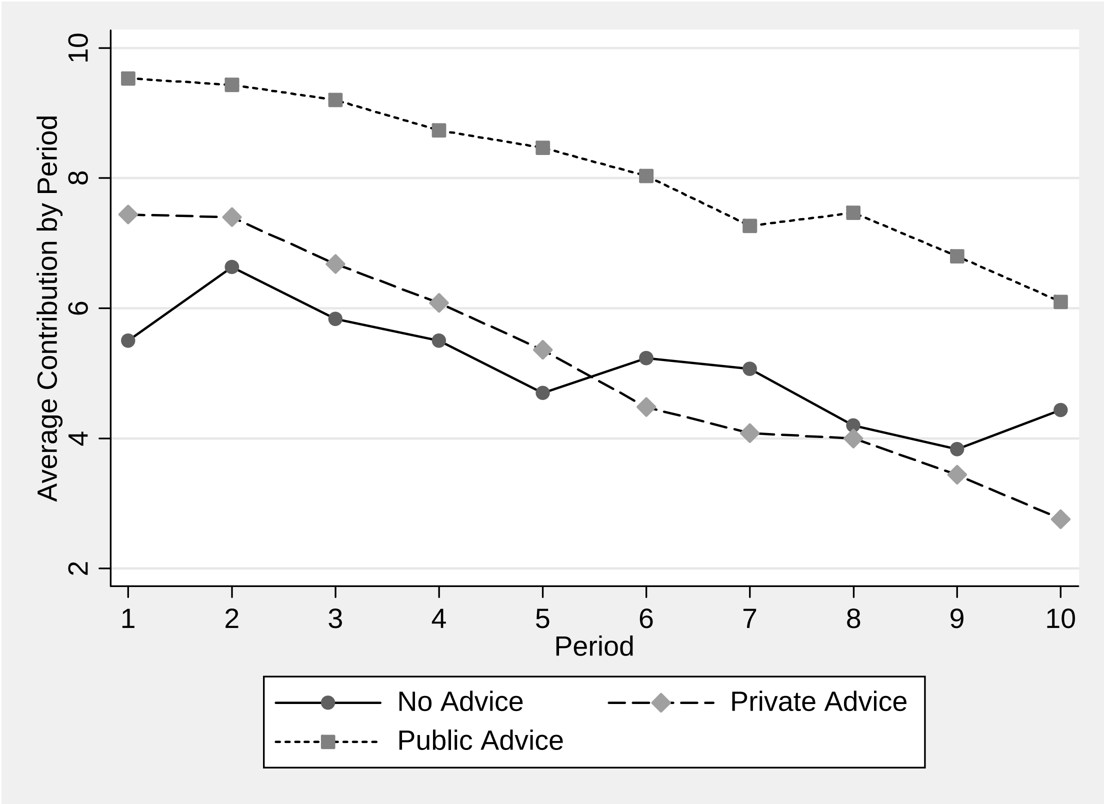
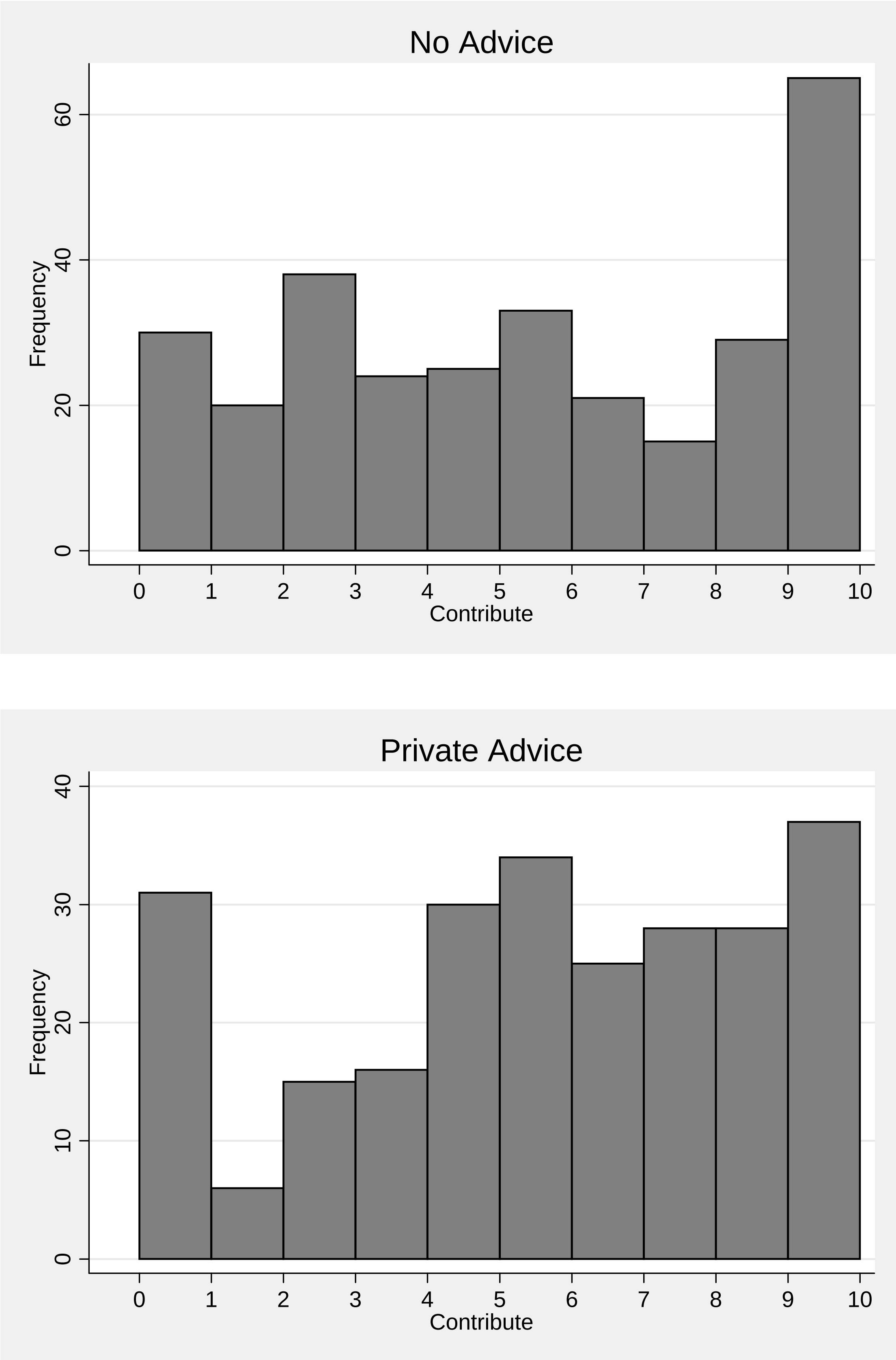


