Behavioural Mechanisms of Cooperation and Coordination

Lu Dong, MSc.

Thesis submitted to the University of Nottingham
for the degree of Doctor of Philosophy

February 2017

Abstract

This thesis consists of three independent chapters investigating behavioural mechanisms of cooperation and coordination. In particular, chapter 1 analyses a voluntary contribution game and proposes a simple behavioural mechanism to achieve social efficiency. Specifically, in this mechanism, each player can costlessly assign a share of the pie to each of the other players, after observing the contributions, and the final distribution of the pie is determined by these assignments. In a controlled laboratory experiment, I find that participants assign the reward based on others' relative contributions in most cases and that the contribution rates improve substantially and almost immediately with 80 percent of players contribute fully. Chapter 2 studies the effects of costly monitoring and heterogeneous social identities on an equity principle of reward allocation. The investigation is based on the mechanism proposed in chapter 1. I hypothesised that the equity principle may be violated when participants bear a personal cost to monitor others' contributions, or when heterogeneous social identities are present in reward allocations. The experimental results show that almost half of the allocators are willing to sacrifice their own resources to enforce the social norm of equity principle. Likewise, with the presence of heterogeneous social identities, though a few participants give more to their in-group member, the majority of them still follow the equity principle to allocate. Chapter 3 explores the behavioural mechanism of communication and leadership in coordination problems. Specifically, I consider two types of leaders: cheap-talk leaders who suggest an effort level, and first-mover leaders who lead by example. I use experimental methods to show the limits of these two mechanisms in avoiding coordination failure in a challenging minimum effort game, with low benefits of coordination relative to the effort cost. The results suggest that both types of leadership have some ability to increase effort in groups with no history, but are insufficient in groups with a history of low effort.

Acknowledgements

I am deeply grateful to my supervisors Alex Possajennikov and Maria Montero. It has been an honour to be their Ph.D. student. They have taught me, both consciously and unconsciously, how good research is done. I appreciate all their contributions of time and ideas to make my Ph.D. experience productive and stimulating, especially during tough times in the pursuit.

I would also like to thank Centre for Decision Research and Experimental Economics (CeDex), for contributing immensely to my personal and professional development. The research centre has been a source of friendships as well as good advices and collaborations. I am particularly indebted to Robin Cubitt, Simon Gächter, Daniele Nosenzo, Martin Sefton and Chris Starmer. I also have had the pleasure to work with or alongside graduate students Zahra Murad, Francesco Fallucchi, Simone Quercia, Antonio Alonso, Benjamin Beranek, Cindy Fu, Di Wang, Xueheng Li.

Many stimulating conversations with people who I met in conferences and other various occasions have also contributed to my thesis. They include (but not limited to) Yan Chen, Peter DeScioli, Aidas Masillunas, Joel Sobel, Brock Stoddard, Robert Sugden.

I gratefully acknowledge the funding sources that made my Ph.D. work possible. I was funded by Nottingham University School of Economics for my study and my work was supported by the CeDEx.

Lastly, I would like to thank my family for all their love and encouragement. For my parents who raised me and supported me in all my pursuits. And most of all for my loving, encouraging, and patient husband Lingbo Huang whose faithful support during this Ph.D. is so appreciated. Thank you.

Lu Dong

February 2017

Contents

Preface

People pursuing their own interest is usually the starting point of modern economic literature. As Adam Smith (1776) famously pointed out, "It is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, but from their regard to their own interest." He implies that as individuals, we hold little regard for the community, and therefore, properly designed institutions are perhaps mankind's best hope for channeling behaviours for the good of all society. In Smith's time, the seemingly chaotic system of voluntary trading in market places was a perfect example.

In the domain of private provision of public goods, however, this self-regard often leads to inefficient under-provisions and mis-coordinations. For example, Hardin (1968) illustrates that users of a commons with pure self-interest would be trapped in an inexorable tragedy of drastic destruction. It has therefore inspired economic theorists to design and create institutions for the implementation of efficient provisions of public goods (e.g., Clarke, 1971; Groves, 1973; Groves and Ledyard, 1977; for a survey see Laffont, 1987). This traditional mechanism design literature has provided a theoretical framework for us to understand the available incentive-compatible mechanisms. However, most of the proposed mechanisms are often rather complicated and, hence, difficult to implement. As Laffont (1987 p.567) prescribed, "considerations such as simplicity and stability to encourage trust, good will and cooperation, will have to be taken into account (in real applications)."

On the other hand, numerous evidence from charitable donations to experimental games challenge the pure self-interest assumption and suggest that people have other-regarding preferences in which their utility depend not only on their own earnings but also on the earnings of the other people. We see individuals voluntarily contributing to a public good, trusting and cooperating with others (see surveys in Ledyard, 1995; Camerer, 2003). Those behaviours reflect the extent of altruistic, reciprocal or other fairness-valuing preferences in populations (e.g., Levine, 1998; Fehr and Schmidt, 1999; Cox, et al., 2007). However, the prosocial preferences can be rather unstable and subject to framing effects (e.g., Nikiforakis, 2008; Herrmann et al., 2008; Dana et al., 2007). Thus, a complete replacement of the selfishness assumption is unreliable.

We need mechanisms that take into account various forms of social preferences and at the same time simple enough to implement. So, the question is whether we can have such mechanisms and how effective are they? This thesis is a contribution to the understanding of some of these mechanisms. Specifically, it collects three independent yet closely related essays: Chapter 1 studies the voluntary contribution games and proposes a novel mechanism that relies on people's distributional preference of equity principle to achieve full cooperation. Chapter 2 explores the validity of the equity principle in the presence of costly monitoring and heterogeneous social identity; and Chapter 3 studies the effectiveness of leadership and communication mechanisms in a coordination game.

I investigate the mechanisms under the context of team work, in which team members make their voluntary contributions to a team project. The nature of the production function creates different social dilemmas. For example, chapter 1 and chapter 2 consider a production function where members' contributions are substitutable, and it creates free-rider problems. The purpose of a mechanism is thus to address individual's incentive and to improve team cooperation rates. On the other hand, chapter 3 studies a production function where team members' contributions are complementary. Such production functions generally result in multiple contribution equilibria with different social efficiencies (Harsanyi and Selten, 1988).

Teams are usually trapped in equilibria with low personal risk but also low efficiency (Van Huyck et al., 1990). The purpose of a mechanism is hence to resolve the conflict between personal risk and group benefit and, consequently, to help the team to coordinate on a more efficient equilibrium.

Measuring the effectiveness of a mechanism, we cannot simply rely on its theoretical predictions. Good theoretical properties do not equal to good empirical performance (e.g., Chen and Plott, 1996; Bracht et al., 2008; for a survey see Chen, 2008). Survey data, on the other side, can also be unreliable, as the extent to which these answers to hypothetical questions can be quite different when participants experience real situations with real people and money. I use experimental methods to provide empirical evidence for each mechanism.[1] Though an economic laboratory shares little similarity with the complicated world we live in, it at least allows us to examine in a controlled fashion how different mechanisms really affect individuals' behaviour (Smith, 1982). Below, I outline each chapter in details.

Chapter 1, which is a joint work with Rod Falvey and Shravan Luckraz, is a study of a simple decentralised mechanism that can be applied in many organisational settings. The mechanism works as follows: After a production phase, each team member is assigned a certain fraction of the surplus to be divided among other team members, and the final distribution of the surplus depends on all team members' allocations. To be concrete, if a team of N members produces a surplus of Π, each member gets to allocate $\frac{\Pi}{N}$ between other $N-1$ players, him/herself excluded. Because individual's payoff depends on others' allocation toward him/her, any deviations from any allocation strategies would not affect their payoffs and hence are all Nash equilibria in the allocation stage. In this chapter, I prove that given certain allocation rules, this mechanism would deliver an efficient outcome to reach maximum contributions. But the empirical and perhaps more important question is how do people actually allocate?

[1]Research protocols in all three chapters have been approved by the Nottingham School of Economics Research Ethics Committee (NSE-REC), which exempted me from obtaining informed consent.

I apply the mechanism to a simple team production problem where efforts are perfect substitutes, and I test its performance using experimental methods. In each experimental round, participants are randomly divided into three-person groups to make two simple decisions. First, they voluntarily decide how much to contribute their private resource to a group production, and second, given their group members' contributions, they decide how to allocate the one-third of the group productions between the others. The results show that most participants (80 percent) distribute the surplus according to others' contribution decisions. This result supports an equity principle of allocation in which each team member's reward is proportional to their contributions (Homans, 1958; Selten, 1978). The mechanism also observes a remarkable increase in the contribution level (more than 90 percent in later rounds).

The equity principle of reward allocation underpins the superior performance of this mechanism. The result is quite intuitive: suppose people anticipate that others would reward them according to their contributions, it is in their best interest to cooperate fully. Chapter 1 thereby illustrates an example of a simple mechanism that depends on people's ingrained behavioural regularity (here the equity principle of allocation) when designing economic mechanisms. Under a more complicated (and more real) environment, however, this equity principle might be challenged by other factors. Chapter 2 investigates two such factors in the context of the mechanism introduced in chapter 1. Specifically, I want to understand how the presence of costly monitoring and heterogeneous social identities would affect the validity of the equity principle.

First, costly monitoring. The ability to monitor people's contributions is a necessary condition to implement the equity principle because otherwise the rewards cannot be related to the contributions. But in many settings such monitoring can be costly; individuals have to sacrifice their personal resources to monitor others without any benefit. The clear prediction from the traditional noncooperative game theory is that people will not monitor at all under the circumstance. On the other hand, behavioural studies suggest that people have strong preferences to enforce certain social norms (e.g., Fehr and Gächter (2002); Fehr and Fischbacher, 2004). Using experimental methods, I find that about half of the participants are willing to monitor others at their own cost, and the average contribution rate is maintained above 64 percent. The experiment thus provides another example of how people's social preference, contradicting the pure self-interest assumption, in sustaining human cooperations.

The motivation to study heterogeneous social identities on equity principle is the possibility of reciprocal rewarding within small groups. Specifically, the equity principle is violated if the reward is based on team members' social identities rather than their relative contributions. Using the mechanism introduced in chapter 1, I induce heterogeneous identities within the three-person group using artificial tasks (Tajfel, 1982). From the experimental result, I find almost no evidence of in-group favouritism; the majority of the participants still follow the equity principle and the contribution rate is maintained above 80 percent. Combining the results from chapter 1 and chapter 2, we have a very promising mechanism with robust behavioural underpinnings in solving free-rider problems in team production.

Chapter 3, which is written jointly with my supervisors Alex Possajennikov and Maria Montero, investigates the effectiveness of communication and leadership mechanisms in team coordination problem. Specifically, players in the game choose costly individual efforts, and the team output is determined by the minimum of the efforts chosen. The nature of the production function dictates that coordinating on a high effort is best for the team but risky for an individual; choosing the lowest possible effort is the safest option. The purpose of introducing a mechanism is to help the team to coordinate on a high effort equilibrium. We consider two types of leaders: leaders who suggest a certain effort level to coordinate, and leaders who lead by example. The effectiveness of the leadership mechanisms is investigated either from the beginning to see whether it can prevent coordination failure or after a history of coordination failure to determine whether it can restore efficiency.

Unlike previous literatures where they find leaders' power in both preventing and restoring efficient coordination (e.g., Brandts and Cooper, 2007; Blume and Ortmann, 2007; Chaudhuri et al., 2009; Sahin et al., 2015), the result in this chapter shows the limited ability for leaders to prevent coordination failure. To be specific, we find leadership has no effect in restoring efficiency after a history of coordination failure, and no group is able to escape the coordination trap in this case. This chapter thus contributes to our understandings of the limits of the leadership mechanism, especially in its minimal implementation. In particular, leaderships are randomly assigned to participants, and the communication only consists of a single number in our experiment. We argue that for the leadership mechanisms to be empirically effective, one has to consider other aspects of the implementations, such as leader-election scheme or text-rich messages.

CHAPTER 1

Fair Share and Social Efficiency: A Mechanism In Which Peers Decide On the Payoff Division

1.1 Introduction

It can be difficult for a principal to observe individual agent's effort levels, particularly when agents work in teams. The extensive monitoring that would be required may not be feasible or cost-effective. Profit sharing has been suggested as a response (Weitzman and Kruse, 1990), since giving each of the agents a stake in an enterprise's profits does provide a link between agent effort and agent reward that is missing from a fixed wage or salary structure. But under an equal sharing rule, which is the natural allocation for a principal to impose when she cannot observe individual agent's behaviour, a free-rider problem arises since each agent bears the full cost of their effort but only reaps $\frac{1}{N}$th of the benefit in an N-agent team. Unless the costs of effort are low or the interdependencies between agent productivities in team production are high (Heywood and Jirjahn, 2009), agents will not contribute their social optimal effort under an equal-sharing regime. If rewards are not related to effort, an agent who feels under-compensated may end up reducing her effort.

Although the principal may be unable to observe agents' efforts, there will be occasions where the agents themselves are in a position to observe each others' actions.[1] The challenge then for the principal is to design a mechanism that elicits and uses this information to induce the appropriate levels of effort from the agents. In this context, we consider a simple mechanism in which agents are not only able to monitor each other, but also in positions to determine each other's payoffs. The mechanism we propose takes the form of a two stage game. In the first stage, each player chooses some effort level and in the second stage, after having observed each others' efforts, each player proposes a fraction of the total surplus to be received by each of the remaining players. A player's final share depends on the other players' allocation toward her.

We label our mechanism the "Galbraith Mechanism" (GM hereafter) as the idea is inspired by John Kenneth Galbraith who, in an aside in *The Great Crash 1929*, described a bonus sharing scheme used by the National City Bank (now Citibank) in the U.S. in the 1920s. Under this scheme each officer would sign a ballot giving an estimated share of the bonus pool towards each of the other eligible officers, himself/herself excluded. The average of these shares would then guide the final allocation of the bonus to each of the officers (Galbraith, 1963, p.171). This sharing mechanism can be applied to many economic problems including games with positive externalities and principle-agent problems in which the principal needs to distribute some common resource amongst the agents.[2]

The crucial feature of the GM is that how a player allocates shares in the second stage does not affect her own payoff. Therefore, players are able to reward or punish their peers based on the first stage observed actions. A number of studies have demonstrated that

[1] Freeman (2008) reports survey results showing: that most workers believe that they are able detect shirking by co-workers; that those participating in a profit-sharing scheme are more likely to act against shirking; and that such anti-shirking behaviour tends to reduce shirking.

[2] A familiar example where our mechanism might be applied is the division of marks in university level group assignments. Professors typically observe only the final output but wish to award marks based on individual students' inputs. In such a situation, our mechanism can be described by a two stage game in which students choose how much effort to exert in the first stage and in the second stage, after observing each others' efforts, each student proposes a fraction of the total marks (the sum of marks given to all students in the group) to be given to each of the remaining students in his group.

players exhibit social preferences to “punish" those who free-ride on the group production (Fehr and Gächter, 2000) and to “reward" those who contribute more than the group average (Sefton et al., 2007; Nosenzo and Sefton, 2012). While such social preferences move the outcome towards social efficiency, self-interest tends to restrict their application and the social costs that these punishments and rewards impose on all parties involved tend to limit their ultimate success (see Chaudhuri (2010) for a review). The practicality of implementing “costly punishment" within organisations remain unclear (Nikiforakis, 2008). The GM is based on an endogenous payoff allocation in which players can freely decide on some fraction of the co-players' payoffs. Players are free to punish, to reward, to allocate equally or even to allocate randomly to the remaining players, while no costs are incurred by any players in the allocation exercise.

The GM is also “simpler" than other endogenous mechanisms proposed to solve social dilemma problems. For example, Andreoni and Varian (1999) studied a mechanism where players can agree on a pre-play contract before the prisoner's dilemma game. However, their mechanism does not perform well when tested in laboratory settings (Bracht et al., 2008), and while there are other mechanisms that perform better in the laboratory, for example, Falkinger et al. (2000); Masuda et al. (2014) and Stoddard et al. (2014), they demand either an enforcement institution, or require the intervention of an informed third party (see Chen (2008) for a review of mechanisms tested in the laboratory). In contrast, the GM operates in a decentralised manner, with no external allocator required.[3] Provided the players are inclined to reward effort in the second stage, and they anticipate this happening at the

[3]Perhaps the model closest to ours is Baranski (2016) which also has a contribution stage followed by an allocation stage. His contribution stage is equivalent to ours, but the allocation stage is of a Baron and Ferejohn (1989)'s bargaining procedure. The bargaining patterns reveal that players are concerned with the allocations to other partners as well as their own. The final outcome achieves 80% efficiency in Baranski (2016). Our mechanism obtains a higher efficiency (90%) in the laboratory and is much simpler, players only allocate to each other which avoids the need for a bargaining process.

first stage, the GM should yield outcomes closer to social efficiency than an equal shares mechanism.[4]

The behavioural assumption underpinning the GM can be related to the notion of fairness. While some theoretical literature on fairness has focused on equality (e.g. Fehr and Schmidt, 1999), a growing empirical literature appeals to other fairness criteria to justify unequal allocations, e.g. Adams (1965); Konow(1996, 2000, 2009); Gächter and Riedl (2006); Cappelen et al. (2007); Shaw (2013); Cappelen et al. (2013).[5] For example, in one of Konow's (2000) experiments, when asked to divide some surplus among a pair of participants, a disinterested third party almost always allocates the surplus proportional to each group member's contribution to that surplus. This "proportional rule" also features prominently in our experimental results.

The remainder of the chapter is organised as follows. Section 1.2 presents our mechanism and its assumptions. Section 1.3 describes the experimental design. Experimental results are discussed in section 1.4 and section 1.5 concludes.

1.2 Galbraith Mechanism

We propose a simple model of team production that is represented by the following two stage game. There are three players. Each player, indexed i, has an initial endowment of $\bar{e} > 0$ and takes an action $e_i \in E_i = \{0, 1, ..., \bar{e}\}$ in the first stage. The players' actions determine a joint monetary outcome $\Pi = \beta \cdot \sum_{i=1}^{3} e_i$, which must be allocated among the players and where $\beta > 1$ is a parameter that represents the scale of returns of the production function. The allocation takes place in the second stage as follows. Each player i observes all actions taken in the first stage and proposes share a_{ij} of the outcome to each player j such that

[4] In practice discriminatory preferences or collusion by subgroups of players could reduce the efficiency of the mechanism. These possibilities are excluded by players' anonymity and rotation between rounds in the experiments that we report below.

[5] The literature distinguishes between two type of allocators: stakeholders and spectators. Stakeholders can allocate stakes to themselves in the allocation decisions and a self-biased fairness view may occur (Konow, 2000). Spectators allocate between the others and therefore are more likely to maintain impartiality. Under the GM, all allocators are spectators because their allocation decisions do not affect their own earnings.

$a_{ii} = 0$, $a_{ij} \in [0, 1]$ $\forall i \neq j$ and $a_{ij} + a_{ik} = 1$. In other words, each player proposes a fraction of Π to be received by each of the other players. We let q_i denote player i's final share of the outcome Π and assume that it is determined by equation: $q_i = \frac{a_{ji}+a_{ki}}{3}$. Finally, we let player i's payoff be given by $\pi_i = \bar{e} - e_i + q_i\Pi$.[6] We call this mechanism the "Galbraith Mechanism" (GM).

While the GM has freed each player from the constraint of having to protect her own interests at the second stage, at this point, we have no unique theoretical prediction on how a player i might allocate between the other two players. In fact, she can allocate all to one player, allocate equally, or even allocate randomly. But if the GM is to achieve efficient contributions, the second stage allocations must be positively related to the first stage contributions and this must be anticipated by players when they make these contributions. Motivated by previous research on the theory of distributive justice,[7] we will take the view that the players' allocations to others are consistent with some notion of "fairness" relating to the others' respective contributions. We begin by considering the "proportional rule" noted in the introduction.

1.2.1 Proportional Rule

Under this rule we assume that, in stage two, each player's allocation to the other two players is in proportion to their relative contributions (i.e., relative to each other). That is player i's allocation to player j, a_{ij}^p, is given as follows.

[6] Since $\sum_{i=1}^{3} \pi_i = 3\bar{e} + (\beta - 1)\sum_{i=1}^{3} e_i$, it is socially optimal for players to contribute $\bar{e}$ or 0 as $\beta > 1$ or < 1.

[7] According to the equity theory (Adams, 1965), the fair proportion of player i's share of the total outcome should be $q_i = \frac{e_i}{e_i+e_j+e_k}$. Under the GM, perfect implementation of the fair proportion rule cannot always be achieved. This is because the highest fraction one player can get is two-thirds. Suppose player i deserves more than two-thirds under the fair proportional rule, i.e., $q_i = \frac{e_i}{e_i+e_j+e_k} > \frac{2}{3}$, then this cannot be implemented under the GM. However, the allocation rule described in equation (1) is equivalent to what has be described as the accountability principle in Konow (1996) and the Liberal fairness rule in Cappelen et al. (2007) from a spectator's point of view.

$$a^p_{ij} = \begin{cases} \frac{e_j}{e_j+e_k} & \text{if } e_j + e_k > 0 \\ \frac{1}{2} & \text{if } e_j + e_k = 0 \end{cases} \tag{1.1}$$

The next result shows that if players allocate according to Equation 1.1 in the second stage, then the GM implements full effort in the first stage under some conditions.

