Using Public Goods Game Experiments to Design Cooperative Environments

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Statement of Originality

This is to certify that to the best of my knowledge, the content of this thesis is my own work. This thesis has not been submitted for any degree of other purposes.

I certify that the intellectual content of this thesis is the product of my own work and that all the assistance received in preparing this thesis and sources have been acknowledged.

Danielle Monique Merrett

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Contents

Abstract

This PhD thesis demonstrates how Public Goods Game experiments can be used to design and test cooperative environments. In chapter two I propose an intergroup competition scheme (ICS) to theoretically solve the free-riding problem in the public goods game. The key feature of the ICS is a transfer payment from the group with the lowest contribution to the group with the highest contribution that is proportional to the difference in the overall contribution between the groups. The ICS is trivial to implement, requires minimal information, makes the efficient contribution a dominant strategy and is budget balanced across the groups. Consistent with the theory, the experimental results demonstrate that the ICS significantly raises contributions to almost reach optimality.

Chapter three examines the effects of in and out-group social comparisons on cooperation in team situations. Performance benchmarking, where firms compare their performance to other firms, is one channel firms can use to motivate free-riders to contribute greater effort. Three competing models are put forward to explain how comparative information might affect contribution preferences: conformity, competition, and selfish biased conditional cooperation. This study varies in-group and out-group comparative information to experimentally test the models driving behavior. Social comparisons raise cooperation with the highest level of cooperation observed when both in-group and out-group comparisons are provided. However there are differences in how in-group and out-group comparisons influence cooperation.

Chapter four compares the performance of alternative estimation approaches for Public Goods Game data. A leave-one-out cross validation was applied to test the performance of five estimation approaches. Random effects is revealed as the best estimation approach because of its un-biased and precise estimates and its ability to estimate time-invariant demographics. Surprisingly, approaches that treat the choice variable as continuous out-perform those that treat the choice variable as discrete. Correcting for censoring is shown to induce biased estimates. A finite Poisson mixture model produced relatively un-biased estimates however lacked the precision of fixed and random effects estimation.

List of Figures and Tables

List of Figures

List of Tables

Chapter 1: Introduction

Danielle Merrett

In the crisp early hours of an autumn morning in 2006, more than 60 fire fighters and three ladder trucks fought a raging inferno that engulfed the iconic inner city Sydney church, St Barnabas Broadway. The church building was used for congregational gatherings, and was also frequently used during the week to host community and outreach events. The contributions of the original building donors, from over 150 years ago, provided the city with a public good that lasted several generations. In the years following the fire that destroyed the church building, St Barnabas Church raised voluntary contributions to build a new church building. Cooperation from the community was strong with a non-trivial proportion of the money raised coming from individuals who were not members of the church.

The St Barnabas church building is one of many examples of public goods that have been voluntarily provided. Another less obvious example of a public good is the product of the effort contributed within a team setting such as a large choir. It is only when all the members of a choir cooperate to sing together, that the splendour of Handel's *Messiah* is made manifest for all to hear. Conversely, there are also many examples of free-riding where individual interests conflict with collective interests such that voluntary contributions fail to provide a collectively efficient amount of public good. Income tax is compulsory for this reason. The attributes of a public good that facilitates the unique opportunity for free-riding are non-excludability and non-rivalry. Non-excludability means that non-contributing members cannot be excluded from using the good. In the example of the church building above, individuals who did not make a donation to the building cannot be excluded from attending church services or community events.

And similarly, it is hard to identify and exclude any choir member who chooses to lip-sync difficult verses. Non-rivalry means that more than one person can enjoy the good at any one time. A song produced from a live choir is non-rivalrous as many can listen at once. Yet headphones, for example, are a rivalrous good because only one person can use them at a time.

Maintaining public goods through cooperation is essential to economic development. Yet many economic settings for providing public goods are informal and voluntary. For example, effort in team work is voluntary because it is hard to contract against free-riding. Most economic activity involves such informal interactions; therefore, it is important to understand how the provision of public goods can be sustained under moral hazard. The policy problem of limiting global carbon emissions looms as the largest public goods challenge of our lifetime. Each country has an incentive not to reduce emissions, yet the socially efficient outcome is lower total global emissions. Our challenge as economists is to design schemes and institutions that can overcome the barriers to productive cooperation.

The question then arises as to why under some institutions, despite the opportunity to free-ride, the voluntary provision of public goods is successful, and in others voluntary provision fails. A Public Goods Game experiment can be very useful in examining this question. A basic Public Goods Game experiment is an experiment that elicits an individual's choice of contribution towards a public good in a controlled setting. A controlled setting allows the investigator to examine the effect of a single intervention while holding all other possible effects constant. Experiments overcome the problem of observing public goods contribution in a natural world environment where many effects may operate simultaneously. In natural world settings it is hard to separate out the impact of different effects on individual contributions. Such separations are crucial for making correct inferences in order to guide the design of schemes that are to be effective in providing the socially efficient level of public goods.

The earliest public goods experiments in the early 1970s were framed in an institution known as the Lindahl mechanism (Ledyard, 1995). These experiments required subjects to nominate their willingness-to-pay for various amounts of a public good (see for example, Bohm's (1972) field experiment on willingness to pay for a TV broadcast). This approach reflected the public goods literature at the time where there was a focus on how to elicit an individual's true willingness-to-pay for a public good in the presence of incentives for over and under-stating one's preferences. Contemporary public goods experiments typically use a more abstracted institution known as the Voluntary Contribution Mechanism (VCM). In VCM experiments, each individual belongs to a group of a known number (however members remain anonymous to each other) and individuals are given an amount of money (an endowment) in which they must decide an amount to contribute to a public (group) account. The aggregate contributions in the public account are then multiplied by the experimenter and divided equally to every member regardless of the amount that individual contributed. Aggregate contributions are multiplied by a number chosen by the experimenter to model the additional social benefits reaped from providing a public good over the sum of the benefits of the individual private goods. A public library, for example can provide much more value to society than the sum value of the separate private contributions.

The origins of the VCM can be traced to a public goods field experiment designed by two sociologists from Wisconsin (Marwell and Ames, 1979). The Marwell and Ames (1979) study was seminal since it was the first to demonstrate that the effects of free-riding were much weaker than predicted from theory. Gerald Marwell and his PhD student Ruth Ames set about on the ambitious task of conducting a public goods field experiment on a random sample of 256 high school students in the Madison area. The experiment was conducted via several telephone calls from the experimenters to each student. Students were given the decision task to decide how to invest their $5 between a private account called an “individual exchange” and a public account called a “group exchange”. Marwell and Ames’s (1979) experimental design was subsequently used by experimental economists in the first laboratory public goods experiments by Kim and Walker (1984) and Isaac, McCue and Plot (1985) using university students with paper and pencils. Isaac, Walker and Thomas (1984) extended the design by computerizing the experiment and adding 10 repeated VCM rounds. Since then Public Goods Game experiments have been extensively[1] used not only by experimental economists but by anthropologists (for example Gintis, Bowles, Boyd and Fehr, 2005), political economists (for example, Ostrom, Walker, Gardiner, 1992), and evolutionary scientists (for example Hauert, De Monte, Hofbauer, and Sigmund, 2002) to examine different social dilemmas ranging from voting to punishment.

This PhD thesis demonstrates how Public Goods Game experiments can be used to design and test cooperative environments. In a standard VCM game, the dependent variable is the amount contributed by an individual as a proportion of the endowment. The amount contributed to a public good can also be thought of as a measure of cooperation. High contributions reflect a high level of cooperation within the group whereas low contributions reflect a low level of cooperation. Contributions in VCM games can therefore be used as an index of pure cooperation and used to test the success of different interventions in raising cooperation.

Chapter 2 “Intergroup competition and the efficient provision of public goods” is a study that uses the Public Goods Game experiment in a laboratory setting to test a scheme designed to raise cooperation within

[1] For example a review paper by Chaudhuri (2011) cites 146 Public Goods experiment publications.

teams belonging to the same firm (for example two sales teams). In this paper we propose an intergroup competition scheme to solve the free-riding problem in team production. In this scheme, two groups compete in contributions and the prize to the winning group is paid by the losing group. The advantages of the scheme are that it requires little monitoring and is budget balanced across the groups. Our experimental results demonstrate that the scheme significantly raises contributions to almost reach optimality compared to the control case. If implemented in an environment of team production, we would expect a significant increase in cooperation within each team.

Chapter 3 “Social Comparisons and Cooperation” is a second study that uses the Public Goods Game experiment to examine how social comparisons can be used to raise cooperation in team situations. This study has direct implications for the design of benchmarking programs. My experimental results show that comparisons to other groups have more effect if they are also reported with the team’s own performance. Further, upward comparisons to other groups have greater effects on cooperation than downward comparisons. Benchmarking programs that implement a design guided by the results of this study are expected induce greater cooperation within teams than designs that only report industry benchmarks.

Chapter 4 “Estimation of Public Goods Game data” is the final study in this thesis that offers experimentalists an evidence-based prescription for the best estimation approach for using VCM data to guide the design cooperative environments. While experiments solve the confound inference problems associated with the natural environment, making correct statistical inferences from public goods games can still be difficult. This is because the distribution of choice data for this game is highly non-standard and is complicated by its discrete, censored and often panelled nature. A leave-one-out cross validation reveals that the right choice of estimation approach can lead to significant improvements in the precision and unbiasedness of VCM model estimates.

Chapter 2: Intergroup competition and the efficient provision of public goods*

2.1 Introduction

Since early discussions of the tragedy of the commons, economists have been interested in the provision of public goods and the inefficiencies due to free-riding. Samuelson (1954) formulated the seminal analytical model of the public goods problem. Since the early 1970s, theorists have proposed a variety of taxation, transfer and subsidy schemes that usually rely on accurate information about individual behaviour to solve the free rider problem (e.g. Clark 1971; Groves and Ledyard 1977; Green and Laffont 1979; Holmstrom 1982; Falkinger 1996). Experimental evidence hints that free riding is a somehow less acute problem than the theory suggests (Sweeny, 1973; Marwell and Ames, 1979; Andreoni, 1988); typically, as first shown in Andreoni (1988), contributions in finitely repeated public goods games start substantially higher than zero (on average around 50% of the individual endowments) but decline over time and approach zero in the last period. While efficiency is significantly higher than the no contributions theoretically predicted, it remains far from the socially optimal (full) contributions. In the lab a few schemes have been found to achieve greater efficiency, however they typically rely on participants having knowledge of their peers' individual contributions, and/or being willing to engage in costly punishment (Fehr and Gaechter 2000a; Nikiforakis 2010). For instance, Falkinger's (1996) scheme in which a principal sets up transfers based on information about individual contributions was able to increase contributions in the lab (Falkinger et al., 2000).

We propose a new intergroup competition scheme (ICS) to solve free riding in a linear version of the public goods game using the well

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* Joint with Dr Pablo Guillen and Professor Robert Slonim

known voluntary contributions mechanism (VCM). The proposed ICS works in the following simple manner. The difference between the aggregate contributions of two groups playing the VCM is multiplied by a parameter (δ). This product is subtracted from the payoff of each member of the group with the low aggregate contribution and added to the payoff of each member of the group with the high aggregate contribution. Increasing contribution to the public good by one unit increases a player's payoff by his return from the public good plus δ. If this sum is bigger than one, the efficient (full in the VCM) contribution to the public good is a dominant strategy.

The proposed ICS has three desirable properties. First, it requires minimal information since it only requires knowing the aggregate contribution of each group. Second, it is theoretically efficient since full contribution, the socially optimal contribution in the VCM, becomes the dominant strategy. Third, the ICS is budget balanced across the two groups involved; no money needs to be injected from a principal, thus in equilibrium no transfers occur. We show that the proposed ICS has the same properties when applied to more general, non-linear versions of the public goods game and can be trivially extended to different group sizes and any finite number of groups.

A natural application involves using the ICS to solve the moral hazard problem in team production. Imagine a firm has several teams producing the same output (e.g., sales or manufacturing units) and that each team member's wages are a percentage of their team output. Further, assume workers are endowed with similar ability and perform equally productive activities so that output is a good proxy of effort towards the public good. In this case, workers face the standard free-rider problem. With the ICS, a worker's earnings are further affected by a transfer from the less to more productive team proportional to the difference in output between the teams. Note that the ICS requires no additional information than the compensation scheme without it. Solving the free-rider problem, the ICS improves the welfare of all parties involved (both the firm and team members).[1] Besides team

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[1] If we assume team members have heterogeneous ability, then there can be differences in output across teams and transfers in equilibrium. Nonetheless, the ICS offers the right incentive to members to contribute the efficient amount regardless of the behavior of other members of his team since the efficient contribution remains a

production, the ICS can also be the basis for other incentive schemes designed to solve public goods problems such as an energy saving initiative in which groups compete to save the most energy to reduce negative externalities.

To test the ICS, we ran two experiments. The first experiment tests the efficacy of the ICS and the second experiment isolates the various factors that change from the standard public goods to the ICS situation to understand the behavioural effects of each factor. Experiment 1 tests a version of the ICS in which the marginal per capita return from the public good, $MPCR^{PG}$, is 0.5 and the marginal per capita return from the group transfer, $MPCR^{T}$, is 0.75, against a control VCM with a MPCR = 0.5 since MPCR = 0.5 has been examined extensively in the literature. Over all 10 periods, we find that the ICS increased the average contribution by over 50 percentage points compared to the control treatment. We observed a strong and typical (e.g. Andreoni 1988) end effect in the control with contributions declining to just 10% on average by period 10 whereas in sharp contrast in the ICS there is no end game decline in contributions; subjects contributed 80% of their endowment in the last period.

The ICS changes several dimensions from the standard public goods environment, any of which may have affected subjects' behaviour. First, the ICS includes additional information on the other's group total contribution, and thus introduces additional payoff comparisons. Second, there is the possibility of out of equilibrium transfers in ICS that could invoke a psychological framing of winning and losing. Third, the MPCRs, including the transfer in the ICS, differ between the standard public goods game and the ICS. Experiment 2 examines several treatment conditions to isolate each of these three factors to tease apart their distinct effects. First, we find that the information provided by the ICS does not raise contributions *per se*.

Second, we tested two parameterizations where competition is introduced, but keeping the dominant strategy at the level of contributing nothing. When intergroup competition is introduced, but the total MPCR is kept constant with respect to the standard public

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dominant strategy, and moreover, new team members can be attracted with the incentive for optimal effort regardless of other team member's current productivity.

goods game (standard PG game with MPCR= 0.5; ICS with $MPCR^{PG}$ = 0.25, $MPCR^{T}$ = 0.25) contributions increased significantly, achieving roughly half of the gain to the ICS when contributing everything is the dominant strategy. When intergroup competition is introduced and the total MPCR is raised with the $MPCR^{PG}$ kept constant (0.5) with respect to the control, but the total MPCR still lower than one (MPCR= 0.75, $MPCR^{PG}$ = 0.5, $MPCR^{T}$ = 0.25) contributions increase even more, and overall contributions are now not significantly different compared to when contributing everything is the dominant strategy. Thus, competition alone appears to explain roughly half of the increased contributions in the ICS in Experiment 1 and it is not necessary to raise the MPCR over 1 to make full contributions a dominant strategy in order to raise contributions even further.

Last, we examined one final treatment in Experiment 2 in which we altered the ICS to have an external party provide the compensation to the group contributing more instead of a transfer from the group contributing less. In this condition, that has been proposed and explored by others, the unique equilibrium is for only one group to contribute 100% and for the other group to contribute nothing. Our results are consistent with this theoretical prediction and indicate that the ICS proposed here with an internal transfer produces greater efficiency theoretically and experimentally than an externally funded intergroup competition scheme due to causing both groups to contribute more.

This paper is structured as follows. Section 2 briefly discusses the relevant literature. Section 3 presents the ICS theory for two groups in the linear public goods condition and the appendix presents the more general ICS theory. Section 4 provides the experimental design and results for the standard VCM and proposed ICS conditions. Section 5 contains results regarding all the explanatory treatments. Section 6 includes a discussion and concludes.

2.2 Literature

Most of the literature on intergroup competition examines schemes where members of the winning group receive a bonus or reward paid by a third party, typically a principal. Thus, in contrast to our design, no transfer between the groups occurs under these schemes.

Rapoport and Bornstein (1987) introduced the intergroup competition (IC) paradigm to address social dilemmas. They proposed a binary public goods game where two groups compete in aggregate contributions to earn a reward. The primary motivation for their setup was to examine the effect of differing endowment sizes, group sizes and game structure on contributions in environments with intergroup conflict (Rapoport, Bornstein and Erev 1989; Bornstein, Erev and Goren 1994; Bornstein 2003). This early stream of literature examined IC as an economic and societal problem (e.g., IC exacerbated inefficiencies in the Chicken game in Bornstein, Budescu and Zamir 1997).

A discussion involving intergroup competition to achieve socially efficient outcomes emerges in Borstein, Erev and Rosen (1990), Bornstein, Gneezy and Nagel (2002), and Gunnthorsdottir and Rapoport (2006). Intergroup competition is shown to reduce free riding in laboratory social dilemma experiments (Tan and Bolle 2007; Reuben and Tyran 2010) and raise effort levels in a field study involving team production (Erev, Bornstein and Galil 1993). However, neither of the former schemes is budget balanced and neither makes the socially optimal contribution a dominant strategy.

2.3 The intergroup competition scheme (ICS)

We model a public goods situation using a standard Voluntary Contributions Mechanism (VCM) (Davis and Holt 1993; Ledyard 1995). Participants have the same endowment w and are in groups of N. Each individual has to decide how much of his endowment to allocate to a public account t_i and how much to keep for himself w-t_i. For each group, the sum of the individual allocations to the public good $\sum_{j=1}^{N} t_j$ is then multiplied by a factor a ($N > a > 1$), to model the additional value generated from the public nature of the good. The final value of the public account is then shared equally among the group members. The payoff therefore of player *i* under a VCM is given by:

$$\pi_{\text{i}} = (w - t_i) + \frac{a}{N}\sum_{j=1}^{N} t_j \ .$$

Since $\dfrac{\partial \pi_i}{\partial t_i} = -1 + \dfrac{a}{N} < 0,$ under the assumptions of selfishness (i.e., players only care about maximizing their own payoff) and common knowledge of rationality, the dominant strategy for each individual is to free ride (i.e., to allocate nothing to the public good). However, maximum efficiency occurs when everyone allocates their entire endowment to the public good, $t_i = w, \forall i$.

We propose an Intergroup Competition Scheme (ICS) to theoretically eliminate the free rider problem. This scheme involves competition between two groups where the prize to the winning group is funded by a transfer, proportional to the difference in total contributions, from the losing group. Let w > 0 for each individual *i* and assume there are two groups, denoted A and B, with N members in each group. Further, let the difference in aggregate allocation between the two groups be multiplied by a parameter δ. This product is then subtracted from the payoff of each member of the group with the lower aggregate contribution and added to the payoff of each member in the group with the higher aggregate contribution. Participants will now not only receive the marginal per capita return from the public good, $\text{MPCR}^{\text{PG}} = \dfrac{\alpha}{N}$ but will also receive an additional marginal per capita return from the transfer, $\text{MPCR}^{\text{T}} = \delta$. Formally, the payoff of member *i* belonging to group A and B, respectively, is:

$$\pi_i^A = (w - t_i^A) + \frac{\alpha}{N}\sum_{j=1}^{N} t_j^A + \delta(\sum_{j=1}^{N} t_j^A - \sum_{j=1}^{N} t_j^B) \ and$$

$$\pi_l^B = (w - t_l^B) + \frac{\alpha}{N}\sum_{j=1}^{N} t_j^B + \delta(\sum_{j=1}^{N} t_j^B - \sum_{j=1}^{N} t_j^A).$$

Now $\dfrac{\partial \pi_l^k}{\partial t_i} = -1 + \dfrac{\alpha}{N} + \delta$ for $k = [A, B]$.

If δ + α/N > 1, then $t_i^k = w$ (with *k* = *A or B*) is a dominant strategy under the standard assumptions of selfishness and common knowledge of perfect rationality.[2] Regardless of the contributions of anyone else, it will always be in an individual's best interest to contribute fully to the public account. The unique Nash equilibrium requires all players to contribute the maximum amount, and no transfers will occur in equilibrium. The appendix shows that the ICS can trivially be extended to any number of groups, any group size and to non-linear public goods situations. The theoretical considerations lead to the following hypothesis that we test experimentally in Experiment 1:

H1: If $\delta + \alpha/N > 1$, $N > a > 1$, then contributions will be higher with ICS than in the standard public goods VCM game.

2.4 Experiment 1: Testing the ICS

To test H1, we run a simple experiment (Experiment 1) with one control condition and one treatment condition.

2.4.1 Experiment 1 Design

To test the efficiency of the proposed ICS, Experiment 1 examines contributions in a control (C) condition and the ICS condition (ICS-dom) where $\delta + \alpha/N > 1$ so full contribution is the dominant strategy and 100% efficiency occurs in the unique Nash equilibrium.

Each condition (in both experiments) has two stages. In Stage 1 (S1) all subjects played a standard 'partners' VCM public goods game. Subjects were randomly and anonymously assigned to groups of N=4 and played the same game with the same partners for 10 periods. In Stage 2 (S2) subjects in the control condition played another 10 periods of the VCM game with *new partners*. In ICS-dom, subjects played a 10 period intergroup competition game with *new partners*. The order of events is the following:

Order of Events

Condition	**Stage 1** (10: periods w/same partners)	**Between Stages**	**Stage 2** (10 periods w/same partners)
Control C	Standard VCM	assigned	Standard VCM
ICS-dom	Standard VCM	new partners	ICS-dom

[2] Under fairly general assumptions the scheme would also work for conditional co-operators.

Having all subjects initially play the same Stage 1 VCM game establishes a baseline level of contribution for all subjects that provides greater precision for estimating treatment effects. With this design, we estimate various versions of the following difference-in-difference (DD) model:

$$(2.1) \quad y_{i,s} = \beta_0 + \beta_1 * S2_i + \beta_3 * ICS\_dom_i + \lambda * S2_i * ICS\_dom_i + \varepsilon_i,$$

where $y_{i,s}$ is subject i's contribution in Stage s to the public good that will be either his average contribution over all ten periods of the stage or for a specific period within the stage. $S2_i$ is a dummy variable indicator for Stage 2 so β_1 estimates changes that occur when the 10 period VCM game is repeated. ICS_dom_i is a dummy variable indicator for the ICS-dom condition so β_2 estimates any baseline difference in contributions between subjects in the VCM and the ICS condition. Most importantly, λ is the DD estimator measuring how subjects' contributions changed in the ICS-dom condition from the VCM to the ICS condition compared to how subjects contributions changed in the control condition when subjects repeated the VCM game.[3]

Procedures: One-hundred and twenty subjects participated in Experiment 1.[4] Upon arrival, subjects were randomly assigned seats in private cubicles with partitions to prevent subjects from seeing or interacting with each other. Once all subjects were seated, they were given the Stage 1 instructions.[5] The S1 instructions informed subjects that there would be two stages in the experiment. No further information on S2 was given during S1. After reading the S1 instructions, subjects answered a series of questions to ensure they

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[3] In addition to the greater precision, including S1 provided additional earnings that would minimize the risks of potential bankruptcy from potential losses in S2 in the intergroup competition conditions. No subject in ICS ever came close to going bankrupt; the lowest balance a subject ever experienced at any time in ICS was $15.00.

[4] Recruitment involved the on-line email invitation system ORSEE (Greiner, 2004) inviting students to participate who had volunteered to be in the subject pool for laboratory experiments at the University of Sydney. The experiment was computerized using zTree software (Fischbacher, 2007).

understood the task, and then they played a 10 period partners VCM (Andreoni 1998) with an MPCR of 0.5 (α/N=0.5). Subjects were randomly matched into groups of four for S1 and were informed that they would remain in the same group throughout S1. Subjects were given an endowment of 100 cents each period and could contribute between 0 and 100 cents to a neutrally framed "project" in each period. At the end of each period subjects received feedback on four pieces of information: their contribution that period, their group's aggregate contribution that period, their income that period and their income from all periods.