Figure 2: Histogram of Contributions by Treatment



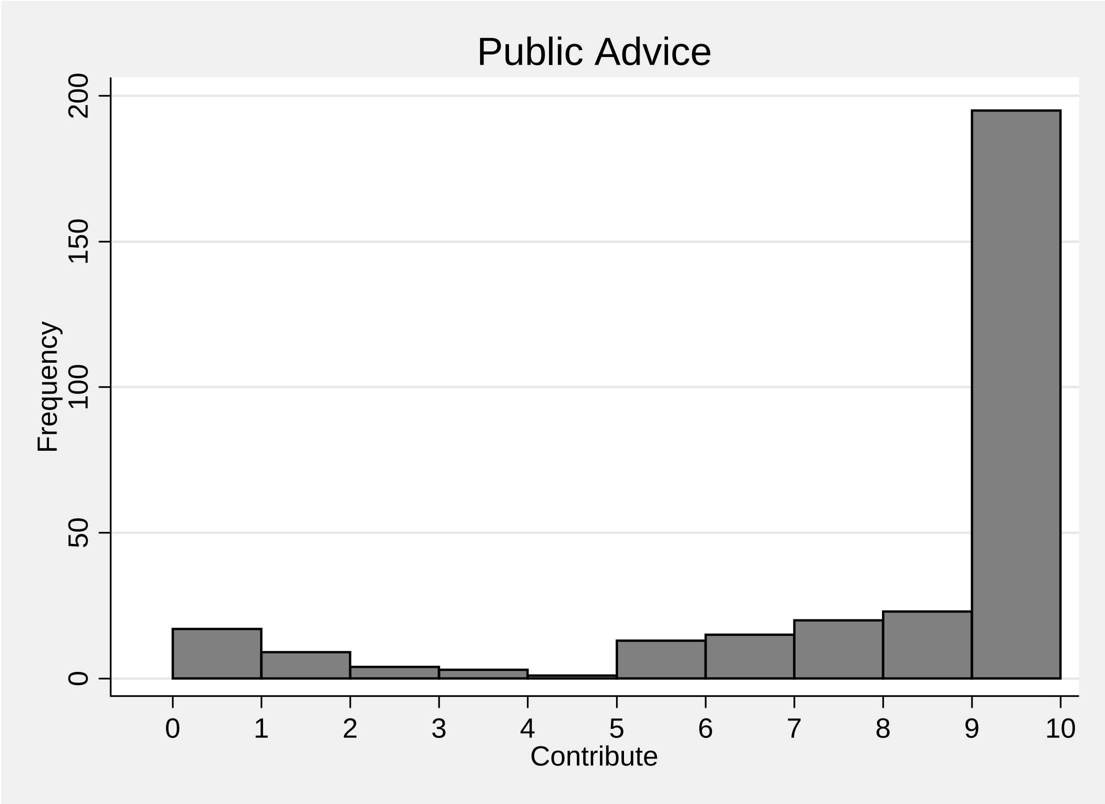

Public Advice
Frequency
0
50
100
150
200
0
1
2
3
4
5
6
7
8
9
10
Contribute