Proposition 1. *Suppose that each player i uses the proportional rule in the second stage, then the strategy profile in which $e_i = \bar{e}$ for each i is a first stage dominant strategy equilibrium*[8] *if and only if $\beta > \frac{3}{2}$.*

Proof. See Appendix A1.1. □

Proposition 1 shows that as long as the joint returns to effort are sufficiently high and players take account of effort in their second stage allocations, then the GM induces maximal effort in the first stage.[9] The mechanism solves the free-rider problem in the model as players correctly anticipate that their final payoffs depend on how much effort they contribute in the first stage. Indeed, the proportional allocation rule acts as a credible threat that deters players from shirking in the first stage. Such an allocation rule is allowed by the design of the mechanism as players, after observing the action history of the first stage, can allocate freely in the second stage and hence, can freely and costlessly choose to punish, reward or even be indifferent to each others' first stage effort. It is this particular feature of this mechanism that distinguishes itself from others found in the existing literature.[10]

[8] Let $\mathbf{e} = (e_1, e_2, e_3)$ be a triple denoting an action profile of the first stage of the game. Sometimes we denote $\mathbf{e}$ by (e_i, e_{-i}). Then the set of all first stage strategy profiles is given by $\prod_{i=1}^3 E_i$. Since the players observe all actions at the end of stage one, a stage two strategy for player i is a function $a_i : \prod_{i=1}^3 E_i \to \prod_{i=1}^3 [0,1]_i$ satisfying $a_{ii}(\mathbf{e}) = 0$ and $a_{ij}(\mathbf{e}) + a_{ik}(\mathbf{e}) = 1$ for all $\mathbf{e}$ and all $i \neq j \neq k$. A stage 2 strategy profile is then given by $\mathbf{a} = (a_1, a_2, a_3)$. We call $\omega = (\mathbf{e}, \mathbf{a})$ a strategy profile of the full game. For some given $\mathbf{a}$, we say that $\omega = (\mathbf{e}, \mathbf{a})$ is a *stage one dominant strategy equilibrium* if and only if $\mathbf{a}$ is a stage 2 Nash Equilibrium and $\pi_i((e_i, e_{-i}), \mathbf{a}) > \pi_i((e'_i, e_{-i}), \mathbf{a})$ for all $e'_i \neq e_i$, for all e_{-i}.

[9] Note that the proposition can be easily extended to the n player case, that is, $e_i = \bar{e}$ for each i is a dominant strategy equilibrium in the first stage if and only if $\beta > \frac{n}{n-1}$.

[10] It can be shown that for the same lower bound on β, an alternative mechanism which fixes an equal sharing rule (commonly used in the literature) would fail to implement full effort. Indeed, it can be shown that a larger bound on β is needed to implement maximal effort for an equal division allocation.

On the other hand, given that players can allocate freely in our mechanism, one may wonder whether allocation rules that rely on “stronger" notions of justice may also work. In particular, in a case where player j puts in more effort than player k in stage one, one may ask whether an allocation rule in which player i over-compensates player j and under-compensates player k, might still implement full effort as a stage one dominant strategy equilibrium. It turns out that such a rule would also work. This is because a player who contributes maximal effort is allocated at least as much under such a rule as she would be under the proportional rule as compared to any other effort level, for all possible effort level combinations of the other players, while her costs due to exerting effort are unchanged. Therefore maximal effort remains a dominant strategy for each player (again subject to the same threshold on β).

But what if joint returns to effort are lower than this threshold? Then maximal effort may not be a dominant strategy for each player. It is straightforward to show that when $\beta < \frac{3}{2}$, $e_i = 0$ for each i is a Nash equilibrium under the GM if players use the proportional rule in the second stage. We now show the conditions under which the strongest form of such over- and under-compensation—where the largest contributor is awarded the entire allocation—leads to maximum effort.

1.2.2 Largest Contribution Rule

Under this rule we assume that, in stage two, each player's allocation to the other two players is to reward only the largest contributor, if their contributions differ, and to reward them equally if their contributions are the same. That is player i's allocation to player j is given as follows.

$$a_{ij}^l = \begin{cases} 1 & \text{if } e_j > e_k \\ \frac{1}{2} & \text{if } e_j = e_k \\ 0 & \text{if } e_j < e_k \end{cases} \tag{1.2}$$

The next result shows that if players allocate according to Equation 1.2 in the second stage, then the GM implements full contribution in the first stage under some conditions.

Proposition 2. *Suppose that each player i uses the largest contribution rule in the second stage, then the strategy profile in which $e_i = \bar{e}$ for each i is a first stage dominant strategy equilibrium if $\beta > \frac{3}{2}$.*

If $\frac{12}{11} < \beta < \frac{3}{2}$, then each player's best response is to match the others' contributions if they both choose the maximum ($\bar{e}$) or the minimum (0) and to contribute more than the second largest contributor by the minimal amount, in our case, $\Delta e_i = 1$.[11]

Proof. See Appendix A1.2. □

What about the other allocation rules that are in between the proportional rule and the largest contribution rule? Indeed, they can be generalised to an enhanced proportional rule by introducing a parameter γ. We can show that maximal effort may also be a Nash equilibrium under the enhanced proportional rule, subject to certain conditions.

1.2.3 Enhanced Proportional Rule

Under this rule we assume that, in stage two, each player's allocation to the other two players is based on their relative contributions with some adjustment for the difference in relative contributions. Let $b_{ij} = \frac{\gamma e_j + (1-\gamma)e_k}{e_j + e_k}$, where $\gamma > 1$. Then player i's allocation to player j, a_{ij}^h, is given by:

[11] In the GM, we assume the minimal incremental amount of the contribution Δe_i is 1. If Δe_i can be arbitrarily small, the result will be true for $1 < \beta < \frac{3}{2}$.

$$a_{ij}^h = \begin{cases} 1 & \text{if} \quad b_{ij} \geq 1 \quad \text{i.e.} \quad e_j \geq \frac{\gamma}{\gamma-1} e_k, \\ b_{ij} & \text{if} \quad \frac{\gamma}{\gamma-1} e_k > e_j > \frac{\gamma-1}{\gamma} e_k, \\ 0 & \text{if} \quad b_{ij} \leq 0 \quad \text{i.e.} \quad e_j \leq \frac{\gamma-1}{\gamma} e_k. \end{cases} \tag{1.3}$$

Note that the proportional rule is a limiting case here when γ converges to unity from above, and that $b_{ij} = \frac{1}{2}$ if $e_j = e_k$. The larger is γ, the greater the deviation from the proportional rule; the smaller the range $\frac{\gamma}{\gamma-1} e_k > e_j > \frac{\gamma-1}{\gamma} e_k$, and hence the larger the ranges for the extreme values, for a given e_k. We can also write

$$b_{ij} = a_{ij}^p + (\gamma - 1) \frac{e_j - e_k}{e_j + e_k}$$

which allows us to explicitly consider deviations from the proportional rule using

$$a_{ij}^h - a_{ij}^p = \begin{cases} 1 - a_{ij}^p & \text{if} \quad (\gamma-1)\frac{e_j - e_k}{e_j + e_k} \geq 1 - a_{ij}^p, \\ (\gamma - 1) \dfrac{e_j - e_k}{e_j + e_k} & \text{if} \quad 1 - a_{ij}^p > (\gamma-1)\frac{e_j - e_k}{e_j + e_k} > -a_{ij}^p, \\ -a_{ij}^p & \text{if} \quad -a_{ij}^p \geq (\gamma-1)\frac{e_j - e_k}{e_j + e_k}. \end{cases} \tag{1.4}$$

We can interpret the magnitude of $\gamma - 1$ as indicating the extent to which the player rewards the larger contributor and punishes the smaller contributor, relative to the proportional rule. We can show that if players allocate according to Equation 1.3 in the second stage, then the GM implements full effort as a Nash equilibrium in the first stage under some conditions.

Proposition 3. *Suppose that each player allocates using the enhanced proportional rule outlined in Equation 1.3 in the second stage and that $\beta > \frac{3}{2\gamma}$. Then $(\bar{e}, \bar{e}, \bar{e})$ is a Nash equilibrium of the game.*

Proof. See Appendix A1.3. □

Given that $\beta > 1$, Proposition 2 shows that $\gamma \geq \frac{3}{2}$ is sufficient for full contribution to be a Nash equilibrium under Equation 1.3.

Motivated by this discussion, we now investigate whether the experimental findings will support these predictions. In particular, we run experiments to try to answer the following two questions: first, how do people allocate in the second stage, and second, how do people contribute in the first stage? Since Propositions 1 and 2 indicate that the success of the GM in implementing full contributions is likely to be sensitive to the scale of returns on the production function (β), we run two sets of experiments under the GM. Those labelled GM1.8 set $\beta = 1.8$, which is above the threshold $\frac{3}{2}$; while those labelled GM1.2 set $\beta = 1.2$ which is below the threshold. The results of this section lead us to expect greater success for the GM when β is above the threshold.

1.3 Experimental Design and Procedure

We ran 24 experimental sessions at the Centre for Decision Research and Experimental Economics (CeDEx) in Nottingham in February 2015. In total, 216 university students from various fields of study took part, with 9 participants in each session. Participants were allowed to participate in only one session. Those participants were drawn from the CeDEx subject pool, which was managed using the Online Recruitment System for Economic Experiments (ORSEE; Greiner, 2015). The experiment was programmed in z-Tree (Fischbacher, 2007). Each session lasted about 60 minutes and the average payment was £8.34 (equivalent to $12.93 or €11.65 at the time of the experiment).

Upon arrival, participants were asked to randomly draw a number from a bag and they were seated in a partitioned computer terminal according to that number. The experimental instructions were provided to each participant in written form and were read aloud to the subjects (the instructions can be found in Appendix A2). The experiment only started after all participants had given the correct quiz answers with respect to the instructions. The experiments have three treatments: one control treatment and two GM treatments.[12]

[12] We had 10 sessions each of the GM but only 4 sessions of the Control treatment because previous studies strongly and robustly predict that equal share produces a low contribution rate (Ledyard, 1995). This conclusion was also supported by all four sessions of the control treatments.

Table 1.1: Experiment Design

Sessions	Treatment Round1-10	Treatment Round11-20	β	Matching Protocol	No. of subjects	Indep. Groups
Control	Equal Share	Equal Share	1.8	Random	36	4
T0	Equal Share	GM1.8	1.8	Random	90	10
T1	Equal Share	GM1.2	1.2	Random	90	10
Total:					216	24

Each experiment contained 20 rounds of decision making tasks that can be divided into two segments of ten rounds (see Table 2.1). The instructions for the second ten-round segment were distributed only after the completion of the first ten rounds. In each round, the computer program draws three participants to form a group, and the group composition reshuffles every round.[13] Participants were asked to complete a post-experimental survey (see Appendix A4) before they were paid privately.

Equal sharing rules were applied to all participants in the first ten rounds.[14] We used neutral terminology in the experiment and the contribution question formulated on the computer screen was “Tokens you want to add to the Group Fund:_ _." In each round, each player i chose an integer from 0 to 10, which represented her contribution, e_i. The production function was $\Pi(e_1, e_2, e_3) = \beta(e_1 + e_2 + e_3)$, and each player's payoff function was $\pi_i = 10 - e_i + \frac{1}{3}\Pi$. Two alternative values of β are considered. In sessions Control and T0, β equals 1.8, and in sessions T1, β equals 1.2.[15] At the end of each round, players were

[13] The matching of the three-person group was pre-determined by the computer software. Specifically, each participant would never be in the same group with the two other participants twice during the whole experiment. We randomized the display of players' contribution details on the screen in each round; in this way players were not able to track the identities of other players across rounds.

[14] The equal sharing rule, where the final production is equally divided among group members, is equivalent to the voluntary contribution mechanism. To compare with other studies (e.g., Andreoni and Varian (1999); Fehr and Gächter (2000); Falkinger et al. (2000)), we introduce our mechanism after ten rounds of the equal sharing rule.

[15] In session T1, β equals 1.2 in all rounds to minimize the variable changes during the experiment. As will be shown in section 4.1, the first ten rounds of all three treatments yield very similar results.

informed about all group members' contributions and payoffs and were reminded that they would not be in the same group again.

In rounds 11-20, there were two decision stages in each round of the GM. The first stage decision was the same as in the Equal Share treatment, that is, each player voluntarily chose an integer from 0 to 10. In the second stage, the computer screen displayed each group members' contribution decision in the first stage and the value of the group fund. Each player's task was to divide $\frac{1}{3}\Pi$ between the other two group members with a resolution of 0.1. The allocation question formulated on the computer screen was "Please divide [insert $\frac{1}{3}\Pi$] between player A and player B". In other words, player i allocated $\tilde{a}_{ij}$ to player j and allocated the remaining $\tilde{a}_{ik} = \frac{\Pi}{3} - \tilde{a}_{ij}$ to player k.[16] Player i's share of the group production was determined by the allocation decisions by player j and player k. Her payoff function was $\pi_i = 10 - e_i + \tilde{a}_{ji} + \tilde{a}_{ki}$. In the control treatment, players simply repeated the same decision task as in rounds 1-10 for another ten rounds.

1.4 Experimental Results

We split the analysis into three parts. Section 1.4.1 looks at the difference in the contribution decisions across treatments. Section 1.4.2 analyses the participants' allocation decisions, and Section 1.4.3 studies how allocation choices affect the players' contribution decisions.

1.4.1 Contributions

Figure 2.1 displays the time-path of the average contributions over all 20 iterations for each treatment. In the first ten rounds, when the equal sharing rule is used, we observe a steady decline in the level of contributions over time. Participants start with an average contribution level of 3.32 (3.72 if β equals 1.8 and 2.92 if β equals 1.2) and end up with 0.63 in round

[16] In section 1.2, a_{ij} is defined as the proportion player i allocates to player j, and $a_{ij} + a_{ik} = 1$. To calculate player's final profit, a_{ij} would be normalized by dividing by the group number and multiplying by the joint profit, i.e., $\pi_i = 10 - e_i + \frac{a_{ji}+a_{ki}}{3}\Pi$. To make our experiment cognitively easy, we asked participants to divide $\frac{\Pi}{3}$ ex ante. In other words, we set $\tilde{a}_{ij} = \frac{\Pi}{3} a_{ij}$.

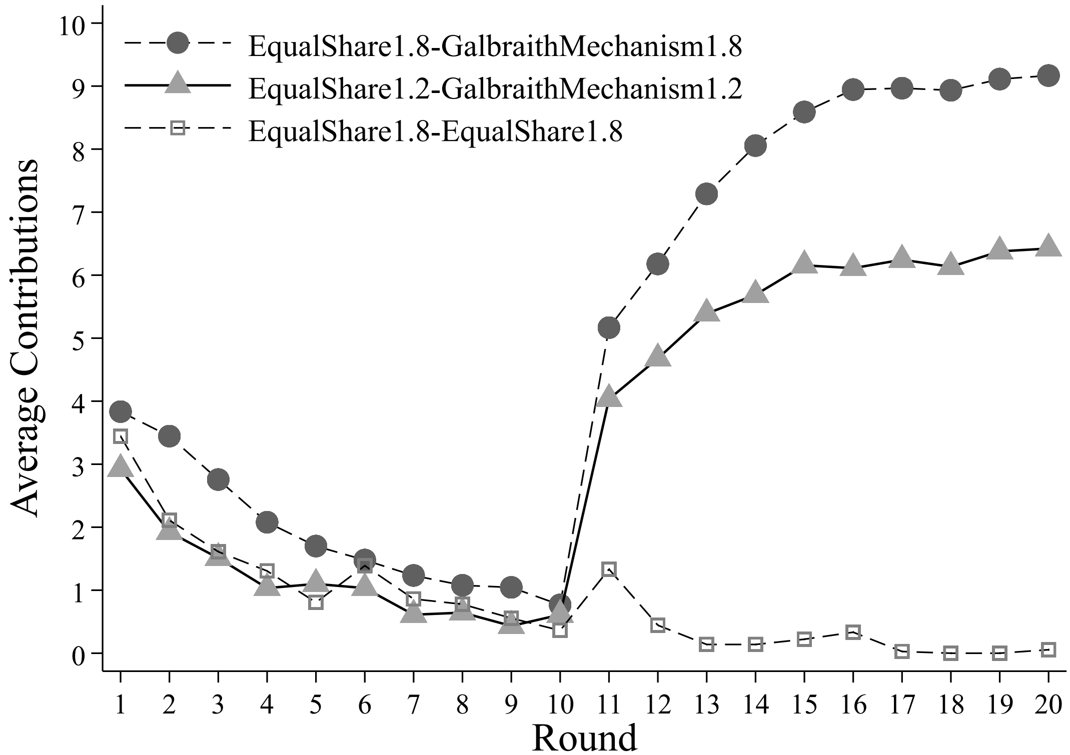


Figure 1.1: Time-path of the Average Contribution by Treatment

10 (0.65 if β equals 1.8 and 0.61 if β equals 1.2). The average contribution levels do not differ across treatments ($p > 0.1$). This finding is consistent with results from other studies in which group compositions are reshuffled every round (e.g.,Croson, 1996; Fischbacher and Gächter, 2010).

At the beginning of round 11, we introduce the GM in 20 out of 24 sessions. This introduction triggers a dramatic increase in the contribution level. Specifically, in round 11, the contribution level in both the GM1.8 (mean = 5.17) and the GM1.2 (mean = 4.03) are significantly higher than the contribution in the control treatment (mean = 1.33, Mann-Whitney test, $p < 0.01$).[17] Over the ten rounds of the GM1.8, the average contribution is 8.0 and the final round contributions reach an average of 9.16. Indeed, in round 20, most players (82.8%) in the GM1.8 contribute fully to the group fund, and 21 out of 30 three-

[17]Because the Mann-Whitney test requires independent observations, the tests are conducted on sessions' average contribution level. The two-sided p values are reported.

player groups coordinate on the (10,10,10) equilibrium. Compared to the GM1.8, the average contribution in the GM1.2 (mean=5.72) is relatively lower ($p < 0.05$).[18] Specifically, in the last round of the GM1.2, the average contribution is 6.42; 38.9% of the players contribute fully and 7 out of 30 groups coordinate on the (10,10,10) equilibrium. But there are also 11.1% of the players contributing zero and 1 of the 30 groups coordinates on the (0,0,0) equilibrium. On the other hand, almost all players (35 out of 36) in the control treatment have zero contribution in later rounds. Appendix A3 shows more comparative statistics of the contribution decisions across sessions and treatments.

Result 1. *Both the GM1.8 and the GM1.2 produce a much higher average contribution level compared to the control treatment, particularly in later rounds. The average contributions in the GM1.2 are lower than those in the GM1.8, consistent with our expectations. But even with the lower β value, we observe that the GM has greatly improved the contribution rate relative to equal sharing.*

1.4.2 Allocation Decisions

In this section, we investigate players' allocation decisions. Recall that for each round in the GM treatment, participants need to decide on how to allocate between the other two group members. The allocation must sum up to one third of the group fund, that is, $\tilde{a}_{ij} + \tilde{a}_{ik} \equiv \frac{\Pi}{3}$. In the following analysis, we only consider each player i's allocation to player j (randomly determined from the data), $\tilde{a}_{ij}$, because the allocation to each player k is automatically determined by $\tilde{a}_{ik} \equiv \frac{\Pi}{3} - \tilde{a}_{ij}$.

We represent players' allocation choices from the GM1.8 and GM1.2 in Figure 1.2a and Figure 1.2c, respectively. The horizontal axis indicates the fraction player j contributes relative to player k, that is, $\frac{e_j}{e_j+e_k}$, and the vertical axis shows the actual fraction i allocates to player j, that is, $\frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}}$. The size of the circle indicates the relative frequency of the

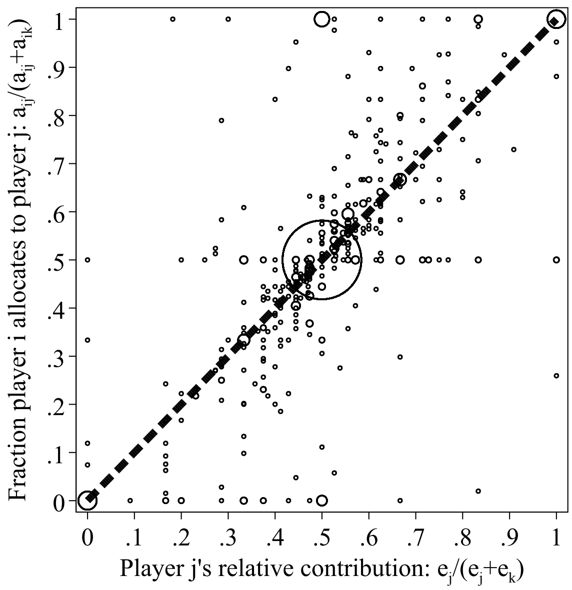
[18] Baranski (2016) assumes $\beta = 2$ in his experiment, which makes his results more comparable with those for the GM1.8.

observation. Around 55.4% of the observations in the GM1.8 and 41.7% in the GM1.2 fall exactly on the 45-degree line where $\frac{e_j}{e_j+e_k} = \frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}}$. This means a large number of players allocate proportionally according to the others' relative contributions. Indeed, the fractional allocations (mean of $\frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}}$ equals 0.502) are very close to players' relative contributions (mean of $\frac{e_j}{e_j+e_k}$ equals 0.501, t-test, $p > 0.1$).