At the completion of S1 subjects were given instructions for Stage 2. In S2 subjects were rematched into new groups of four which they remained in throughout S2. Each subject was informed that none of the participants in her S1 group would be in her S2 group. In the ICS condition, subjects were also informed that their group was randomly matched to another group. After the S2 instructions, subjects were given review questions regarding S2.

In the control condition, subjects' payoffs and feedback in S2 were determined identically to S1. In ICS-dom in S2 subjects payoffs also depended on the difference in aggregate contributions between their group and the group that they were matched with; each member of the group with the higher contribution received 75 percent of the difference in group contributions while each member of the group with the lower aggregate contributions had their income reduced by 75 percent ($\delta = 0.75$) of the difference in group contributions. In the event that both groups had equal contributions, no money was transferred. Feedback in S2 in ICS included the information in S1 plus the aggregate contribution of the other group and the difference in aggregate contributions between their group and the other group. The experiment concluded at the end of Stage 2.

Table 2.1 summarizes the key parameters and number of groups for all the conditions in Experiments 1 and 2 (Experiment 2 will be discussed below). In the control condition, the MPCR for each subject is the MPCR from the public goods game, $MPCR^{PG}$=0.5. In the ICS-dom condition, the MPCR equals 1.25 which is the sum of the $MPCR^{PG}$ from the public goods game (0.5) plus the $MPCR^{T}$ from the transfer (δ=0.75). In S1, there were 12 groups of four subjects in the control condition and 18 in the ICS-dom. In S2, there were, rearranging the group members, again 12 and 18 groups in the control and ICS-dom conditions, respectively. However, in the ICS-dom condition, since each group was matched with another group and payoffs and feedback were affected by the other group's contributions, there were only 9 independent pairs of groups.

Table 2.1: Summary of experimental conditions

Condition	**Independent Observations**	**MPCR PG (a/N)**	**MPCR T (δ)**	**MPCR = MPCR^{PG} + MPCR^{T}**	**Nash Equilibrium^**
Experiment 1					
Control	S1: 12 groups S2: 12 groups	0.5	-	0.5	$t_i = 0$
ICS-dom	S1: 18 groups S2: 9 pairs of groups	0.5	0.75	1.25	$t_i = 100$
Experiment 2					
ICS0.5	S1: 18 groups S2: 9 pairs of groups	0.25	0.25	0.5	$t_i = 0$
INF0.5	S1: 18 groups S2: 9 pairs of groups	0.5	-	0.5	$t_i = 0$
ICS0.75	S1: 18 groups S2: 9 pairs of groups	0.5	0.25	0.75	$t_i = 0$
ICS-EXT	S1: 16 groups S2: 8 pairs of groups	0.5	0.75	1.25	$t_i = 100$* $t_i = 0$*

^ Based on the standard assumptions; * The Nash Equilibrium requires members of one group to contribute everything and members of the other group to contribute nothing.

Subjects received the sum of their earning across all periods in S1 and S2. On average, subjects earned 31.40 Australian Dollars in Experiment 1 and $31.50 on average over Experiment's 1 and 2). At the time of the experiment the exchange rate between the U.S. and Australian Dollar was almost exactly one to one.

2.4.2 Experiment 1 Results

We first present results for overall contributions and then look at the period by period behavior. In all analyses we find a significant increase in contributions and hence efficiency with ICS-dom.

Result 1: The ICS significantly raises contributions.

Average contributions across all 10 periods in S2 were 31 percentage points higher in the ICS-dom (80.9) than in C (49.9). Mean, standard deviation and median contributions in S2 are reported in Table 2.2a.

Table 2.2a: Average contribution in S2

	Conditions				
	Experiment 1		***Experiment 2***		
	C	***ICS-dom***	***INF0.5***	***ICS0.5***	***ICS0.75***
Mean	**49.86**	**80.92******	34.73*	57.09	67.61**
Standard Deviation	14.20	10.78	11.74	16.36	13.69
Median	50.31	81.11	36.54	55.25	69.38
N (independent groups)	12	9	9	9	9

Significance levels: *** p<0.01, ** p<0.05, * p<0.1 for Mann Whitney tests, each condition compared to C.

Table 2.2b: Average contributions in S1

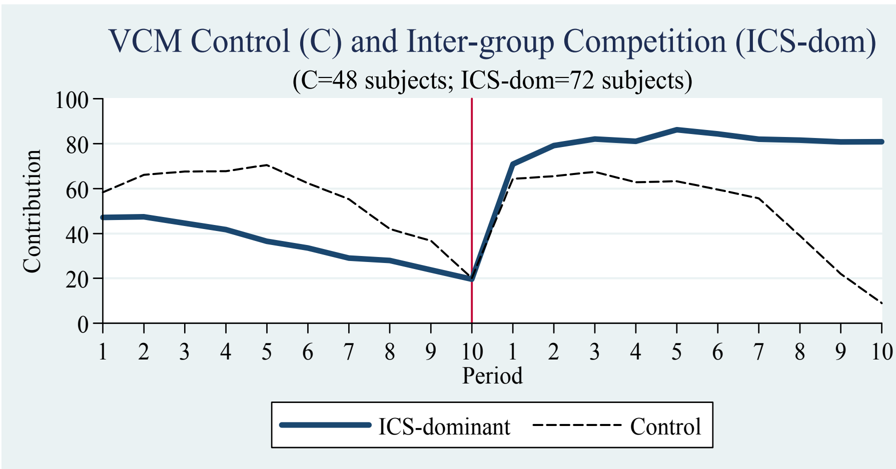
	Conditions				
	Experiment 1		***Experiment 2***		
	C	***ICS-dom***	***INF0.5***	***ICS0.5***	***ICS0.75***
Mean	**54.69**	**35.12**	41.07	37.88	37.14
Standard Deviation	17.82	18.24	14.79	22.90	22.56
Median	52.36	38.53	41.78	31.11	34.39
N (independent groups)	12	18	18	18	18

Table 2.2c: Difference in average contributions (S2-S1)

	Conditions				
	Experiment 1		***Experiment 2***		
	C	***ICS-dom***	***INF0.5***	***ICS0.5***	***ICS0.75***
Mean	-4.83	45.8	-6.34	19.21	30.47
Standard Deviation	-3.62	-7.46	-3.05	-6.54	-8.87
Median	-2.05	42.58	-5.24	24.14	34.99
N (independent groups)	12	18	18	18	18

The higher average contribution in ICS than C is significant at the 0.1% level (Mann-Whitney $p<0.001$).[6] Table 2.2b shows that there are some differences in the average contributions across the five treatments in S1 when facing the identical VCM public goods game; most notably, subjects in the Control condition contributed 19 percentage points more than in ICS-dom (p=0.004). To address this underlying difference, we focus on how the change in contributions between S1 and S2 differs between the conditions. Table 2.2c shows that the average contributions between S1 and S2 falls by 4.8 percentage points in the control condition, whereas it increases by 45.8 percentage points in ICS-dom. In other words, the *net* effect of introducing ICS-dom is a relative increase of 50.6 percentage points. This difference between S1 and S2 contributions in the ICS-dom is highly significant (Mann-Whitney $p<0.001$) whereas there is no significant difference between S1 and S2 contributions in the control condition (Mann-Whitney p=0.817).

Figure 2.1: Average contributions over time: Control (C) versus ICS-dom



Average contributions per period for the control condition (C) and Inter-group Competition (ICS-dom). S1 (S2) is shown to the left (right) of the red line.

[6] The Mann-Whitney test reported here and below uses the average contribution at the group level in C (12 observations) and the average contribution across the paired groups in ICS-dom (18 observations) for the independent observations.

Result 2: The effect of ICS is immediate.

Figure 1 shows average contributions per period for the C an ICS-dom conditions across S1 and S2. Contributions in Period 1 of S1 show that despite identical initial conditions, subjects contributed almost 11 percentage points more in C than ICS-dom. Moreover, contributions remain 10 percentage points or more higher in C than ICS-dom in S1 through Period 9, and as much as 15 percentage points higher in Periods 5 and 6. The behavior in S1 suggests that despite the sample population size (48 in C and 72 in ICS-dom), subjects appear to contribute more in C than ICS-dom. This difference stresses the importance of having the S1 baseline contributions to use for control in the analyses. Figure 1 also shows that when the ICS-dom scheme is introduced, a fairly dramatic change in contributions occur; whereas contributions in Period 1 of S2 compared to Period 1 in S1 are only slightly higher in C, contributions in ICS-dom are over 20 percentage points higher in Period 1 of S2 than S1.

Table 2.3 presents estimates from Equation (2.1) for the effect of ICS-dom on contributions at the subject level. Column 1 present estimates for Period 1. The regression includes two observations per subject (for each subject's contributions in Period 1 of S1 and S2); given two observations per subject, we cluster errors at the subject level. Estimates in Column 1 thus measure the immediate effect of the scheme. Random effects (RE) estimation was used as in both column 1 and 2 to control for individual effects in the panel data[7]. Consistent with Figure 2.1, the estimates in Column 1 shows that subjects contributed somewhat less on average in ICS-dom than C (marginally significant), and contributions are insignificantly higher in Period 1 of S2 than S1.

[7] We also estimated Equation (1) using Tobit Random Effects estimation but subsequently excluded these results because Tobit-Random Effects estimates of VCM data were found to be highly biased and less precise than Random Effects estimates (See Chapter 4).

Table 2.3: Control (C) versus ICS-dom

Dependent variable: Contribution		
	(1) RE [ab]	(2) RE [abc]
	Period 1	Periods 1-10
Constant	51.62***	55.89***
	(1.497)	(6.966)
ICS-dom	-11.19*	-7.347
	(6.242)	(12.68)
Stage2	5.980	-4.31
	(4.543)	(6.044)
ICS-dom*S2	17.78***	31.11***
	(6.030)	(10.95)
ICS-dom*S2*period2		14.62**
		(6.364)
ICS-dom*S2*period3		19.91***
		(6.338)
ICS-dom*S2*period4		26.60***
		(8.237)
ICS-dom*S2*period5		39.13***
		(8.636)
ICS-dom*S2*period6		35.92***
		(8.488)
ICS-dom*S2*period7		34.81***
		(8.736)
ICS-dom*S2*period8		38.91***
		(8.975)
ICS-dom*S2*period9		54.02***
		(8.125)
ICS-dom*S2*period10		54.65***
		(8.832)
R-square (overall)	0.080	0.464
N	240	2,400
Subjects	120	120

Models (1) and (2) are estimated using individual random effects. Robust standard errors in parentheses; Significance levels: *** p<0.01, ** p<0.05, * p<0.1. [a] Hausman Tests: model (1) ($Prob>chi^2 = 1$); model (2) ($Prob>chi^2 = 0.999$). [b] Standard errors clustered by subject.
[c] In Model (2) Group dummies, Period dummies and Period interactions with Treatment and Stage were controlled for in the regression (output excluded).

The key effect of the ICS scheme is the DD estimate *ICS-dom*S2*. The highly significant interaction (p<0.001) suggests that the introduction of the ICS raises contributions immediately, even when there has been no opportunity to learn. ICS-dom increases contributions 18 percentage points on average in Period 1.

Result 3: The effect of ICS alleviates the decay in contributions over time.

Figure 2.1 shows that the difference in contributions between ICS-dom and C increases slowly through Period 7 in S2, and dramatically during the final 3 periods. This increasing difference in contributions is due to contributions in the ICS-dom no longer deteriorating over time as is the normal pattern in VCM experiments, seen in S1 for both conditions, and seen in S2 in the Control condition. Columns 3 and 4 in Table 2.3 present difference in the period-by-period and stage 1 and stage 2 diff-in-diff-in-diff (DDD) estimates. These estimates show the relative change in contributions in ICS-dom than C in S2 compared to the changes that occurred in S1, period 1. In addition to the estimates shown in Table 2.3, the regressions include controls for period and the interaction terms for each period by S2 and for each period by ICS-dom. The DDD terms ICS-dom*S2*periodK estimate the difference in contributions between ICS-dom and C in S2 in Period K relative to Period 1 compared to the difference in contributions ICS-dom and C in S1 in Period K relative to Period 1. Column 2 shows that the average DDD increase is significant (p<0.05) from Period 1 to Period 2, and highly significant by Period 3 and thereafter. To see the large magnitude, note that in S1 the contributions are increasingly larger in C than ICS-dom over the first six periods whereas in S2 during the same first six periods contributions are increasingly larger in ICS-dom than C, and over the last three periods in S2 the contributions in ICS-dom become dramatically larger than C (essentially because contributions collapse in C while they stay high, around 80%, in ICS-dom).

Discussion: Although the effect of ICS-dom is dramatic, increasing average contributions by over 50 percentage points overall (Result 1), with the effect occurring immediately (Result 2) and preventing decay in contributions over time (Result 3), it is interesting that average contributions in ICS-dom remains below 100 percent. This is mostly due to a small percentage of subjects who in the last period contributed nothing (6 percent) or contributed 50 (6 percent). Conversely, the remaining subjects contributed on average nearly 90 percent.

In sum, Experiment 1 shows clear evidence that the intergroup competition scheme proposed here increases contributions dramatically overall compared to the control group (over 50 percentage points), and the effect is immediate and increases overtime, with no endgame collapse commonly seen in public goods game VCM experiments.

2.5 Experiment 2: understanding why the ICS raises contributions

In ICS-dom, with MPCR > 1, full contribution is a dominant strategy whereas in the control, with MPCR < 1, contributing nothing is the dominant strategy. However, this is not the only factor that differs between C and ICS. The ICS also includes:

i). An informational element; subjects in ICS are informed of the contribution of another group.

ii). A competitive element; in ICS a group will 'win' or 'lose' money from the other group.

iii). A higher MPCR; besides full contributions becoming the dominant strategy in ICS-dom, the MPCR is higher in ICS than C.

Each of these factors can theoretically change contribution levels if subject's preferences include concerns related to social comparisons (i), utility of winning or disutility of losing (ii) or other regarding preferences (iii). Experiment 2 will tease apart these effects to determine if any of them can help explain the significantly higher contributions in ICS-dom than C.

Treatments in Experiment 2 were run in an identical manner to those in Experiment 1. Each treatment included the identical Stage 1 VCM game (e.g., with four partners staying together for all 10 periods) to again establish a baseline level of contributions for comparison with all other treatments. All treatments then had each subject rematched into groups with entirely new partners to play the VCM game in the new treatment condition in Stage 2 for 10 periods. Subjects in these new treatments were again paid for their earnings from all 20 periods over both stages.

5.1 The effect of information: INF0.5

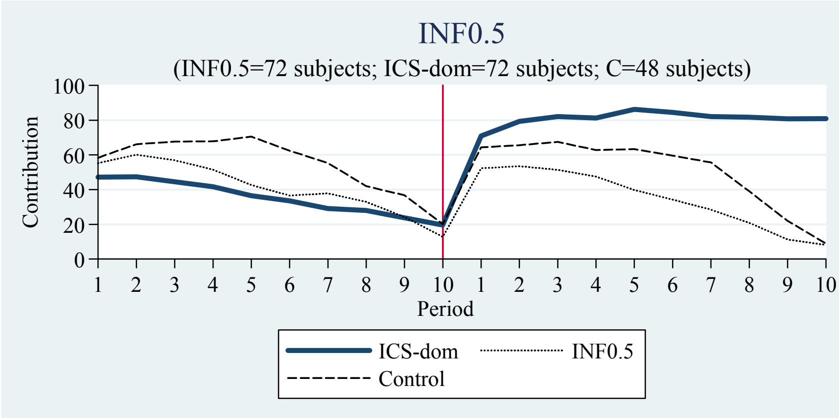
The difference in the amount of information provided to subjects in ICS-dom and C may explain the higher contribution ICS-dom in Experiment 1. In S2 of ICS-dom, in addition to receiving information on their own group's aggregate contribution each period, subjects also received information on the aggregate contribution of the group they were paired with. It is thus possible that subject's contributions were affected by the additional information rather than the other features of ICS-dom. For instance, group identity theory (e.g., Rabbie and Horwitz 1969; Tajfel et al 1971; Eckel and Grossman 2005; Chen and Li 2009) suggests that people could gain utility from seeing their group (in-group) do better than the other group (out-group).

To test this information hypothesis, we examined a condition, INF0.5, where subjects played the same VCM in S2 identical in every way to C (i.e., same earnings calculation, same MPCR=0.5, and same own group feedback), except that each group was also *paired with another group* at the beginning of S2 (which stays the same for all 10 periods) and subjects were given the aggregate contribution of this other group after each period, identical to the contribution information subjects were given each period in ICS-dom.

Result 4: information alone cannot explain the higher contributions in ICS-dom

The period by period contributions in INF0.5 are displayed in Figure 2, the overall average contributions are reported in Table 2a-c and Table 4 presents the individual level regressions. Average contributions in INF0.5 fell 6.3 percentage point from S1 to S2, similar to the decrease in C (4.8) (Mann-Whitney p=0.211). Figure 2.2 shows that contributions

Figure 2.2: Average contributions over time, Control (C), ICS-dom and INF0.5



S1 (S2) is shown to the left (right) of the vertical line.

in INF0.5 were lower than in C in both S1 and S2 by similar amounts, suggesting that the subjects participating in the INF0.5 condition contribute less than subjects in C in the identical condition (during S1) and that the INF0.5 condition had no further effect between the conditions. Figure 2.2 also shows that in INF0.5 in S2, similar to C and in contrast to ICS-dom, contributions decrease across periods.

Individual level regression analysis (Table 2.4) confirms that the introduction of information did not raise contributions in INF0.5. The estimated INF0.5**Stage2* interaction effect is not significant in Column 1 and is actually directionally negative, indicating that the providing information regarding another group's aggregate contributions cannot by itself explain the initially higher contributions on average due to ICS-dom. Thus, information had no institutional effect. Further, the sign on INF0.5**Stage2 + INF0.5*S2*periodK* in Column 2 is negative in all periods, indicating that in every period contributions in S2 are directionally lower in INF0.5 than C compared to the difference in contributions in S1 between INF0.5 and C for the identical period. This result indicates that information alone cannot explain the higher contributions in ICS-dom than C in any period.

Table 2.4: Control (C) vs. INF0.5

Dependent variable: Contribution		
	(1) RE [ab] Period 1	(3) RE [abc] Periods 1-10
Constant	58.33***	55.89***
	(4.77)	(6.97)
INF0.5	-3.08	10.69
	(6.31)	(9.28)
Stage2	5.98	-4.31
	(4.55)	(6.04)
INF0.5*Stage2	-8.98	-17.06*
	(5.96)	(8.83)
INF0.5*Stage2*period2		2.97
		(6.53)
INF0.5*Stage2*period3		3.59
		(6.94)
INF0.5*Stage2*period4		9.94
		(8.41)
INF0.5*Stage2*period5		13.36
		(8.76)
INF0.5*Stage2*period6		9.44
		(8.69)
INF0.5*Stage2*period7		-0.75
		(9.33)
INF0.5*Stage2*period8		-0.09
		(8.96)
INF0.5*Stage2*period9		10.78
		(8.08)
INF0.5*Stage2*period10		15.49*
		(8.00)
R-square (overall)	0.015	0.371
N	240	2,400
Subjects	120	120

Models (1) and (2) are estimated using individual random effects. Robust standard errors in parentheses; Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. [a] Hausman Tests: model (1) (Prob>chi^2 = 1); model (2) (Prob>chi^2 = 0.999). [b] Standard errors clustered by subject. [c] In Model (2) Group dummies, Period dummies and Period interactions with Treatment and Stage were controlled for in the regression (output excluded).

2.5.2 Effect of competition: ICS0.5

In addition to making contributions a dominant strategy with MPCR > 1 and providing additional information regarding another group's contribution, ICS-dom also introduced an element of competition. In particular, members of a group may perceive their group as "winning" a game when their group contributes more than the other group and likewise as "losing" if their group contributes less, regardless of how much they win or lose. It is possible that people get utility from winning or disutility from losing that goes beyond the monetary amount. For instance, Ku et al (2005) find evidence of 'competitive arousal' consistent with people gaining utility from winning live and online internet auctions. Moreover, to the extent that subjects in the group that contributed less to the public good in ICS-dom perceive the resulting transfer to the other group as a loss, loss aversion (Kahneman and Tversky 1979; Tversky and Kahneman 1992) predicts that these subjects will incur greater disutility than an equally sized monetary gain; in other words, loss aversion suggests that subjects may contribute more in ICS-dom to avoid additional disutility associated with losses. Thus, the element of competition introduced by ICS-dom, beyond making full contributions a dominant strategy, could explain higher contributions in ICS-dom than in the standard VCM control game.

To test the role of competition, we examine a new condition, ICS0.5, in which the competitive element is introduced similar to ICS-dom, but we hold constant the MPCR at the same level as in the control condition, at MPCR=0.5. In ICS0.5, we have MPCR = 0.5, with $MPCR^{PG}$ = 0.25 and $MPCR^{T}$ = 0.25, so that now subjects face the competitive element since there is the opportunity to win or lose as well as face monetary losses to the other group if the other group contributes more. To test the effect of competition we ran the ICS0.5 condition identically to the ICS-dom condition except with $MPCR^{PG}$ = $MPCR^{T}$ = 0.25.

Result 5: The competitive element in ICS can explain part, but not all, of the higher ICS-dom contributions.

Table 2.2c shows that average contribution over all 10 periods increased in ICS0.5 by 19.2 percentage points from S1 to S2 in contrast to a 4.8 percentage point decrease in C. This 24 percentage point net increase in contributions from S1 to S2 in ICS0.5 is significant (Mann-Whitney, p=0.004). Figure 2.3 shows that contributions in S2 are nearly identical between ICS0.5 and C except in the last three periods, whereas in S1 contributions are lower among subjects in ICS0.5 than in C, suggesting ICS0.5 had a positive effect. We thus again turn to the individual level analyses to tease apart subject differences from treatment effects.

The individual analysis (Table 2.5, Column 1) shows that subjects in ICS0.5 contributed 15.2 percentage points less than Control subjects on average in S1. This lower contribution is significant (p<0.05). Given this difference between the subjects, the same level of contributions in S2 between ICS0.5 and C suggests, at least directionally, that ICS0.5 raised contributions. As suggested by Figure 2. 3, Column 1 shows that the interaction ICO0.5**Stage2* is positive, indicating that ICS0.5 increased contributions by 8.8 percentage points in Period 1 of S2, however this higher level of contributions does not reach a conventional level of significance. Column 2 in Table 2.5 shows, however, that the effect of introducing competition, while holding the overall MPCR constant, raised contributions in S2 in virtually all periods in ICS0.5 than in C compared to the difference using the same periods in S1. Thus, the introduction of competition, in the absence of any change in the MPCR is sufficient to at least partially explain the higher contributions in ICS-dom.

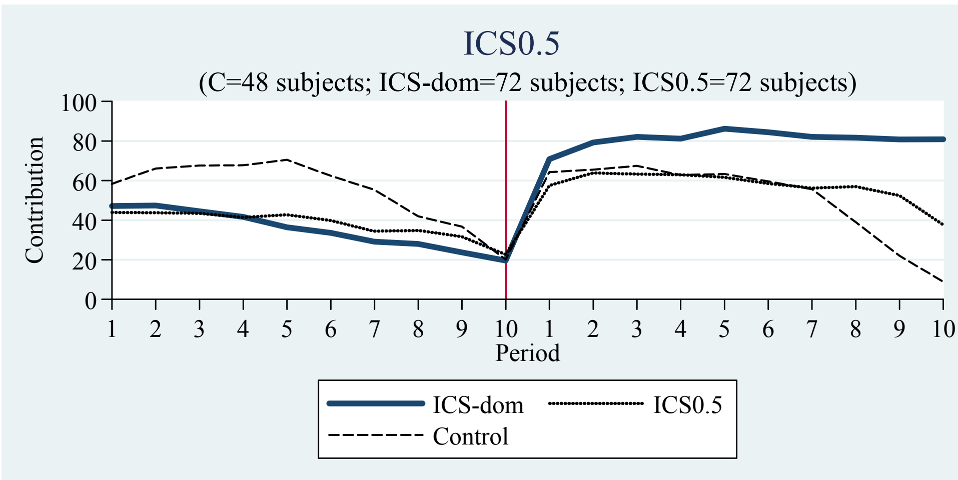
Table 2.5: C vs. ICS0.5

Dependent variable: Contribution		
	(1) RE [ab] Period 1	(3) RE [abc] Periods 1-10
Constant	58.33*** (4.764)	55.89*** (6.936)
ICS0.5	-15.16** (6.406)	11.27 (13.78)
Stage2	5.980 (4.55)	-4.31 (6.02)
ICS0.5*Stage2	8.82 (6.70)	18.80** (7.65)
ICS0.5*Stage2*period2		13.16* (7.03)
ICS0.5*Stage2*period3		12.34* (7.30)
ICS0.5*Stage2*period4		18.87** (8.94)
ICS0.5*Stage2*period5		18.51** (9.13)
ICS0.5*Stage2*period6		13.87 (9.14)
ICS0.5*Stage2*period7		13.85 (9.38)
ICS0.5*Stage2*period8		17.53* (9.95)
ICS0.5*Stage2*period9		27.98*** (9.28)
ICS0.5*Stage2*period10		18.59* (10.11)
R-square (overall)	0.0447	0.2877
N	240	2,400
Subjects	120	120

Models (1) and (2) are estimated using individual random effects. Robust standard errors in parentheses; Significance levels: *** p<0.01, ** p<0.05, * p<0.1.[a] Hausman Tests: model (1) (Prob>chi^2 = 1); model (2) (Prob>chi^2 = 0.999). [b] Standard errors clustered by subject. [c] In Model (2) Group dummies, Period dummies and Period interactions with Treatment and Stage were controlled for in the regression (output excluded).