Figure 3: Proportion Contributing the Maximum (=10), by Period and Treatment

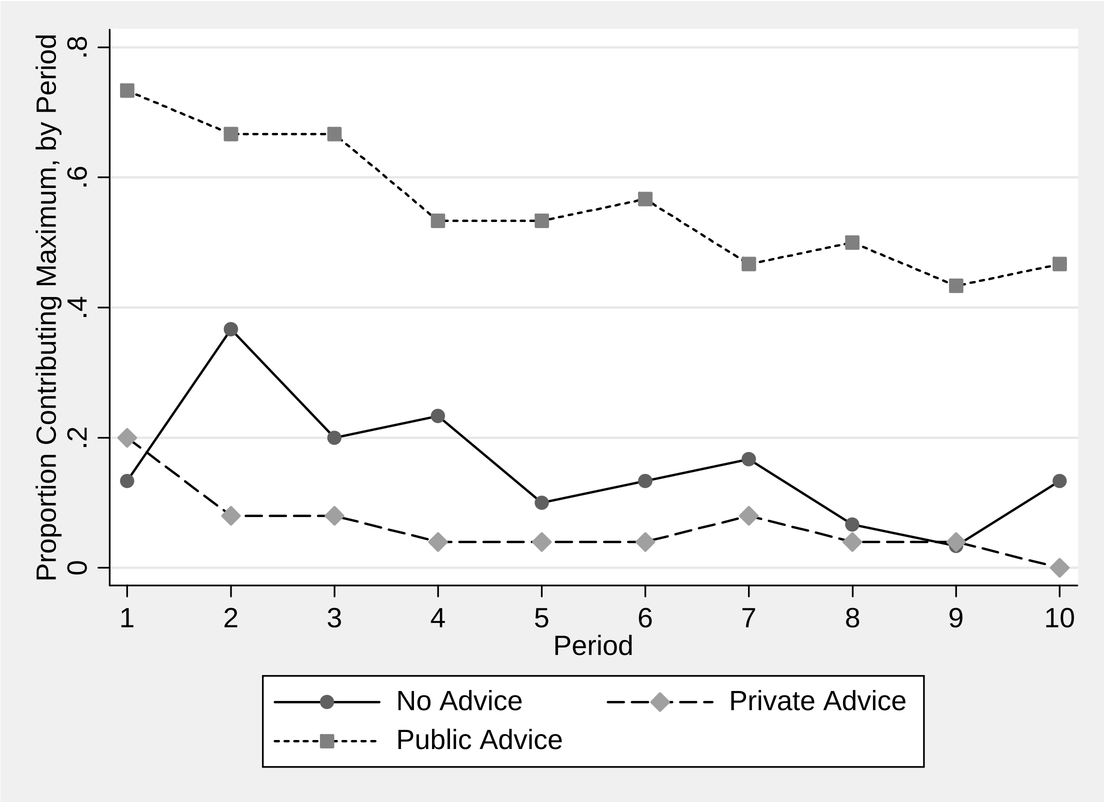
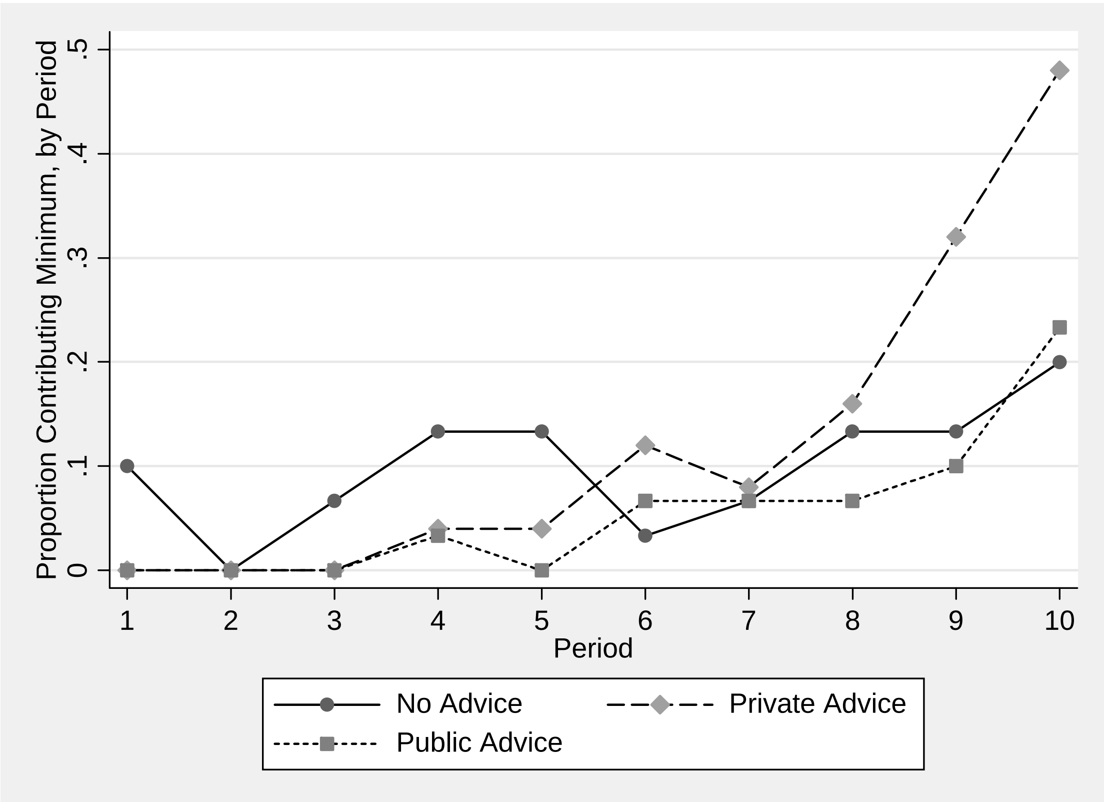