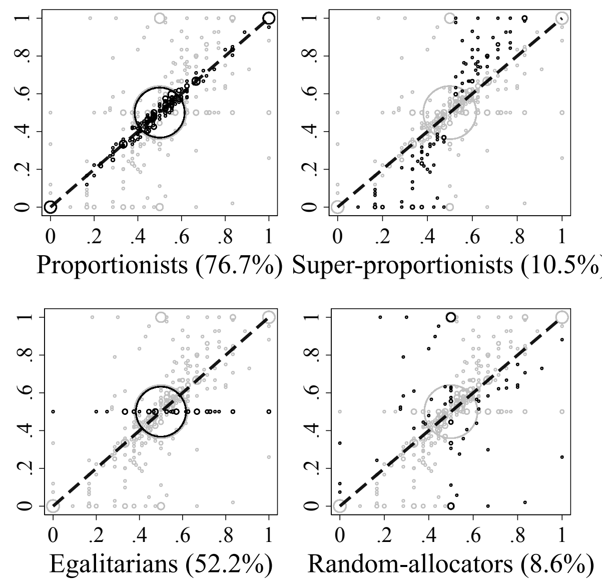
Table 1.2 presents the results of further investigation of contributions using random effects regressions. In these regressions the dependent variable is the fraction player i allocates to player j, and the independent variable is the contribution of j relative to k. The proportional rule predicts the coefficient of $\frac{e_j}{e_j+e_k}$ equals 1 and the intercept term equals zero, and the estimates of these parameters in both regressions are consistent with this prediction. For both the GM1.8 and the GM1.2, the estimated coefficients (0.919 and 0.933, respectively), are different from zero ($p < 0.01$) and not significantly different from one (F-test, $p > 0.1$). The intercept, meaning the fraction player i allocates to player j when player j deserves zero, is not significantly different from zero ($p > 0.1$).

But even a casual look at the spread of observations in Figure 1.2a and Figure 1.2c suggests that *not all* players are in fact following the proportional rule. Not all observations are gathered about the 45-degree line. In particular, there appears to be some clustering of observations above this line when $\frac{e_j}{e_j+e_k} > \frac{1}{2}$, and a corresponding clustering below this line when $\frac{e_j}{e_j+e_k} < \frac{1}{2}$, in both cases. After further inspection we find that we can categorise allocators into four different types as follows.

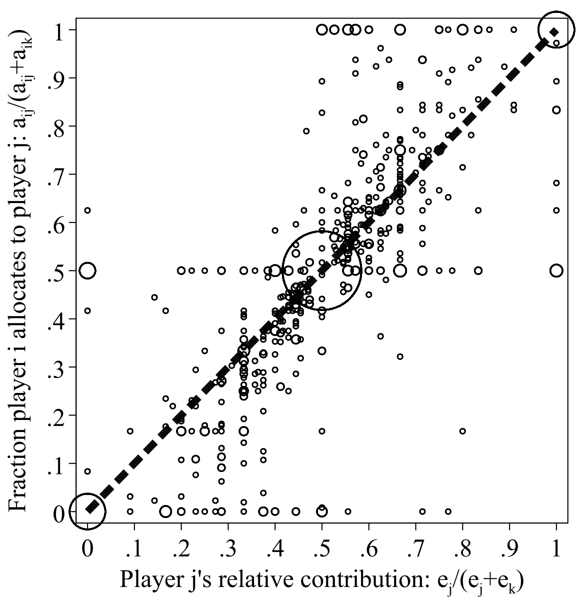
Proportionists These are the players who allocate strictly according to other's relative contributions, that is, $\frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}} = \frac{e_j}{e_j+e_k}$. Because our software only allows the input with a resolution of 0.1 and $\frac{e_j}{e_j+e_k}$ may not always be a fraction of ten, we define player i as a Proportionist if $\mid \frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}} - \frac{e_j}{e_j+e_k} \mid \leq 0.05$ (see the category highlighted as Proportionists in Figure 1.2b and 2d). In most situations (76.7% in the GM1.8 and 64.4% in the GM1.2), players allocate like proportionists.



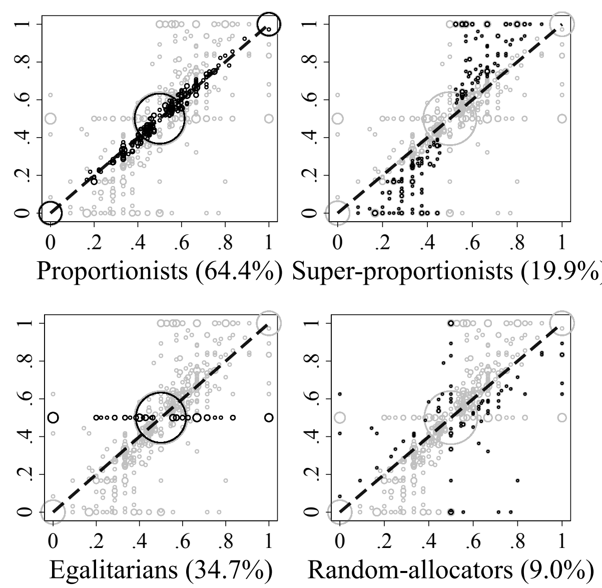
(a) GM1.8: Allocation Choices



(b) GM1.8: Four types of allocators



(c) GM1.2: Allocation Choices



(d) GM1.2: Four types of allocators

Notes: Figure (a) and Figure (c) includes 900 allocation decisions each from the GM1.8 and the GM1.2 respectively. The size of the circle indicates the relative frequency of the observation. Observations lying on the 45-degree line means player i use the proportional rule to allocate.

Figure 1.2: Allocation Decisions in the Galbraith Mechanism

Table 1.2: Allocation Choice: Random Effects

	Dep. Variable: Fraction Player i Allocate to Player j	
	(1)	(2)
Player j's relative contribution: β_1	0.919***	0.933***
	(0.068)	(0.068)
Intercept: β_0	0.044	0.041
	(0.034)	(0.038)
#Data Used	GM1.8	GM1.2
#Observations	900	900
#Clusters	10	10
$H_0 : \beta_1 = 1$	1.43	1.39
	($p = 0.231$)	($p = 0.238$)

Notes: (1) The table reports the regression results for random-effects model with the standard error clustered at the session level. *** indicates significance at 1% level. (2) Period dummies are controlled for in the regression: the estimated coefficients are between -0.027 to 0.022, and they are not significantly different from zero at conventional significance levels. (3) We report the test statistics for the hypotheses tests and 2-sided p values are in the brackets. (4) Hausman tests for random vs fixed effects model for both regressions yield p values greater than 0.1.

Egalitarians These are the players who allocate equally to the other two group members regardless of their contributions. In 52.2% of the observations from the GM1.8 and 34.7% from the GM1.2, players allocate using the egalitarian rule (see the category highlighted as Egalitarians in Figure 1.2b and 1.2d). Note that proportionists and egalitarians are not mutually exclusive. For example, if the other two players contribute the same amount, both the proportional and the egalitarian rules predict an equal allocation. This is not a rare case especially in later rounds (rounds 16-20), where full contributions of all three players are frequently observed. In total, there are 48.2% (GM1.8) and 28.0% (GM1.2) of the observations that can be classified under both proportional and egalitarian rules. However, when conditioning on the inequality of contributions between the other two players, only less than 5% of players choose to allocate equally.

Super-proportionists If player j contributes less than player k, player i, under the "super-proportionists" category, rewards player j with less than what a proportionist would give. The other player, player k, is consequently over-compensated. 10.5% (GM1.8) and 19.9% (GM1.2) of the observations fall under this rule (see the category highlighted as super-proportionists in Figure 1.2b and 1.2d).[19] In other words, super-proportionists tend to "punish" the player who has a lower relative contribution and "reward" the player who has a higher relative contribution. Note that the punishment possibility in the GM is different from the "punishment mechanism" in Fehr and Gächter (2000). In their setting, players can choose to incur a cost to destroy part of the other players' payoff. With the GM, however, players bear no cost to punish others. Moreover, if a super-proportionist punishes one player, the other group member will be over-compensated automatically. The overall welfare, hence, remains the same.

Random-allocators The remaining 8.7% (GM1.8) and 9.0% (GM1.2) of observations that cannot be captured by any of the three rules listed above, we label as random allocators (see the last panel of Figure 1.2b and 1.2d).

Any super-proportional allocation can be captured by an enhanced proportional rule with the appropriate γ, although where the allocation is extreme (i.e. all to the highest contributor) this γ will not be unique. But it seems extremely unlikely that the participants in our experiments are explicitly applying a formula like this, and the value of γ implicit in their allocations may well differ from one round to the next. We therefore resist any temptation to impose the rule at the individual level, and instead simply use the formula as a way of capturing in a single parameter (γ), the overall willingness to reward and punish of those players adopting a super-proportionist allocation. Recall that in equation (3), the difference between the share allocated by i to j and j's relative contribution is explained

[19] Note that unequal contributions is a necessary condition for an allocation to be classified as super-proportional. When contributions are equal, super-proportionists cannot be distinguished from proportionists or egalitarians. In the GM1.8, 51.8% of the allocation decisions are unequal while in the GM1.2, 72.0% of them are unequal. If we only count the unequal contributions, 21.9% of the allocation decisions in the GM1.8 and 28.6% in the GM1.2 can be classified as super-proportional.

by the difference in the relative contributions of j and k, $(\frac{e_j - e_k}{e_j + e_k})$, multiplied by parameter $\gamma - 1$, which reflects the willingness to reward/punish relative to the proportional rule. This relationship has upper and lower bounds which are also triggered by the same explanatory variable. We fit this equation by conducting a grid search on γ and finding the value that minimises the sum of squared residuals. A bootstrapping procedure is then used to determine the distribution of this estimate.[20] Our estimates of γ turn out to be very similar in the two regimes, i.e., 1.377 in the GM1.8 and 1.320 in the GM1.2. Both are significantly higher than unity ($p < 0.001$) and are not significantly different from each other ($p > 0.1$).[21] This indicates that on average super-proportionists reward and punish by adjusting the share allocated from the proportional rule by about 35% of the difference in relative contributions.[22]

To understand what may help to explain players' different allocation rules outlined above, we use probit regressions of the form:

$$Pr\{A_{ij,r} = 1\} = \Lambda(\alpha_1 Contri_{i,r} + \alpha_2 OtherContri_{i,r} + \alpha_3 \frac{e_j}{e_j + e_k} + \alpha_4 GM1.2 + \alpha_5 Round_i + \varepsilon_{i,r})$$

where $A_{ij,r} = 1$ if the subject chooses a certain allocation rule to allocate to player j in round r, and zero otherwise. We use the allocation rules defined in section 1.4.2 to classify $A_{ij,r}$. That is, in models (1)-(4), $A_{ij,r}$ indicates whether or not player i is using the proportional rule, the egalitarian rule, the super-proportional rule or a random allocation to allocate in round r, respectively. $Contri_{i,r}$ is player i's own contribution in round r. The variable $OtherContri_{i,r}$ is the average contribution of the other two group members in round r. The variable $\frac{e_j}{e_j + e_k}$ is player j's contribution relative to player k, the variable $GM1.2$ equals 1 if the player belongs to the GM1.2 treatment, and $\varepsilon_{i,r}$ is the error term. We exclude observations

[20] We searched for γ over the range 1.001 to 1.5 in STATA and bootstrapped with 1000 replications to obtain the standard error of the estimate.

[21] From Proposition 2, full contributions will be a Nash equilibrium in a game played by super-proportionists with $\gamma = 1.32$ and $\beta = 1.2$.

[22] If we retain the focus on observations with unequal contributions but also include egalitarians and randomists the resulting estimate of γ is 1.076 and is not significantly different from unity ($p > 0.1$).

Table 1.3: The Determinants of Allocation Rules

	1 if player choose the allocation rule:			
Dependent Variables:	(1) Proportional	(2) Egalitarian	(3) Super-proportional	(4) Random
$Contribution_i$	0.059**	-0.225***	0.063**	-0.054***
	[0.023]	[-0.021]	[0.020]	[-0.012]
	(0.021)**	(0.045)***	(0.023)**	(0.015)***
$Others'Contribution_i$	-0.056	0.017	-0.009	0.088**
	[-0.022]	[0.002]	[-0.003]	[0.019]
	(0.029)	(0.028)	(0.027)	(0.036)**
Relative Contribution: $\frac{e_j}{e_j+e_k}$	-0.082	0.071	-0.185	0.358
	[-0.033]	[0.007]	[-0.057]	[0.078]
	(0.108)	(0.202)	(0.100)	(0.219)
The GM1.2 Treatment	-0.083	-0.217	0.357***	-0.184
	[-0.033]	[-0.020]	[0.111]	[-0.040]
	(0.151)	(0.160)	(0.100)***	(0.184)
$Round_i$	-0.010	-0.002	-0.009	0.036
	[-0.004]	[-0.000]	[-0.003]	[0.008]
	(0.021)	(0.030)	(0.021)	(0.034)
Constant	0.283	-0.344	-1.019**	-1.905**
	(0.313)	(0.406)	(0.349)**	(0.598)**
pseudo R^2	0.016	0.207	0.025	0.034
Log-likelihood	-758.797	-259.277	-606.177	-441.223
Clusters	20	20	20	20
Number of observations	1114	1114	1114	1114

Notes: The table shows four Probit estimates of the propensity for players to choose each allocation rule. Standard errors clustered at the session level are reported in brackets and the implied average marginal effects are shown in parentheses below the coefficient estimates. *,** and *** denote, respectively, significance level at the 10%, 5% and 1% levels.

where the proportional rule and the egalitarian rule predict the same outcome, because they do not help us to distinguish how players choose different rules.

Table 1.3 presents the estimated parameters for the model. (1) The proportional rule is chosen by high contributors. (2) The egalitarian rule tends to be chosen by low contributors. (3) The super-proportional rule tends to be chosen by high contributors, and players in the GM1.2 treatment are more likely to choose this rule. (4) Random allocations tend to made by low contributors when others' contributions are high.

Result 2. *Most allocations are related to players' contributions and the overall outcomes are consistent with players following the proportional rule. Super-proportionists reward and punish by about 35% of the difference in relative contributions on average. There are also a small number of egalitarians and random allocators.*

1.4.3 Allocation Received and Contribution Decisions

So far, we have established that most players do allocate according to others' relative contributions and the contribution rate is high. In this section, we check the causal relationship between these two events. Specifically, we look at the effect of the allocation players received in the previous round on their contribution decisions in the current round. Note that the allocation a player receives in a certain round is the aggregate result of her two group members' allocation rules, and we categorise these as in the previous section.[23] Most players are treated by the proportional rule under most circumstances (77.5% in the GM1.8 and 67.7% in the GM1.2), and in many cases all three group members are rewarded with equal shares (47.7% in the GM1.8 and 30.2% in the GM1.2). Note that, in 45% of the total observations in the GM1.8 and 25.7% in the GM1.2, these two rules overlap because all three players contribute equally. Since these observations cannot be assigned to a specific allocation rule we exclude them from the following analysis.

In this restricted dataset ($n = 415$ in the GM1.8 and $n = 669$ in the GM1.2), players are treated by the proportional rule in 59.1% of the cases in the GM1.8 and 56.5% in the GM1.2. Players are subject to a "super-proportional" allocation in about 26.5% of the cases in both regimes. So players are treated to an allocation based on their relative contributions in over 80% of the cases under each regime. But when considering responses

[23] The share player i should receive according to the proportional rule is $q_i^p = \frac{1}{3}(\frac{e_i}{e_i+e_j} + \frac{e_i}{e_i+e_k})$. When $e_i + e_j = 0$ or $e_i + e_k = 0$, $q_i^p = \frac{1}{6}$ and when $e_i + e_j + e_k = 0$, $q_i^p = \frac{1}{3}$. The actual fraction player i receives is $q_i = \frac{a_{ji}+a_{ki}}{3}$. Player i is treated by the proportional rule if $|q_i - q_i^p| \leq 0.05$. Player i is treated by the egalitarian rule if $q_i = \frac{1}{3}$. When $q_i < q_i^p < \frac{1}{3}$ and $q_i^p - q_i > 0.05$, we say player i is being under-compensated, or punished. When $q_i > q_i^p > \frac{1}{3}$ and $q_i - q_i^p > 0.05$, we say player i is being over-compensated, or rewarded. If player i's allocation does not fit any of these classifications we refer to it as "random".

to past allocations it is useful to distinguish between those"punished" (under-compensated relative to the proportional rule) and those "rewarded" (over-compensated relative to the proportional rule) in the previous period. In 12.6% of the cases in the GM1.8 and 15.1% in the GM1.2, players are punished; and in 13.8% of the cases in the GM1.8 and 15.1% in the GM1.2, players are rewarded. Players are treated by the egalitarian rule in 4.9% of the cases in the GM1.8 and 6.1% in the GM1.2. In the remaining 9.7% of cases in the GM1.8 and 10.9% in the GM1.2, players appear to be treated by a random allocation.

Table 1.4 presents the one-round change in the contributions according to whether or not the player is treated by a certain allocation rule from the previous round. We want to discover whether there are differences in the one round change based on different allocations players received from the last round. For example, the first row in the table shows that players who are "punished" increase their contributions in the next round (mean = 1.85 in the GM1.8 and mean = 0.84 in the GM1.2) by more than those players who are *not* punished ($p < 0.1$). While receiving a proportional allocation increases players' contributions (mean = 0.90 in the GM1.8 and mean = 0.47 in the GM1.2) more than being treated by other rules ($p < 0.05$), being rewarded stimulates a smaller increase (mean = 0.36 in the GM1.8 and mean = 0.15 in the GM1.2).[24] Players who receive an equal allocation despite their unequal contributions lower their next round contributions (mean = -0.19 in the GM1.8 and mean = -0.10 in the GM1.2; $p < 0.01$). Lastly, being treated by random allocations have strong negative effects on players' contributions in the subsequent round; on average, those players lower their contributions (mean = -0.21 in the GM1.8 and mean = -0.74 in the GM1.2) compared to those players who are treated by alternative rules ($p < 0.01$).

We use regression analysis to further investigate the relationship of how a player is treated in the previous round affects her next round contribution. Our behavioural equation of the change in contribution for player i in round r, $\Delta e_{i,r}$ is given by:

[24] Players who are being overcompensated have an average contribution of 8.69 in the GM1.8 (6.75 in the GM1.2) and 70.6% of these players have full contributions (37.6% in the GM1.2). Therefore, it is difficult for over-compensation to stimulate further increases in contribution. Previous studies also find that the effect of punishment is stronger than the effect of reward (e.g. Andreoni et al., 2003; Sefton et al., 2007)

Table 1.4: Allocation rule received and the one round change in contributions

Allocation Rule Received	Fractions		One-round change		Hypothesis Test (p-value)	
	GM1.8	GM1.2	GM1.8	GM1.2	GM1.8	GM1.2
Be under-compensated	12.6%	11.4%	1.85	0.84	0.064	0.007
Be treated proportionally	59.1%	56.5%	0.90	0.47	0.006	0.020
Be over-compensated	13.8%	15.1%	0.36	0.15	0.030	0.069
Be treated by Egalitarian rule	4.9%	6.1%	-0.19	-0.10	0.040	0.015
Be treated by Random allocation	9.7%	10.9%	-0.21	-0.74	<0.001	<0.001

Notes: (1) The results are based on 415 allocation decisions in the GM1.8 treatment and 669 allocation decisions in the GM1.2 treatment from round 12 to round 20 classified by the allocation rules players received in the previous round. We exclude the observations where all three players contribute equally, as both the proportional rules and the egalitarian rule yield the same prediction. (2) The hypothesis tests are ranksum tests with the null hypothesis of equal one-round change in the contributions between the specific allocation rule received and other rules received. The tests are clustered at the individual level and the two-sided p-values are reported.

$$\Delta e_{i,r} = \kappa_0 + \mathbf{B_{i,r-1}}\theta + \kappa_1 OtherContribution_{i,r-1} + \kappa_2 GM1.2 + \kappa_3 Round_i + \varepsilon_{i,r}$$

Here $\mathbf{B_{i,r-1}}$ is a set of dummy variables which indicate how the player was treated at the allocation stage in the previous round. Specifically, $B^{EGA}_{i,r-1}$, $B^{UNDERCOMP}_{i,r-1}$, $B^{OVERCOMP}_{i,r-1}$, $B^{RANDOM}_{i,r-1}$ are dummy variables indicating whether the player was treated by the egalitarian rule, was undercompensated, was overcompensated or was subject to a random allocation, respectively, in the previous round. Being treated by the proportional rule is taken as the base case. The variable $OtherContribution_{i,r-1}$ represents the average contribution of the other two members in the player's group in the previous round. This is intended as a proxy for the player's belief about the likely contributions of the other group members in the current round. The behavioural regulation of conditional cooperation (i.e. matching the other group members' contributions) is well documented in the literature (e.g. Fischbacher et al., 2001; Fischbacher and Gächter, 2010). $GM1.2$ equals 1 if the player belongs to the treatment GM1.2, $Round_i$ captures the time trend and $\varepsilon_{i,r}$ is an unobservable variable that

Table 1.5: Determinants of One-Round Contribution Change

Dependent Variable:	One-round Change in Contribution		
Treated by the egalitarian rule	-0.748***	-0.725***	-0.210
	(0.147)	(0.108)	(0.316)
Being under-compensated	0.437	0.603**	3.003**
	(0.288)	(0.256)	(0.794)
Being over-compensated	-0.305*	-0.396**	0.236
	(0.159)	(0.114)	(0.290)
Treated by random allocations	-1.009***	-0.851**	-0.991**
	(0.223)	(0.244)	(0.462)
Others' average contribution	0.131***	0.128***	0.377**
	(0.0237)	(0.0224)	(0.105)
The GM1.2 Treatment	-0.0348	0.0263	0.0485
	(0.112)	(0.0946)	(0.345)
Round	-0.131**	-0.139***	
	(0.0371)	(0.0254)	
Constant	1.940**	2.023***	-0.831
	(0.550)	(0.378)	(0.634)
Round used	12-20	12-20	12
Data Excluded	Yes	No	No
Clusters	20	20	20
Observations	1003	1620	180
Adjusted R^2	0.108	0.127	0.269

Notes: All results are from OLS regression. Standard errors clustered on session level are reported in brackets. *,** and *** denote, respectively, significance level at the 10%, 5% and 1% levels. Column 2 excludes observations where all three players contribute equally.

is assumed to have mean zero and is uncorrelated with other explanatory variables. The estimation method is OLS with robust standard errors clustered at the session level.