To test whether the introduction of competition alone, in the absence of increasing the overall MPCR, explains all of the effect of ICS-dom in Experiment 1, we next examine whether there is any difference in contributions between ICS-dom and ICS0.5. The results (Table 2.2a) indicate that the average contribution in S2 is significantly higher in ICS-dom (80.92) than in ICS0.5 (57.09) (Mann-Whitney; p=0.014), suggesting that overall, the higher MPCR remains critical to fully explain the higher contributions in ICS-dom. Table 2.6 contains the individual analysis for the comparison between ICS-dom and ICS0.5. The difference between ICS-dom and ICS0.5 both becomes significant and increases over time (Column 2). In sum, there is an effect of competition in raising contributions under the ICS when the overall MPCR is kept constant with respect to condition C, but the ICS-dom effect is still significantly greater than the ICS0.5 on contributions.

Figure 2.3: Average contributions over time, ICS0.5 versus C and ICS-dom



S1 (S2) is shown to the left (right) of the red line.

2.5.3 Effect of a higher MPCR: condition ICS0.75

The results of the first two treatments in Experiment 2 suggest that neither the extra information nor the introduction of a competitive element can explain all of the increase in contributions in ICS-dom. Another possibility is that subjects have heterogeneous other-regarding preferences in which they gain heterogeneous levels of utility from other subjects earnings (e.g., Bolton and Ockefels 2000; Fehr and Schmidt 1999). With other-regarding preferences, it is possible that contributions to the public good will be utility maximizing even with

MPCR < 1, so that as the MPCR increases, more subjects will find full contributions to be the utility maximizing choice.

Table 2.6: ICS-dom vs. ICS0.5

Dependent variable: Contribution		
	(1) RE [ab] Period 1	(3) RE [abc] Periods 1-10
Constant	43.24***	66.91***
	(4.27)	(11.87)
Condition	3.90	-19.11
	(5.87)	(15.76)
Stage	14.68***	14.47***
	(4.91)	(4.713)
ICS-dom*Stage	9.09	12.00
	(6.31)	(10.11)
ICS-dom*Stage*period2		1.458
		(6.19)
ICS-dom*Stage*period3		7.57
		(7.279)
ICS-dom*Stage*period4		7.736
		(8.35)
ICS-dom*Stage*period5		20.61***
		(7.61)
ICS-dom*Stage*period6		22.06***
		(8.51)
ICS-dom*Stage*period7		20.96**
		(8.45)
ICS-dom*Stage*period8		21.37**
		(9.28)
ICS-dom*Stage*period9		26.04***
		(8.83)
ICS-dom*Stage*period10		36.06***
		(10.07)
R-square (overall)	0.0871	0.3816
N	288	2,880
Subjects	144	144

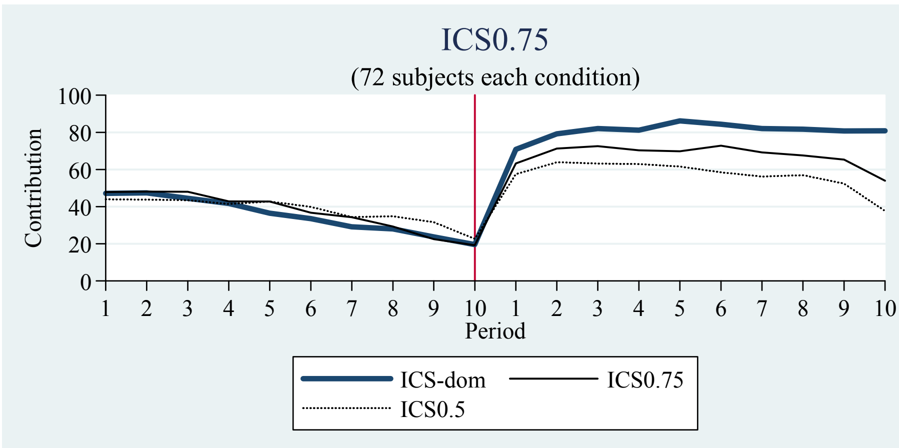
Models (1) and (2) are estimated using individual random effects. Robust standard errors in parentheses; Significance levels: *** p<0.01, ** p<0.05, * p<0.1.[a] Hausman Tests: model (1) (Prob>chi^2 = 1); model (2) (Prob>chi^2 = 0.999). [b] Standard errors clustered by subject. [c] In Model (2) Group dummies, Period dummies and Period interactions with Treatment and Stage were controlled for in the regression (output excluded).

Evidence of contributions increasing with the MPCR was shown in Palfrey and Prisbrey (1996) and Brandts and Schram (2001). In both studies the MPCR varied from zero to more than 1, and the results showed that contributions did not change dramatically when the MPCR was slightly higher than one to when the MPCR was slightly less than one as theory would predict without other-regarding preferences.

To test effect of the ICS when the overall MPCR is lower than one, but is higher than the MPRC in the Control condition, we ran condition ICS0.75 where the $MPCR^{PG} = 0.5$ is kept constant with respect to C and we set $MPCR^{T} = 0.25$ so that overall MPCR = 0.75. We set MPCR = 0.75 not only so that $MPCR^{PG} = 0.5$ is consistent with the control condition and the overall MPCR = 0.75 is less than 1, but also so that the competitive element, $MPCR^{T} = 0.25$, is kept constant with respect to ICS0.5.

Result 6: Increasing the MPCR, even if it remains less than 1, is sufficient to explain a significant majority of the increase due to ICS-dom. However this higher MPCR does not fully explain the last period higher contributions in ICS-dom.

Figure 2.4: Average contributions over time, ICS0.75 versus ICS0.5 and ICS-dom



S1 (S2) is shown to the left (right) of the red line.

The results of ICS0.75 are presented in Figure 2.4 and Tables 2.2c, 2.7 and, 2.8. The change in average contributions (Table 2.2c) from S1 to S2 was significant in ICS0.75 (average contributions rose 30.4 percent)

(Mann Whitney p = 0.000). Average contributions in S2 between ICS-dom (80.92) and ICS0.75 (67.61) are significantly different (Mann Whitney p = 0.047), but this is not the case when comparing ICS0.75 (67.61) with ICS0.5 (57.09) (Mann Whitney p = 0.200). Figure 2.4 shows that contributions in ICS0.75 do not decay over time as in C and is typically observed in standard public goods VCM experiments.

Table 2.7: C vs. ICS0.75

Dependent variable: Contribution		
	(1) RE [ab] Period 1	(3) RE [abc] Periods 1-10
Constant	58.33*** (4.767)	55.89*** (6.97)
ICS0.75	-10.49* (6.28)	-16.49 (11.02)
Stage2	5.98 (4.55)	-4.31 (6.04)
ICS0.75*Stage2	9.48 (5.94)	23.79* (12.56)
ICS0.75*S2*period2		14.28** (6.54)
ICS0.75*S2*period3		15.31** (6.96)
ICS0.75*S2*period4		22.95*** (8.69)
ICS0.75*S2*period5		24.88*** (9.341)
ICS0.75*S2*period6		29.51*** (8.95)
ICS0.75*S2*period7		25.01*** (9.38)
ICS0.75*S2*period8		31.87*** (9.82)
ICS0.75*S2*period9		48.03*** (9.25)
ICS0.75*S2*period10		36.44*** (9.30)
R-square (overall)	0.040	0.396
N	240	2,400
Subjects	120	120

Models (1) and (2) are estimated using individual random effects. Robust standard errors in parentheses; Significance levels: *** p<0.01, ** p<0.05, * p<0.1.[a] Hausman Tests: model (1) (Prob>chi^2 = 1); model (2) (Prob>chi^2 = 0.999). [b] Standard errors clustered by subject. [c] In Model (2) Group dummies, Period dummies and Period interactions with Treatment and Stage were controlled for in the regression (output excluded).

Table 2.8: ICS0.75 vs ICS-dom

Dependent variable: Contribution		
	(1) RE [ab] Period 1	(3) RE [abc] Periods 1-10
Constant	47.85*** (4.08)	40.89*** (8.51)
ICS-dom	-0.71 (5.73)	7.83 (13.60)
Stage2	15.46*** (3.81)	17.14 (11.56)
ICS-dom*S2	8.31 (5.50)	9.74 (14.75)
ICS-dom*S2*period2		0.33 (5.61)
ICS-dom*S2*period3		4.60 (6.93)
ICS-dom*S2*period4		3.65 (8.08)
ICS-dom*S2*period5		14.25* (7.85)
ICS-dom*S2*period6		6.417 (8.29)
ICS-dom*S2*period7		9.792 (8.43)
ICS-dom*S2*period8		7.04 (9.13)
ICS-dom*S2*period9		5.99 (8.79)
ICS-dom*S2*period10		18.21** (9.25)
R-square (overall)	0.089	0.472
N	288	2,880
Subjects	144	144

Models (1) and (2) are estimated using individual random effects. Robust standard errors in parentheses; Significance levels: *** p<0.01, ** p<0.05, * p<0.1.[a] Hausman Tests: model (1) (Prob>chi^2 = 1); model (2) (Prob>chi^2 = 0.999). [b] Standard errors clustered by subject. [c] In Model (2) Group dummies, Period dummies and Period

interactions with Treatment and Stage were controlled for in the regression (output excluded).

Table 2.7 compares ICS0.75 to C and the results are mostly similar to the comparison of ICS-dom with C reported in Table 2.3. There is no immediate effect ICS-dom as in this case *ICS0.75*Stage2* is not significant in model (2.1), but has a strong, significant and increasing effect over time. Table 2.8 indicates few period by period significant differences between ICS-dom and ICS0.75; a modest end game effect becomes significant in period 10.[8]

2.5.4 The effect of internal funding

One of the fundamental ways in which the ICS proposed here differs from past IC literature is that our mechanism includes an internal transfer from the group contributing the least to the group contributing the most to induce an equilibrium in dominant strategies in which each individual fully contributes to the public good. In other proposed IC plans (e.g., Reuben and Tyran 2010) the group contributing the most is funded by a bonus at least partly paid by an outside party, and the group contributing the least does not have to pay any of the bonus. In this alternative externally funded IC plan, if group A contributes more on aggregate then the payoff to members of group A and B, respectively, would be:

$$\pi_i^A = (w - t_i^A) + \frac{a}{N}\sum_{j=1}^{N} t_j^A + \delta\left(\sum_{j=1}^{N} t_j^A - \sum_{j=1}^{N} t_j^B\right) \textit{ and}$$

$$\pi_i^B = (w - t_i) + \frac{a}{N}\sum_{j=1}^{N} t_j .$$

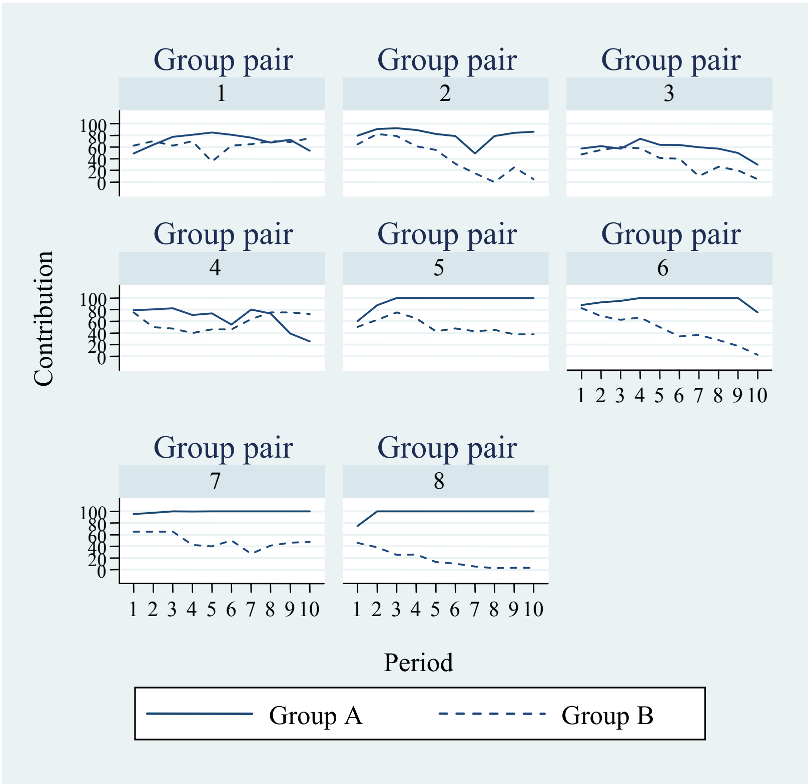
---

[8] While the difference in contributions are only significant in the last period, suggesting a very small difference in efficiency overall, the difference in the last period could be important if the stage game is repeated many times. Selten and Stoecker (1986) and Engle-Warnick and Slonim (2004) show that cooperation and trust, respectively, are initially high in finitely repeated supergames with the exception of the last period. However, with repetition of the supergame, both cooperation and trust begin to unravel from the last period, along the lines of subjects learning backward induction. In both studies, cooperation and trust unravel to increasingly earlier periods over 25 to 50 repetitions of the supergames. If this behavior occurs in the VCM games as well, then the lack of any collapse in ICS-dom compared to the 10th period decrease in contributions in both ICS0.5 and ICS0.75 suggests that repetition of the supergame could lead to a fall in contributions across all periods when $a/N + \delta < 1$, and thus the small differences observed in the current experiment could get magnified in the long run.

If both groups contribute the same aggregate amount than individuals receive:

$$\pi_i = (w - t_i) + \frac{a}{N}\sum_{j=1}^{N} t_j$$

Figure 2.5a: ICS-ext

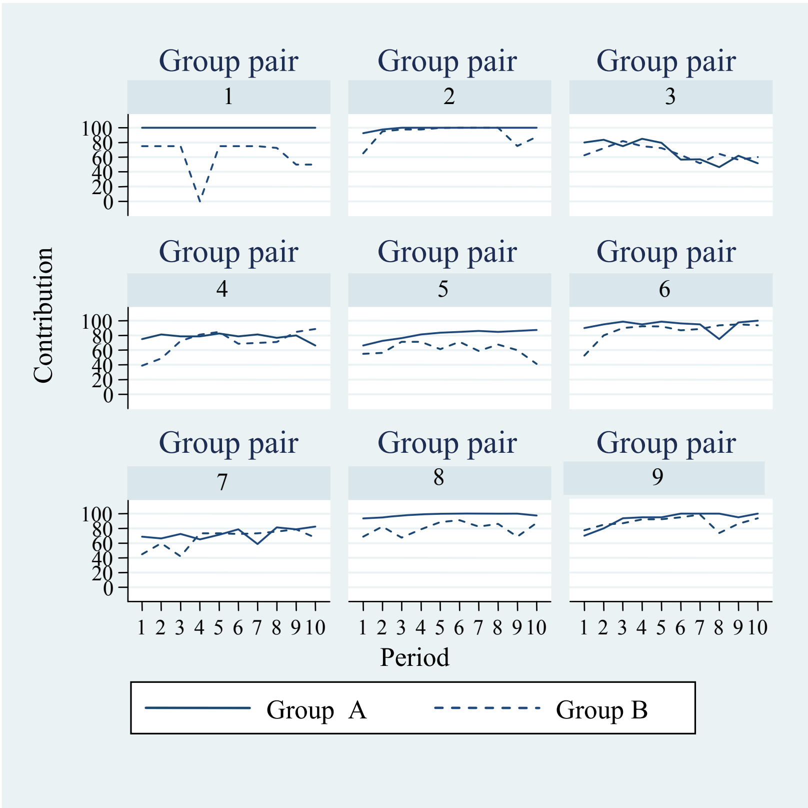


In this setup, it is easy to show that the two equilibria involve full contributions for all players in one group and no contributions by any player in the other group when $a/\mathrm{N} < 1$ and $a/\mathrm{N} + \delta > 1$. Thus, the current ICS-dom condition is predicted to provide higher contributions for both groups compared to when the funding is external, in which case only one group will fully contribute.

As an additional check, we ran an auxiliary condition ICS-ext where all conditions are identical to ICS-dom except that the group contributing the most receives the $\delta = 0.75$ transfer from the experimenter rather than from group contributing less. For ICS-ext, we had 64 subjects participate with 16 groups in S1, and 16 groups and 8 pairs of groups in S2.

Figures 5a and 5b show the average contributions for ICS-ext and ICS-dom, respectively, for each group contributing the most and the least within each pairing during S2. In six of the nine pairs in ICS-ext the differences in average contributions in the last period are at least 50 percentage points whereas in ICS-dom there are no pairings that

Figure 2.5b: ICS-dom



exceed a difference in contribution of 50 percentage points. Table 2.9 shows the average contribution in the last period of S2 for the ICS-ext and ICS-dom conditions for the groups that contributed the most and the least. As anticipated, there is little difference in the contributions by the groups that contributed the most in ICS-dom and ICS-ext (Mann Whitney $p = 0.318$). For the group that contributed the least, the ICS-dom subjects continued to contribute a high amount (Average = 71.00) while ICS-ext contributions collapsed, and the difference in contributions between the groups that contributed the least in ICS-dom and ICS-ext is highly significant (Mann Whitney $p < 0.01$). In sum, the ICS-dom theoretically increases contributions to participants in both groups whereas the externally funded ICS-ext scheme theoretically increase contributions to just one group, and the experimental evidence supports this distinction.

Table 2.9: Contributions in the last period of S2 in ICS-dom, ICS-EXT and the Control

	Group contributing the most		Group Contributing the least	
	Mean	s.d.	Mean	s.d.
ICS-dom (N=9)	90.69	13.30	71.00	20.37
ICS-EXT (N=8)	79.84	23.43	22.53	21.41

2.5.5 Other possible solutions

One important advantage of the proposed ICS is that it requires minimal information. The current ICS proposal only requires knowing the resulting aggregate contribution of each group. If more information is available, then additional solutions will also be available. For instance, as mentioned in section 2, other schemes have been proposed that can achieve greater efficiency if individual level contributions are known. Moreover, if more information is known, including the maximum possible contribution each team member can make, then an alternative solution exists that would indeed involve no intergroup competition at all, but rather would involve imposing a penalty to each team member in a group that is equal to δ times the difference between the maximum possible group contribution and the teams actual contribution. In this situation, the overall MPCR would again be $a/N + \delta$, so setting $a/N + \delta > 1$ would change the dominant strategy to full contributions. This solution has two concerns. First, losses can occur of equilibrium. Second, since effort is unobservable, determining the maximum output, which depends on effort, could be difficult since group members could have strategic motives to understate their maximum effort.

2.6 Summary

In this paper we proposed a solution to the free-rider problem. In theory, with the right parameters, $MPCR^{PG} + MPCR^{T} > 1$, the ICS induces the efficient contribution to the public good. Our experiments suggest that, to a large extent, optimality can be obtained, and sustained, in the lab. This result can be only partially explained by a ‘taste’ for competition. On the other hand, and in line with previous research (Palfrey and Prisbrey 1996; Brandts and Schram 2001; Tan and Bolle 2007), high contributions can be achieved with the ICS even if the combined MPCR is lower than one but only when the return from the public good is sufficiently high.

In contrast with previous schemes, the currently proposed ICS is budget balanced, so money does not need to be injected externally, and the internal transfer setup makes the optimal contribution a dominant strategy without having to rely on a taste for cooperation.[9] In addition, and what seems most practical, the ICS proposed here requires little extra information, and in the some cases where group aggregate contributions are already known, no extra information is required.[10]

These characteristics make the ICS useful in potentially many situations such as teamwork production. The introduction of competition within the firm through the ICS would alleviate the moral hazard problem and make both principal and agents (team members) better off in the Pareto sense. For the ICS to work it is enough to know the aggregate contribution (or just the aggregate output as a proxy); for other IC schemes to work the principal needs to know about individual contributions.

[9] In theory, without imposing much structure, the ICS would also work for pure and conditional cooperators.

[10] An alternative solution to the IC-dom scheme without the competitive element is to add to the public goods payoff scheme a penalty to be paid by all members of the group that is equal to δ times the difference in aggregate contributions of the group. In this setup, each player’s dominant choice is to contribute 100% of their endowment identical to the ICS-dom scheme when $a/N + \delta > 1$. However, the drawback of this setup is that additional information regarding the amount of full contribution is now needed. If this is not known, and so the target is set too high, all players would suffer losses, and if the target is set too low then players would only have an incentive to contribute up to the amount of the target.

Chapter 2 Appendix A: ICS Extensions

I show that the ICS results established in the text for n = 2 groups of equal size can trivially be extended to (1) any finite number of groups n ≥ 2 (A.1), (2) groups of different sizes (A.2), and (3) extended to non linear public goods games.

A.1 The ICS extended to any number of groups n ≥ 2:
For n ≥ 2, the ICS can be implemented with a balanced budget. I first demonstrate this with n = 3 groups: The payoff function for member *i* in group's A, B and C, respectively, are:

$$\pi_i^A = (w - t_i^A) + \frac{a}{N}\sum_{j=1}^{N} t_j^A + \delta\left(\sum_{j=1}^{N} t_j^A - \frac{(\sum_{j=1}^{N} t_j^B + \sum_{j=1}^{N} t_j^C)}{2}\right),$$

$$\pi_i^B = (w - t_i^B) + \frac{a}{N}\sum_{j=1}^{N} t_j^B + \delta\left(\sum_{j=1}^{N} t_j^B - \frac{(\sum_{j=1}^{N} t_j^A + \sum_{j=1}^{N} t_j^C)}{2}\right),$$

$$\pi_i^C = (w - t_i^C) + \frac{a}{N}\sum_{j=1}^{N} t_j^C + \delta\left(\sum_{j=1}^{N} t_j^C - \frac{(\sum_{j=1}^{N} t_j^A + \sum_{j=1}^{N} t_j^B)}{2}\right).$$

To satisfy a balanced budget, the transfers to/from member *i* in each group must sum to zero. This is indeed the case as the sum of the transfers:

$$\delta\left(\sum_{j=1}^{N} t_j^A - \frac{(\sum_{j=1}^{N} t_j^B + \sum_{j=1}^{N} t_j^C)}{2}\right) + \delta\left(\sum_{j=1}^{N} t_j^B - \frac{(\sum_{j=1}^{N} t_j^A + \sum_{j=1}^{N} t_j^C)}{2}\right) +$$

$$\delta\left(\sum_{j=1}^{N} t_j^C - \frac{(\sum_{j=1}^{N} t_j^A + \sum_{j=1}^{N} t_j^B)}{2}\right)$$

Reduces to:

$$2\sum_{j=1}^{N} t_j^A - \sum_{j=1}^{N} t_j^B - \sum_{j=1}^{N} t_j^C + 2\sum_{j=1}^{N} t_j^B - \sum_{j=1}^{N} t_j^A - \sum_{j=1}^{N} t_j^C + 2\sum_{j=1}^{N} t_j^C - \sum_{j=1}^{N} t_j^A - \sum_{j=1}^{N} t_j^B = 0$$

To generalize, note that with n > 3 groups, if there is an even number of groups then we can arbitrarily assign groups to parings to play the ICS. If there are an odd number of groups then we can arbitrarily choose three groups to play the ICS as described above, and the remaining groups can be arbitrarily assigned to pairings. It immediately follows from above that it is optimal for each individual in each group to contribute 100%.