Figure 4: Proportion Contributing the Minimum (= 0), by Period and Treatment



Appendix 1: Experimental Instructions

Subject ID (Student ID #)____________________

Instructions

This is an experiment in economic decision-making. Wellesley College has provided the funds 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. At the beginning of each round each participant will have an endowment of 10 tokens. In each round, each participant will choose how many tokens (ranging from 0 to 10) to allocate to a private account and how many tokens (ranging from 0 to 10) to allocate to a public account. For each round, these two numbers should add to 10, the total number of tokens you have for that round. At the beginning of each round you will write the number of tokens you wish to contribute to the public account on a slip of paper 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. Your personal earnings for this round will equal the number of tokens you invested in your private account plus the number of tokens you get back from the public account (the latter may be a fractional amount). You will keep track of your contributions to each account and your earnings on the Record Sheet on the next page. Please take a look at the Record Sheet now.

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.

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 single subject who participated in the experiment immediately prior to you. At the end of your 10 decision rounds you will leave advice to a new subject on how to make decisions. 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 the subject to whom you give advice. Please write your advice on the sheet provided, and write or print legibly. 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 (Student ID #)____________________

Please answer the following question after the instructions have been read and before the first round begins.

What is the average contribution to the public account that you expect from the other subjects in your group? Do not include yourself, and round to the nearest integer if necessary. Please choose one:

____ 0	____ 3	____ 6	____ 9
____ 1	____ 4	____ 7	____ 10
____ 2	____ 5	____ 8	

Record Sheet

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

Background Information
(Optional)

What is your year? __________

What is your GPA? __________

What is your ethnic background? (Please choose **one** that you feel describes you best.)

_____ African-American
_____ Asian
_____ Hispanic
_____ White (Caucasian, non-Hispanic)
_____ Other, specify: ________________

ADVICE:
Please write your advice to the next player here. Continue on reverse if necessary.