Table 1.5 presents the estimated equations. The result in Columns 2 and 3 confirm that, relative to the proportional rule, egalitarian or random treatment in the previous round results in a lower increase (or larger decrease) in contribution in the current round. The estimates in Column 2 exclude the “dual" observations (i.e. those that meet both the proportional and egalitarian criteria), and there we find that players who were punished in the previous round seem to increase their current contribution (the estimated coefficient is 0.437, two sided $p = 0.15$) and those who were over-compensated tend to decrease their current contribution (the estimated coefficient is -0.305, two sided $p = 0.07$). These results are strengthened when we include the dual observations under the egalitarian dummy in Column 3. We also find no difference between the treatments as the magnitude of the estimated coefficients of $GM1.2$ are quite small and are not significantly different from zero. The estimated coefficient on $OtherContribution_{i,r-1}$ is consistently positive and significant indicating that a higher average contribution by the other group members in the previous round generates a larger increase (or smaller decrease) in a player's contribution in the current round. Likewise the negative coefficient on *Round* indicates that the increase in contributions get smaller as the rounds progress, other things equal. This is consistent with the concavity of the plots in Figure 2.1. Finally, the last column reports the results based on observations from round 12 only, which is the first round in which the players receive feedback on the allocations made by the other group members. Noteworthy here is the magnitude of the estimated coefficient on $B_{i,r-1}^{UNDERCOMP}$, which suggests that, other things equal, players who are being "punished" (under-compensated) in round 11 increase their contributions dramatically (by an average of 3.0 out of 10 tokens) in round 12 ($p = 0.001$).

Result 3. *How players are treated in the allocation stage affects their contribution decisions in the subsequent round: those players who are treated by the proportional rule, are punished, or are rewarded by their group members increase their contributions; on the other hand, those players who are treated by the egalitarian rule or a random allocation by their group members, reduce their contributions in the next round.*

1.5 Conclusion

Our goal in this study was to propose and experimentally test a simple mechanism in which peers decide on others' payoffs after a joint production stage. This mechanism does not involve bargaining and the resulting allocation is undistorted by self-interest. We tested the mechanism in an economic laboratory with groups of three players under two regimes differing in the scale of returns of the production function, we found that the majority of participants allocated according to other players' relative contributions in both regimes. Consequently, we observed average levels of contribution in both regimes much higher than those occurring under an equal sharing regime, and almost full contribution in the production stage in the later rounds of the experiment in the regime with the higher scale of returns. We interpret our result as a successful attempt to improve social efficiency by combining social preference with the right form of institution.

The pursuit of self-interest is an important assumption in the traditional mechanism design literature, as elsewhere in Economics. Because the GM permits players no role in determining their own share of the surplus at the allocation stage, they are free to allocate shares to other players without concern for protecting their interests. Thus even purely self-interested players may allocate on the basis of relative contributions, particularly if they believe that other players will anticipate such behaviour and make a full contribution leading to the socially optimal outcome. But of course players need not have this belief and need not allocate on this basis.

Recent behavioural and experimental studies find evidence of various degree of "other-regarding" preferences (e.g., Fehr and Schmidt, 1999; Charness and Rabin, 2002; Cox et al., 2007; Benabou and Tirole, 2011). Although not indended as a replacement of pure self-interest, richer behavioural assumptions such as fairness and other moral standards can be valuable in the design of effective institutions. In our study, we demonstrate that a small intrinsic concern for justice (allocating on the basis of relative contributions), when utilized in an appropriate social institution (the GM), has significant advantages in overcoming the free-rider problem in team production and improving social efficiency.

In our view, such a simple mechanism is worthy of further study. Its simplicity should be an advantage in practical applications. In our introduction we noted its potential in assigning individual marks for student group work, and reported Galbraith's observations of its use for allocating bonus payments to some New York bankers in the 1920s. Baranski (2016) notes that similar profit allocation decisions arise in certain types of business partnership, such as accounting firms, law firms, management consultants, medical groups, and architects' consortiums. Of course any practical application can bring with it complications, such as differences in participants' endowments and the possibility of repeated interactions, not considered in our experiments; hence the importance of further study.

1.6 Supplementary Treatment: Fixed Matching

In addition to the treatments outlined in section 1.3, in this section, I report another experimental treatment where the group composition is fixed instead of reshuffling every round. I put this treatment in a separate section because if the GM is already effective in stimulating almost full contribution in the random matching setting, it won't be a surprise to find that the GM is equally effective in the fixed matching setting. However, the repetitive interacting with the same group members enables us to study another interesting feature of the GM, that is, the possibility of collusion in the allocation stage. For the rest of the section, I will present the experimental design and the results followed by a discussion of the collusion behaviour in the GM.

Experimental Design: The GM1.8_Fix Treatment

I ran another four experimental sessions of fixed matching (Session T2 in Table 1.6). Each session had 9 participants and they were randomly divided into three 3-person group. The matching of the 3-person group remained unchanged during all 20 rounds of the experiment. We label the GM part as the GM1.8_Fix. The recruitment and other aspects of the experiments are the same to the GM1.8 reported in section 1.3.

Table 1.6: Experiment design with fixed matching treatments

Sessions	Treatment		β	Matching Protocol	No. of subjects	Indep. Groups
	Round1-10	Round11-20				
Control	Equal Share	Equal Share	1.8	Random	36	4
T0	Equal Share	GM1.8_Rd	1.8	Random	90	10
T1	Equal Share	GM1.2_Rd	1.2	Random	90	10
T2	**Equal Share**	**GM1.8_Fix**	**1.8**	**Fixed**	**36**	**12**
Total:					252	36

Experimental Results: Contributions

Figure 1.3 displays the time-path of the average contributions over all 20 iterations for each treatment. From round 1 to round 10, the production is equally shared. In the GM1.8_Fix, the contribution levels (mean = 5.81) in each round are all significantly higher than the treatments where the group composition reshuffles very round ($p < 0.01$). It means, even for the equal sharing rule, reputation building of the fixed matching alone can achieve a relatively high contribution rate.

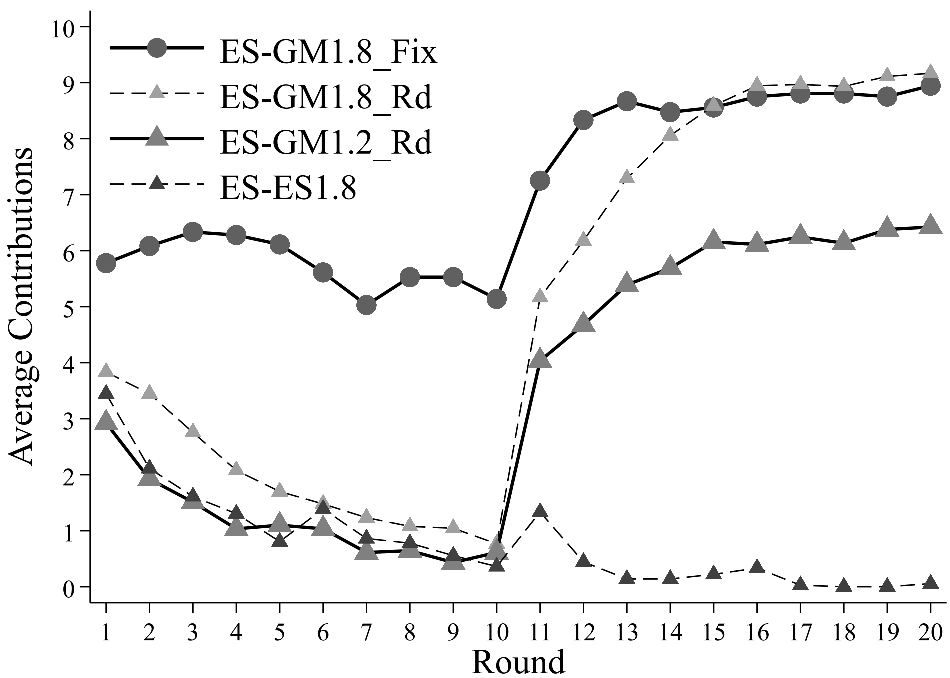


Figure 1.3: Time-path of the Average Contribution by Treatment

From round 11 to round 20, players use the GM to divide the production. To understand the behaviour difference between the fixed matching and the random matching, we focus our comparison between the GM1.8_Fix and the GM1.8. Table 1.7 shows the average contribution in each round of the GM1.8_Fix and the p-values of hypothesis tests. The average contribution in the GM1.8_Fix (mean = 8.53) is not significantly different from the GM1.8 (mean = 8.04, $p > 0.1$). According to Figure 1.3 and Table 1.7, the differences in the average contribution between the two treatments are significant in round 11-13, but are

Table 1.7: Descriptive analysis of contribution choices by treatment

	GM1.8_Fix		Alternative Hypotheses		
Round	Mean	S.D.	GM1.8_Fix ≠GM1.8	GM1.8_Fix ≠ES1.8(control)	ES1.8_Fix(r1-10) ≠GM1.8_Fix(r11-20)
11	7.25	2.81	0.03	0.00	0.06
12	8.33	2.59	0.01	0.00	0.03
13	8.67	2.37	0.02	0.00	0.02
14	8.47	2.70	0.15	0.00	0.04
15	8.56	2.55	0.30	0.00	0.05
16	8.75	2.53	0.13	0.00	0.01
17	8.81	2.46	0.23	0.00	0.01
18	8.81	2.47	0.23	0.00	0.01
19	8.75	2.70	0.23	0.00	0.01
20	8.94	2.67	0.21	0.00	0.00

Notes: Column 2-3 lists the average contribution and their standard deviation in the GM1.8_Fix treatment. Column 4-5 shows the p-values of the ranksum tests between treatments and column 6 shows the p-values of the signrank tests for the comparison within the treatment, for example round 11 is compared with round 1.

negligible from round 14 onwards. It implies that reputation building with the fixed matching is not a necessary part for the GM to work; fixed matching protocol does not further increase the contribution rate in the GM comparing to the random matching protocol. On the other hand, although the contribution rate in the equal sharing rules with the fixed matching are high, it can be further improved by introducing the GM. Specifically, comparing the ES1.8_Fix in round 1-10, the increase in the contribution with the GM is significant in each respective round (see column 5 in Table 1.7).

Experimental Results: Allocations

In this section, we analyze players' allocation decisions. Similar to the allocation choices in the GM1.8 reported in the main chapter, about half of the observations (58.6%) in the GM1.8_Fix follows the exact proportional rule, that is, $\frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}} = \frac{e_j}{e_j+e_k}$. If we relax the criterion to a 5% confidence region following the proportional rule, 73.3% observations in the GM1.8_Fix use the proportional rule to reward others, that is to say, a large number of players allocate proportionally according to the other's entitlement.

Table 1.8: Allocation Choice: Random Effects

Treatment: GM1.8_Fix	*Dep. Variable:* Fraction Player i Allocate to Player j		
	(1)	(2)	(3)
Fraction j Deserved: β_1	0.916***	0.791***	0.977***
	(0.068)	(0.137)	(0.150)
Intercept: β_0	0.013	0.094	-0.098
	(0.026)	(0.116)	(0.087)
#Data Used	All	$e_j > e_i$	$e_j < e_i$
#R-square	0.461	0.406	0.564
#Observations	360	61	58
#Clusters	12	12	12
$H_0 : \beta_1 = 1$	$\chi^2(1) = 1.53$	$\chi^2(1) = 2.34$	$\chi^2(1) = 0.02$
	$(p = 0.216)$	$(p = 0.127)$	$(p = 0.877)$

Notes: The table reports the regression results for random-effects model with the standard error clustered at the independent group level. *** indicates significance at 1% level. Period dummies are controlled for in the regression: the estimated coefficients are between 0.005 to 0.054, and they are not significantly different from zero at 1% significance level.

Table 1.8 presents additional support for the use of the proportional rule from random effects regressions. The dependent variable is the fraction player i allocates to player j, and the independent variables is the relative contribution of j from player i's perspective. The proportional rule predicts the coefficient of $\frac{e_j}{e_j+e_k}$ equals one and the intercept term equals zero. Overall, the estimates of these parameters are consistent with the hypothesis: the estimated coefficient of $\frac{e_j}{e_j+e_k}$, 0.92 in the GM1.8_Fix, is different from zero ($p < 0.01$) and not significantly different from 1(F-test, $p > 0.1$). The intercept, meaning the fraction player i allocates to player j when player j deserves zero, is not significantly different from zero ($p > 0.1$, see model 1 in Table 1.2). The results are very similar to the GM1.8, suggesting that the use of the proportional rule are consistent across treatments.When dividing the data between the case where the targeted player contributes more than the other player ($e_j > e_i$) and the case where the targeted player contributes less ($e_j < e_i$). The results of both cases in the GM1.8_Fix treatment are similar to the aggregated data (see model 2-3 in Table 1.2).

Table 1.9: Four types of allocators

Treatment	Proportionists	Egalitarians	Super-proportionists	Random-allocators
GM1.8_Rd	76.8%	52.2%	10.5%	8.7%
GM1.8_Fix	73.3%	58.6%	7.0%	18.3%

Notes: (1) Player i is defined as a proportionist in a certain round when $\left| \frac{\tilde{a}_{ij}}{\tilde{a}_{ij}+\tilde{a}_{ik}} - \frac{e_j}{e_j+e_k} \right| \leq 0.05$; (2) Egalitarians are those who allocate the $\frac{1}{3}\Pi$ equally; (3) If player j contributes less than player k, player i, under the "super-proportionists" category, rewards player j with less than what a proportionist would give; (4) We label players as random allocators when their allocation decisions cannot be captured by any of the three rules listed above.

Table 1.9 lists the fraction of four types of allocators according to the definitions given in section 1.4. Comparing to the GM1.8, there is a much higher fraction of random-allocators in the GM1.8_Fix treatment. We will explore some explanations for this result in the next section.

Collusion in the GM1.8_Fix

The fixed matching design of the GM enables us to detect possible collusions among the group members. Specifically, in the GM1.8_Fix, players are identified as player A, B, or C, and their roles remain unchanged in all rounds. Since the GM put no restrictions on allocation rules, two players, say player A and player B, can allocate more than a proportional amount to each other. This creates opportunity for two players to form coalition against a third player.

We can detect such collusion behavior by counting the number of times two players allocating more than the proportional amount to each other. The upper panel of Table 1.10 lists the number of times *a player allocates more than the proportional amount to other players* in each of the 12 three-person groups. For example, in matching group 2, for 9 (out of 10) times, the player i allocates more than the fair amount to the player j; and for 6 (out of 10) times, the player j allocates more than the fair amount to the player i. Since both the player i and player j frequently get more than what they deserve, the player k often results

Table 1.10: Collusion Detection

Matching	i and j		j and k		i and k	
Groups	i to j	j to i	j to k	k to j	i to k	k to i
1	1	2	3	2	1	1
2	**9**	**6**	0	2	1	1
3	2	5	5	3	1	4
4	2	0	**5**	**5**	3	1
5	4	2	5	3	5	3
6	**7**	**6**	4	5	3	4
7	0	0	0	0	0	0
8	1	1	1	0	0	0
9	0	1	0	1	2	1
10	0	0	1	0	3	2
11	0	0	0	0	1	0
12	0	1	4	0	**8**	**5**

Notes: This table shows the number of times a player, say player i, allocates more than the proportional amount to player j. The highlighted four groups, group 2, 4, 6, 12, are where two players allocate more than the proportional amount to each other for at least 5 times.

Matching	i and j				j and k				i and k			
Groups	i to j		j to i		j to k		k to j		i to k		k to i	
1	0	(9)	1	(9)	1	(8)	1	(9)	0	(9)	1	(10)
2	0	(10)	0	(10)	0	(1)	1	(10)	1	(1)	1	(9)
3	0	(10)	3	(8)	2	(3)	2	(8)	0	(1)	0	(2)
4	2	(7)	0	(10)	0	(4)	0	(4)	0	(9)	0	(10)
5	1	(7)	1	(9)	3	(8)	1	(7)	5	(9)	1	(9)
6	3	(10)	0	(8)	0	(2)	2	(6)	1	(1)	3	(6)
7	0	(10)	0	(10)	0	(10)	0	(10)	0	(10)	0	(10)
8	1	(10)	1	(10)	1	(10)	0	(10)	0	(10)	0	(10)
9	0	(9)	1	(10)	0	(10)	0	(9)	0	(9)	0	(9)
10	0	(10)	0	(10)	1	(9)	0	(9)	3	(10)	2	(10)
11	0	(9)	0	(9)	0	(10)	0	(10)	0	(10)	0	(10)
12	0	(10)	1	(10)	4	(10)	0	(10)	**6**	**(8)**	**4**	**(8)**

Notes: This table shows the number of times a player, say player i, allocates more than half to player j, while player j contributes less or equal to player k. Number in the parenthesis indicates the number of times player j contributes less or equal to player k.

in less than the fair amount in the allocation. In this matching group, we have reasons to suspect that the player i and the player j colluded against the player k.

There is, however, one potential problem with the above method to detect collusions. Two players allocate more than a fair amount to each other could also be due to the lack of contribution of the third player. Recall that under the “super-proportional" rule, there is a tendency to reward a player with more than their fair share if they contribute more than the other player. To separate collusion from the “super-proportional" way of allocation, we can look at the cases where *a player, say player* i*, allocates more than half to player* j*, while player* j *contributes less or equal to player* k*.* The lower panel of Table 1.10 reports the numbers of the occurred cases. For example, in matching group 3, the player j allocates more than half to the player i in the three out of eighth times where player i contributes less than player k. Eye-balling the table, we do not find a significant amount of such cases on average.

There can be several reasons why we do not find significant collusions in the data. First, it can be because the motive to be fair drives out the motive to collude. If this is true, the result contributes to the robustness of the GM, i.e., the mechanism is immune to collusions. Second, we may hypothesize that the conditions for collusion in the current design is not strong enough. Though players can track the other group members' contribution and allocation decisions from the previous round, it is not clear whom they should colluded with. For example, when player A is deciding a potential member to collude with, she will have to guess who, either player B or player C, is more likely to get the signal and colludes back with her.

The potential collusion opportunities of the GM can be a major obstacle for its real world implications. At the same time, it can be an interesting extension for future studies. Next, I outline three possible scenarios in which lab experiments can be useful in this pursuit. First, manipulating the identity of group members. Suppose two out of the three players share a common identity (in chapter 2, we create an artificial identity among group members), it then provides these two players the opportunity to collude. Second, manipulating the timing of the group. This can be the case where two out of the three group members are the incumbents of the team and the third player is the new comer. Last, manipulating the way of communication. For instance, only two out of the three players are allowed to communicate.

CHAPTER 2

Equity Principle Meets Costly Monitoring and Social Identities

2.1 Introduction

Team work is a ubiquitous feature of economic and social life. People corporate with each other to achieve certain goals in many settings. When it comes to distributing the team profits, a reasonable principle is to reward each individual member according to their contributions to the team work, as Aristotle (1566) puts it, “for everyone agrees that justice in distribution must be in accordance with some kind of merit". This type of behaviour, or the equity principle of reward allocation, has been well documented in early social science studies (Homans, 1958; Adams, 1965; Selten, 1978), and has been recently supported by laboratory experiments (Konow, 2000) and neural studies (Fliessbach et al., 2007; Cappelen et al., 2014).

In practice, however, the implementation of the equity principle may not be straightforward. For example, the perception of the “merit" in team work varies. Even if we assume perfect observability of the effort, team members' contributions can be due to different productive rates (Cappelen et al., 2007) or risk factors (Cappelen et al., 2013). Furthermore, the equity principle of reward allocation can be challenged by other competing factors. Notably, self-interest can constrain the equity principle when the allocatees involve oneself (Ruffle, 1998; Frohlich et al., 2004). In this chapter, we investigate two other factors. First, the effect of costly monitoring to obtain the necessary information for equity allocation and second, the effect of social identity in influencing allocation decisions.

We study these two factors in the context of an interesting mechanism where the profit distribution is conducted in a decentralised manner so that each team member can decide other members' payoff. Concretely, the game is of two stages: in the first stage, each team member chooses to put some effort in a team project; and in the second stage, after observing each others' effort, each member can costlessly assign a share of the pie to each of the other members. The final distribution is determined by these assignments. The novel design of this mechanism is that it eliminate the self-interest motive in the allocation stage. Since players are only permitted to allocate among others, but not to him/herself, this mechanism allows the allocators to reward or punish according to their valued principle. To my knowledge, the description of this mechanism first appear in John Kenneth Galbraith's book *The Great Crash 1929* (1963, p.171), where the author noted such a bonus sharing scheme used by the Citibank in the U.S. in the 1920. We thereby name this mechanism as the Galbraith Mechanism or the GM.[1]

Under a controlled laboratory experiment, chapter 1 demonstrates the effectiveness of the GM in achieving almost full social efficiency. Its experimental results show that the majority of the participants (80 percent) allocate using equity principle and the contribution rate reaches 90 percent. The results are intuitive: suppose people anticipate that others would reward them according to their first-stage efforts, it is in their best interest to put full efforts, given certain returns of scales. In other words, the equity principle of reward allocation underpins the effectiveness of the GM. With this principle, the GM can almost ensure team members' payoffs proportional to their contributions, and hence, a high productivity can be expected.

[1]For a formal description of the GM, see chapter 1 section 1.2.

The findings in chapter 1 are certainly exciting; it shows the prevailing use of the equity principle in reward allocation and highlights the potential of the GM to be used in a more general setting or even outside the laboratory. But in chapter 1's experiment, without competing factors, the equity principle seems straightforward; we need to evaluate the robustness of this principle under a more complex environment. In particular, this chapter looks at how the equity principle may be challenged by two practical aspects: costly monitoring and heterogeneous social identities.