A.2 The ICS extended to pairings of groups with different sizes

Let group A have N members and group B have M members and $N \neq M$. For simplicity, assume $w_i = w_j \forall w$. A modified ICS can be implemented where the aggregate contributions of the second group B are transformed into a value that is comparable to the aggregate contributions of group A. This is done by converting the aggregate contributions of group B into a proportion of its group's aggregate wealth then expressing this in terms of the aggregate wealth of group A.

$$\pi_i^A = (w - t_i^A) + \frac{a}{N}\sum_{j=1}^{N} t_j^A + \delta\left(\sum_{j=1}^{N} t_j^A - \frac{\sum_{j=1}^{M} t_j^B \cdot \sum w^A}{\sum w^{\mathbf{B}}}\right)$$

The payoff function for an individual in group B becomes:

$$\pi_i^B = (w - t_i^B) + \frac{a}{N}\sum_{j=1}^{N} t_j^B + \delta\left(\frac{\sum_{j=1}^{N} t_j^B \cdot \sum w^A}{\sum w^B} - \sum_{j=1}^{M} t_j^A\right)$$

Because only one group's aggregate contributions are transformed (in terms of the other) the inter-group MPCR (δ) is invariant to the transformation.

Further, this transformation still satisfies the condition for a balanced budget:

$$\delta\left(\sum_{j=1}^{N} t_j^A - \frac{\sum_{j=1}^{M} t_j^B \cdot \sum w^A}{\sum w^{\mathbf{B}}}\right) + \delta\left(\frac{\sum_{j=1}^{N} t_j^B \cdot \sum_{j=1}^{M} t_j^A}{\sum w^B} - \sum_{j=1}^{M} t_j^A\right) = 0$$

A.3 The ICS can apply to groups of any size

Both intra-group and inter-group MPCRs are invariant to increases in *N*. However, the maximum size of the inter-group transfer $\delta(\sum_{j=1}^{N} t_j^B - \sum_{j=1}^{N} t_j^A)$ increases with *N*. Large group sizes can increase the opportunity for bankruptcy in out of equilibrium play[11]. The ICS works best in small groups.

A.4 ICS Extension to a non linear public goods game

A non linear version of the public goods game has, in general, an interior solution. That is, optimality is achieved by allocating a positive amount lower than the endowment. In this case, under the ICS, there is a risk of overshooting and therefore providing too much of the public good. Overshooting can be avoided and efficiency can still be achieved in a more general public goods environment. Consider for instance the simplest of the Holmstrom (1982) team production models. In that model *n* individuals who take a costly non-observable action (that can be understood as a contribution to a public good) $a_i \in A_i = [0, \infty)$ with a private (nonmonetary) cost $v_i : A_i \rightarrow \Re; v_i$ is strictly convex, differentiable and increasing with $v_i(0) = 0$. Let $a = (a_1 ... a_n) \in A \equiv \underset{i=1}{\overset{n}{X}}$. The actions taken by the *n* individuals determine a monetary outcome $x : A \rightarrow \Re$, that must be allocated among them. The function *x* should be strictly increasing, differentiable and concave with $x(0) = 0$. Finally, $s_i(x)$ is the share of agent *i* in the output. The preference function of agent *i* is supposed to be additively separable in money and action and linear in money. Holmstrom demonstrates the non-existence of Pareto efficient, budget balanced sharing rules.

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[11] This is not the case with the size of the intra-group return $\frac{a}{N}\sum_{j=1}^{N} t_j^A$ which remains constant with increases of *N* due to its denominator.

In the Holmstrom model, efficiency can be achieved through inter-group competition. As earlier we assume there are two groups, A and B, with the same number of members. Let us suppose the sharing rule is simply $\frac{x^K}{n}$, $K = A$ or B, group members share the output equally. The maximization problem for a member of group A is

$$\max_{a_i^A} \frac{x^A}{n} - v_i^A(a_i^A) + \delta(x^A - x^B).$$

The first order condition characterizes the optimal effort: $(\frac{1}{n} + \delta)\frac{\partial x^A}{\partial a_i^A} - \frac{\partial v_i^A}{\partial a_i^A} = 0$. Following Holmstrom, Pareto optimality implies $\frac{\partial x^A}{\partial a_i^A} - \frac{\partial v_i^A}{\partial a_i^A} = 0$. Hence an efficient solution can be achieved through inter-group competition if $\frac{1}{n} + \delta = 1 \Leftrightarrow \delta = \frac{n-1}{n}$.

Chapter 2 Appendix B: Instructions

Directly below are the stage one experimental instructions used for all treatments.

Instructions

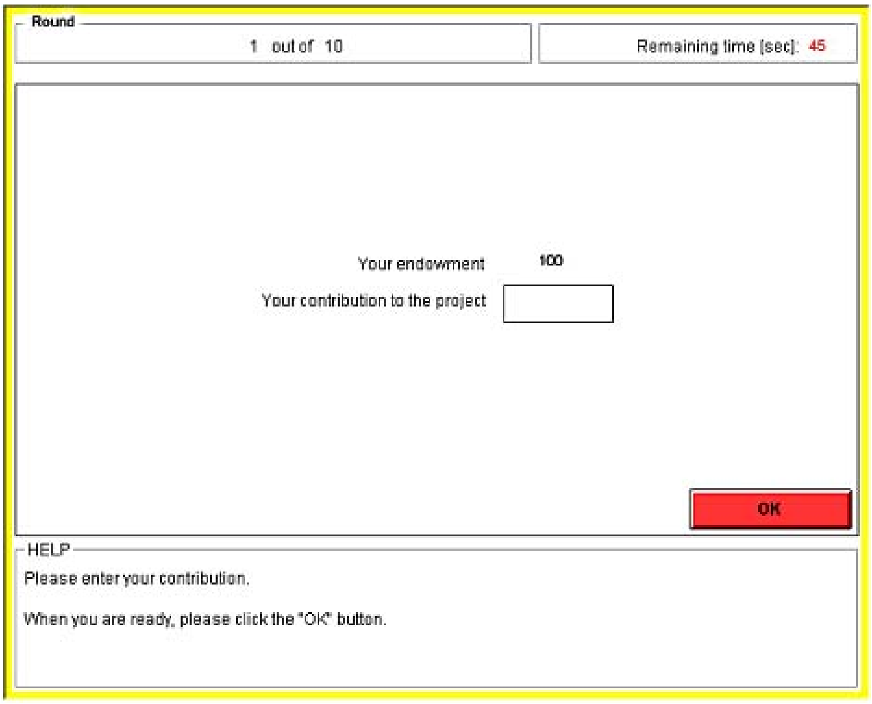
This is an experiment on the economics of decision making. Please do not communicate with each other during the experiment. If you have any questions, raise your hand and the experimenter will come and help you. If you read the instructions carefully, you will have the opportunity to make a significant amount of money based on the decisions you make in the experiment. During the experiment, you will earn money which will be paid to you in cash at the end of the experiment.

Procedure

There are *two* stages to the experiment. **Stage one** of the experiment is divided into 10 rounds. Participants are divided into groups of four before the first round commences. You will therefore be in a group with three other participants. **The composition of the groups will stay the same for all ten rounds.**

At the beginning of each round, each participant receives **100 cents**. We call this your endowment. Your task is to decide how to use your **endowment**. You have to decide how many cents you want to contribute to a **project** and how many of them to keep for yourself. The consequences of your decision are explained in detail below. At the beginning of each round the following input-screen for the first stage will appear:

Input screen

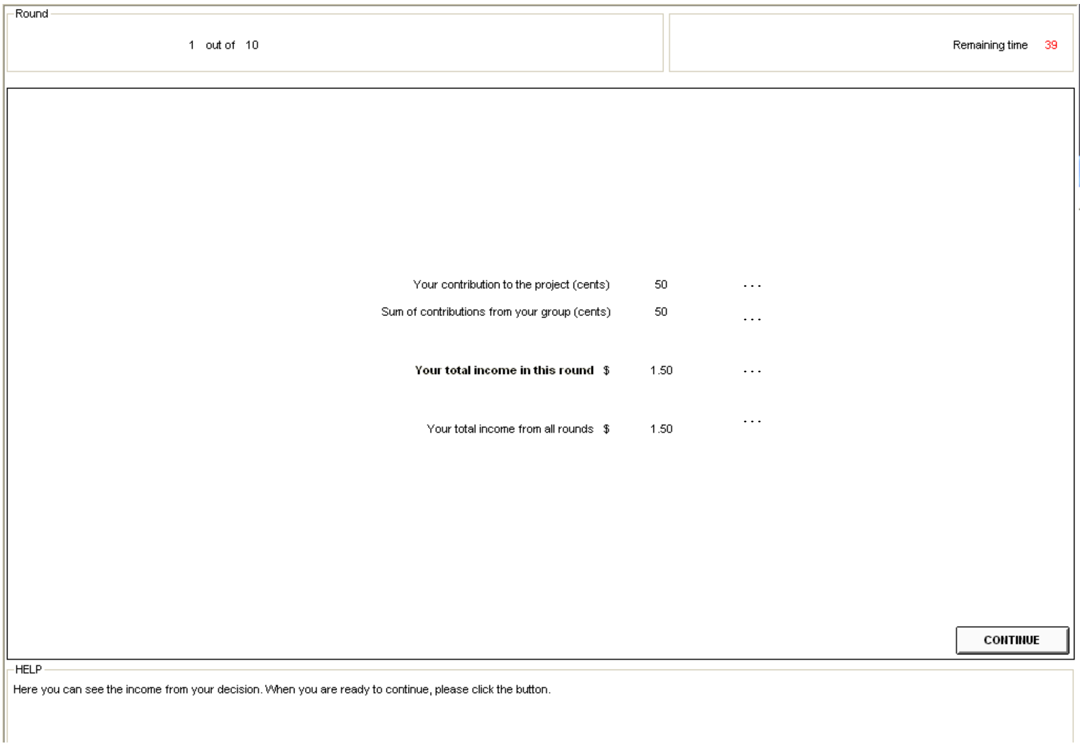


The round number appears in the top of the screen. In the top right corner you can see how many more **seconds** remain for you to decide on your contribution. You will have 45 seconds in the first two rounds and 30 seconds in the remaining rounds. Your decision must be made within the time limit.

Your **endowment in each round is 100 cents**. You have to decide how many cents you want to contribute to the project by typing a number between 0 and 100 in the input field. This field can be reached by clicking it with the mouse. As soon as you have decided how many cents to contribute to the project, you have also decided how many cents you keep for yourself: this is (**100 – your contribution**). After entering your contribution you must click the O.K. button. Once you have done this, your decision can no longer be revised.

After all members of your group have made their decision, the following screen will show you the total amount of money contributed by all four group members to the project (including your contribution). This screen also shows you how much money you have earned that round.

Result screen



Your **income** consists of two parts:

(1) The money which you have kept for yourself ; and

(2) The income received from the project. This is calculated as:

Income from the *project* = 0.5 *times* the total contributions to the project.

Your **income (in dollars)** of a round is therefore:

= [(100 – your contribution to the project) + 0.5*(total contributions to the project)] ÷ 100

The income of each group member from the project is calculated in the same way, i.e., each group member receives the same income from the project. Assume, for example, that the sum of the contributions of all group members is 150 cents. In this case each member of the group receives an income from the project of: 0.5*150 = 75 cents.

Each cent you keep for yourself will be added to your income. Suppose you instead contributed this cent to the project, then the total contributions to the project would rise by one cent. Your income from the project would rise by 0.5*1=0.5 cents. However the income of the other group members would also rise by 0.5 cents each, so that the total income of the group from the project would rise by 2 cents. Your contribution to the project therefore also raises the income of the other group members. On the other hand, you also earn income for each cent contributed by the other members to the project. For each cent contributed by any member, you earn 0.5*1=0.5 cents.

Do you have any questions?

---------------------------------Scratch paper for working----------------------

Control questions [*displayed on computer screen*]

Please answer all control questions. They serve as a test for your understanding of income calculations.

1. Each group member has an endowment of 100 cents. Suppose nobody (including you) contributes money to the project. What is:
Your income this round (in dollars)?...........
The income of the other group members?...........

2. Each group member has an endowment of 100 cents. Suppose you contribute 100 cents to the project. All other group members each contribute 100 cents to the project. What is:
Your income this round (in dollars)?...........
The income of the other group members?...........

3. Each group member has an endowment of 100 cents. Suppose the other three group members contribute a total of 60 cents to the project.
a) What is your income if you contribute zero cents to the project (in dollars)?...........
b) What is your income if you contribute 30 cents to the project?...........

Stage two instructions for the ICS-dom condition are below. The instructions and control questions for other treatments were adapted accordingly.

Instructions

The second stage of the experiment is **similar** to the first stage except that:

1. You will be assigned to a **new group** with three different participants. **None** of the participants in your last group will be in your new group. The composition of your new group **will stay the same** for all the following ten rounds.

2. Your new group will also be **paired with another group** in the room for ten rounds and your income will now **also depend** on the **difference** in contributions between your group and the other group.

   If the "**Difference of group contributions"** displayed in the results screen is a negative (-) number, then your group has contributed less than the other group. If it is a positive number, then your group has contributed more than the other group.

   If your group contributes **more**, then money will be **transferred to** you and the other members of your group from the members of the other group. If your group contributes **less** than the other group, then money will be **transferred from** you and the members of your group, to the members of the other group.

   The amount of money transferred **from each** person in the **group with the lower** total contributions will be **75 percent of the difference in group contributions**. **Each** member of the group with the higher contributions will receive 75 percent of the difference in group contributions.

   Note that it is possible to earn a **negative** income in a round. Any losses in a round will be covered by your previous earnings in stage one.

Procedure

Stage two of the experiment is divided into 10 rounds. Participants are divided into groups of four before the first round commences. You will therefore be in a group with three other participants. **The composition of the groups will stay the same for all ten rounds** and the group that you are **paired** with will stay the same for all ten rounds.

At the beginning of each round, each participant receives **100 cents**. We call this your endowment. Your task is to decide how to use your **endowment**. You have to decide how many cents you want to contribute to a **project** and how many of them to keep for yourself.

Your **income is calculated differently** in stage two. There is now an **extra component** where your income can either be increased or decreased by 75 percent of the difference in total contributions between your group and the group you are paired with.

Your **income (in dollars)** of a round

= [(100 – your C) + 0.5*(Sum your group's C) + 0.75*(Sum your group's C – Sum other group's C)] ÷ 100

C: contribution to the project

The experiment will conclude at the end of stage two. After stage two you will be asked to answer a few questions and then you will be paid anonymously and privately before you leave.

Do you have any questions?

----------------------------------Scratch paper-----------------------------------

Control questions [*displayed on computer screen*]

Q1. Each group member has an endowment of 100 cents. Suppose nobody in your group (including you) contributes any money to the project and the other group has a **total** contribution of 200 cents. What is:

a) Your income this round (in dollars)? $ **…….**
b) The income of the other members of your group? $ **……**

Q2. Each group member has an endowment of 100 cents. Suppose you contribute 100 cents \b0 to the project. All of your group members also **each** contribute 100 cents to the project and the other group has a total contribution of 200 cents. What is:

a) Your income this round (in dollars)? $ **……**
b) The income of the other members of your group? $ **……**

Q3. Each group member has an endowment of 100 cents. Suppose the other three group members contribute a **total** of 60 cents to the project and the other group has a total contribution of 200 cents.

a) What is your income if you contribute **zero** cents to the project (in dollars)? $ **……**
b) What is your income if you contribute 40 cents to the project $ **……**

Chapter 3: Social Comparisons and Cooperation*

3.1 Introduction

It is common for production to be done in teams to fully utilize a broader range of skills, information and talents. However, one drawback is the potential incentive it creates for workers to free ride on the efforts of others (Olson, 1965; Liden, Wayne, Jaworski, and Bennett, 2004). Performance benchmarking, where firms compare their performance to other firms, is one channel firms can use to motivate free-riders to cooperate more by contributing greater effort. This study will gauge the effectiveness of different types of comparative information that might be used in benchmarking programs and recommend optimal designs for implementation.

Research shows that social comparisons affect behavior. Frey and Meier (2004) for example, show that providing information to undergraduates, that told them that most other students donated to charitable funds, increased their likeliness to contribute. Shang and Crosen (2009) investigated the effect of providing a comparison to donors to a public radio station and found that subjects donated more on average given a high comparison. Croson and Shang (2008) then went on to investigate the effect of downward social comparisons on donations and found that subjects adjusted their donations downward in the direction of the given comparison. Their studies suggest that social comparisons may affect behavior via people's motivation to conform to a norm. This study will address the gap of previous studies that typically provide only single comparative information between the individual and their in-group by examining the effect of providing comparative out-group information. Little is known of which type of comparative information is most salient (in-group or out-group) in changing an individual's revealed preferences and the interaction effects (if any) of in and out group comparisons.

*I am grateful to Simon Gächter, Robert Slonim, Pablo Guillen and Stephen L. Cheung for their helpful suggestions and comments.

In this study, the effect of comparative information in a public goods situation is tested using a linear public goods laboratory experiment. Two manipulations are performed. The first is the presence of in-group or out-group contribution information given to subjects. The second is the effect of a high (upward) out-group comparison verses the effect of low (downward) out-group comparison. The results of this study show that in-group comparisons initially raise contributions with subjects appearing to subsequently adjust their contributions in the direction of the comparison consistent with (Croson and Shang, 2008). In contrast, providing downward out-group comparisons in addition to in-group comparisons further raises contributions. This result can be explained by the differences in how in-group and out-group comparisons affect cooperation.

The practice of performance benchmarking was popularized by the success of Xerox's benchmarking scheme introduced in the 1980s. The photocopier company was able to significantly raise its market position by lowering its production costs by comparing their costs to their Japanese competitors (Camp, 1993). The practice was subsequently taken up by managers in many industries from education to logistics. By 1995, benchmarking was so embedded as a key management strategy that over 40 books had been published specifically on the topic (Yasin, 2002). In contrast to process benchmarking, which compares organizational processes, this chapter is concerned with the more traditional competitive benchmarking analysis that has been used for decades as a means of comparing a firm's sales, costs, and output to that of their competitors (Yasin, 2002). Although the amount of academic literature published on benchmarking has increased in line with its use in the business sector, Dorsch and Yasin (1998) observe that most of the knowledge we have on benchmarking has come from the results of practitioners' efforts. Yasin (2002) also highlights the problem that the field of benchmarking lacks a unifying theory to advance academically. This paper goes some way to addressing this problem by empirically testing three competing hypothesis explaining the effects of benchmarking comparisons.

In a review of 382 publications on benchmarking, the majority (at 50 percent) were case studies on specific applications and innovations (Dattakumar and Jagadeesh, 2003). While case studies provide a rich source of information, the idiosyncratic nature of this methodology makes it difficult to develop any unifying theory to guide the development of future benchmarking schemes. Laboratory experiments can illuminate new knowledge on benchmarking previously unavailable from field research. The effects of internal and competitive benchmarking can be disentangled in the lab because we are able to control all aspects of the environment. We can then determine the "pure effect" of an intervention, without it being confounded by endogeneity and other factors such as management style, firm culture, and the economic climate. Another advantage of investigating benchmarking using a laboratory experiment is that many more observations can be rigorously tested than is typically available in a natural environment.

This study will also make a contribution to the literature on identity and inter-group rivalry. Previous experimental studies involving out-group identity have demonstrated its effect of potentially causing inter-group conflict by inducing in-group favoritism and out-group discrimination (Rabbie and Horwitz, 1969; Tajfel, Billig, Bundy, and Flament, 1971; Chen and Li, 2009). Eckel and Grossman (2005) found that voluntary contributions in the Public Goods Game could be raised by enhancing the in-group identity strength. Inducing a common group identity was similarly found to raise effort levels in the Minimum Effort Game (Chen and Chen, 2011). This study will examine whether the presence of information on an out-group raises contributions via inter-group rivalry. Under such a mechanism, comparing contribution levels to a high contributing out-group would induce higher contributions in an attempt to beat the other team. However, competition in the Public Goods Game need not be a race to the top. It could well be that we find that teams compete in a race to the bottom by competing to contribute less than the other team. In either case, the important theoretical feature of this mechanism is that the effect of the information is asymmetric. Under the race to the top (bottom) case, low (high) out-group comparisons should have no effect on in-group contributions.

Another alternative explanation on why social comparisons may change behavior is that it makes a social norm more explicit. This story is one of conforming. Less ambiguity in what the "correct" behavior is results in higher conformity to the norm. Under a conformity mechanism, additional out-group contribution information in the Public Goods Game would move contributions of the in-group towards the level of contributions of the out-group. If the out-group contributions were low, then the comparative information would lower contributions, and if the out-group contributions were high, the comparative information would raise contributions. In a similar study to this one, Bardsley and Sausgruber (2005) examine the effects of conformity in Public Goods Game contributions with a design where individuals in one group observe the contributions of another group. Their study was a correlational study. The authors make the conclusion that any observed correlation is evidence for conformity. Such a conclusion ignores the possibility of individuals responding to inter-group rivalry. By controlling the comparative information given to individuals this paper is able to dis-entangle the effects of conformity and inter-group rivalry and move beyond correlational inferences.

Previous research suggests that subjects in repeated Public Goods Games tend to behave in a conditionally cooperative manner (see, for example, Keser and van Winden (2000); Fischbacher and Gächter (2010)). Individuals will only contribute if others contribute as well. This implies that in-group comparative information has the potential to not only raise contributions of the below average contributors but lower contributions of the above average contributors. Neugebauer, Perote, Schmidt, and Loos (2009) compared contributions of a ten round VCM with no feedback to a condition with feedback on the sum of the other members' contributions after each round. They found contributions were higher on average in the no information condition. They concluded that the information drove contributions down by the mechanism of selfish-biased conditional cooperation and the downward adjustment of the beliefs of what others will contribute. Under selfish-biased conditional cooperation, individuals prefer to contribute if others contribute, but at a level a little below everyone else (Fischbacher et al., 2001). If in-group information drives contributions down, it might be reasonable to hypothesise that the addition of comparative out-group information would further drive contributions down because of the additional information from the out-group to condition upon. Out-group comparisons would therefore have the effect of further reducing contributions over time. The implications of this result would have direct implications for the effectiveness of benchmarking programs.

I use a computerized Public Goods Game using the Voluntary Contributions Mechanism (VCM) (see Davis and Holt (1993); Ledyard (1995)) to test the null hypothesis that comparative out-group information has no effect on contribution preferences. In other words, the null hypothesis predicts that participants' payoffs are only affected by the aggregate contributions of their own group not the other group. A purely rational subject will disregard the information on the other group's contributions.

This chapter is structured as follows. Section 2 provides a theoretical framework for the investigation. Section 3 contains the experimental design. Section 4 explains the competing hypotheses. Section 5 summarizes the results and section 6 concludes.

3.2 Theoretical framework

In the absence of any comparative information, let an individual i's chosen level of cooperation be x_0. Let $x_0 \in [0, 100]$ be a function of many potential dimensions such as payoff and priors such as the beliefs of others' contributions. The exact function of x_0 is not important in this framework as the predictions of the following models, as we will see, do not depend on x_0.

Drawing on the approach of Bolton and Ockenfels (2000) and Fehr and Schmidt (1999) and further guided by experimental literature in behavioral economics, I propose three falsifiable models as a motivation for behavioral change induced by social comparisons[1].

In each of the models, x captures the revealed choice of cooperation in the presence of comparative information. This revealed choice x may be a function of a social norm (see 3.2.1), or it may be a function of rivalry (see 3.2.2) or it may be a function of selfish bias (3.2.3). The three models nest a range of parameters that are falsifiable by the different predicted marginal effects of the comparative information x_c on contribution x.

The models can be applied separately to in-group and out-group comparisons.