2.1.1 Costly Monitoring

A necessary condition to implement the equity principle is the availability of the contribution information. Rewards cannot be related to contributions unless the allocators know each allocatee's contributions. Such contribution information can be obtained through mutual peer monitoring. While peer monitoring is costless in some workplaces, for instance, assembly lines; in other situations, it is costly. For example, in modern office buildings, one may have to knock on their co-workers' doors or to read through complicated reports to determine each of her team members' contributions. If the monitoring activity is at one's own cost and yield no direct monetary benefit for himself, the clear prediction from traditional noncooperative game theory is that players will not monitor at all. The absence of peer monitoring makes the equity principle of reward allocation impossible, and consequently, it poses a serious threat to the effectiveness of the GM. We therefore postulate the following two hypotheses.

Hypothesis 1 (No Monitoring)**.** *In the GM, if monitoring each others' contribution involves cost c, where $c > 0$, players will not monitor.*

Hypothesis 2 (No Contribution)**.** *In the GM, if monitoring each others' contribution involves cost c, where $c > 0$, players will not contribute in the first stage.*

The dilemma associated with costly monitoring is whether individuals are willing to incur a personal cost to enforce a social norm (the equity principle in the GM). We can find similar dilemma in other scenarios. For example, in some real forest management projects, farmers can voluntarily conduct patrols through the forest to maintain the commons, and they can report the case of forest overuse to an executive committee. Such patrols cost farmers personal time and effort but generate group benefits. In a study by Rustagi et al. (2010), they found a substantial amount of farmers doing the patrols voluntarily.[2] Moreover, studies using empirical dataset also suggest that costly monitoring is a necessary condition for successful resource management (e.g., Gibson et al., 2005; Chhatre and Agrawal, 2008)

While in resource management projects, farmers ultimately get some benefits from the commons by doing the patrols, there are studies show people's willingness to sacrifice their own resources to enforce certain social norms even without any monetary benefit. For example, Fehr et al. (2002) find that more than 80 percent of the experimental participants are willing to incur a personal cost to "punish" low contributors in one-shot public good games. Moreover, Fehr and Fischbacher (2004) show that disinterested third-party players are willing to spend their own money to "punish" those who violate the group norms. These costly punishing behaviour yield no monetary benefit for the punishers, yet is beneficial in maintaining group cooperative norms. Later studies not only replicate the robustness of the results (Andreoni et al., 2003; Bernhard et al., 2006; Sefton et al, 2007; Gächter et al., 2008), but also attempts to use evolutionary game theory and computer simulations to establish the necessity of such "altruistic punishment" behaviour in human evolution (Boyd et al., 2003; Tatsuya et al., 2003). In this chapter, we use experimental methods to explore the willingness of costly enforce the norm of equity principle.

[2]There are other studies investigating whether players would spend a personal cost to improve group coordinations. For example, Kriss et al. (2016) study whether individuals will send messages to others to assist coordination, and they find that, once the message is costly, the frequency of the message exchange reduces dramatically. Fehr (2016) also find similar effects of unwillingness to do costly communications in coordination problems.

2.1.2 Social Identities

The second factor that may challenge the equity principle is the presence of heterogeneous social identities, for it is seldom the case that all team members share a homogeneous background in the modern world. Previous studies suggest that social identity plays an important role in people's decision making and that people's behaviour towards in-group members can be different from out-group members (Akerlof and Kranton, 2000; Fershtman and Gneezy, 2001; Bernhard et al., 2006; Goette et al., 2006; Eckel and Grossman, 2005; Charness and Rabin, 2007; Chen and Li, 2009).[3] For example, when allocating a given amount of money in an experiment, Chen and Li (2009) find that experiment participants tend to allocate more to their in-group members than out-group members.

Social identity can assist coalition formation in the organisations. For example, in Tremewan (2010), when dividing a given amount of money, even splits between the players who share the same social identity and nothing to the player who does not share their identity occupy the majority of the allocation outcomes. Laroze et al. (2016) studies the impact of social identity in three-person Baron and Ferejohn (1989)'s bargaining game, where profit allocation is determined as long as two out of three players agree. Players are informed of each group member's gender, race, and political position, but the authors find no evidence of coalition based on these features. One interpretation is that coalition formation can be ambiguous in the presence of multiple dimensions of social identities. There are also many studies show real world evidences of in-group favouritism. For example, in Laband and Piette (1994)'s investigation of 1051 articles published in top economic journal in year 1984, the authors find that journal editors are more likely to accept papers from their professional connections. Rivkin et al. (2005) show that students are more likely to give higher teaching

[3]Literatures distinguish between two ways of creating group identity in laboratory settings. The first way is to use participants' natural identities, for example, gender, race, ethnical group or nationality. The second way is to divide groups using artificial tasks, for example assigning random colours to participants (Tajfel, 1982; Tajfel and Turner, 1986; Hargreaves Heap and Zizzo, 2009). They will neither learn the true identity of the others nor even saw them, the only information they have is whether they belong to their own artificial group or not. Both ways are shown effective in changing participants' behaviour in the laboratory. In this study we use artificial tasks to induce group identity.

evaluation scores to teachers who come from their own nationality. Likewise, Feld et al. (2016) show that teachers are more likely to give higher grade to those students who shares their nationality.

Equity principle of reward allocation can be challenged by heterogeneous social identities especially when the team profit is allocated in a decentralised way. Incumbents may discriminate against new-comers in the reward allocation, and people who are from the same ethnical background may form coalitions to better reward each other at the cost of people who are not from their coalitions (Brick et al., 2006). Cronyism or reciprocal rewarding within small groups are possible in the GM, because the mechanism puts no restrictions in the allocation. As a result, the equity principle can be violated if the reward allocation is based on team members' social identities. On the other hand, deviations from the equity principle also means that people who are not in the alliance would be under-compensated. The anticipation of the exploitation in the reward allocation would reduce their motive to contribute in the first stage in the GM. Labour economics have long documented the harm of wage discrimination and nepotism on firm productivities (Becker, 1971; Goldberg, 1982).

Once the social identity is accentuated before the GM, we hypothesise that the allocation would be biased towards the allocator's in-group member. Note that the allocation stage of the GM is essentially the same as in Chen and Li (2009)'s experiment: players can propose any division between the other two players. But the crucial difference is the presence of the contribution stage in the GM. People now face conflict of interests between favouritism and fairness: the motive to favour their in-group member and the motive to follow the equity principle. Little is known about how people resolve this conflict. Formally, we propose the following two hypotheses.

Hypothesis 3 (Biased Allocation Toward In-group Member)**.** *In the three person GM, if only two out of the three players share a same group identity, these two players will allocate more than the proportional amount to each other and the third player will be under-compensated.*

Hypothesis 4 (Lower Contribution). *In the three person GM, suppose only two out of the three players share a same group identity, the average contribution rate will be lower compared to a homogeneous group.*

Motivated by the above discussions, my research strategy is to carry out these investigations and test the hypotheses in a controlled laboratory experiment where individuals make allocation and contribution decisions in the GM with the presence of either costly monitoring, or heterogeneous social identities, or both. The remainder of the chapter is organised as follows. Section 2.2 describes the experimental design, section 2.3 discusses the experimental results and section 2.4 concludes.

2.2 Experimental Design and Procedure

I ran 24 independent computerised sessions at the Centre for Decision Research and Experimental Economics (CeDEx) in Nottingham in March 2016. In total, 288 university students from various fields of study took part, with 12 participants in each session. Those participants were drawn from the CeDEx subject pool, which was managed using the Online Recruitment System for Economic Experiments (ORSEE, Greiner (2015)). The experiment was programmed in z-Tree (Fischbacher, 2007).

Participants were randomly seated in a partitioned computer terminal upon arrival. The experimental instructions (see Appendix B1) were provided to each participant in written form and were read aloud by the experimenter in each session. The experiment started when all participants provide the right answers to the quiz questions with respect to the instructions. The experimental instructions had a neutral frame. At the end of each session, participants filled a post-experiment survey including questions about demographics and strategies used during the experiment (see Appendix B2). I used experiment currency units (ECUs) to represent the money units during the experiment. After completing survey questions, participants were privately paid with each ECU worth 4 UK pence (0.04 pounds) and left the laboratory one at a time. Average earnings per participant were £8.67 (equivalent to \$13.44 or €12.14 at the time of the experiment). Each experiment session lasted about 50 minutes on average.[4]

The experiment was a two (free or costly monitoring) by two (homogeneous or heterogeneous group composition) between-subject design, yielding four experimental treatments: free monitoring with homogeneous group composition (FreeHom), costly monitoring with homogeneous group composition (CostHom), free monitoring with heterogeneous group composition (FreeHet), and costly monitoring with heterogeneous group composition (CostHet). In each treatment, there were twelve decision rounds, and in each round, the computer program drawn three participants to form a group. The group composition reshuffled every round.[5] Table 2.1 summarises key features of each treatment.

The Free Monitoring and Homogeneous Group (FreeHom) Treatment

In the FreeHom treatment, the decision tasks were the same as in chapter 1's GM treatment; it served as a control in this study. Specifically, in each round, participants made two decisions.

Contribution Decision.—At the beginning of each round, players received an endowment of 10 ECUs and each had to decide simultaneously and privately how many ECUs (if any) to contribute to a group project, i.e., e_i. Players could keep the money that they did not contribute to the project. ECUs invested in the project were summed up and multiplied by 1.8, i.e., $\Pi = 1.8 \sum_{i=1}^{3} e_i$. Thus, the group return was 1.8 ECUs. Players then received the information of the total value of the group project (i.e., Π) and each of their co-players' contributions.

[4]Sessions with Homogeneous group composition lasted about 45 minutes whereas sessions with heterogeneous group composition lasted about one hour. This was because participants in heterogeneous group composition went through a 15-minutes painting task.

[5]The matching of the three-person group was pre-determined by the computer software. Specifically, each participant would never be in the same group with the two other participants twice during the whole experiment. We randomised the display of players' contributions on the screen in each round; in this way, players were not able to track the identities of other players over rounds.

Allocation Decision.—Players had to divide $\frac{1}{3}\Pi$ or $0.6\sum_{i=1}^{3} e_i$ between the other two group members. That is, each player i had to decide an allocation a_{ij} to player j and $a_{ik} = \frac{1}{3}\Pi - a_{ij}$ to player k. Note that there are no restrictions on how players shall divide the $\frac{1}{3}\Pi$. Player i's own share of the group project was determined by the allocation from player j and player k. To be precise, a player's earning in that round was $\pi_i = 10 - e_i + a_{ji} + a_{ki}$. At the end of each round, players are informed about the contributions and earnings of each of the group member, they are also reminded that they will never meet the same set of two other participants again for the rest of the experiment.

The Costly Monitoring and Homogeneous Group (CostHom) Treatment

The purpose of the CostHom treatment was to investigate how imposing a monitoring cost would affect players' allocation and contribution decisions in the GM (see Hypothesis 1 and Hypothesis 2). The treatment was like the CostHom treatment, but with one difference about the availability of other players' contribution information. In each round of the CostHom treatment, between the contribution decision and the allocation decision, there was an additional decision—monitoring decision.

Monitoring Decision.—At the end of the *contribution decision*, players were informed about the total value of the group project, but *not* each of their co-players' contribution decisions. Players were asked whether or not to spend a small cost (0.5 ECU) to reveal the other players' contributions. If they decided to monitor, the information would be available in the *allocation decision*;[6] if they chose not to buy, they would not know this information during the whole experiment.

The Free Information and Heterogeneous Group (FreeHet) treatment

In the FreeHet treatment, we wanted to discover whether heterogeneous social identities would bias participants' allocation decisions and affect their contribution decisions in the GM

[6] When players bought the information about their group members, they learned each of their group members' contribution decisions.

(see Hypothesis 3 and Hypothesis 4). Before participants were introduced to the GM, they first went through a *Painting Task* designed to induce the heterogeneous social identities.

Painting Task.—I adopted the classical Klee/Kandinsky painting preference task followed by a ten minutes group chat (e.g., Tajfel et al., 1971; Chen and Li, 2009): 12 participants in each session indicated their preferences over five pairs of paintings, each of which contained one painting by Paul Klee and one painting by Wassily Kandinsky.[7] They were then assigned into two painting groups of six people based on their preferences.[8] Next, participants were shown two additional paintings and their task is to determine, within ten minutes, which artist (Klee or Kandinsky) painted each of these final two paintings.[9] During this ten minutes, players in the same painting group were encouraged to chat via a z-Tree chat-box.[10] For each correct answer, a participant earned 15 ECUs, though she was not told the correct answer until the end of the experiment.

After the *painting task*, players faced 12 rounds of the GM just like the FreeHom treatment. In each round, the three-person group was randomly formed with two group members (the majority players) from the same painting group and the other group member (the minority player) from the other painting group. Players were assigned at least once the role of minority player and at least once that of the majority player. They were informed about their group composition, whether each of their group members came from their own

[7] I'm grateful to Yan Chen for sending me the paintings and the program. All the paintings are shown on the computer screen as well as in printed form. The five pairs of paintings are: 1A *Gebirgsbidung*, 1924, by Klee; 1B *Subdued Glow*, 1928, by Kandinsky; 2A *Dreamy Improvisation*, 1913, by Kandinsky; 2B *Warning of the Ships*, 1917, by Klee; 3A *Dry-Cool Garden*, 1921, by Klee; 3B *Landscape with Red Splashes I*, 1913, by Kandinsky; 4A *Gentle Ascent*, 1934, by Kandinsky; 4B *Hoffmannesque Tale*, 1921, by Klee; 5A *Development in Brown*, 1933, by Kandinsky; 5B *The Vase*, 1938, by Klee.

[8] Our procedure differed from Chen and Li (2009)'s group assignment in two ways. First, instead of a binary choice, we gave players four options: strongly prefer A, weakly prefer A, weakly prefer B, or strongly prefer B. Second, to ensure each painting group has an equal number, players were notified that their group assignments were based on their painting preferences relative to other people's preferences in the room. So players were not necessarily placed in the group for which they expressed stronger preferences, but selecting a high number of painting by a given artist and indicating "strongly prefer" the paintings from that artist increased the probability of being in that group.

[9] Painting number 6 is *Monument in Fertile Country*, 1929, by Klee, and Painting number 7 is *Start*, 1928, by Kandinsky.

[10] The 12 copies of the paintings were also on their desk for the reference during the chat. A participant was neither required to contribute to the discussion nor to give answers that conform any decision reached by the group.

Table 2.1: Experiment Design

		Experimental Treatments			
		FreeHom	CostHom	FreeHet	CostHet
Before the GM	Painting Task	No	No	Yes	Yes
Decisions in One Round	Contribution Decision	Yes	Yes	Yes	Yes
	Monitoring Decision	No	Option to buy	No	Option to buy
	Allocation Decision	Yes	Yes	Yes	Yes
Number of subjects (total: 288)		72	72	72	72
Number of sessions (total: 24)		6	6	6	6

painting group or the other painting group *before* they proceed to make contribution and allocation decisions.

The Costly Monitoring and Heterogeneous Group (CostHet) treatment

The CostHet treatment was designed to study players' decisions in the GM in the presence of both costly monitoring and heterogeneous group composition. It had all the elements mentioned in the previous treatments. Players started by the *painting task* to induce heterogeneous social identities. Just like the FreeHet, in each round, players were informed of each group member's associated painting group before they made their contribution decision. Then, they were presented the monitoring decision should they wish to know other two group members' contribution decisions like CostHom. Lastly, they made allocation decisions.

2.3 Experimental Results

I split the analysis into three parts. First, I look at the contribution decisions in all four treatments. Second, I study the monitoring decisions in treatments when the monitoring

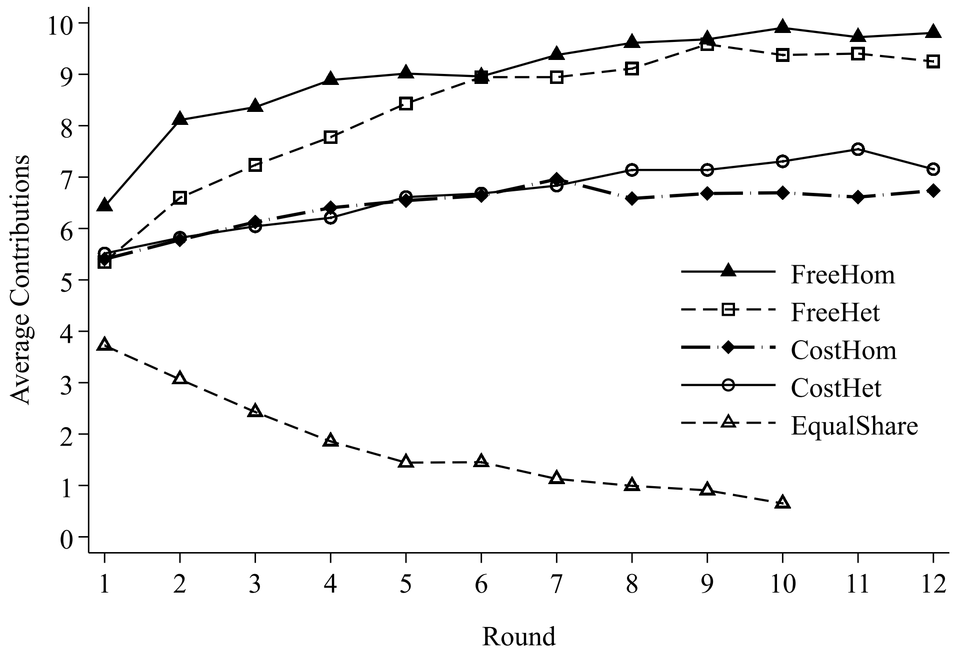


Figure 2.1: Time-path of the Average Contribution by Treatment

is costly. Finally, I investigate how the monitoring decisions and the heterogeneous social identities affected participant's allocation choices.

2.3.1 Contribution Decisions

In this section, we want to discover whether contributions differ across treatments. Figure 2.1 displays the time path of the average contributions of the four experimental treatments and a control treatment where equal sharing rule is enforced.[11] Appendix B3 provides further statistics with confidence interval and split by sessions and treatments. The left-hand panel (column 2-5) in Table 2.2 shows the round-by-round mean contributions and their standard deviations in each treatment, and the right-hand panel (column 6-9) reports the p-values for the rank-sum tests with the null hypotheses of equal contributions.

[11]The treatment with equal sharing rule is equivalent to the voluntary contribution mechanism. The data is from chapter 1 where three-player group's composition reshuffles every round for ten rounds. It is consistent with previous studies where the contribution gradually declines (Ledyard, 1995).

Table 2.2: Descriptive Analysis of Contribution Decisions by Treatment

Round	Average Contributions (Standard Deviations)				Alternative Hypotheses			
	FreeHom	FreeHet	CostHom	CostHet	FreeHom> FreeHet	FreeHom> CostHom	FreeHet> CostHet	CostHom≠ CostHet
1	6.43 (2.88)	5.35 (3.21)	5.51 (3.07)	5.40 (3.38)	0.02	0.02	0.37	0.75
2	8.11 (2.21)	6.60 (2.88)	5.82 (3.03)	5.78 (3.31)	0.05	0.00	0.26	1.00
3	8.36 (2.18)	7.24 (2.60)	6.04 (3.25)	6.12 (3.18)	0.10	0.01	0.17	0.69
4	8.89 (1.74)	7.78 (2.59)	6.21 (3.20)	6.40 (3.12)	0.10	0.01	0.05	0.87
5	9.01 (1.77)	8.43 (1.84)	6.61 (3.06)	6.54 (3.34)	0.37	0.01	0.01	0.87
6	8.96 (1.87)	8.94 (1.64)	6.68 (3.08)	6.64 (3.08)	0.34	0.05	0.01	0.75
7	9.38 (1.48)	8.94 (2.08)	6.83 (3.04)	6.96 (3.18)	0.29	0.00	0.03	0.75
8	9.61 (1.00)	9.11 (1.95)	7.14 (2.69)	6.58 (3.59)	0.21	0.00	0.02	0.75
9	9.68 (0.99)	9.58 (0.92)	7.14 (2.83)	6.68 (3.67)	0.37	0.01	0.01	0.75
10	9.90 (0.45)	9.38 (1.73)	7.31 (2.91)	6.69 (3.43)	0.07	0.00	0.02	0.81
11	9.72 (1.35)	9.40 (1.76)	7.54 (2.75)	6.61 (3.31)	0.26	0.02	0.01	0.52
12	9.81 (1.02)	9.25 (2.21)	7.15 (3.32)	6.74 (3.62)	0.06	0.01	0.04	0.94
Mean/Overall:	9.00 (1.94)	8.33 (2.53)	6.67 (3.07)	6.43 (3.36)	0.02	0.00	0.00	0.82

Notes: Column 2-5 report the average contributions of 72 observations in each round of the treatments. Column 6-9 present the p-value for the rank-sum test treating sessions' average contribution (each treatment has six sessions) as independent observations. The null hypothesis is of equal contributions.

Our first observation is that the contribution levels in the FreeHom are among the highest of all treatments, with an average of 9.0. This result successfully replicates chapter 1's findings.[12] Comparatively, when the heterogeneous social identities are introduced, in the FreeHet, the contribution rate (mean = 8.33) is lower: A rank sum test[13] shows that the contribution in the FreeHet is lower than the FreeHom ($p = 0.025$, one-sided). It is consistent with Hypothesis 4. Note that this difference is especially salient in the first two rounds (mean difference = 1.30, $p = 0.004$, one-sided); from round 3 onward, the gap in the average contribution between the FreeHet and the FreeHom is small (mean difference = 0.43, $p = 0.033$, one-sided). This result suggests that, when the monitoring is free, heterogeneous social identities only slightly lowers the contribution level.