3.2.1 Norm Behavior (Conforming Mechanism)

I apply a model developed by Benjamin, Choi, and Fisher (2010) which was originally inspired by Akerlof and Kranton (2000) to represent a norm preference relation. Let x denote an individual's contribution and x_c the comparative contribution such that the individual chooses x to maximize:

$$U = -(1-\theta)(x-x_0)^2 - \theta(x-x_c)^2 \text{ where } 0 < \theta \leq 1, x \in [0, 100] \quad (3.1)$$

Under this model, deviating from the norm (x_c) causes a disutility. The weight θ represents a trade off between choosing a contribution close to the individual's preference in the absence of comparative information (x_0) and choosing a contribution level closer to the norm. By definition, x_0 is exogenous. Any effect of x_c on behavior is captured by x. The first-order condition for (3.1) gives the optimal action $x^* = (1-\theta)x_0 + \theta x_c$

3.2.2 Intergroup Rivalry Mechanism

Under an inter-group rivalry mechanism I extend the model to incorporate competitive influences between the groups. Now, disutility from choosing an x different from x_c is in the loss domain only. Under

[1] Drawing on literature on social norms (for example Ostrom (2000); Fehr and Gächter (2000)). Inter-group rivalry (for example (Amnon Rapoport, 1987; Bornstein, 2003) and selfish-biased conditional cooperation (for example Fischbacher and Gächter (2010)).

this mechanism, there is no utility lost from contributing more than the comparative level.

$$U = -(1-\theta)(x-x_0)^2 - \theta(\min\{(x-x_c),0\})^2 \text{ where } 0 < \theta \leq 1 \quad (3.2)$$

When $x_c < x_0$ the optimal action is $x^* = x_0$. When $x_c > x_0$ the optimal action is $x^* = (1-\theta)x_0 + \theta x_c$.

3.2.3 Selfish Bias Mechanism

Under a selfish biased mechanism, an individual chooses x to maximize:

$$U = -(1-\theta)(x-x_0)^2 - \theta(x-(x_c-\epsilon))^2 \text{ where } 0 < \theta \leq 1, \epsilon > 0 \quad (3.3)$$

This is very similar to the norm relation except that individuals now prefer to contribute ϵ units less than x_c. The first-order condition for (3) yields the optimal action $x^* = (1-\theta)x_0 + \theta(x_c - \epsilon)$.

3.2.4 Theoretical Predictions

Let H denote a high comparison and L denote a low comparison. Then the derivative of x^* with respect to x_c yields the following predictions:

H_1 Norm (conforming) behaviour $\quad \dfrac{\partial x^*}{\partial x_c} > 0, |x_0 - x^*H| = |x_0 - x^*L|$

H_2 Inter-group rivalry $\quad \dfrac{\partial x^*}{\partial x_c} \begin{cases} = 0 & \text{if } x_c < x_0 \\ > 0 & \text{if } x_c > x_0 \end{cases}$

H_3 Selfish biased conditional cooperation $\quad \dfrac{\partial x^*}{\partial x_c} > 0, |x_0 - x^*H| < |x_0 - x^*L|$

These predictions are used to guide the experimental design of this study. From this, an experimental manipulation of x_c can directly test an inter-group rivalry hypothesis against the competing two hypotheses. How might we distinguish norm behavior from selfish biased conditional cooperation? Note that if $x_c > x_0$ then, $\epsilon > 0$, and when $x_c < x_0, \epsilon < 0$. In the first case (H), under selfish biased conditional cooperation, the distance between x_0 and x^* is reduced by ϵ, and the distance is increased by ϵ in the second case (L) giving rise to the following asymmetry . No such asymmetry is predicted for norm behavior. If we assume that in the absence of $x_c, x^* = x_0$, then norm behavior can be tested against selfish biased conditional cooperation econometrically using baseline conditions that withhold x_c.

3.3 Experimental Design

The effect of comparative information in a public goods situation is tested using a standard Voluntary Contributions Mechanism (VCM). Participants are given the same endowment w so that each participant has the same budget. In this study, $w = 100$ cents (AUD). Participants interact in groups of N. Each individual has to decide how much of his endowment to allocate to a public account ti and how much to keep for himself $w - t_i$. For each group, the sum of the individual allocations to the public good $\sum_{j=1}^{N} t_j$ is then multiplied by a factor a (where $N > a > 1$) to model the additional value generated from the public nature of the good. The final value of the public account is then shared equally among the group members. The payoff therefore of player i under a VCM is given by:

$$\pi_i = (w - t_i) + \frac{a}{N} \sum_{j=1}^{N} t_j$$

A factor of $a = 2$ is used in this experiment among groups of $N = 4$ subjects yielding a marginal per capita return (MPCR) of 0.5. The experiment involved a 2 x 2 factorial (out-group comparisons: high/low, and in-group comparisons: available/not available) between subjects design (Fig. 3.1). The in-group comparison in this study is the comparison between a subject's own contribution and the aggregate contributions of their group. The out group comparison is the comparison between a subjects own contribution and the aggregate contributions of another group (this was manipulated by the experimenter). A total of 192 university students were recruited to participate in one of 9 experimental sessions of the computerized VCM experiment (Fischbacher, 2007; Greiner, 2004) conducted at the Behavioral Experimental Lab at the University of Sydney. To avoid duplication, the standard VCM game baseline data used in this study were generated from subjects in a prior study (Guillen, Merrett, and Slonim, 2012) which involved 328 subjects in addition to the 192 subjects specifically recruited for this study. The instructions used in both studies were identical (full instructions contained in supplementary material). The game consists of 10 rounds. This experiment uses partners matching, so that a subject's group members remains the same for the entire 10 rounds. In each of the ten rounds subjects receive an endowment of 100 cents and are asked to decide how many cents to contribute to a public account and how much of their endowment to keep for themselves. At the end of each round, total contributions to the public project are multiplied by two and then shared equally between the four members of their group. The instructions carefully explain the experiment within a neutral frame and how subjects' payoffs are calculated using a formula and examples[2].

[2] The instructions were derived from those originally created by (Herrmann, Thöni, and

Table 3.1: Summary of experimental conditions

Baselines	In-group feedback	Low (Out-group Comparison)	High (Out-group Comparison)
C1 (zero feedback) 24 subjects	**No feedbac**k	T1 24 subjects	T2 24 subjects
C2 (VCM) 320 subjects	**Feedback**	T3 60 subjects	T4 60 subjects

Notes: Conditions T3 and T4 have more subjects than C1, T1 and T2, because the unit of independent observation is at the group level.

In the C1 control, subjects are given no feedback until the end of the 10 rounds. The C2 control is the standard VCM game where subjects are given feedback on their group's aggregate contributions after each round. In conditions T1 and T2 subjects are only given feedback on the total contributions made to the public project by another group and not their own. The subjects are simply told that their group will be paired to another group and that the other group will remain the same for the entire 10 rounds. In T1, the "other group" is a group pre-selected from the set of real contribution data from the C1 VCM condition which consisted of 82 groups. The group contributing in the bottom 5 percentile was chosen for the "low" out-group comparison[3]. In T2, the group contributing in the top 5 percentile was chosen as the "high" out-group comparison. The data from these groups was removed from the C2 VCM dataset and are displayed in Fig. 3.1. The T1 and T2 conditions were conducted during the same session with half the groups receiving the high out-group comparison and half the groups receiving the low comparison. Conditions T3 and T4 were different from T1 and T2 only in that they also received feedback on their own group's aggregate contributions as well as the low/high out-group feedback.

3.4 Falsifying Hypotheses

Using the theoretical predictions derived in section 2, the criteria in Table 2 can be used to falsify each and all models. The criteria are outcomes that would reject the theoretical predictions. Mean contributions in the presence of comparisons can be assessed against the criteria using a statistical level of significance.

There may be one model that predicts the effects of in-group comparisons and another model that predicts the effects of out-group comparisons. The

Gächter, 2008)

[3]The choice of the top and bottom 5th percentile was guided by the results of Shang and Crosen (2009) who found that the most influential level of social comparison information was drawn from the 90th to the 95th percentile of previous contributions.

Period	Low comparison	High comparison
1	85	310
2	50	320
3	35	320
4	20	295
5	35	290
6	75	270
7	90	300
8	95	285
9	60	295
10	40	210

Figure 3.1: Out-group comparison information for low and high treatments

models are not assumed to be the same between the different comparisons. The criteria therefore can be applied separately to revealed choices under in-group comparisons and revealed choices under out-group comparisons. The out-group predictions can be tested using aggregate analysis of mean contributions by condition and the in-group predictions can be tested using individual panel regressions.

3.5 Results

3.5.1 Aggregate Analysis

Average group contributions between conditions were found to be significantly different (Kruskal-Wallis $p<0.01$). The condition with both in-group and a high out-group comparison, T4, yielded the highest average contribution of all the conditions with an average more than double that of the condition with no comparisons C2 (aggregate contributions are summarized in Table 3.3).

Result 1. The more social comparisons provided, the greater the contribution

In the absence of any comparisons, Table 3.3 shows the mean contribution for C1 is 24.23. When in-group aggregate contribution information is provided average contributions jump to 40 percent. When subjects are provided with a high out-group comparison with no information on their own group's contribution, average contributions rise from 24.23 to 39.82 percent. The highest contributions resulted when both in-group and out-group social comparisons were provided.

Table 3.2: Criteria for falsifying hypotheses

Model	**Falsified by:**
1. Conforming (H_1)	• Upward (high) comparisons have a negative effect on contributions • Downward (low) comparisons have a positive effect on contributions • Downward (low) comparisons have a greater effect on contributions
2. Inter-group Rivalry (H_2)	• Upward (high) comparisons have a negative effect on contributions • Downward (low) comparisons have a greater effect on contributions
3. Selfish Biased Cond. Coop. (H_3)	• Upward (high) comparisons have a negative effect on contributions • Downward (low) comparisons have a positive effect on contributions

Table 3.3: Mean, median contributions and Mann-Whitney

Condition	**Mean**	**St Dev**	**Independent Observations**	**t-test (condition/control)**
Out-group only comparison				
C1 (zero info)	24.23	27.09	24	
T1 (low)	28.84	28.85	24	p=0.57
T2 (high)	39.82	27.50	24	p=0.05
				p=0.18(T1/T2)
In + out-group comparison				
C2 (VCM)	40.60	18.18	80	
T3 (VCM + low)	50.15	22.12	15	p= 0.10
T4 (VCM + high)	53.09	18.69	15	p= 0.02
				p= 0.61 (T3/T4)

Notes: For conditions C2, T3 and T4 the appropriate independent observation is average group contribution. As C1, T1 and T2 received no in-group feedback during the experiment, average individual contributions was used as the independent observation.

Result 2. Out-group information raises cooperation through competitive motivations.

The rivalry model predicts that only high comparisons will have a significant effect on average contributions (H_2, or conversely only low comparisons if groups compete to contribute the least. Whereas the other two models predict that in addition to high comparisons raising contributions (H_1, H_3), low comparisons will also significantly reduce average contributions. The results from Table 3 support the rivalry hypothesis for out-group comparisons. The average contributions of the high comparison conditions T2 and T4 were both significantly higher than their controls. Whereas the low comparison condition T1 was not significant at a 5 percent level in the out-group only condition.

Result 3. Interaction effects between in-group and out-group comparisons.

Introducing in-group comparisons results in an interaction where low out-group comparisons also marginally raises average contributions (p=0.10) (Table 3.3). This is interesting because low comparisons had no effect on contributions in the absence of in-group comparisons. While the effect is statistically marginal, average contributions are raised by 10 percentage points, which is a non-trivial increase in average contributions.

3.5.2 Temporal Analysis

The C2 control condition replicates the temporal results of earlier VCM experiments (Ledyard, 1995) where mean contributions start between 40 percent to 60 percent of the endowment and decline to close to zero (Figure 1). Figure 1 shows that although contributions in the C1 zero information condition did decline in early periods, they were relatively constant for the rest of the game. This result is consistent with the results of both Neugebauer, Perote, Schmidt, and Loos (2009) and Sell and Wilson (1991) no information 10 round VCM game conditions.

Result 4. In-group information raises cooperation through anticipation.

An interesting observation can be made by observing the intercepts of the first column in Figure 3.2 and comparing them to the intercepts of the second column where in-group comparisons are now provided. Significantly higher first period contributions are observed[4] in the conditions where in-group feedback is to be provided compared with conditions where no in-group feedback is provided (t-test p=0.004). Simply telling subjects that they will receive information on the aggregate contributions of their own group raised their contribution before they even received any information. The anticipation of the information drove an increase in contributions as opposed to the information itself. This result is consistent with Sell and Wilson (1991) who also found higher contribution levels when in-group feedback was provided to subjects.

3.5.3 Individual Analysis

Panel analysis provides greater power to test the effects of condition comparisons on contribution levels. Table 4 contains estimations for condition dummies using random effects estimation[5]. Robust standard errors are clustered at the individual level while Period dummies were included because mean contributions are observed to be changing over time.

Result 5. High out-group comparisons have a stronger effect than low comparisons

[4]Each contribution can be treated as an independent observation for statistical analysis for first period contributions because subjects at this stage receive no feedback from about their group members' contributions

[5]Equation (1) was also estimated using Tobit Random Effects estimation but the results were subsequently excluded because Tobit-Random Effects estimates of VCM data were found to be highly biased and less precise than Random Effects estimates (Merrett, 2012).

Table 3.4: Estimation results: Random Effects

Variable	**Coefficient**	**(Std. Err.)**
Dependent variable: Contribution		
C2 In-group	16.442***	(5.599)
T1 Low out-group	4.621	(7.845)
T2 High out-group	15.592**	(7.731)
T3 In-group + Low out-group	25.928***	(6.346)
T4 In-group + High out-group	28.867***	(6.308)
Period 2	2.680**	(1.169)
Period 3	0.349	(1.351)
Period 4	-2.583*	(1.488)
Period 5	-4.610***	(1.552)
Period 6	-9.105***	(1.620)
Period 7	-11.051***	(1.707)
Period 8	-15.375***	(1.768)
Period 9	-20.958***	(1.857)
Period 10	-30.676***	(1.902)
Intercept	33.358***	(5.497)
R-Squared (overall)	0.109	N=5440
Rho	0.500	

Notes: Robust standard errors are clustered by subject. Baseline condition is C1 (no feedback between periods). Hausman Test: ($Prob > chi^2 = 1$). Level of significance denoted as $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

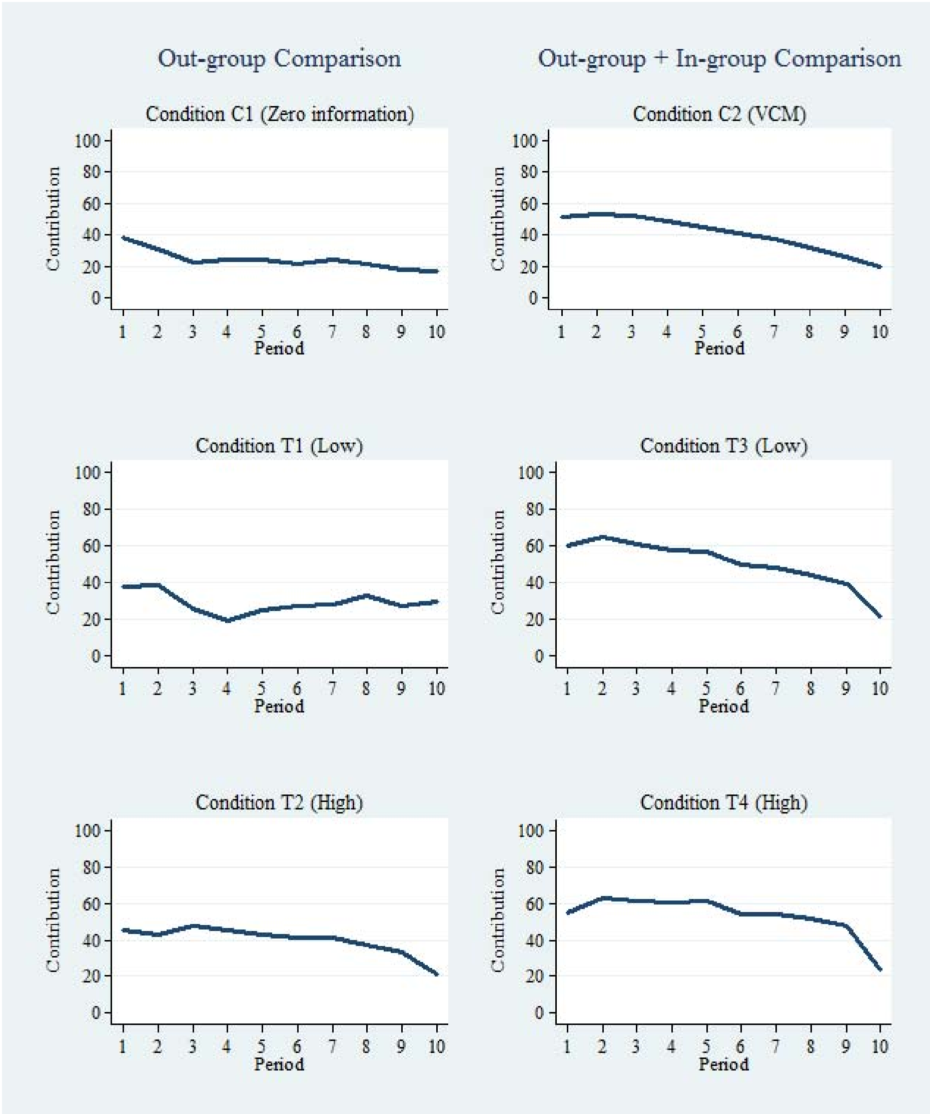


Figure 3.2: Mean contributions over time. Higher intercepts are observed in the second column conditions where in-group comparisons were given.

The regressor estimates in Table 4 are discussed in the order of the estimated parameter size. High (upward) out-group comparisons have a greater impact on contributions than low (downward) out-group comparisons. However low out-group comparisons did not lower contributions as would be predicted under a conformity model. In the presence of in-group comparisons, the addition of low out-group comparisons (T3) *increased* average contributions. The greatest marginal effects of social comparisons are observed when in-group comparisons are coupled with high out-group comparisons (T4).

To investigate the dynamic influence of out-group and in-group comparisons on contributions (C), I estimate the following Instrumental Variable (IV) dynamic panel model:

$$\Delta C_{it} = \beta_0 + \lambda \Delta C_{it-1} + \sum \beta_k \Delta x_{kit-1} + \sum \beta_j \Delta P_{jt} + \Delta u_{it}$$

The dependent variable ΔC_{it}, allows us to understand what may have motivated an individual to change their contribution from one period to the next. Period dummy variables (P) are added to control for variation in C over time. The estimation results of above IV model are contained in Table 5. The regressor *LD.outgroup comparison* LD stands for lagged difference in Table 5 measures the comparative out-group information effect on the change in contributions in the next period. Similarly, the effect of presenting aggregate contributions of one's own group on the choice of contribution in the next round is captured by *LD.ingroup comparison.* The significance (at 1 percent level) of *LD.ingroup comparison* in model 2 suggests that subjects adjust their contributions upward in response to higher average contributions of their group members. To econometrically test the presence of in-group selfish biased conditional cooperation the interaction variable *LD.more*ingroup comparison* was used to test the theoretical asymmetry $\mid x_0 - x^*H \mid < \mid x_0 - x^*L \mid$ The variable *low* is a dummy variable denoting T3 and *high* is a dummy variable denoting T4.

Result 6. No significant dynamic effects from out-group comparisons

There are no significant dynamic effects from out-group comparisons (model 1) even when they are coupled with in-group comparisons (model 2). A distinction should be made between dynamic effects and non-dynamic effects. Dynamic effects measure the responsiveness in contribution choice to changes in the comparison. It does not imply that out-group comparisons have no effect on average contributions at all , but simply that any dynamic changes appear to have no effect.

Interestingly, the variable *LD.contribution* is not significant, indicating that when only out-group information is provided, an individual's previous contribution is not predictive of their choice of contribution.

Result 7. Significant in-group dynamic effects

The significance of the in-group comparison variables in model 2 of table 5 suggests that subjects adjust their contribution according to the contributions of their co-members. The significance of the variable *LD.ingroup comparison* (Table 5) suggests that individual contributions are adjusted upwards on average by 0.6 cents for every additional cent contributed by their co-members in the previous period.

Table 3.5: Dynamic Effects IV-Panel Regression

Dependent variable: D.Contribution				
	Model 1 (N=336) Out-group feedback conditions		Model 2 (N=1008) In-group feedback conditions	
Variable	**Coefficient**	**(Std. Err.)**	**Coefficient**	**(Std. Err.)**
LD.Contribution	0.009	(0.116)	0.490***	(0.164)
LD.Out-group comparison	-0.289	(0.385)	0.087	(0.465)
LD.In-group comparison			0.576***	(0.121)
LD.More*in-group comparison			-0.488***	(0.111)
D.Low*out-group comparison			-0.132	(0.437)
D.High*out-group comparison			0.389	(0.319)
D.Period 3	-26.083	(33.553)	-69.444**	(30.972)
D.Period 4	-25.354	(27.847)	-55.514**	(26.748)
D.Period 5	-19.881	(25.425)	-42.633*	(23.679)
D.Period 6	-13.467	(18.729)	-34.501*	(18.743)
D.Period 7	-7.123	(13.596)	-18.560	(13.129)
D.Period 8	-0.122	(7.847)	-9.193	(7.561)
Intercept	-5.358	(5.120)	-12.244**	(5.287)

Notes: *D.Contribution*=subject's current period contribution - previous period contribution; The instrumental variable, $Contribution_{t-2}$ was used as the instrument for *LD.Contribution* (lagged difference contribution); *LD.Out-group Comparison*=lagged difference in other group's average contributions; *More*=1 if own contribution > average contributions of other members in subject's own group and =0 if otherwise; *Low*=1 if T3 condition, and =0 if otherwise; *High*=1 if T4 condition and=0 if otherwise; Robust standard errors are clustered by subject. Level of significance denoted as $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

The interaction variable *LD.more*ingroup comparison* tests for selfish-biased conditional cooperation. The negative sign of the estimated parameter indicates that subjects adjust their contributions downwards if they contributed more than their co-members on average in the previous round. The significance level (1 percent) indicates the adjustment effect is on average greater when subjects contributed more than the average contributions of their co-members in the previous round than when they did not contribute more. This is consistent with the findings of Fischbacher et al. (2001) and Neugebauer et al. (2009).

3.6 Conclusion

This chapter demonstrates that social comparisons raise cooperation. The results suggest that in-group comparisons raise cooperation through selfish biased conditional cooperation while out-group comparisons raise cooperation by inducing rivalry. Cooperation is more dynamically sensitive to changes in in-group comparisons than out-group comparisons.

There also appears to be an interaction effect between in-group and out-group information. Low out-group comparisons start to induce higher contributions when in-group information is provided. The combination of in-group and out-group comparisons could create a situation of hyper-rivalry where the players are not simply satisfied to beat the other group, but now want to beat them by the biggest margin possible.

This study suggests that the optimal benchmarking scheme is one where feedback is given on both the in-group and an out-group. But not just any out-group, the best results come from comparing against the best out-group. A benchmarking scheme where individuals are only compared to the performance of an out-group, say for example another company, will be limited. The information becomes more meaningful when the performance of the out-group can be compared to the performance of one's own group. In this situation, even downward out-group comparisons have the potential to raise in-group cooperation.

Further research exploring the effects of being observed could follow on from this study. The large difference in period one effects between the in-group comparison conditions and the no in-group comparisons suggest these effects could be quite important. A follow up experiment that might be designed is one where the subjects would not observe the comparative information but would know that other subjects were observing the information. For example, to test the effect of being observed by an out-group, subjects would play a 10 period VCM game knowing that their aggregate contributions were being observed by another group without having the opportunity to observe the aggregate contributions of another group themselves. Similarly this could be done with in-group feedback. The effects of observation using out-group comparisons could be compared to the effects of observation using in-group comparisons.

This chapter explores how to incorporate out-group comparisons into the literature of social comparisons. It shows that out-group comparisons are important in influencing behavior and that they do not influence behavior in the same way as comparisons within one's own group.