In treatments with heterogeneous identities, we also want to know whether contributions and earnings differ between the majority and the minority players. Table 2.3 reports the

[12] In chapter 1, the GM is preceded by ten rounds of equal sharing rule in which the contribution reached almost zero in round ten. In their study, the GM manages to restore the contribution in the second ten rounds with an average contribution of 8.0. Using Kolmogorov-Smirnov tests of equality of distribution for last round contribution decisions, we find that the result is not significantly different from theirs ($p = 0.519$).

[13] Since the rank sum test requires independent observations, we use each session's average contribution in the test throughout the analysis. p-values reported in this chapter is two-sided unless otherwise stated.

Table 2.3: Average Contributions and Earnings

Contribution	Heterogeneous		Homogeneous	Alternative Hypotheses		
	Majority	Minority		Maj> Min	Maj≠Hom	Hom>Min
Free Monitor	8.607	7.784	8.988	0.000	0.039	0.000
	[576]	[288]	[864]	[72]	[144]	[144]
				(3.753)	(2.066)	(3.946)
Costly Monitor	6.661	5.965	6.665	0.001	0.666	0.114
	[576]	[288]	[864]	[72]	[144]	[144]
				(3.061)	(0.432)	(1.207)

Earnings	Heterogeneous		Homogeneous	Alternative Hypotheses		
	Majority	Minority		Maj> Min	Maj≠Hom	Hom>Min
Free Monitor	17.369	15.260	17.191	0.000	0.901	0.000
	[576]	[288]	[864]	[72]	[144]	[144]
				(4.226)	(0.124)	(5.220)
Costly Monitor	15.205	14.433	15.089	0.003	0.737	0.074
	[576]	[288]	[864]	[72]	[144]	[144]
				(2.750)	(0.336)	(1.786)

Notes: Columns 2-4 show the average contributions and earnings under each situations. Earnings are in ECUs. Numbers of observations are in square brackets. Columns 5-7 report the p-values with the null hypothesis of equal contribution or equal earnings. We use signrank tests for column 5 and ranksum tests for column 6-7. All the tests are conducted at the individual average contributions or earnings, and numbers of independent observations used in the test are in square brackets. t-statistics are in the parentheses.

average contributions and earnings by the majority and the minority players sorted by monitoring conditions. We use t-tests clustered at individual level to find out the difference, and column 5 to 7 present the results. We find that players in the minority position contribute and earn less than players in the majority position or players in the homogeneous treatments, especially when the monitoring is free ($p < 0.001$).

When the monitoring is costly (i.e., the CostHom and the CostHet), Figure 2.1 indicates a noticeably lower average contribution comparing to the free monitoring treatments. Results from the right-hand panel of Table 2.2 validate the observations: the contribution in the CostHom (mean = 6.67) is significantly lower than the contribution in the FreeHom

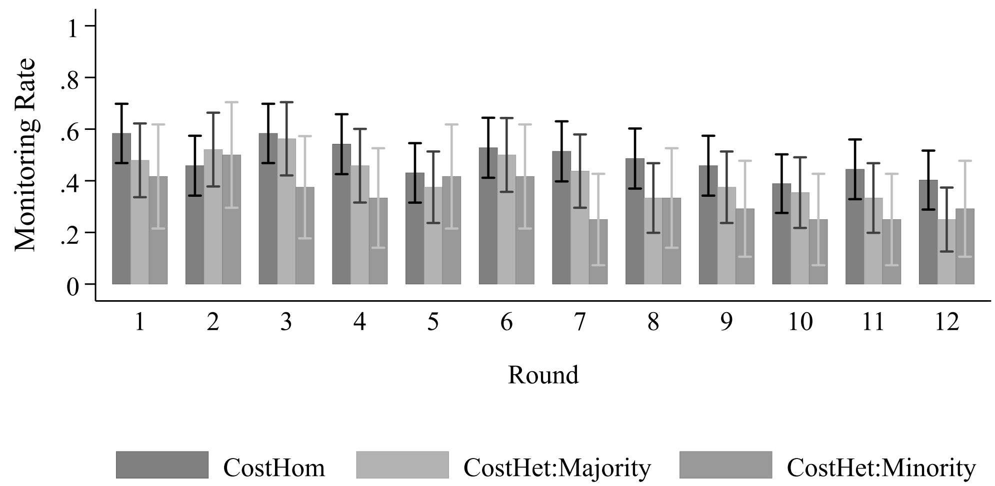
($p < 0.001$), and the overall contribution in the CostHet (mean = 6.43) is also significantly lower than the contribution in the FreeHet ($p < 0.001$). Note that the disparity is already present in the first round in the homogeneous treatment, while it is only significant from round four onward in the heterogeneous treatment. On the other hand, the contribution rates with costly monitoring are significantly higher than zero ($p < 0.001$) rejecting Hypothesis 2 in both treatments. A ranksum test indicates that there is no difference of the contribution level between the CostHom and the CostHet ($p = 0.819$, two-sided). We summarise our result below.

Result 1 (Contribution)**.** *The Galbraith Mechanism has significantly improved the average contribution in all treatments compared to the mechanism with equal allocations. It reaches the highest average contribution (90 percent) when the group composition is homogeneous and the monitoring is free. Introducing heterogeneous social identities reduces the average contribution, so does the costly monitoring. In treatments with group heterogeneity, players in the minority position contribute and earn less than players in the majority position.*

2.3.2 Monitoring Decisions

In this section, we focus on the costly monitoring decisions (i.e., the CostHom and the CostHet). Figure 2.2 shows the percentage of players who bought the information in each round of the treatments. From the figure, we observe a substantial amount of costly monitoring in both treatments (the mean monitoring rate is 48.5 percent in the CostHom and 39.1 percent in the CostHet), thus rejecting Hypothesis 1. This result implies that participants are willing to voluntarily spend money to monitor others' contributions. We also find that, with and without heterogeneous social identities, the monitoring frequencies are significantly different ($p < 0.001$, probit regression).

We next explore under which conditions players are more likely to monitor others. Table 2.4 displays the results of two conditional logit specifications with odds ratios reported in the squared brackets. In the regressions, we use whether each participant chose to spend



Notes: This figure shows the average monitoring rate in each round for the respective treatments. The average monitoring rates are 48.5 percent (CostHom), 41.5 percent (CostHet, Majority) and 34.4 percent (CostHet, Minority). The capped spikes indicate 95 percent confidence intervals.

Figure 2.2: Round-by-round Average Monitoring Rate

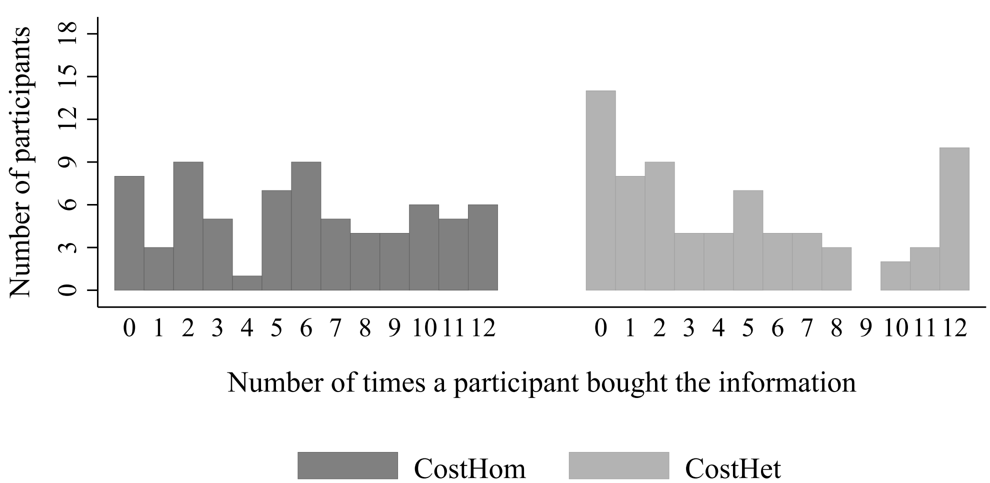


Figure 2.3: Distributions of Individual's Monitoring Decisions

Table 2.4: Determinants of Costly Monitoring: Conditional Logit Regression

Dependent Variable:	1 if players chose costly monitoring	
Treatments:	CostHom	CostHet
Own contributions	0.342***	0.418***
	[1.407]	[1.519]
	(0.076)	(0.077)
Others' contribute fully	-4.510***	-2.575***
	[0.011]	[0.076]
	(0.645)	(0.740)
Others' total contributions	0.018	-0.015
	[1.019]	[0.985]
	(0.054)	(0.033)
Round	-0.047	-0.214***
	[0.954]	[0.807]
	(0.043)	(0.046)
Minority player		-0.514*
		[0.598]
		(0.269)
#Number of Oberservations	696	576
#Number of Individuals	58	48

Notes: The table shows conditional logit regression of the likelihood to monitor others in the CostHom and CostHet. Odds ratios are reported in square brackets. Standard errors in the parentheses, clustered at the individual level. Significant at the* $p < 0.01$, ** $p < 0.05$ and *** $p < 0.001$ levels.

money to monitor others in each round as our dependent variable, and player's own contributions, whether others contribute fully, the total contribution of the others, round, and whether the participant is a minority player as our independent variables. Note that conditional logit regressions drop observations where participants have never monitored or always monitored during the twelve rounds. Figure 2.3 shows the distribution of individual's monitoring decisions; specifically, we drops 14 individuals (8 never, 6 always) in the CostHom and 24 individuals in the CostHom (14 never, 10 always).

The results show that participants are more likely to monitor others if their own contributions are higher. The odds of a participant monitoring others whose own contribution is 1 ECU higher is 41-52% higher ($p < 0.001$). We also find that players are not likely to monitor if they observe full contributions from the other two players, i.e., knowing the other two players contribute fully in a round reduces player's odds to monitor significantly (about 90 times less in the CostHom and 13 times less in the CostHet, $p < 0.001$).[14] Others' total contributions has no impact on participants' monitoring decisions as the coefficients are small and not significantly different from zero. Furthermore, round has a small negative effect on monitoring others in the CostHet; the frequency of monitoring decreases slightly over time. Lastly, being a minority player in the CostHet reduces participants likelihood to monitor; a signrank test for individual player's matched-pair decisions also indicates the monitoring rate differs between being a majority player and a minority player ($p = 0.007$). It seems that being a minority player, facing two out-group members, reduces one's incentive to monitor.

Result 2 (Monitoring Decisions)**.** *Participants choose to monitor others at their own cost in many situations (43.8 percent). The monitoring ratios differ between treatments: it is higher in the treatment with homogenous group composition (the* CostHom*) than the treatment with heterogeneous group composition (the* CostHet*). Within the* CostHet*, the majority players choose to monitor more frequently than the minority players.*

2.3.3 Allocation Decisions

In this section, I explore how the costly monitoring and the heterogeneous social identities affect players' allocation decisions. I organise the analysis into three parts. In the first part, I look at the allocation decisions in the FreeHom, so as to provide a baseline for later

[14]Note that if a player observes a total contribution of others equals 20, she can easily infer that each of the other two players contributes 10. From the data, the probability a player observes the full contributions is 18.4 percent in the CostHom and 11.6 percent in the CostHet. Of these observations, in more than 90 percent of the cases, players choose not to monitor (91.2 percent in the CostHom and 92 percent in the CostHet).

analysis. In the second part, I investigate allocation decisions in the situations where players chose not to monitor, and in the last part, I study the situations where the monitoring occurred.

Before going into the analysis, I first introduce a useful way to present players' allocation decisions. Let the horizontal axis represents the relative fraction player j contributes compared to player k, or $\frac{e_j}{e_j+e_k}$, and let the vertical axis represents the fraction player i allocates to player j.[15] In the case where player i faces an in-group member and an out-group member (i.e., the majority players in heterogeneous treatments), player j is their in-group member; otherwise, player j is randomly determined. Table 2.5 lists the allocation decisions classified by treatments and information conditions. In each figure, the size of the circle represents the relative frequency of the observation. Note that each observation on the 45-degree line presents an allocation where player i gives a proportional amount to player j. On the other hand, if player i allocates equally between j and k, the observation will lie on the horizontal line where the vertical axis equals 0.5.

Allocation Decisions in the FreeHom

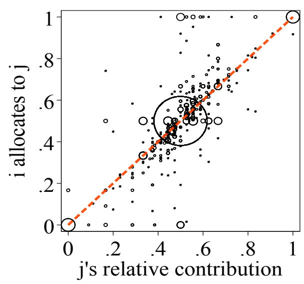
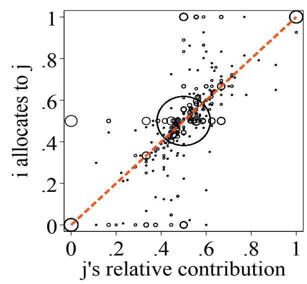
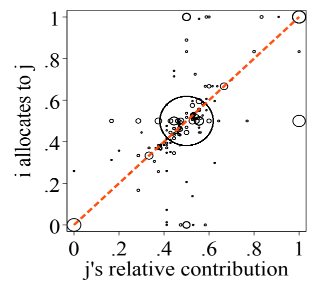
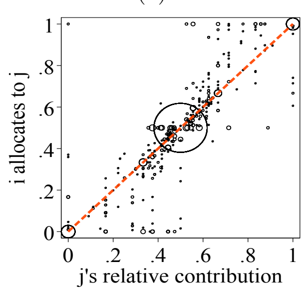
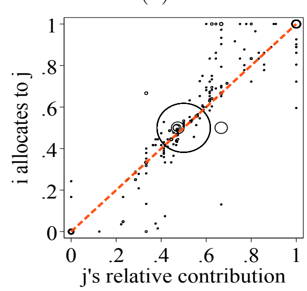
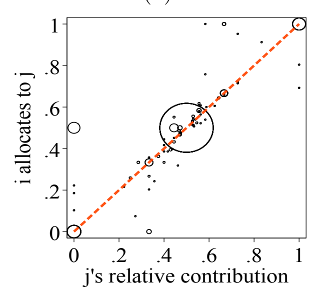
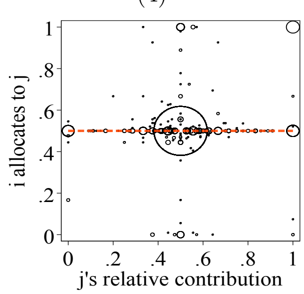
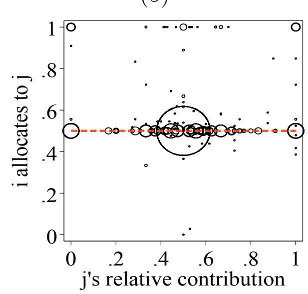
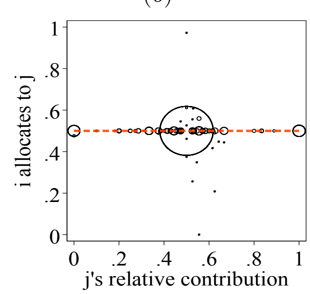
Table 2.5(1) depicts participants' allocation decisions in the FreeHom. More than half of the observations (56.1 percent) lie exactly on the 45-degree line; those are the decisions where players allocate precisely according to the other players' relative proportions.[16] If we allow a 5 percent deviation from the 45-degree line, then 82.9 percent of the observations fall into the category of proportionists.[17] OLS regression of the fraction player i allocates to

[15] Recall that the participants need to decide on how to allocate between the other two group members. The allocation must sum up to one-third of the group fund, that is, $a_{ij} + a_{ik} \equiv \frac{\Pi}{3}$. In the following analysis, we only consider each player i's allocation to player j, a_{ij}, because the allocation to each player k is automatically determined by $a_{ik} \equiv \frac{\Pi}{3} - a_{ij}$.

[16] The "big circle" in the middle of the figure (52.9 percent) includes situations in which players make equal splits when two other players contribute equally. In such situations, we cannot distinguish whether players are using the equity principle or the equal sharing principle. If we exclude the "big circle", among the rest of the observations, only in 5.5 percent of the cases players make equal shares while in 64.3 percent of the cases the observation fall closely on the 45-degree line.

[17] Specifically, we proportionists are those whose allocation satisfy $\mid \frac{a_{ij}}{a_{ij}+a_{ik}} - \frac{e_j}{e_j+e_k} \mid \leq 0.05$. We use this definition because some exact proportional rule may not always be feasible as the computer software only allows an input with a resolution of 0.1.

Table 2.5: The Monitoring, Heterogeneous Identities, and Allocation Decisions

	Homogeneous	Heterogeneous Treatment	
		Majority	Minority
Free Monitoring	 [864] (1)	 [576] (2)	 [288] (3)
Costly Monitoring	 [419] (4)	 [239] (5)	 [99] (6)
No Monitoring	 [445] (7)	 [337] (8)	 [189] (9)

Notes: (i) The horizontal axes in all figures represent the fraction player j contributes relative to player k, i.e., $\frac{e_j}{e_j+e_k}$. The vertical axes in all figures represent the actual fraction player i allocates to player j, i.e., $\frac{a_{ij}}{a_{ij}+a_{ik}}$. For the majority player in the heterogeneous treatment, player j is the in-group member. In other cases, player j is randomly determined. (ii) The size of the circles in each figure represents the observed relative frequency. (iii) The number of observations used to portray each figure is listed in the squared brackets.

player j ($\frac{a_{ij}}{a_{ij}+a_{ik}}$) on player j's relative contribution ($\frac{e_j}{e_j+e_k}$) lends additional support yielding a highly significant ($p < 0.001$) slope coefficient of 1.015 while the constant is close to zero (-0.007) and not significant ($p = 0.759$). Furthermore, an F-test does not reject the hypothesis that the slope coefficient is different from one ($p = 0.773$). Hence, for each 1-unit increase in player j's relative contribution, $\frac{e_j}{e_j+e_k}$, player i's relative allocation to player j ($\frac{a_{ij}}{a_{ij}+a_{ik}}$) also increases by approximately 1-unit on average, supporting the equity principle of reward allocation.

Allocation Decisions Without Monitoring

If players chose not to monitor in the CostHom and the CostHet, it is impossible for them to reward based on others' contributions. In those situations, most players allocate equally between the other two players, consistent with previous studies (e.g., Chen and Li, 2009) and inequality aversion models (e.g., Fehr and Schmidt, 1999). Specifically, 78.2 percent allocation decisions in the CostHom and 88.9 percent in the CostHet of the minority players are equal splits. The allocation decisions do not differ between these two cases ($p = 0.146$), and the mean fraction of allocation is not significantly different from 0.5 (mean $= 0.503$, t-test,[18] $p = 0.979$). We can therefore conclude that when players chose not to monitor and face homogeneous group compositions, in most cases, they make equal shares between the other two players.

Without monitoring, most majority players in the CostHet, facing one in-group and one out-group members, also tend to make equal shares between two other players (75.0 percent). But they give more than equal share to their in-group members in 20.5 percent of the cases (see panel 8 in Table 2.5). The average fraction allocated to their in-group member is 53.4 percent, and a t-test indicates significant difference comparing to 50 percent ($p = 0.003$, two-sided).[19] The result implies that without monitoring, players allocate more to their in-

[18] t-test takes into account of the multiple individual observations. It is conducted based on the average of the fraction player i allocates to player j when player i do not monitor

[19] t-test and the statistics are clustered on individual level.

Table 2.6: Allocation Decisions with Monitoring When Facing Homogeneous Group

	Dep. Variable: Fraction Player i Allocate to Player j	
	Estimated Coefficient	Standard Error
j's relative contribution: γ_1	0.989***	0.025
FreeHom: γ_2	-0.013	0.010
Minority × FreeHet: γ_3	-0.013	0.012
CostMonitored × CostHom: γ_4	-0.017	0.012
CostMonitored × Minority × CostHet: γ_5	0.019	0.016
$H_0 : \gamma_1 = 1$	$\chi^2(1) = 0.19$	$p = 0.659$
$H_0 : \gamma_2 = \gamma_3 = \gamma_4 = \gamma_5 = 0$	$\chi^2(4) = 2.12$	$p = 0.713$
# Observations: 1670	$R-$square : 0.662	Hausman test: $p = 0.960$

Notes: (1) The data used in the random effects regression include allocation decisions from the FreeHom, players who choose to do costly monitoring in CostHom, minority players in the FreeHet and the minority players who bought information in the CostHet. (2) *** denotes significance at the 1 percent levels. Standard errors are clustered on the session level.

group member compare to otherwise homogenous group members. It is consistent with Chen and Li (2009)'s finding where allocators tend to give more to their in-group members than out-group members. It also partially explains why the minority players earn less compare to the majority players in the costly monitoring treatment (see Table 2.3).

Result 3 (Allocation Decisions Without Monitoring)**.** *Without monitoring, most allocation decisions are of equal splits between the other two players if players face homogeneous group compositions. When facing heterogeneous group compositions, participants tend to allocate slightly more to their in-group members.*

Allocation Decisions With Monitoring

In this section, our analysis is focused on situations where the monitoring had occurred. In particular, we are interested in two questions. First, whether the costs of monitoring, either free or costly, affects players' allocation decisions. Second, when players face heterogeneous group compositions, whether they reward their in-group members more than proportional amounts.

To understand the effects of costly monitoring on the allocation decisions, we use a random effects model. The dependent variable is the fraction player i allocates to player j, and the independent variable is the relative fraction player j contributes. We also add four dummy variables to capture the treatment differences. For the heterogeneous treatments, we only include data from the minority players since they face allocation decisions between two out-group members. Table 2.6 shows the results. We first note that the slope coefficient in the regressions is highly significant ($p < 0.001$) and is not different from one ($p > 0.1$) indicating that players follow the equity principle to allocate. Furthermore, the result shows no treatment differences: Each of the estimated coefficients of the dummy variable is not significantly different from zero ($p > 0.1$) and Wald test fails to reject the hypothesis that they are all equal to zero ($p > 0.1$). The result implies that most players allocate based on the equity principle when the monitoring occurred, and whether the monitoring is free or costly does not affect their decisions.

Next, we address the question of whether people allocate differently towards their in-group member with monitoring. Note that Table 2.3 shows players in minority positions earn less than players in majority positions (mean difference = 1.44 ECUs). There can be two possible explanations. First, because the minority players contribute less on average (mean difference = 0.76 tokens), they thus deserve less compared to the majority players according to the equity principle. Secondly, the earning gap can be caused by majority players allocate more to each other than the proportional amount leaving the minority player under-compensated.

Formally, we use random effects models to investigate these two explanations. The data include all allocation decisions from homogeneous treatments and majority players' allocation decisions from heterogeneous treatments (both with the monitoring occurred).[20] The dependent variable is the fraction player i allocates to player j in round t. Note that a player j is the in-group member for the majority player in heterogeneous treatments and is ran-

[20]I have also tried to include the minority players' allocation decisions from the heterogeneous treatments, where they face a homogeneous group composition (two out-group members), the results are reported in Appendix B3 and are very similar to the case of exclusion.

domly determined in homogenous treatments. The independent variables include the relative fraction player j contributed in round t, a dummy variable indicating whether player j is an in-group member (for heterogeneous treatments) and another dummy variable describing whether the monitoring is free or costly. We thus have between-subject comparisons of the allocation decisions in the regression: majority players from heterogeneous treatments and players from homogeneous treatments. We also include an interaction term between player j's relative contribution and the in-group dummy variable. This interaction term allows us to test the in-group contingent effect in the allocation decisions.

Table 2.7 presents the regression results. Column 1 includes all the observations. Column 2 and column 3 separate the datasets to give us more insights on players' allocation decisions. Specifically, column 2 looks at the cases where player j contributes more than player k, and column 3 considers the cases where player j contributes less than player k.

Results in column 1 show that most players follow the equity principle to allocate: both the estimated coefficient on player j's relative contribution and its sum with the interaction term are significantly different from zero ($p < 0.001$) and are not significantly different from one ($p > 0.1$). We also found no evidence of the effect of in-group favouritism in the allocation decisions: the estimated coefficients of both the In-group dummy variable and the interaction term are not significantly different from zero ($p > 0.1$). The result implies that the majority players generally follow the equity principle to allocate, and on average their allocation decisions do not bias towards their in-group members.[21]

Column 2 in Table 2.7 only includes the observations where player j contributes more than player k in a certain round t. In the heterogeneous treatment, they are the situations where the in-group member contributes more than the out-group member. Again, we found no evidence of in-group favouritism: the estimated coefficients of neither the in-group dummy nor the interaction term is significantly different from zero ($p > 0.1$). In other words, the

[21] Appendix B3 includes an OLS regression focusing only on the first round allocation decisions. Though we use randomised stranger matching protocol in our experiment, the first round is the round where participants receive no feedbacks from the previous rounds. The results show no in-group favouritism on average.

Table 2.7: Allocation Decisions With Monitoring (Random Effects Regressions)

	Dep. Variable: Fraction Player i Allocate to Player j		
	(1)	(2)	(3)
j's relative contributions: β_1	1.015***	0.877***	0.824***
	(0.029)	(0.028)	(0.049)
j is In-group: β_2	0.055	-0.060	-0.021
	(0.033)	(0.039)	(0.024)
j's relative contributions	-0.052	0.077	0.186***
$\times$ j is In-group: β_3	(0.044)	(0.045)	(0.056)
Costly Monitoring: β_4	-0.010	0.023	-0.032**
	(0.011)	(0.016)	(0.011)
Intercept: β_0	-0.005	0.094***	0.053**
	(0.014)	(0.023)	(0.019)
R-square	0.698	0.676	0.759
#Data Used when	All	$e_j > e_k$	$e_j < e_k$
#Observations	2098	635	625
#Clusters	24	24	24
Hausman test for random	3.50	0.16	0.94
vs fixed effects	($p = 0.173$)	($p = 0.922$)	($p = 0.625$)
$H_0 : \beta_1 = 1$	0.29	18.80	12.93
	0.589	0.000	0.000
$H_0 : \beta_1 + \beta_3 = 1$	1.19	1.97	0.13
	($p = 0.275$)	($p = 0.161$)	($p = 0.722$)

Notes: (i) $\frac{a_{ij}^t}{a_{ij}^t + a_{ik}^t} = \beta_0 + \beta_1 \times \frac{e_j^t}{e_j^t + e_k^t} + \beta_2 \times \text{Ingrp}_j + \beta_3 \times \frac{e_j^t}{e_j^t + e_k^t} \times \text{Ingrp}_j + \beta_4 \times \text{CostMonitor}_i^t + u_i + \varepsilon_i^t$ (ii) Column (1) includes 2098 decisions where monitoring has occurred (either free or costly): 1283 decisions from the homogeneous treatments and 815 decisions from the majority players in the heterogeneous treatment. Appendix B3 shows our results are robust to adding minority players' allocation decisions. (iii) Column (2) is based on the decisions where player j contributes more than player k. Column(3) is based on the decisions where player j contributes less than player k. (iv) An interaction term between the variables CostMonitor and Ingrp has tried to be added. The effect is not significant. We therefore omit this interaction term in the regression. (v)*, ** and *** denote, respectively, significance at the 10-percent, 5-percent and 1-percent levels. Standard errors clustered on the session level are in the brackets. (vi) We report the test statistics for the hypotheses tests and 2-sided p values are in the brackets.

majority players allocate no more than the proportional amount to their in-group member when their in-group member contributes more than their out-group member. Interestingly, we found the intercept term, 0.094, is significantly different from zero ($p < 0.001$). It implies that player i on average allocates more than the proportional amount to the player j when player j contributes more than player k. Reflecting on panel 1-2 and 4-5 in Table 2.5, we found that most allocation decisions tend to cluster above the 45-degree line when player j's relative contribution is higher than 0.5. We can thus conclude that players reward more than proportional amounts to those who have relatively higher contributions regardless of their group identities.

Column 3 Table 2.7 looks at the situations where player j's contribute less than player k's. In the heterogeneous treatment, they are the situations where the ingroup member contributes less than the outgroup member. The regression result suggests that players allocate more to their in-group matches in the heterogeneous treatment compared to homogeneous treatment. In particular, this effect of group identity on the allocation decision is through the interaction with the player's relative contributions. The marginal effect of the interaction term of the in-group dummy and player j's relative contribution is 0.186 ($p < 0.001$), meaning that players are rewarding more to their in-group members for each unit increase in their relative contributions compared with otherwise homogeneous group members. Furthermore, we also found that the estimated coefficient of player j's relative contributions, 0.824, is significantly different from one ($p < 0.001$), meaning that player i generally allocate less than the proportional amount to player j (82.4 percent of her relative contributions), when player j contributes less than player k. But the sum of it with the estimated coefficient of the interaction term is very close to one ($p > 0.1$), suggesting that the allocation towards the in-group member follows the equity principle. Reflecting on panel 1-2 in Table 2.5, allocation decisions tend to cluster below the 45-degree line when player j's relative contribution is less than 0.5. We can interpret it as a punishment to player j for a lower relative contribution, but when this player j is an in-group member, this punishment effect is no longer there.

This result is consistent with Chen and Li (2009)'s finding that players tend to be more lenient about their in-group members' misbehaviour. Additionally, the estimated coefficient of the dummy variable for Costly Monitoring is negative and significantly different from zero ($p < 0.05$). It implies that, when the monitoring is costly, players tend to reward less than the proportional amount to those who have a lower relative contributions compare to the situations where the monitoring is free. We summarise our findings below.

Result 4 (Allocation Decisions With Monitoring)**.** *When the monitoring has occurred, either it is free or costly, most participants allocate using the proportional rule if they face a homogeneous group composition. When facing heterogeneous group compositions, the allocation decisions are not biased towards the in-group member overall, though when the in-group member contributes less than the out-group member, the allocator is more lenient towards their in-group member.*

2.4 Conclusion

The main purpose of this study is to show how costly monitoring and social identities challenge people's distributional preference of reward allocation. In particular, I conduct my investigations in a mechanism (the GM) where the equity principle plays a key role in achieving social efficiency. I hypothesised that the equity principle may be violated when players have to bear a personal enforcement cost without benefit or when heterogeneous social identities are present in the allocation stage. I therefore designed an experiment varying the monitoring cost and group compositions to learn how the equity principle will be affected by these two factors, and ultimately, to evaluate the performance of the GM.

To study the effects of costly monitoring, instead of making the peer monitoring of the contribution information costless, players are given opportunities to buy such information before the allocation stage. Even though this spending does not yield any monetary benefit for the buyers themselves, it turns out that participants choose to monitor others at their own cost in about half of the cases. We also find that most participants who bought the information allocate following the equity principle. Thus, this result provides a further example for the notion of strong reciprocity (Fehr et al., 2002; Gintis et al., 2003) . The questionnaire results (see Appendix B2) are consistent with the view that the altruistic costly monitoring decisions are driven by fairness concerns.

To understand the effects of heterogeneous social identities on the allocation decision, before introducing the GM, we use the classical Klee and Kandinsky painting task with a ten-minutes communication phase designed to build participants' group identities. In the GM, we set the groups where only two out of three group members are from the same painting group. We find that though a few participants give more to their in-group member, the majority of them still follow the equity principle to allocate. We can interpret this result as evidence that the equity principle stands as a robust way in reward allocation even with the presence of heterogeneous social identities.

Finally, we find very high contribution rates in all treatments of our experiment. Though the contribution rates in the costly monitoring treatments are significantly lower than treatments with free information, the average is still above 64 percent. Furthermore, the contribution rates in treatments with heterogeneous social identities are only marginally lower than treatments with homogeneous group. Our results thus demonstrate great potentials of the GM to stimulate high productivity in team work, even with the presence of the costly monitoring and heterogenous social identities.

There are, of course, important differences between our experiment and real world settings. For instance, in most situations, the monitoring cost are implicit and groups are based on shared beliefs. Nevertheless, the findings of this paper gives some insights for organisational design. For example, there are great benefit in encouraging mutual monitoring of productivity among workers, and social norms such as the equity principle can be a reliable source of incentive when designing institutions. This chapter also highlighted the potential and significance of the GM in practical use. Important and fruitful avenues for future research are to study the GM in the field through natural experiment in real team production.

CHAPTER 3

Communication, Leadership and Coordination Failure

3.1 Introduction

Coordination problems arise in many organisations. There are often complementarities between members' choices, and these complementarities can lead to multiple stable outcomes. Organisations may be successful in coordinating on a good outcome, or they may become trapped in an inefficient situation even though better outcomes are also potentially stable.

Few coordination problems are as stark as those arising in the *minimum effort* game (also called *weakest-link* game), analysed first in Van Huyck et al. (1990). In this game, a player's payoff depends, in addition to the player's own choice, on the minimum choice in the group.[1] This game is a coordination game with multiple Pareto-ranked equilibria: any situation where all players make the same effort is a Nash equilibrium, but equilibria with a higher effort level have greater payoffs for all players.

[1]Examples of such situations include the classical stag-hunt game (Rousseau, 1755), and, more modernly, writing joint reports with several sections where completion of the report requires all sections to be completed (Weber et al., 2001) and airline departures, where for a plane to be able to depart several separate tasks must be completed (Knez and Simester, 2001).

Van Huyck et al. (1990) find that failure to coordinate on the efficient outcome in the minimum effort game is common in the laboratory. They point out that this coordination failure is due to the effects of strategic uncertainty: players do not choose the efficient action because they cannot be sure that all others will choose it.[2] These findings have been confirmed by later studies (see Camerer, 2003, Ch. 7; Devetag and Ortmann, 2007, for an overview). A typical pattern of behavior found in minimum effort game experiments is that initially many subjects choose high levels of effort, but after several rounds the majority choose a low effort.[3]

Coordination failure in a minimum effort game could be *prevented* if the game is modified from the beginning (e.g. by introducing a leadership mechanism), thus avoiding the decline of effort choices to a low level. However, it is also worth asking whether and how a group can *restore* coordination on a higher effort level, thus overcoming coordination failure after a history of being trapped in an inefficient equilibrium (the "turnaround game" of Brandts and Cooper, 2006). Most organisations have existed for a period of time and a mechanism that works with zero-experience groups might not work with groups that already have a history. For example, a device that is successful in a new company might not work in restructuring an old one.

In this chapter we investigate a minimum effort game with a low benefit-to-cost ratio and focus on two leadership mechanisms to improve coordination. One mechanism involves *cheap-talk* (CT) one-way pre-play communication, where one of the group members acts as a leader by suggesting an effort level; after observing the suggestion, all players choose an effort level simultaneously. The second mechanism entails a *first-mover* (FM) leader that leads by example. One player chooses an effort level prior to his followers, who observe this choice and then choose their own effort simultaneously. We compare the ability of these

[2] Van Huyck et al. (1990) distinguish two possibilities for players failing to coordinate on the efficient equilibrium: playing a Pareto-dominated equilibrium instead or not playing an equilibrium at all. They use the term *coordination failure* to refer to the first situation.

[3] The prevalence of coordination failure is higher if the benefits from coordinating on a higher effort are low relative to the cost of effort; coordination failure is also more likely with more players (Devetag and Ortmann, 2007).

mechanisms to prevent coordination failure in groups without history, and to restore high effort in groups with a history of coordination failure.

Both CT and FM mechanisms are expected to help players to coordinate on a more efficient equilibrium since in both cases the leader's suggestion or choice may act as a focal point. In addition, in the leading-by-example case, observing the leader's effort reduces the strategic uncertainty faced by the followers. On the other hand, commiting to a choice is more risky for the leader than making a non-binding suggestion. Which mechanism is more successful overall is not clear a priori. A novel aspect of our experiment is to elicit responses of followers to all possible suggestions or choices by the leader using the strategy method. This allows us to analyze followers' behavior more systematically by classifying their strategies into different types. In this way we can measure how their responsiveness to the leader's choice in the two mechanisms changes over time. In addition, we are able to conduct a counterfactual analysis of the consequences of alternative choices by the leaders.

Mechanisms similar to the ones we use have been applied previously to prevent coordination failure, albeit in less challenging environments. For a stag-hunt two-player game, Cooper et al. (1992) find that one-way pre-play communication improves coordination on the efficient equilibrium; two-way communication does even better. In a minimum effort game, Blume and Ortmann (2007) find that multilateral communication significantly increases efficiency relative to the treatment without cheap talk.[4] For the first-mover mechanism, Cartwright et al. (2013) observe that it increases effort in some groups, although not many groups reached the maximum possible effort. Sahin et al. (2015) compare the two mechanisms (one-way communication and leading-by-example) and find that both lead to an increased group effort compared with the baseline treatment, and the magnitude of the increase is similar for both mechanisms.

[4]The results are sensitive to the cost and clarity of messages, as Manzini et al. (2009) and Kriss et al. (2016) find.

The studies above show that both mechanisms are at least partially effective in preventing coordination failure in some parametrizations of the minimum effort game.[5] We study these mechanisms in a tougher environment in the sense of lower benefits of coordination relative to the cost of effort, and we also study whether the mechanisms can overcome coordination failure (without changing other aspects of the game).[6] For this purpose we use the parametrisation of the minimum effort game introduced by Brandts and Cooper (2006) to induce coordination failure in the absence of any mechanism. In our experiment leaders are chosen randomly,[7] and the leader-communicator in our cheap-talk mechanism can only suggest an effort level rather than send a more complicated message.[8] Our implementations of the leadership mechanisms are thus minimal as they do not require extended messages or (potentially costly) elections to determine the leader. By using a challenging environment (especially after a history of coordination failure) and minimal implementations of the mechanisms, we explore the limits of what these mechanisms can achieve.

After having confirmed that coordination failure happens in our tough environment without a mechanism present, we find that this history of coordination failure is a powerful attractor, and the leadership mechanisms fail to provide a means to overcome it in the long run. Nevertheless, shortly after the introduction of the mechanisms, average effort is higher as some subjects do attempt to make use of the mechanisms. Even without a history of coordination failure, both types of leadership have only a limited ability to prevent it in our environment, with only about 30-40% of the groups having their minimum effort above the lowest level.

[5]Other mechanisms that have been shown to be able to prevent coordination failure in minimum effort games to some extent include advice from previous cohorts of players (Chaudhuri et al., 2009), post-play disapproval messages (Dugar, 2010), and inducing social identity (Chen and Chen, 2011).

[6]There are several studies on the use of financial incentives, possibly together with communication, to overcome coordination failure (Brandts and Cooper, 2006, 2007; Hamman et al., 2007; Brandts et al., 2015). Increasing the benefits of coordination is found to improve efficiency, albeit to a lesser degree than communication. Efficiency is also found to increase once post-play monetary punishment is introduced (Le Lec et al., 2014).

[7]Alternative ways of choosing a leader can involve letting players volunteer to be the leader (Cartwright et al., 2013; Préget et al., 2016), elections (Brandts et al., 2015) or administering a test (Sahin et al., 2015).

[8]Free-form communication by a leader is found to shift group effort upward in Brandts et al. (2015).

Given the relatively poor performance of the mechanisms in terms of escaping from and even preventing coordination failure, what are the reasons for this? Is it due to an ineffective leadership or to the reluctance of other players to follow? We find that followers do follow the leader's suggestion or choice to some extent (more in the first-mover case than in the cheap-talk one, and more without a history of coordination failure) but there is a sizeable minority that always chooses the lowest possible effort. We also find that not all leaders dare to choose a high effort (even after having suggested it); hence, both leaders and followers can be blamed for the poor performance to some degree. Using the data from the strategy method, we find that even if leaders had chosen a higher effort, they would not have increased their payoff. The presence in a group of just one player who is not responsive to the leader's suggestion or choice makes it impossible to avoid coordination failure, as it is then individually rational for the leader and for any of the followers to choose the lowest possible effort.

The remainder of the chapter is organised as follows. Section 3.2 provides a general background on the minimum effort game and a discussion of possible effects of the leadership mechanisms. Section 3.3 describes the experimental design and hypotheses. The results of the experiment are discussed in section 3.4 and section 3.5 concludes.

3.2 Effects of leadership in the minimum effort game

3.2.1 The minimum effort game

In the minimum effort game there are n players. Player i's strategy is denoted by $x_i \in X_i \subseteq \mathbf{R}_+$, where X_i is a finite set. Players' strategies can be interpreted as effort levels. The payoff function of player i is

$$u_i(x_1, x_2, ..., x_n) = a + b \cdot \min\{x_1, ..., x_n\} - c \cdot x_i,$$

where a, b, c are exogenous constants with $b > c > 0$.

Any strategy profile in which all players in the group choose the same effort is a Nash equilibrium. A unilateral increase in x_i incurs a cost without changing the minimum. A unilateral decrease in x_i reduces the minimum; the effect of this reduction outweighs the saving on cost since $b > c$. The multiple Nash equilibria in the game can be Pareto-ranked according to the players' choice: any equilibrium with a higher choice Pareto-dominates any equilibrium with a lower choice.

Every player choosing the highest possible effort is the payoff-dominant equilibrium and thus it would be selected by Harsanyi and Selten's (1988) primary selection criterion. However, choosing a high effort is risky because a player may incur a large cost if the group's minimum effort happens to be low. There is a conflict between the Pareto-efficiency property of everybody choosing the highest possible effort and the insurance value for an individual player of choosing the lowest effort. The lower uncertainty associated with the choice of a lower effort is related to Harsanyi and Selten's (1988) secondary risk-dominance selection criterion. One generalization of this criterion to n-player potential games (of which the minimum effort game is an example) is maximization of the potential function (Monderer and Shapley, 1996; Goeree and Holt, 2005). In the minimum effort game, maximization of the potential selects coordination on the highest effort level if $n < b/c$ and on the lowest effort level if $n > b/c$.[9]

3.2.2 Effects of leadership

In a game with multiple equilibria, players' beliefs about the strategies of the other players are important for equilibrium selection. We will discuss how our two leadership mechanisms, while not altering the payoff function of the game, can affect players' beliefs and therefore possibly change their behavior allowing coordination on a different equilibrium. In our experiment we have three types of games based on the payoff function above but differing in the dynamic structure. The baseline game is the *simultaneous* game, where all players

[9] Experimental evidence tends to support this prediction (Goeree and Holt, 2005; Chen and Chen, 2011).

make their choices at the same time. The other two games correspond to our mechanisms. Recall that in the *cheap-talk* (CT) mechanism, an exogenously chosen leader-communicator sends a message from the set X_i of possible effort levels. This message is interpreted as a suggestion to the players. The message is seen by all players; then all players (the leader and the $n-1$ followers) choose an effort level simultaneously. In the *first-mover* (FM) mechanism an exogenously chosen leader makes a choice first. The other $n-1$ players (followers) observe this choice and then make their choices simultaneously.

Cartwright et al. (2013), who discuss only the game corresponding to our FM game, offer two reasons why leadership may increase the minimum effort in the group. First, the leader's choice may act as a focal point that facilitates coordination. Second, the leader's choice reduces the strategic uncertainty faced by the followers, who are now effectively playing a coordination game with $n-1$ players. Both effects are present in our FM game but only the focality effect is present in our CT game. We discuss the differences between the games below; a more formal analysis can be found in Dong et al. (2015).