Chapter 3 Appendix

C1 CONDITION INSTRUCTIONS

Instructions

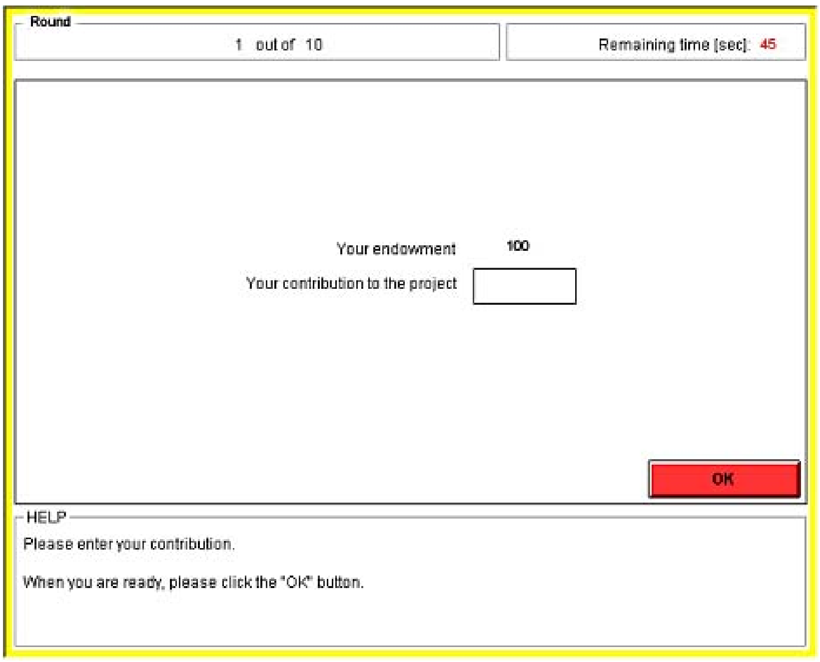
This is an experiment on the economics of decision making. Please do not communicate with each other during the experiment. If you have any questions, raise your hand and the experimenter will come and help you. If you read the instructions carefully, you will have the opportunity to make a significant amount of money based on the decisions you make in the experiment. During the experiment, you will earn money which will be paid to you in cash at the end of the experiment.

Procedure

This experiment is divided into 10 rounds. Participants are divided into groups of four before the first round commences. You will therefore be in a group with three other participants. **The composition of the groups will stay the same for all ten rounds.**

At the beginning of each round, each participant receives **100 cents**. We call this your endowment. Your task is to decide how to use your **endowment**. You have to decide how many cents you want to contribute to a **project** and how many of them to keep for yourself. The consequences of your decision are explained in detail below. At the beginning of each round the following input-screen for the first stage will appear:

Input screen

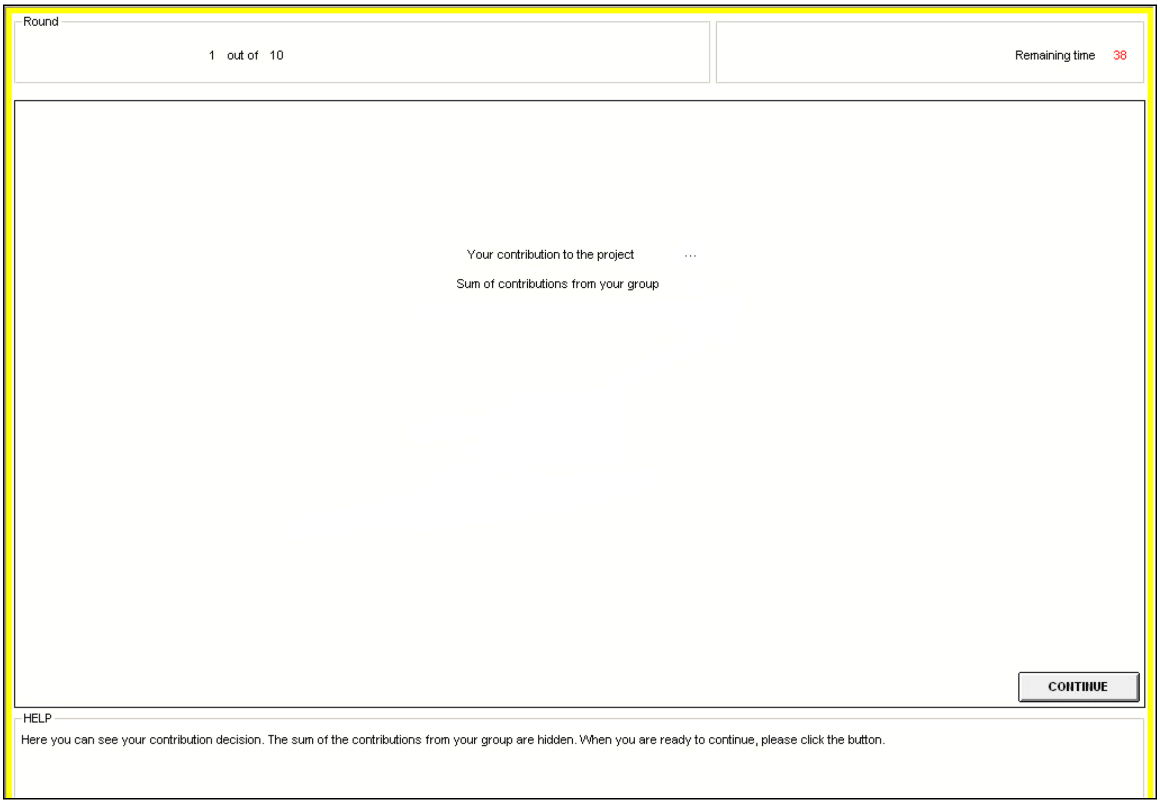


The round number appears in the top of the screen. In the top right corner you can see how many more **seconds** remain for you to decide on your contribution. You will have 45 seconds in the first two rounds and 30 seconds in the remaining rounds. Your decision must be made within the time limit.

Your **endowment in each round is 100 cents**. You have to decide how many cents you want to contribute to the project by typing a number between 0 and 100 in the input field. This field can be reached by clicking it with the mouse. As soon as you have decided how many cents to contribute to the project, you have also decided how many cents you keep for yourself: this is (**100 – your contribution**). After entering your contribution you must click the O.K. button. Once you have done this, your decision can no longer be revised.

After all the members of **your** group have made their decision, the amount of money you have earned that round will be calculated and the following screen will display the total amount of money contributed by you to the project. Your income each round will depend on the sum of your group's contributions. You will **not** be able to see the sum of the contributions from your own group (this will be left blank). At the end of the ten rounds you will be shown your total income, calculated as the sum of the income you earned each round.

Results screen



Your **income** consists of two parts:

(1) The money which you have kept for yourself; and

(2) The income received from the project. This is calculated as:

Income from the *project* = 0.5 *times* the total contributions to the project.

Your **income (in dollars)** of a round is therefore: **= [(100 – your contribution to the project) + 0.5*(total contributions to the project)] ÷ 100**

Your income depends on the sum of **your** group's contributions.

The income of each group member from the project is calculated in the same way, i.e., each group member receives the same income from the project. Assume, for example, that the sum of the contributions of all group members is 150 cents. In this case each member of the group receives an income from the project of: 0.5*150 = 75 cents.

Each cent you keep for yourself will be added to your income. Suppose you instead contributed this cent to the project, then the total contributions to the project would rise by one cent. Your income from the project would rise by 0.5*1=0.5 cents. However the income of the other group members would also rise by 0.5 cents each, so that the total income of the group from the project would rise by 2 cents. Your contribution to the project therefore also raises the income of the other group members. On the other hand, you also earn income for each cent contributed by the other members to the project. For each cent contributed by any member, you earn 0.5*1=0.5 cents.

Do you have any questions?

--------------------------------Scratch paper for working------------------------

INSTRUCTIONS CONDITIONS T1 & T2 (OUT-GROUP COMPARISONS)

Instructions

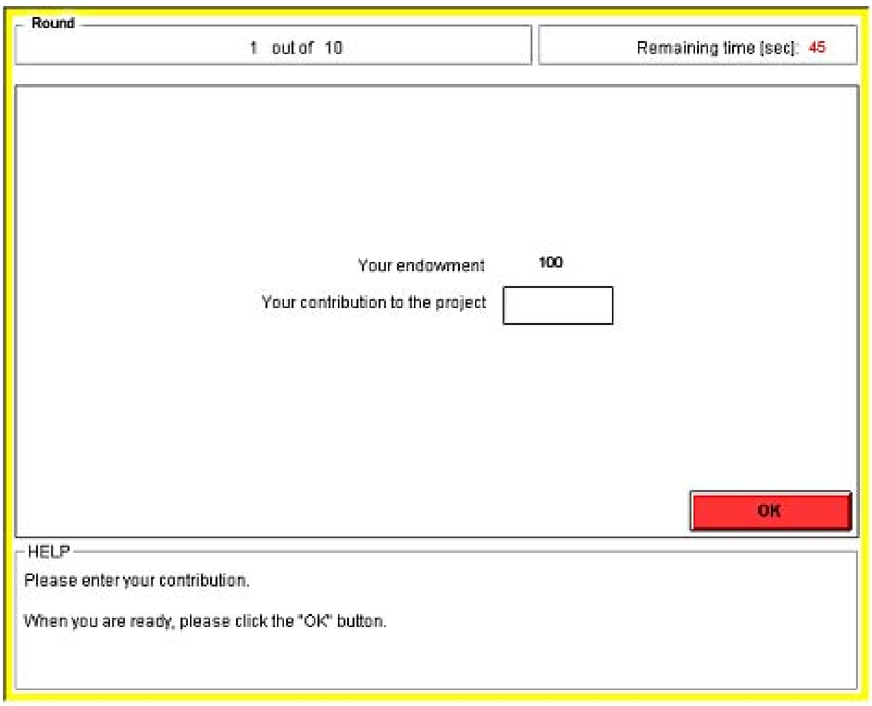
This is an experiment on the economics of decision making. Please do not communicate with each other during the experiment. If you have any questions, raise your hand and the experimenter will come and help you. If you read the instructions carefully, you will have the opportunity to make a significant amount of money based on the decisions you make in the experiment. During the experiment, you will earn money which will be paid to you in cash at the end of the experiment.

Procedure

This experiment is divided into 10 rounds. Participants are divided into groups of four before the first round commences. You will therefore be in a group with three other participants. **The composition of the groups will stay the same for all ten rounds.**

At the beginning of each round, each participant receives **100 cents**. We call this your endowment. Your task is to decide how to use your **endowment**. You have to decide how many cents you want to contribute to a **project** and how many of them to keep for yourself. The consequences of your decision are explained in detail below. At the beginning of each round the following input-screen for the first stage will appear:

Input screen



The round number appears in the top of the screen. In the top right corner you can see how many more **seconds** remain for you to decide on your contribution. You will have 45 seconds in the first two rounds and 30 seconds in the remaining rounds. Your decision must be made within the time limit.

Your **endowment in each round is 100 cents**. You have to decide how many cents you want to contribute to the project by typing a number between 0 and 100 in the input field. This field can be reached by clicking it with the mouse. As soon as you have decided how many cents to contribute to the project, you have also decided how many cents you keep for yourself: this is (**100 – your contribution**). After entering your contribution you must click the O.K. button. Once you have done this, your decision can no longer be revised.

Your group will be **paired with another group** for all ten rounds and you will be able to **see** the sum of the contributions from the other group you are paired with. The other group will remain the same for all ten rounds. After all the members of **your** group have made their decision, the amount of money you have earned that round will be calculated and the following screen will display the total amount of money contributed from the **other group**. You will **not** be able to see the sum of the contributions from your own group (this will be left blank). You only get to see the sum of the contributions of the **other group**. Your income only depends on the sum of **your** group's contributions. At the end of the ten rounds you will be shown your total income, calculated as the sum of the income you earned each round.

Results screen

Your **income** consists of two parts:

(1) The money which you have kept for yourself; and

(2) The income received from the project. This is calculated as:

Income from the *project* = 0.5 *times* the total contributions to the project.

Your **income (in dollars)** of a round is therefore:

= [(100 – your contribution to the project) + 0.5*(total contributions to the project)] ÷ 100

Your income only depends on the sum of **your** group's contributions. The income of each group member from the project is calculated in the same way, i.e., each group member receives the same income from the project. Assume, for example, that the sum of the contributions of all group members is 150 cents. In this case each member of the group receives an income from the project of: 0.5*150 = 75 cents.

Each cent you keep for yourself will be added to your income. Suppose you instead contributed this cent to the project, then the total contributions to the project would rise by one cent. Your income from the project would rise by

0.5*1=0.5 cents. However the income of the other group members would also rise by 0.5 cents each, so that the total income of the group from the project would rise by 2 cents. Your contribution to the project therefore also raises the income of the other group members. On the other hand, you also earn income for each cent contributed by the other members to the project. For each cent contributed by any member, you earn 0.5*1=0.5 cents.

Do you have any questions?

------------------------------Scratch paper for working--------------------------

INSTRUCTIONS FOR C2 (IN-GROUP COMPARISONS)

Instructions

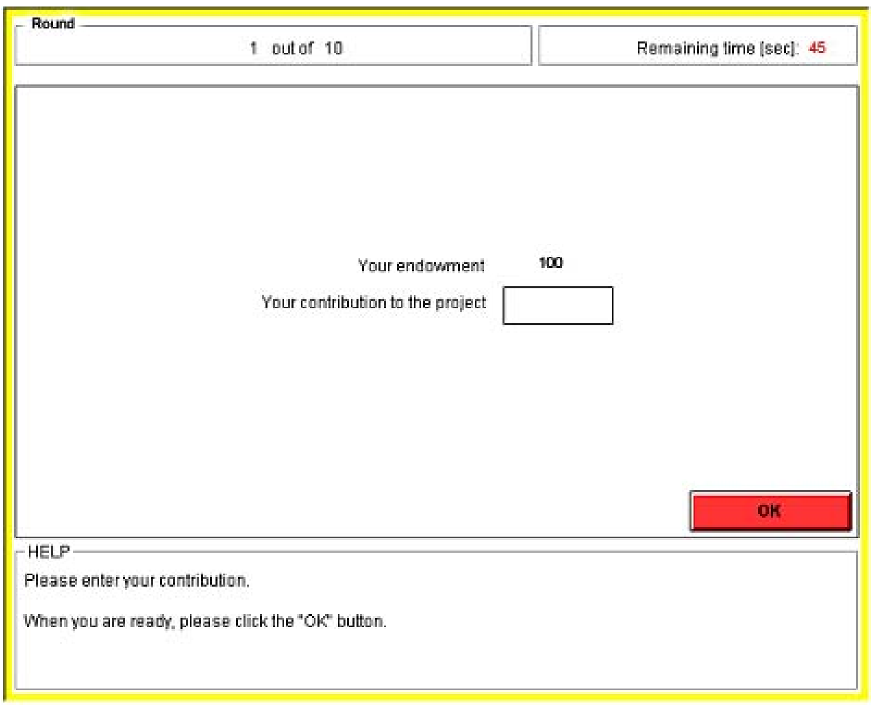
This is an experiment on the economics of decision making. Please do not communicate with each other during the experiment. If you have any questions, raise your hand and the experimenter will come and help you. If you read the instructions carefully, you will have the opportunity to make a significant amount of money based on the decisions you make in the experiment. During the experiment, you will earn money which will be paid to you in cash at the end of the experiment.

Procedure

This experiment is divided into 10 rounds. Participants are divided into groups of four before the first round commences. You will therefore be in a group with three other participants. **The composition of the groups will stay the same for all ten rounds.**

At the beginning of each round, each participant receives **100 cents**. We call this your endowment. Your task is to decide how to use your **endowment**. You have to decide how many cents you want to contribute to a **project** and how many of them to keep for yourself. The consequences of your decision are explained in detail below. At the beginning of each round the following input-screen for the first stage will appear:

Input screen

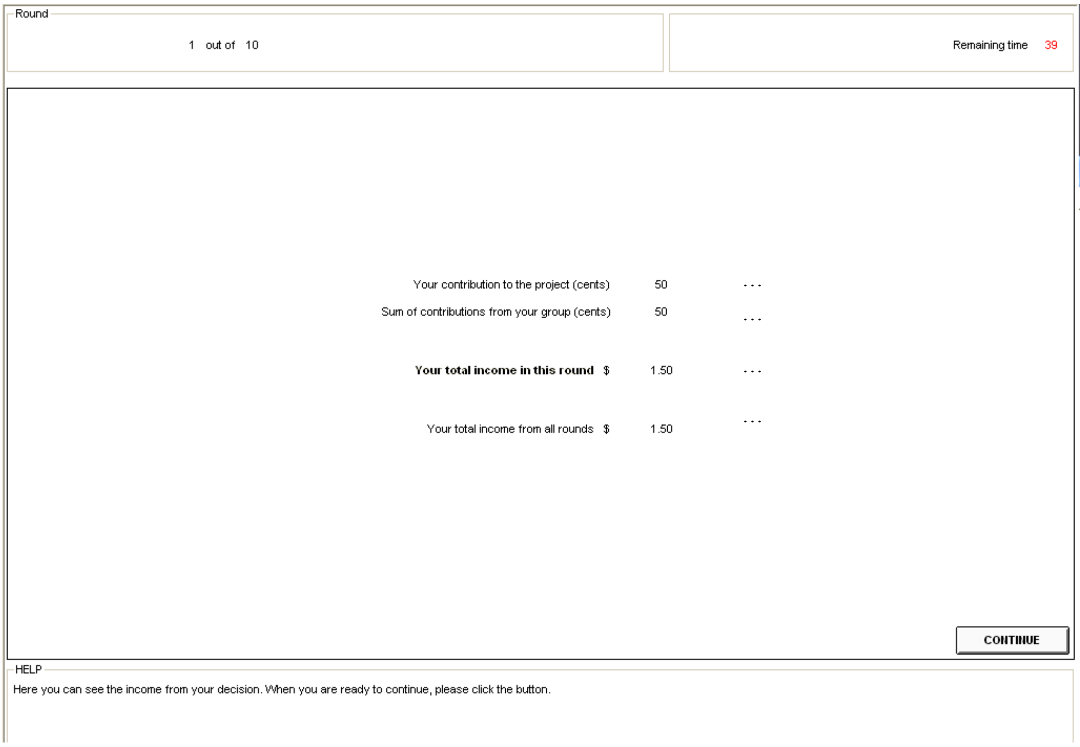


The round number appears in the top of the screen. In the top right corner you can see how many more **seconds** remain for you to decide on your contribution. You will have 45 seconds in the first two rounds and 30 seconds in the remaining rounds. Your decision must be made within the time limit.

Your **endowment in each round is 100 cents**. You have to decide how many cents you want to contribute to the project by typing a number between 0 and 100 in the input field. This field can be reached by clicking it with the mouse. As soon as you have decided how many cents to contribute to the project, you have also decided how many cents you keep for yourself: this is (**100 – your contribution**). After entering your contribution you must click the O.K. button. Once you have done this, your decision can no longer be revised.

After all members of your group have made their decision, the following screen will show you the total amount of money contributed by all four group members to the project (including your contribution). This screen also shows you how much money you have earned that round.

Result screen



Your **income** consists of two parts:

(1) The money which you have kept for yourself ; and

(2) The income received from the project. This is calculated as:

Income from the *project* = 0.5 *times* the total contributions to the project.

Your **income (in dollars)** of a round is therefore:

= [(100 – your contribution to the project) + 0.5*(total contributions to the project)] ÷ 100

The income of each group member from the project is calculated in the same way, i.e., each group member receives the same income from the project. Assume, for example, that the sum of the contributions of all group members is 150 cents. In this case each member of the group receives an income from the project of: 0.5*150 = 75 cents.

Each cent you keep for yourself will be added to your income. Suppose you instead contributed this cent to the project, then the total contributions to the project would rise by one cent. Your income from the project would rise by 0.5*1=0.5 cents. However the income of the other group members would also rise by 0.5 cents each, so that the total income of the group from the project would rise by 2 cents. Your contribution to the project therefore also raises the income of the other group members. On the other hand, you also earn income for each cent contributed by the other members to the project. For each cent contributed by any member, you earn 0.5*1=0.5 cents.

Do you have any questions?

----------------------------------Scratch paper for working-----------------------

INSTRUCTIONS T3 & T4 (IN-GROUP + OUTGROUP)

Instructions

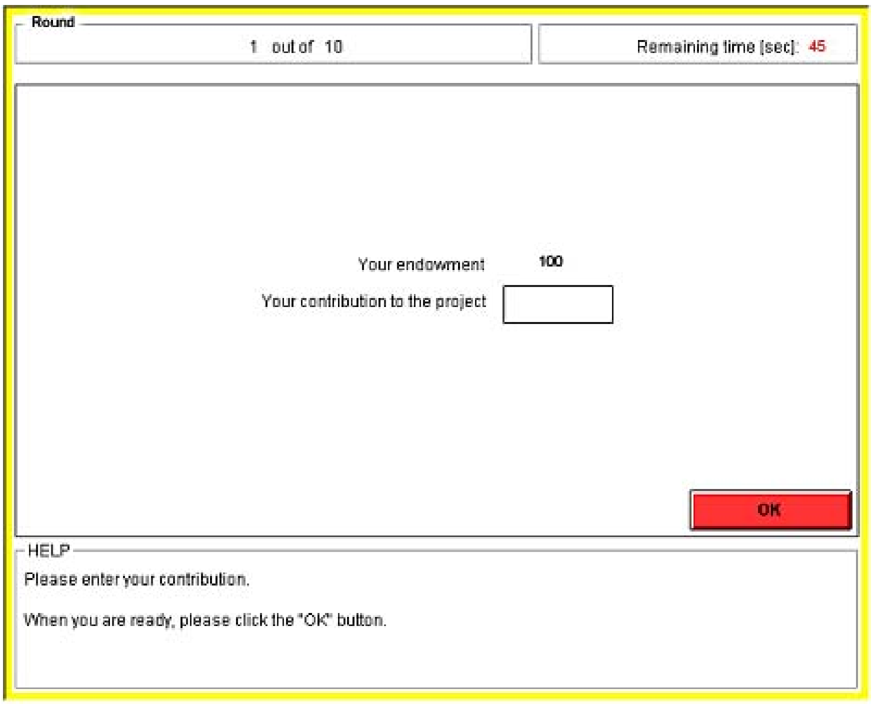
This is an experiment on the economics of decision making. Please do not communicate with each other during the experiment. If you have any questions, raise your hand and the experimenter will come and help you. If you read the instructions carefully, you will have the opportunity to make a significant amount of money based on the decisions you make in the experiment. During the experiment, you will earn money which will be paid to you in cash at the end of the experiment.

Procedure

This experiment is divided into 10 rounds. Participants are divided into groups of four before the first round commences. You will therefore be in a group with three other participants. **The composition of the groups will stay the same for all ten rounds.**

At the beginning of each round, each participant receives **100 cents**. We call this your endowment. Your task is to decide how to use your **endowment**. You have to decide how many cents you want to contribute to a **project** and how many of them to keep for yourself. The consequences of your decision are explained in detail below. At the beginning of each round the following input-screen for the first stage will appear:

Input screen



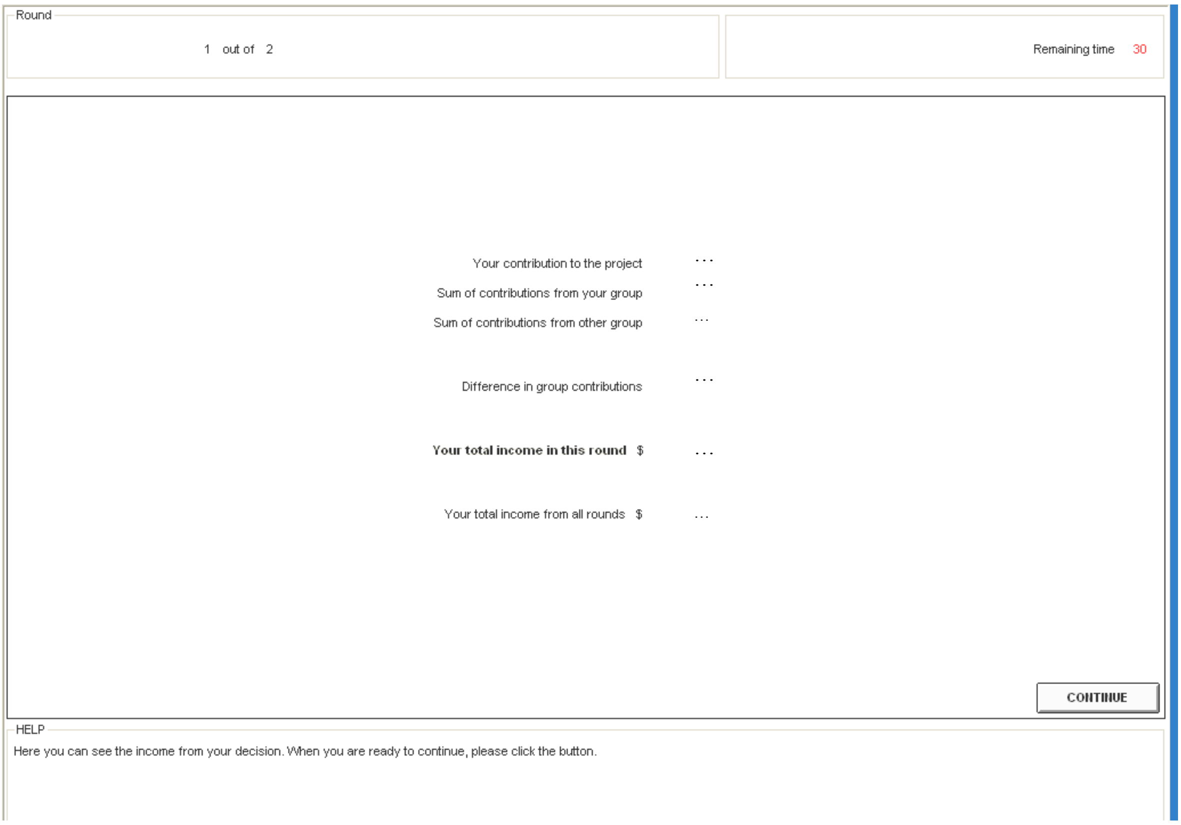
The round number appears in the top of the screen. In the top right corner you can see how many more **seconds** remain for you to decide on your contribution. You will have 45 seconds in the first two rounds and 30 seconds in the remaining rounds. Your decision must be made within the time limit.

Your **endowment in each round is 100 cents**. You have to decide how many cents you want to contribute to the project by typing a number between 0 and 100 in the input field. This field can be reached by clicking it with the mouse. As soon as you have decided how many cents to contribute to the project, you have also decided how many cents you keep for yourself: this is (**100 – your contribution**). After entering your contribution you must click the O.K. button. Once you have done this, your decision can no longer be revised.

After all members of your group have made their decision, the following screen will show you the total amount of money contributed by all four group members to the project (including your contribution). This screen also shows you how much money you have earned that round.

Your group will be **paired with another group** for all ten rounds and you will be able to **see** the sum of the contributions from the other group you are paired with **as well as** the sum of the contributions from your own group in the results screen. The other group will remain the same for all ten rounds. If the "**Difference of group contributions"** displayed in the results screen is a negative (-) number, then your group has contributed less than the other group. If it is a positive number, then your group has contributed more than the other group. Your income only depends on the sum of **your** group's contributions.

Result screen



Your **income** consists of two parts:

(1) The money which you have kept for yourself; and

(2) The income received from the project. This is calculated as:

Income from the *project* = 0.5 *times* the total contributions to the project.

Your **income (in dollars)** of a round is therefore:

= [(100 – your contribution to the project) + 0.5*(total contributions to the project)] ÷ 100

Your income only depends on the sum of **your** group's contributions.

The income of each group member from the project is calculated in the same way, i.e., each group member receives the same income from the project. Assume, for example, that the sum of the contributions of all group members is 150 cents. In this case each member of the group receives an income from the project of: 0.5*150 = 75 cents.

Each cent you keep for yourself will be added to your income. Suppose you instead contributed this cent to the project, then the total contributions to the project would rise by one cent. Your income from the project would rise by 0.5*1=0.5 cents. However the income of the other group members would also rise by 0.5 cents each, so that the total income of the group from the project would rise by 2 cents. Your contribution to the project therefore also raises the income of the other group members. On the other hand, you also earn income for each cent contributed by the other members to the project. For each cent contributed by any member, you earn 0.5*1=0.5 cents.

Do you have any questions?

----------------------------------Scratch paper for working------------------------

OUT-GROUP COMPARISON INFORMATION

Period	**Low comparison**	**High comparison**
1	**85**	**310**
2	**50**	**320**
3	**35**	**320**
4	**20**	**295**
5	**35**	**290**
6	**75**	**270**
7	**90**	**300**
8	**95**	**285**
9	**60**	**295**
10	**40**	**210**

Data obtained from prior public goods experimental study using the same parameters and methods.

Of the 82 groups in the prior study, the contribution data from group contributing on average in the lowest 5 percentile was chosen as low comparison.

The contribution data from the group contributing in the highest 95 percentile was chosen as the high comparison.

Chapter 4: Estimation of Public Goods Game Data*

4.1 Introduction

The Public Goods Game is extensively used by experimental economists[1] as a tool to study social dilemmas and cooperation. However, even though it has been nearly 30 years since the first laboratory Public Goods Game experiments were published (Isaac, Walker, and Thomas, 1984; Kim and Walker, 1984; Isaac, McCue, and Plott, 1985) the empirical analysis of the game choice data has still not moved beyond descriptive statistics in most papers. The likely reason for this is that the distribution of the choice data for this game is highly non-standard and is complicated by its discrete, censored and often panelled nature. With little known about the preciseness or extent of biasedness of estimates for this data under these conditions, many authors have avoided model estimation entirely.

There have been a few exceptions though. Carpenter (2004) for example, used Tobit random effects estimation to account for data censoring to model contribution choice in a 10 period public goods game. Bardsley and Moffatt (2007) made a clear attempt at advancing the analytical toolbox for public goods experiments by proposing that public goods data be modelled using a finite mixture model to incorporate heterogeneity of types within a population with Tobit components to address censoring, and a tremble term to model decision error. Despite the sophistication of the model and compelling rational for the approach, the finite mixture modelling approach was never taken up in the Public Goods experimental literature, probably due to its complexity.

Random effects estimation have been used by (Tan and Bolle, 2007; Nikiforakis, 2010) and more recently, Breitmoser (2010) estimates a nested

*I am grateful to David Drukker, Director of Econometrics at StataCorp, to Professors Adrian Pagan and Robert Slonim for their valuable suggestions and comments, to Professor Simon Gächter for providing the data cited in this paper and to the attendees of the Sydney Experimental Brownbag Seminar for their comments.

[1]For example, a review paper by Chaudhuri (2011) cites 146 Public Goods experiment publications.

ordered logit using Public Goods data in order to compare the internal and external validity of different structural models. This paper differs from Breitmoser (2010) in that this paper is specific to Public Goods experiments only and in this paper the structural model is held constant and the performance of the estimates are compared for different estimation approaches. In contrast, Breitmoser (2010) holds neither the structural model nor estimation approach constant. Different specified models with different estimation approaches are compared using Bayes Information Criteria (BIC) and log likelihoods producing somewhat idiosyncratic results. In line with the results of this paper, Poen (2009) finds evidence of bias in Tobit random effects estimates from simulated public goods game data. However, Poen (2009) suggests the bias is likely due to the inclusion of a feedback variable that may introduce endogeneity. This paper decisively shows, by estimating a model including a feedback variable with and without using a tobit approach, that the source of the bias is not endogeneity but instead from the use of Tobit estimation.

This paper provides Public Goods Game experimentalists with a clear evidence-based prescription for the best estimation approach for Public Goods Game choices. With greater knowledge and certainty as to how different estimators will perform with Public Goods Game data, it is hoped that authors will be more confident in generating inferences from Public Goods Game models.

4.2 Distribution of Contributions

4.2.1 Public Goods Game

I examine a typical public goods situation found in experimental economics literature, a standard Voluntary Contributions Mechanism (VCM) (Davis and Holt, 1993; Ledyard, 1995). Participants have the same endowment w and are in groups of N. Each individual has to decide how much of his endowment to allocate to a public account y_i and how much to keep for himself $w - y_i$. For each group, the sum of the individual allocations to the public good $\sum_{j=1}^{N} y_j$ is then multiplied by a factor $a(N > a > 1)$, to model the additional value generated from the public nature of the good. The final value of the public account is then shared equally among the group members. The payoff therefore of player i under a VCM is given by:

$$\pi_i = (w - y_i) + \frac{a}{N} \sum_{j=1}^{N} y_j$$

The VCM is primarily used to model social dilemmas because the dominant strategy for each player is to free ride by allocating nothing to the public account (assuming players maximize their own monetary payoff and rationality is common knowledge). However, maximum efficiency is achieved when all members allocate their entire endowment to the public account $y_i = w$.

4.2.2 Data

Data was sourced from a previous study that used a 10 period public goods experiment (Guillen, Merrett, and Slonim, 2012). This study used procedures and instructions that closely resembling those from previous literature[2] The data set is a panel of 4000 observations from 400 subjects with each subject making 10 contribution decisions. The Guillen, Merrett, and Slonim (2012) study recruited undergraduates from the University of Sydney, Australia (undergraduates are typically recruited as subjects for Public Goods Game experiments in the literature) and involved two stages. In the first stage all subjects played a standard 10 period VCM game and in the second stage subjects were re-matched into different groups and played a variety of different 10 period public goods games. In this paper only the data from the first stage standard 10 period VCM game is used. Subjects played in groups of $N = 4$ and were given an endowment $w = 100$ cents in which to make a contribution decision $y = \{0, 1, 2...100\}$. The experimenters multiplied contributions by a factor of $a = 2$ thereby giving a marginal per capita return (MPCR) equal to 0.5 for every cent contributed.

4.2.3 Distributions

The contribution data replicate the temporal results of earlier VCM experiments (Ledyard, 1995) where mean contributions start between 40 to 60 percent of the endowment and decline to close to zero (Figure 4.1). The decay in contributions in the standard VCM game has been replicated many times by different authors and is observed across different cultures (Gächter, Herrmann, and Thöni, 2010).

The distributions of contributions across all 10 periods is given in Figure 4.2. The distribution of contributions is similar to those obtained from other VCM studies, see for example, Gächter, Renner, and Sefton (2008)[3] and Herrmann, Thöni, and Gächter (2008)[4] (See Fig. 4.3). The distributions in Fig. 4.2 and Fig. 4.3 are both highly truncated (in Fig. 4.2 almost 40 percent of observations are at one of the two limits), more

[2]The instructions used in the study were adapted from Herrmann, Thöni, and Gächter (2008)

[3]In this study subjects played in groups of 3, were given an endowment of 20 tokens and received a MPCR of 0.5.

[4]This study collected contribution data from subjects in several different countries around the world including Melbourne, Australia. Subjects played in groups of 4, were given an endowment of 20 tokens and received a MPCR of 0.4.

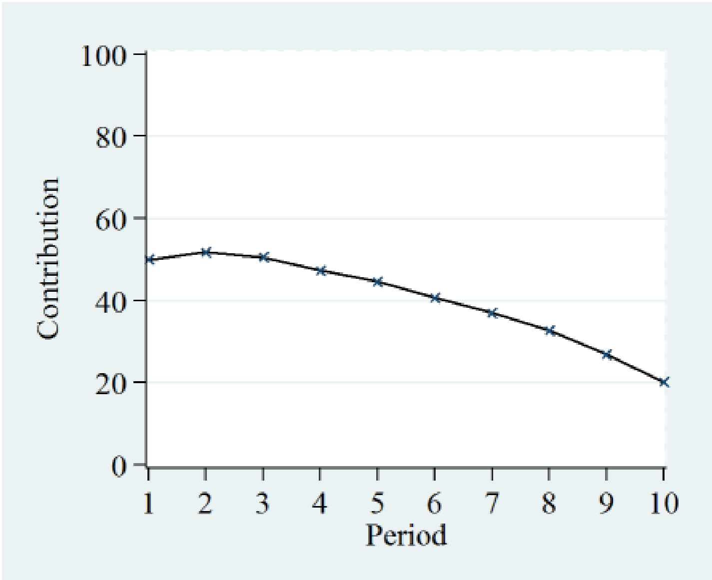


Figure 4.1: Average contributions by period replicate the results of previous experiments ($N = 4000$)

so at the zero end than the 100 end, and display a flat, almost uniform, distribution in between the two limits with a noticeable node at the midpoint. The distributions in Fig. 4.3 also show noticeable modes between the midpoint and zero and the midpoint and upper bound however the distributions are comparable for the most part.

Given that standard parameters and standard experimental methods were used and the contribution distribution replicates previous literature, the dataset used in this paper is representative of standard VCM experimental data. The dataset is also substantial involving 400 subjects and 4000 observations. These attributes make this dataset a good candidate to test the performance of different VCM estimation approaches.

The correct identification of the distribution becomes particularly important when using maximum likelihood estimation (MLE). Whereas least squares estimation only requires that the distribution of the errors be known, MLE requires that the distribution of the dependent variable is known. If the distribution is misspecified then MLE estimates can be inconsistent invalidating standard inference techniques White (1982).
A reasonable assumption might be that contributions are distributed according to a Tobit distribution with lower limit censoring occurring at zero and upper censoring occurring at 100. A simulated Tobit distribution with a

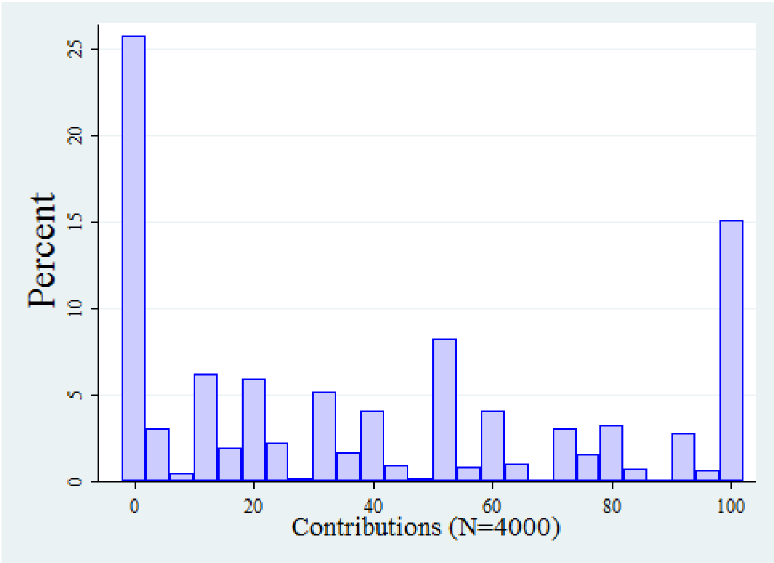


Figure 4.2: Histogram of contribution data used in this paper (all 10 periods)

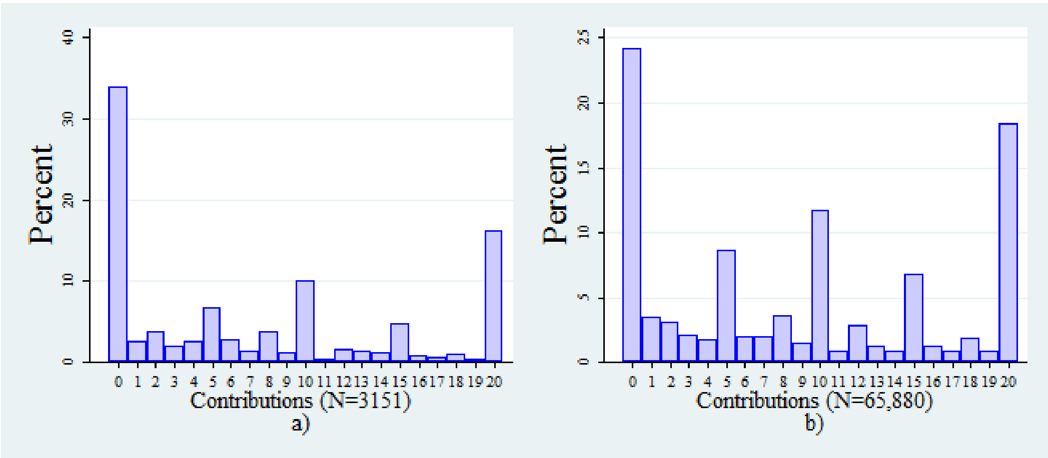


Figure 4.3: Histogram of contributions in the baseline VCM game condition from a) Gächter, Renner, and Sefton (2008) (all 50 periods); and b) Herrmann, Thöni, and Gächter (2008) (all 10 periods).

mean and variance comparable to that of the dataset illustrates how different the Tobit distribution is compared to the distribution of contributions (Figure 4.5). Further examination of contribution distributions for each period shows that none of the periods demonstrate a distribution similar to the simulated Tobit distribution (Figure 4.4).

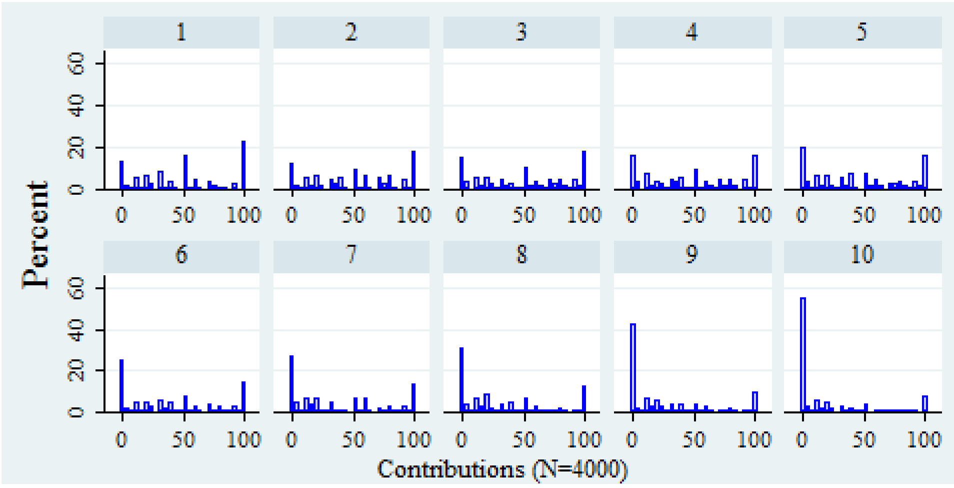


Figure 4.4: Histogram of contributions by period

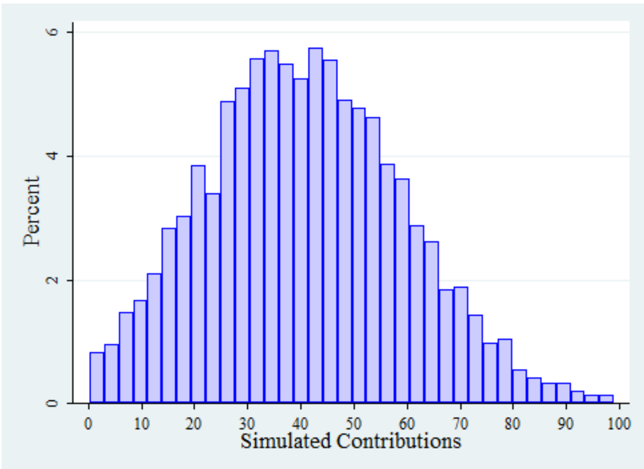


Figure 4.5: Tobit Simulation N=4000 ($\mu = 40, \sigma^2 = 400$)

4.3 Estimation Approaches

Given the unique nature of VCM data (discrete, panel, with a non-standard distribution changing over time) many papers avoid the difficult task of estimating models using VCM data, instead choosing descriptive analysis over regression analysis. From those authors who have, there have been a number of different estimation approaches. The estimation approaches can be grouped into two main categories: the continuous approach (as with generalized least squares for panel data models) and the discrete approach (as with logit and finite mixture models). The estimation approaches chosen for comparison are those used in the literature which include, random effects estimation (Tan and Bolle, 2007; Nikiforakis, 2010), tobit random effects estimation (Carpenter, 2004), ordered logit (Breitmoser, 2010), and finite mixture models (Bardsley and Moffatt, 2007).

In order to compare the performance of different approaches, the same model covariates should be estimated in each approach. To be comparable to the literature, the covariates must also reflect the previous findings. A good model is one that is also parsimonious and incorporates the temporal nature of the data. The following model was created with this criteria in mind and is used to compare the performance of different estimation approaches in sections (4.3.1-4.3.5).

$$y_{ip} = \beta_0 + \beta_1 p + \beta_2 p9 + \beta_3 p10 + \beta_1 AV_{ip-1} + u_{ip} \tag{4.1}$$

The dependent variable in Eq. (1) is an individual's i's contribution y in period p. The covariates are lagged Average Group Contribution (AV) which *excludes* individual i's contribution, period (p) and period 9 and 10 dummies (*p9* and *p10* respectively). The intercept is only estimated when appropriate for that approach.

The covariate AV was included in Eq. (4.1) in order to model the effect of conditional cooperation. There is evidence that people demonstrate conditional cooperation behavior in public goods games (Fischbacher, Gächter, and Fehr, 2001; Keser and van Winden, 2000; Fischbacher and Gächter, 2010). That is, people will contribute to the public account if the others in their group contribute as well. Their contributions therefore, are dependent on the contributions of the others in their group. The covariate p was used to control for the declining trend in contributions over time. To explicitly model any possible end-game effects (Andreoni, 1988) a dummy for each of the last two periods was included.

4.3.1 Fixed Effects

The fixed effect approach is a method of removing the unobserved individual specific effects from panel data by applying a transformation (4.2) to the data prior to estimation (Wooldridge, 2009). The transformation uses the mean of the dependent variable $\bar{y}$ and mean of the regressors $\bar{x}$ to time de-mean the data.

$$y_{ip} - \bar{y}_i = \beta(x_{ip} - \bar{x}_i) + u_{ip} - \bar{u}_i \tag{4.2}$$

The time-demeaned data (time is denoted as period p here) is then regressed using ordinary least squares (OLS). Because the fixed effects approach effectively removes the individual effects it does not require the stricter conditions that random effects estimation (4.3.2) imposes. One drawback though is that the transformation not only removes the individual effects from the intercept, but removes any time invariant variables, for example, gender and race.

Table 4.2 reports the estimation results of model (4.1) using fixed effects estimation. All covariates are significant except *Period 9* and all coefficients have a sign that we would expect. The coefficients are interpreted in the same way as an OLS estimation. *R-Squares* are typically lower in panel models and are less meaningful than those from cross-sectional data. *Rho* reports the correlation coefficient. A correlation coefficient equal to one suggests perfect correlation of contribution choices within the same individual. A *Rho* of 0.498 here suggests suggests that 50 percent of the variance is due to differences between the individuals.

Table 4.1: Estimation results : Fixed Effects

Variable	**Coefficient**	**(Std. Err.)**
Dependent variable: Contribution		
Period	-2.326***	(0.295)
Period 9	-2.193	(1.395)
Period 10	-4.408**	(1.915)
Lag Average Group Contribution	0.385***	(0.037)
Intercept	37.454***	(2.447)
R^2 (overall)	0.226	
Rho	0.498	

Significance: $***1\%, **5\%, *10\%$

Notes: Robust standard errors clustered at subject id level.

4.3.2 Random Effects

Random effects estimation does not remove the individual effects but instead allows each individual to have their own random intercept ie, individual effect, Demidenko (2004). The main advantage of using Random effects over fixed effects estimation is that it allows for covariates that are constant over time. However random effects requires the stricter assumptions that the individual effects are uncorrelated with the covariates and that the individual effects are normally distributed in the population. Allowing for individual effects in the data does create serial correlation which is solved using generalized least squares (GLS). GLS eliminates serial correlation by a similar transformation to (4.2) except that only a proportion of the transformation (λ) is applied in random effects estimation.

$$y_{ip} - \lambda\bar{y}_i = \beta_0(1-\lambda) + \beta_1(x_{ip} - \lambda\bar{x}_i) + (u_{ip} - \lambda\bar{u}_i) \tag{4.3}$$

The proportion is determined by the strength if the individual effects Eq. (4.4). If observations within the same individual are highly correlated, then the within individual variation σ_u^2 will be low relative to the between individual variation σ_b^2 and a greater proportion (λ) of the transformation will applied. In the extreme case where $\lambda = 1$, fixed effects estimation is obtained and when $\lambda = 0$, pooled OLS estimation is obtained.

$$\lambda = 1 - \frac{\sigma_u^2}{\sigma_u^2 + P\sigma_b^2} \tag{4.4}$$

A Hausman test (Hausman, 1978) can be used to help determine whether fixed effects or random effects is the more appropriate estimator to use. The Hausman test involves regressing the model using fixed effects estimation then random effects estimation and the compares whether the estimates are significantly different. If the null hypothesis of no systematic difference is rejected then fixed effects estimation should be used. A rejection of the null suggests that some of the assumptions of random effects estimation have been violated leading to very different results. An LM test (Breusch and Pagan, 1980) can be used to help decide between random effects estimation and a simple OLS regression. If the null hypothesis of no significant differences across individuals is rejected, then OLS estimation should be used.

A Hausman test was applied to model (4.1) obtaining ($\chi^2 = 40.93, P = 0.000$) suggesting that fixed effects is the appropriate estimator for this model and data. This is congruent with the observed distribution of contributions in section 2 in which is appears that the random effects are distributed non-normally. In the interest of understanding how dire such a violation may be to estimation performance when estimating public goods data, I have included random effects estimation in the presence of violations in this paper. The performance of random effects estimation here can alert experimentalists as to the importance of such a mispecification on estimation results.

Table 4.2 reports the estimation results of model (4.1) using random effects estimation. The coefficients are different than those obtained from fixed effects estimation (Table 4.1). However the difference do not seem dramatic and all covariates display the same signs as those obtained from fixed effect estimation.

4.3.3 Tobit Random Effects

Contribution data from VCM public goods games are highly censored (Section 2). Greene (1981) demonstrates that ignoring censoring and proceeding with Least Squares estimation leads to inconsistent and

Table 4.2: Estimation results : Random Effects

Variable	Coefficient	(Std. Err.)
Dependent variable: Contribution		
Period	-2.193***	(0.298)
Period 9 Dummy	-1.986	(1.392)
Period 10 Dummy	-4.013**	(1.902)
Lag Average Group Contribution	0.440***	(0.033)
Intercept	34.233***	(2.438)
R^2 (overall)	0.231	
Rho	0.452	

Significance: $***1\%, **5\%, *10\%$. $N = 3600$
Notes: Robust standard errors clustered at subject id level.

downward biased parameter estimates. A Tobit estimation is sometimes used by authors to address this concern. Statistical packages such as Stata can fit a random effects tobit model by MLE however no statistic exists for tobit fixed effects that would produce un-biased estimates.

Tobit random effects estimation assumes that the random effects, α, are normally distributed. The joint density function is a nested function. The normally distributed random effects nests the tobit distribution of the contributions. The individual level likelihood function is given by

$$l_i = \int_{-\infty}^{\infty} \frac{e^{-\alpha_i^2/2\sigma_\alpha^2}}{\sqrt{2\pi\sigma^2}} \{\prod_{p=1}^{n_i} F(y_{ip}, x_{ip}\beta + \alpha_i)\} d\alpha_i \tag{4.5}$$

where:

$$F(y_{ip}, \mathbf{x}\beta) = \begin{cases} (\sqrt{2\pi\sigma^2})^{-1} e^{-(y_{ip}-\mathbf{x}\beta)^2/(2\sigma_\epsilon^2)} & \text{if } 0 < y_{ip} < 100 \\ \Phi\left(\frac{0-\mathbf{x}\beta}{\sigma_\epsilon}\right) & \text{if } y_{ip} = 0 \\ 1 - \Phi\left(\frac{100-\mathbf{x}\beta}{\sigma_\epsilon}\right) & \text{if } y_{ip} = 100 \end{cases}$$

Table 4.3 reports the estimates of model (4.1) using Tobit random effects estimation. All variables are reported significant and have the signs that we would expect. Tobit estimates predict the average marginal impact of covariates on the dependent variable in its theoretically true uncensored state. For this reason the estimates of a tobit regression are not directly comparable to an OLS regression, which estimates the marginal effects of the covariates only on the *observed* outcomes. The magnitude of Tobit coefficient estimates are often slightly inflated because of this subtle difference. The significance and signs though are directly comparable. If one wished to directly compare Tobit estimates to OLS estimates this can be done by multiplying the Tobit estimate by the adjustment factor $n^{-1}\sum\Phi(x_i\hat{\beta}/\hat{\sigma})$. Tobit regression does not have an R-squared that can be calculated in the same way as those of OLS regression.

Table 4.3: Estimation results : Random Effects Tobit

Variable	**Coefficient**	**(Std. Err.)**
Dependent variable : Contribution		
Period	-2.854***	(0.357)
Period 9 Dummy	-5.585**	(2.460)
Period 10 Dummy	-12.526***	(2.738)
Lag Average Group Contribution	0.664***	(0.040)
Intercept	25.976***	(3.466)
Rho 0.539	left-censored observations	900
	right-censored observations	508

Significance: $***1\%, **5\%, *10\%$. $N = 3600$
Notes: Robust standard errors clustered at subject id level.

4.3.4 Ordered Logit Regression

An ordered logit model fits an ordinal categorical dependent variable on a set of independent variables. This estimation approach allows us to compare the performance of an estimation technique that treats the dependent variable as discrete as apposed to continuous. All the other approaches presented in this paper have assumed the dependent variable as continuous. The results of which approach, discrete or continuous, provides better estimates for VCM data may resolve some debates within the experimental economics community on the issue.

An ordered logit was used on the contribution data instead of multinomial logit regression (MLR) because ordinal information is lost in MLRs which disregards the ordinal nature of the categories. Even though there are 101 possible choices in the contribution set $y = \{0, 1, 2...100\}$, we only observe 75 different contribution choices in the dataset. Therefore a model of 75 categories representing each observed contribution is fit from the data. I chose not to reduce the number of contribution categories into contribution intervals because this would be difficult to compare the predictive performance of the ordered logit to the other estimation approaches.

Table 4.4 shows the estimation results of fitted ordered logistic model. A standard interpretation for the *Lag Average Group Contribution* coefficient is that for every one unit increase in the *Lag Average Group Contribution*, the ordered log-odds of being in a higher contribution category would increase by 0.031 on average, holding other variables constant. The estimated cut off points can be used to find the probability of an individual choosing a particular contribution category. These were estimated (output excluded) and were used to predict contribution choices from the model (in Section 4.4).

Table 4.4: Estimation results : Ordered Logit

Variable	**Coefficient**	**(Std. Err.)**
Dependent variable: Contribution choice		
Period	-0.093***	(0.017)
Period 9 Dummy	-0.186**	(0.082)
Period 10 Dummy	-0.483***	(0.116)
Lag Average Group Contribution	0.031***	(0.003)

Significance: $***1\%, **5\%, *10\%$
Notes: 75 cut-points were estimated (output excluded). Robust standard errors clustered at subject id level.

4.3.5 Finite Mixture Models

The previous estimation approaches assumed that contributions were generated from the same decision making process. Finite mixture models can be used to relax this assumption and explicitly model a finite number of different decision making process (McLachlan and Peel, 2000; Harrison and Rutstrom, 2009; Bardsley and Moffatt, 2007). Under a finite mixture model, agents can be categorized into one of a finite number of groups. A mixture density function is formed by aggregating the category k densities so that:

$$f(y_i;\Psi) = \sum_{k=1}^{g} \pi_k f_i(y_i;\theta_i)$$

With the constraint that $\sum_{k=1}^{g} \pi_k = 1$.

To demonstrate how well a mixture density can fit the observed distribution of the data, an adhoc mixture of a uniform (rounded to the nearest 10) and a discrete distribution was simulated (Figure 4.6). In the adhoc mixture the parameters were not estimated but simply calibrated to reflect the observed distribution. As you can see, it is easy to find an adhoc finite mixture that fits the data very well. The challenge though, is to estimate a finite mixture model that nests a predictive structural model of contributions.

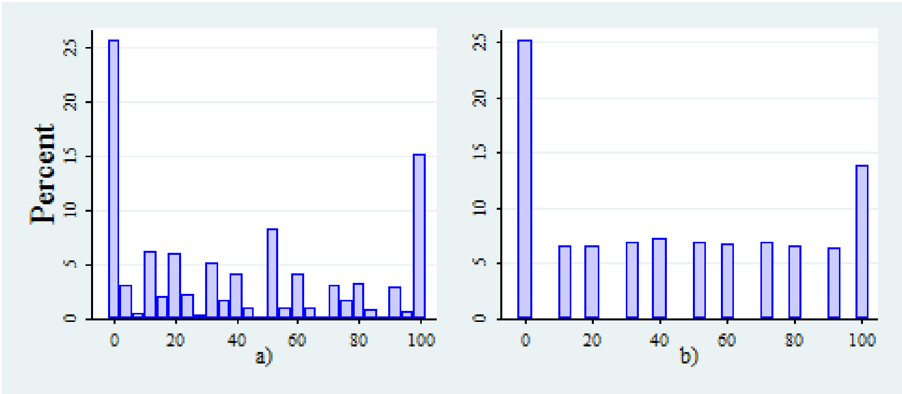


Figure 4.6: a) Distribution of average contributions in the 10 period Public Goods Game. b) Simulated distribution (N=500) containing components of Binomial ($p_1 = 0.30$) and Uniform distributions with ad-hoc component parameters ($\pi_1 = 0.25, \pi_2 = 0.75$) respectively.

As the contribution data is comprised of discrete non-negative integers, the Poisson distribution was chosen as the component densities for the finite mixture model because it models the probability of an occurrence of a discrete non-negative integer. The component distributions need not have the same variance but do need to belong to the same family of distributions. In a finite mixture model (FMM), the number of components are chosen *a priori*. Theory can help guide the choice of components however the choice is largely subjective. Once the number of components is decided the component moments and proportions π_i can then be estimated simultaneously with the coefficients of the structural model. Four components were chosen for the FMM estimated in this paper which replicates the number of components estimated by Bardsley and Moffatt (2007). They chose four types based on the theory that there are four contributor types: Free Riders, Altruists, Strategists and Reciprocators.

Because it is easy to over-fit FMMs it is useful to use the Bayes information criteria (BIC) (Schwarz, 1978) to ensure you haven't over-fit the number of components. These statistics are available post-estimation and is a criteria that incorporates a tradeoff between fit and parsimony. Smaller BICs are preferred as they are associated with higher log likelihoods. BIC is used over Akaike information criteria (AIC) here because there is a tendency for AIC to over-fit models in large sample sizes (Hurvich and Tsai, 1989). Table 4.5 reports the BIC values for FMM of 2 to four components. The four component model has the lowest BIC and is therefore preferred over the other two.

Table 4.5: Goodness-of-fit criteria

Model	**Obs**	**df**	**BIC**
2-Components	3600	9	52897.36
3-Components	3600	14	35934.83
4-Components	3600	19	30913.12

Estimation of FMM can be achieved through GLS, MLE or the Expectation Maximization algorithm. The FMM estimated in this paper was done in Stata through a user-written `fmm` command by Deb (2007) that enables MLE of FMMs using many of the standard distributions. To understand the construction of the grand log likelihood function for the FMM estimated in this paper first consider the likelihood function for a single Poisson distribution:

$$\ell = \prod_{j=1}^{m} \frac{e^{-y_j(\theta' x_j)} e^{-e^{\theta' x_j}}}{y_j!}$$

The log likelihood is

$$\ln \ell = \sum_{j=1}^{m} \left(y_j(\theta' x_j) - e^{\theta' x_j} - \ln y_j! \right)$$

The grand likelihood is constructed from four component Poisson likelihood functions

$$\text{Grand } \ell = \prod_{i=1}^{N} (\pi_1 \ell_1 + \pi_2 \ell_2 + \pi_3 \ell_3 + \pi_4 \ell_4)$$

Taking the log

$$\text{Grand } \ln \ell = \sum_{i=1}^{N} \ln(\pi_1 \ell_1 + \pi_2 \ell_2 + \pi_3 \ell_3 + \pi_4 \ell_4) \tag{4.6}$$

The estimates for each component probability π_k are constrained to be between 0 and 1 and sum to 1, using a post-estimation log-odds transformation (Harrison, 2007).

The mean contributions and standard deviations for the four estimated components of Equation 4.6 are reported in Table 4.6. The first component, or group of contributors, contribute an average of almost 0 cents out of 100 per period. These members could be classified as "free riders". The second group of members contribute on average 90 cents out of 100, these could be classified as "altruists". The third group contribute on average 50 cents out of 100 and their contributions vary during the 10 periods (Std. Deviation is 5.6). The fourth group contributes an average of 16 cents out of 100 per period.

Table 4.6: Summary statistics

Variable	**Mean**	**Std. Dev.**
Component 1	0.123	0.042
Component 2	89.645	2.683
Component 3	46.354	5.609
Component 4	16.021	2.841
Total N=3600		

Table 4.7 reports the estimates of the FMM which nests model (4.1). The first thing to note is that 28 percent of the observations could be classified as free riding[5] ($\pi_1 = 0.278$). The other components are fairly evenly proportioned. The end game effect only seems to be significant for component 1 membership (free riding) with the *Period 10* dummy significant at the 5 percent level. The *Lag Average Group Contributions* significantly affect contributions in all contributor groups except the free riders (component 1). However the altruists (component 2) are the only group to have a significant downward trend in contributions. This result is interesting as the aggregate distributions demonstrate a clear downward trend in contributions. It is surprising to observe the temporal decline is insignificant for most individuals when *Lag Average Group Contributions* are controlled for. One explanation is that the altruists are the instigators of the decline which is magnified by *Lag Average Group Contributions*.

4.4 Comparing the Performance of Different Estimation Approaches

A cross validation method (Stone, 1974) was used to measure the predictive accuracy of each model. In the leave-one-out cross validation (LOOCV) method, one observation is removed from the data set and

[5]Panel FMMs catagorize observations not individuals into types. However it is assumed that individual preferences are stable for at least the duration of the game and therefore all choices from one individual would be catagorized as the same type. Evidence of consistent contribution preferences is observed from subjects whose preferences could be classified by type using the strategy method (Fischbacher, Gächter, and Fehr, 2001). The contribution choices made by individuals in this dataset also set appear to be consistent. Individuals who contributed zero in the first period, typically contributed zero in all rounds.

Table 4.7: Estimation results : Finite Mixture Poisson Model

Variable	Coefficient	(Std. Err.)
Dependent Variable: Contribution		
Equation 1 : component 1		
Period	0.009	(0.087)
Period 9	-0.063	(0.328)
Period 10	-0.975**	(0.454)
Lag Average Group Contribution	-0.011	(0.008)
Intercept	-1.617**	(0.652)
Equation 2 : component 2		
Period	0.007**	(0.003)
Period 9	0.001	(0.017)
Period 10	-0.021	(0.021)
Lag Average Group Contribution	0.001***	(0.000)
Intercept	4.408***	(0.029)
Equation 3 : component 3		
Period	0.002	(0.008)
Period 9	0.055	(0.051)
Period 10	-0.005	(0.060)
Lag Average Group Contribution	0.005***	(0.001)
Intercept	3.617***	(0.062)
Equation 4 : component 4		
Period	0.004	(0.015)
Period 9	0.096	(0.079)
Period 10	-0.095	(0.102)
Lag Average Group Contribution	0.006***	(0.001)
Intercept	2.464***	(0.114)
Proportion: π_1		
	0.278***	(0.012)
Proportion: π_2		
	0.262***	(0.016)
Proportion: π_3		
	0.251***	(0.013)
Proportion: π_4		
	0.204***	(0.012)

Significance: $***1\%, **5\%, *10\%$
Notes: Robust standard errors clustered at subject id level.

used as the test observation. The model is then fit from the remaining data. The value of the test observation is predicted from the fitted model and the predicted residual is calculated from the fit. This is repeated for each observation in the data set and the Mean Squared Error (MSE)

Eqn. (4.7) is calculated from the resulting residuals. The model that has the greatest predictive accuracy is the one with the lowest MSE. The LOOCV method is used as it is a more efficient use of the data than a leave-k-out cross-validation in which k observations are left out at each step.

$$MSE = \sum_{i=1}^{n} (y - \hat{y})^2 / n \tag{4.7}$$

Where $(y - \hat{y})$ is the difference between the observed and predicted contribution also known as the residual.

The predicted contributions for the ordered logit, are given by the contribution category with the highest probability, conditional on the leave one out covariates. There are some contribution categories that are only observed once in the dataset. These observations were dropped before the LOOCV for the ordered logit approach because if they were used as a test observation, their contribution category would not be represented in the fitted set, thus inducing zero probability of an accurate prediction. There were 25 contribution choices that were only observed once reducing the number of categories for which the ordered logit was fit to 51 and the number of total observations to 3575.

The results of the LOOCV for each estimation approach are displayed in Table 4.8. Random effects estimation had the lowest MSE therefore has the highest predictive performance of the approaches examined here. However its performance was only negligibly better than the fixed effects and tobit random effects estimation. The worst performer (by far) was the ordered logit. Surprisingly, the finite mixture model, whose Poisson distributions most closely resembled the distribution of the data, came second last. The mean error (ME) measures estimation bias by sign and magnitude and is the mean of the residuals. MEs suggest that fixed effect estimates are un-biased and random effects estimates are infinitesimally biased. Tobit random effects estimation produces the most biased estimates. The positive bias is most likely due to the mis-specification of the distribution of contributions (Fig. 4.2) as a Tobit distribution and to tobit estimates predicting the latent un-censored variable as apposed to the observed censored variable (as discussed in Section 4.3.3).

The estimation approaches that treat the dependent variable as continuous (random effects, fixed effects and tobit random effects) clearly out-perform the estimation approaches that treat the dependent variable as discrete. One explanation could be that the larger MSEs in the discrete approaches are simply due to rounding to the nearest integer and that these rounding errors are magnified by the square. To test this explanation I ran a second LOOCV on the continuous approaches that rounded predicted contributions to the nearest integer. The MSE was then calculated using the rounded prediction (MSE-integer Table 4.8) making MSE exactly comparable to the discrete MSEs. There is little difference between the MSE and MSE-integer values. Rounding does not explain the poorer performance of the discrete approaches. To see whether dropping the 25 uniquely observed observations might have aversely affected the ordered logit's predictive power, I tested its *in-sample* predictive power by running a cross validation for each variable fitting the model from every observation to give it its best chance at accurately predicting contribution choices. The MSE was just as large (2279.282) suggesting that this was not the cause of its poor performance.

Table 4.8: Leave-One-Out Cross Validation

Model	**Obs**	**MSE** (precision)	**MSE-integer**	**ME** (bias)
Random Effects	3600	1039.959	1040.367	0.001
Fixed Effects	3600	1051.498	1051.799	0.000
Tobit Random Effects	3600	1086.326	1086.209	-4.079
4-Component Poisson Mixture	3600	1267.854		-0.270
Ordered Logit	3575	2285.079		-1.885

4.5 Conclusion

The continuous estimators convincingly outperformed the discrete approaches in both precision (MSE) and un-biassedness (fixed and random effects). The difference in predictive performance between fixed effects, random effects and tobit random effects are negligible. However Tobit random effects estimates are biased. Given that there is no substantial tradeoff in performance and un-biasedness, Random effects estimation is preferred over fixed effects for VCM model estimations as it has the advantage of being able to estimate time in-variant demographic variables.

The MEs suggest that as long as a reasonable model is specified, authors should not be too concerned about the possible biases induced by censoring. In fact trying to correct for censoring will likely induce bias. Greene (1981) even concedes that estimation bias can become negligible even in the presence of severe censoring as the fit of the model increases.

The question raised from these results is why do the discrete estimation approaches perform so badly? For the FMM approach it may be because there are too many points to cluster around. This problem is one of identification. Its performance might be considerably improved by adding more components. If this were the case though, one must then question whether the FMM is reflecting a finite number of contributor types, or is instead clustering around the VCM game groups exogenously randomly determined by the experimenter. In this circumstance, the FMM would not be modeling heterogeneity in contribution preferences but simply reflecting random clustering by experimental design. Further research could be done to investigate the number of mixture components needed to outperform random effects estimation and whether the estimates are clustering on group membership.

The poor performance of the ordered logit might be because the logit is predicting the mode where as the continuous estimators are predicting the mean. If this is the case, we might expect a discrete approach to out-perform a continuous approach under a unimodal data generating process. VCM data though is characterized by multiple modes which is the likely reason the logit estimator performed so badly. In such a circumstance continuous estimators are preferred.

Occam's Razor appears to win the final debate when estimating models using VCM data. As with many things in life, the simplest solution is often the best.

Chapter 5: Conclusion

This thesis makes an original contribution to the question as to why under some institutions, despite the opportunity to free-ride, the voluntary provision of public goods is successful, and in others, voluntary provision fails. My thesis demonstrates that the design of the institution, with regard to both monetary and non-monetary incentives, can induce significant improvements in voluntary provision and free-riding. This thesis also identifies effective institutions that are relatively cheap to implement in terms of the information required and of the cost to the principal. My thesis also makes a methodological contribution by illustrating how the Public Goods Game can be used to guide the design of institutions involving the voluntary contribution of public goods and provides evidence for the best estimation approach to use for VCM data.

The VCM experiments described in Chapter two provide evidence that voluntary contributions to public goods can be significantly increased by introducing an ICS institution. The ICS institution is different from previous schemes that raise voluntary contributions in that it induces incentives for contributions that do not require external funding from a principal. The transfer of monetary benefits from one group to another provides a dominant monetary incentive to contribute as well as providing non-monetary incentives through competitive motivations. My results show that the success of the scheme is a result of an increase in both monetary and non-monetary incentives induced by the design of the ICS.

In Chapter three I demonstrate that voluntary contributions to public goods can be significantly raised by institutions using entirely non-monetary incentives (benchmarking schemes). I report evidence from VCM Public Goods Game experiments that the particular design of a benchmarking scheme is important to its success in raising voluntary contributions. The results show that the optimal benchmarking scheme is one where individuals are given feedback on their own group's contribution to the public good combined with feedback on an external group's contribution. Providing in-group information raises contributions through the anticipation that the information on group performance will be provided (not through the information itself). Where-as out-group information raises contributions through inter-group rivalry where individuals are motivated to beat the other team.

A methodological contribution on how to best model VCM repeated game data is provided in Chapter four. Until this thesis, no evidence had been given to guide the decision of which estimation approach is best for VCM game data. Many approaches are presented in the literature making it difficult for researchers to know which one to use. My empirical evidence through a LOOCV shows that the best estimation approach, in terms of unbiasedness and efficiency, is fixed effects estimation. However, as random effects is very close, and provides the option of including time invariant variables such as gender, I prescribe random effects estimation as the slightly superior estimation approach for VCM data.

The implications of the research from my thesis for economic theory are that non-monetary incentives have a significant effect on the provision of public goods. Nash equilibrium analysis of game theoretic models can be improved by attempting to incorporate non-monetary incentives into best response functions. One important non-monetary incentive for public goods provision identified by my thesis is inter-group competition.

The results of this thesis also have important positive applications for public goods provision in the natural world. New approaches to voluntary public goods dilemmas such as reducing global carbon emissions and raising productivity of working teams can be tested in the laboratory using the Public Goods Game before they are implemented. This reduces the risk of any unexpected perverse effects from new policies. Methodological improvements in estimation approaches also improves the confidence and internal validity of the laboratory results when guiding the design of policy and new institutions.

Fertile opportunities for future research that extend and build on the body of knowledge contributed by this thesis lie in field studies that can test these proposed institutions in a natural setting. A field study using the ICS can be trialled using a randomized controlled trial which pairs office buildings and electricity consumption. The difference in savings from reduced electricity consumption can be transferred from the workers in one building to another creating incentives to mitigate carbon emissions at work. Similarly, an ICS could be used to improve literacy in developing countries by producing more incentives for learning in an otherwise low incentive environment. The opportunities for improving the design of current institutions are boundless.

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