Let L denote the suggestion (in CT) or choice (in FM) by the leader. First, in a given mechanism, suppose players believe that a higher L induces (stochastically) higher choices by the followers. Consider a player that would choose effort level $\hat{k}$ in the simultaneous game. If this player is selected to be the leader, it cannot be optimal to set L strictly less than $\hat{k}$. This is because of the focality effect: setting $L = \hat{k}$ pulls up the effort of players who would have chosen an effort below $\hat{k}$ (it can also pull down the effort of players who would have chosen an effort above $\hat{k}$, but only down to $\hat{k}$ itself). Thus, in FM the leader will choose L (which is the effort choice) not lower than $\hat{k}$. In CT, the leader is not restricted to choose an effort equal to his/her own suggestion L. In this case the leader would find it optimal to suggest the highest possible L but not necessarily follow it.[10] The leader's actual effort in CT still would not be below $\hat{k}$ because the focality effect of the highest possible L cannot

[10]If we interpret the suggestion as a statement of the leader's intention to play, the highest possible L is not *self-signalling*, since the leader has an incentive to shift the followers' beliefs upwards for other intended effort choices as well (a message is self-signalling if the sender wants to send it *if and only if* it is true, see Farrell and Rabin, 1996).

lead to an effort lower than $\hat{k}$ being optimal. Since the leader does not decrease the effort (compared with the choice in the simultaneous game) and other players either match this choice or increase their effort towards it, in a group as a whole the minimum effort cannot be lower with a leadership mechanism than with simultaneous play.

Second, the focality effect is likely to be stronger in the FM game, where the leader is committed to the announced effort level, than in the CT game, where the leader can still make a different choice. Then the choices made by followers in response to a given L should be at least as high in FM as in CT. Due to greater focality in FM, one could expect that leaders would choose a higher effort in FM than in CT. Despite this intuition, it may be optimal for a leader to choose a higher effort in CT than in FM. Suppose that a high L shifts beliefs towards a medium level of effort by followers, whereas a medium L keeps beliefs low. Then the leader in FM would find it optimal to choose a low level of effort. The leader in CT may find it optimal to send a high L and then choose a medium effort level. Therefore we do not have an unambiguous prediction for the comparison between minimum group efforts in CT and in FM.

3.3 Experimental procedures and hypotheses

3.3.1 Experimental design

We use the parametrization of the minimum effort game introduced in Brandts and Cooper (2006). There are four players and five effort levels, $x_i \in \{0, 10, 20, 30, 40\}$. Player i 's payoff is given by

$$u_i = 200 + 6 \cdot \min\{x_1, x_2, x_3, x_4\} - 5 \cdot x_i.$$

Table 3.1 shows the corresponding payoff matrix. This payoff matrix with five Pareto-ranked equilibria along the main diagonal was used by Brandts and Cooper (2006, 2007), Hamman et al. (2007) and Brandts et al. (2015). It is an economical way of "inducing" coordination failure by making $n \gg b/c$ with a relatively small number of players, $n = 4$.[11]

Table 3.1: Minimum Effort Game with $a = 200$, $b = 6$, $c = 5$

		Minimum Effort in the Group				
		0	10	20	30	40
Effort of Player i	0	200	-	-	-	-
	10	150	210	-	-	-
	20	100	160	220	-	-
	30	50	110	170	230	-
	40	0	60	120	180	240

The main part of the experiment consists of two blocks of ten rounds (see table 3.2).[12] In each round, a group of participants play either the baseline game or one of the mechanisms, according to table 2. The group composition remains fixed for the entire experiment. We divide experimental sessions according to the type of leadership mechanism and according to the timing of the introduction of the mechanism. Both mechanisms involve a randomly selected player (a leader) acting before others at the beginning of each round.[13] In our CT treatments, the leader suggests a number; after seeing this number all players (including the leader) simultaneously choose their effort level.[14] In the FM treatments, the leader makes an effort choice before the rest of the group. Having observed the leader's choice, the other players (the followers) make their effort choice simultaneously.

[11]Note that the game has a particularly low ratio of benefits b from coordinating on a higher effort to cost c of effort.

[12]In Restore sessions, there was a third block consisting of ten more rounds of the baseline setup. Since the participants were not informed about how many blocks there would be in the experiment and the instructions for the next block were given only at the beginning of each block, there should be no effect on the first two blocks in the sessions.

[13]Once selected, the identity of the leader remains fixed for the entire block of 10 rounds. Leaders are also fixed for the entire block in Brandts and Cooper (2007), Sahin et al. (2015) and Brandts et al. (2015). Pogrebna et al. (2011) and Cartwright et al. (2013) select a leader separately for each round.

[14]In the instructions, we specified that for the leader "the choice ... is the one used to calculate the points, and it could be different from the suggested number".

Table 3.2: Experimental Design

Timing	**Mechanism**	**Block 1 (Rounds 1-10)**	**Block 2 (Rounds 11-20)**
Restore	CT (15 groups)	Baseline	Cheap Talk Leader
	FM (15 groups)	Baseline	First Mover Leader
Prevent	CT (14 groups)	Cheap Talk Leader	Baseline
	FM (14 groups)	First Mover Leader	Baseline
Control (5 groups)		Baseline	Baseline

We consider two scenarios for the timing of the introduction of the mechanisms. In Restore sessions, the mechanism is introduced in the second block, after the group has played the baseline minimum effort game for ten rounds. This simulates an attempt to turn around an existing organization that has (likely) experienced coordination failure. In Prevent sessions, the order of the blocks is reversed: a group starts with a randomly assigned leader for ten rounds and then plays another block of ten rounds without a leader. We also run a control treatment in which the baseline game is played in all rounds.

At the end of each round, subjects are shown the group minimum effort from the current round and the effort levels selected by all subjects. These efforts are sorted from highest to lowest, so they cannot be traced to individual group members. The feedback format is similar to the one used by Brandts and Cooper (2006).

In the mechanisms, we use the strategy method to elicit followers' decisions. Specifically, we ask followers to enter an effort choice for each possible suggestion (in CT) or effort choice (in FM) of the leader. In this way we are able to collect data on followers' complete strategies rather than only on the choices in response to the actual suggestion/choice of the leader. With these strategies, we are able to test hypotheses about the followers' responses to different suggestions/choices of the leader and conduct a counterfactual analysis of group effort for different leader's choices.[15] For leaders, we elicited their (point) beliefs about the

[15] Experimental results with the strategy method usually do not differ much from results with the direct response method (see Brandts and Charness, 2011, and Fischbacher et al., 2012). We ran two sessions (CT-Restore and FM-Prevent) using the direct response method and confirmed that results are not significantly different.

minimum effort of the followers in response to their actual suggestion or choice; leaders got 20 points if their prediction was correct.

The experimental sessions were conducted in the CeDEx laboratory at the University of Nottingham, United Kingdom. The experiment was computerised using z-Tree (Fischbacher, 2007) and subjects were recruited with ORSEE (Greiner, 2015). Our sample consisted of 252 student participants from various fields of study in 13 sessions with 16-20 participants per session. We ensured the recruited subjects had not participated in a similar experiment (i.e., in a minimum effort game or a public goods game) before. At the beginning of a session, subjects were seated at a computer terminal in a cubicle. An experimenter read the instructions aloud in front of all the participants. Subjects received the relevant instructions only at the beginning of each block. As in Brandts and Cooper (2006) and subsequent papers on the turnaround game, the instructions were framed in a corporate context where the four players in the group are referred to as "employees" and are told that they are working for a "firm". We used "employee X" and "employee Y" to represent the leader and the follower roles, where applicable. Before the beginning of a block, subjects were required to answer several quiz questions regarding the payoff function and procedure details. At the end of a session, subjects were paid in private the amount they earned. The quiz, 20 rounds of decision-making, and the questionnaire lasted approximately one hour and subjects earned on average £9.63 (equivalent to $14.64 at the time of the experiment).

3.3.2 Hypotheses

Based on our discussion of possible leadership effects in section 3.2, we formulate the following hypotheses.[16] As we argued, the minimum effort in a group cannot be lower with a leader than if the players were choosing simultaneously. Therefore,

[16] In the experiment, we have repeated interactions rather than a one-shot game. There is no obvious reason why the focality effects of leadership would not apply with repeated interactions. Also, subjects may have preferences different from the risk-neutral own-payoff-oriented preferences assumed in the discussion. However, risk aversion is expected to exacerbate the difference in focality between CT and FM due to the reduced strategic uncertainty. Reciprocity motives would also tend to favour focality in FM.

Hypothesis 1 *The minimum group effort is higher in CT and FM than in Baseline.*

The history of the group is likely to affect players' beliefs. It can be expected that beliefs are more pessimistic if the mechanism is introduced after experiencing a common history of coordination failure. Our Restore sessions are designed to induce coordination failure, thus beliefs are likely to be more pessimistic in Restore. If beliefs are more pessimistic, then the chosen effort is likely to be lower.

Hypothesis 2 *The minimum group effort is lower in Restore than in Prevent, holding the treatment (CT or FM) constant.*

The previous hypotheses, although formulated on the aggregate level of the group, are based on individual behavior. Our strategy method design is well suited to test hypotheses about the contingent strategies of the followers. We argued in section 3.2 that followers' strategies would be more responsive to the leader's suggestion/choice in FM than in CT. It is also natural to expect the followers to be more responsive in Prevent than in Restore.

Hypothesis 3 *For a given suggestion/choice of the leader, the effort choices of the followers are higher in FM than in CT, and they are higher in Prevent than in Restore.*

We have also argued that a leader's suggestion in CT is expected to be higher than a leader's choice in FM.

Hypothesis 4 *The suggestion of leaders in CT is higher than the choice of leaders in FM.*

Note that this hypothesis is more compelling in a one-shot interaction. Some leaders may realize that, with repeated interactions, not following their own suggestion would reduce the focality effect of it, thus they may decide to suggest the effort they are actually going to choose. This hypothesis would also be less compelling if some leaders are lie averse or guilt averse. If leaders have a disutility from not following their own suggestion or from letting other players down, they may also decide to suggest the effort they are actually going to choose. Because followers are more likely to follow leaders in FM, such CT leaders may suggest (and choose) a lower effort than they would have done in FM.

Nevertheless, even if we assumed that leaders must follow their own suggestion in CT, it cannot be optimal to suggest (and therefore do) less than what a player would have chosen in the simultaneous game. The actual choice of the leader in both treatments should be above the choices in Baseline.

Hypothesis 5 *The effort choice of leaders in CT and FM is higher than in Baseline.*

3.4 Experimental Results

We first present an overview of group outcomes over time in our treatments. We then look at the individual behaviour of the subjects and try to determine what role is played by leaders and followers during the coordination process.

3.4.1 Group effort and coordination with and without leadership

In the analysis below, first we look at whether the mechanisms were successful in the toughest environment, i.e. after a history of coordination failure in Restore sessions. Then we look at their performance in preventing coordination failure (Prevent sessions). Finally, we discuss how the timing of the introduction of the mechanisms influenced overall payoffs.

Trying to overcome coordination failure

For the Restore sessions, a low effort level is a necessary condition to analyse the effectiveness of leadership in overcoming coordination failure. We consider as coordination failure the situation in which the minimum effort in a group is zero. During the first ten rounds in Control and Restore sessions there is a clear trend towards lower effort levels, as seen in figure 3.1, and the minimum effort is zero in round ten in 32 out of 35 groups.[17] There is no significant difference between CT, FM and Control treatments in these ten rounds, reflecting the identical setup across those treatments (the smallest p-value of the two-sided

[17] 14 out of 15 groups in CT, 13 out of 15 in FM, and all 5 groups in Control.

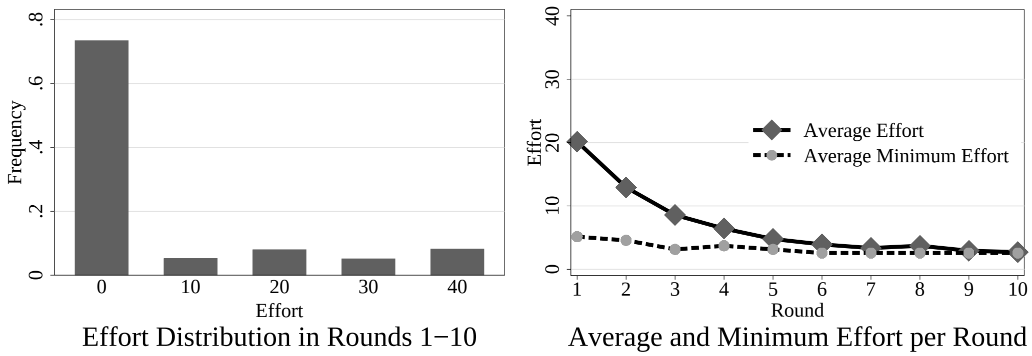


Figure 3.1: Efforts in rounds 1-10 in Restore and Control

Wilcoxon-Mann-Whitney rank-sum tests is 0.111 in round-by-round comparisons of group average or minimum efforts across pairs of treatments).

The results from the first block in Restore and Control sessions confirm the findings in the previous literature (Brandts and Cooper, 2006, 2007; Hamman et al., 2007; Brandts et al., 2015). Coordination failure after ten rounds is not surprising if one realises how tough the environment is. The cost of not being the minimum-effort player is high compared with the benefits of coordination on the most efficient equilibrium. A player who chooses effort 40 instead of 0 gains 40 if all other players choose 40 as well, but loses 200 if any of the other players chooses 0.

In the analysis below we focus on the 32 groups in which coordination failure occurred. Starting from round 11, groups in Restore sessions face a mechanism (either CT or FM). One can expect that players would increase their effort in round 11 compared to round 10. Indeed, 48 out of 108 subjects increase their effort in round 11. The average effort level in round 11 is 14.26 in Restore sessions with a history of coordination failure (13.04 in CT and 15.58 in FM). Figure 3.2 shows the distribution of choices in round 11 and the average payoff obtained for each choice in these groups.

With a leadership mechanism, group average efforts are significantly higher in round 11 than in round 10 (p-value of the two-sided Wilcoxon matched-pairs sign-rank test < 0.001). Group minimum efforts increase only slightly though, and the right panel in figure 3.2 shows

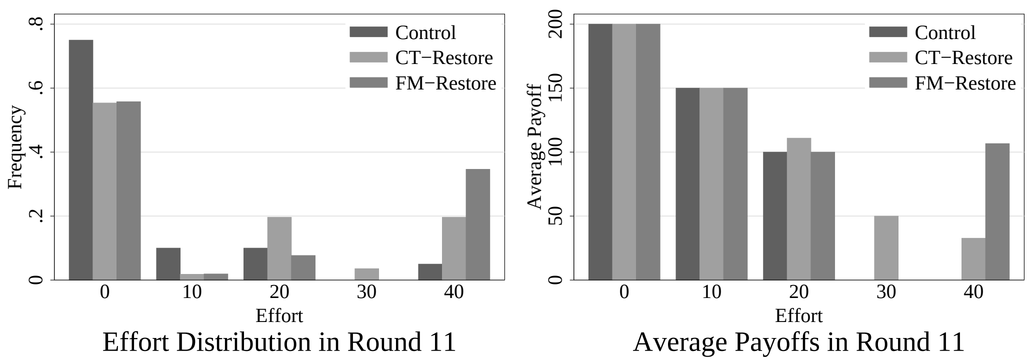


Figure 3.2: Effort and payoffs in round 11 for coordination failure groups in Control and Restore

that players choosing lower efforts still had higher payoffs. Thus it is not surprising that this increase in average effort is short-lived: figure 3.3 shows a clear decrease in effort during the second block. All groups that were trapped in coordination failure in round 10 also experience it in round 20 (the three groups that coordinated on a non-zero effort level in the first ten rounds continued to coordinate on the same level for the rest of the experiment). As can be seen from the figure, there are no clear differences between CT and FM treatments and statistical tests confirm this (p-values for the two-sided rank-sum tests on minimum or average group efforts are > 0.1 for all rounds except for average effort in round 15 where $p = 0.028$).

The increase in effort in round 11 may come partly from a restart effect, as often happens in similar experiments (Brandts and Cooper, 2006; Hamman et al., 2007; Le Lec et al., 2014). There is a visible restart effect in Control treatment in figure 3.3 but it is much smaller than in CT and FM treatments, thus the increase in effort after the mechanism is introduced appears to go beyond the restart effect. Although non-parametric tests are not powerful enough to detect this difference (p-value of the one-sided rank-sum test is 0.105 for round 11), regressing individual efforts in round 11 on the dummy that takes value 1 if a mechanism is present finds that the coefficient on the dummy variable is positive and statistically significant ($p = 0.033$ in an ordered probit regression with standard errors clustered on the group level, and controlling for effort of individuals in round 1).

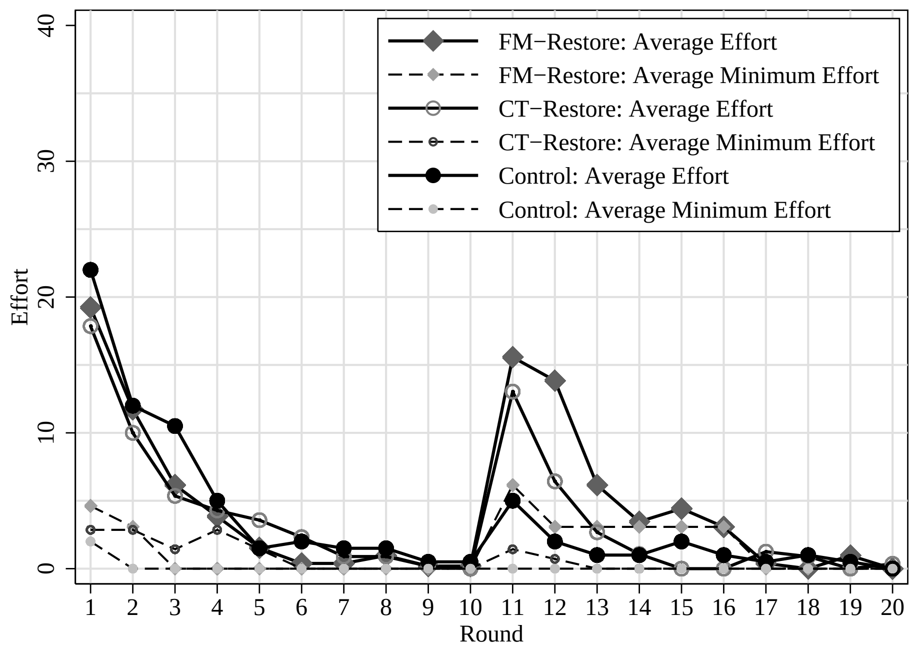


Figure 3.3: Evolution of average and minimum group effort for coordintation failure groups in Control and Restore

Result 1 *After a history of coordination failure, the mechanisms increase individual effort in the short run but not in the long run. They do not have a significant effect on group minimum effort.*

We conclude that the strong form of hypothesis 1 (that mechanisms strictly increase minimum effort) is not confirmed after a history of coordination failure. Could our mechanisms have prevented coordination failure if they were available from the start? The next subsection looks at this question.

Preventing coordination failure

We saw in the previous subsection that neither of the leadership mechanisms was successful in overcoming coordination failure. In our Prevent sessions, one of the mechanisms is present from round 1, thus the first block in those sessions allows the analysis of the effectiveness of communication and leading-by-example in avoiding coordination failure.

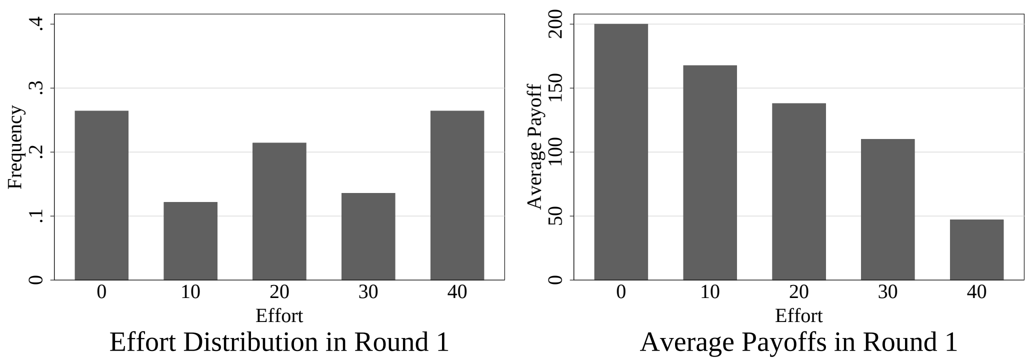


Figure 3.4: Effort distribution and average payoff in round 1 of Control and Restore sessions

The left panel of figure 3.4 displays the distribution of choices in the first round of the simultaneous game (our Control and Restore sessions). Similarly, the left panels of figure 3.5 do the same separately for CT and FM mechanisms in our Prevent sessions and for leaders and followers. As can be seen in the figures, choices in round 1 are quite variable. The average effort in the first round in Control and Restore sessions is 20.14. The average effort of the leaders in round 1 is 21.43 in both CT and FM Prevent treatments; the average effort of the followers is 25.24 in the CT treatment and 19.76 in the FM treatment. The distribution of leaders' efforts, pooled over CT and FM treatments, does not differ significantly from the distribution of first-round choices in Control and Restore treatments (p-value of the one-sided rank-sum test is 0.363). Thus the mechanisms do not per se lead to higher efforts in round 1. However, since followers' efforts are correlated with their group leader's effort, the average minimum effort across groups is higher in Prevent sessions than in Control and Restore sessions (10.71 in Prevent vs 5.14 in Control and Restore). As we see below, this has a significant effect for the evolution of play in subsequent rounds.

The right panel of figure 3.4 shows that in round 1 of Control and Restore sessions players who chose lower efforts got on average a higher payoff. From the right panels of figure 3.5, in round 1 of Prevent sessions average payoffs still tend to decline with effort but sometimes a higher effort leads to a higher payoff. The possibility of getting a higher payoff by choosing a higher effort arises because of the correlation of the choices of the followers.

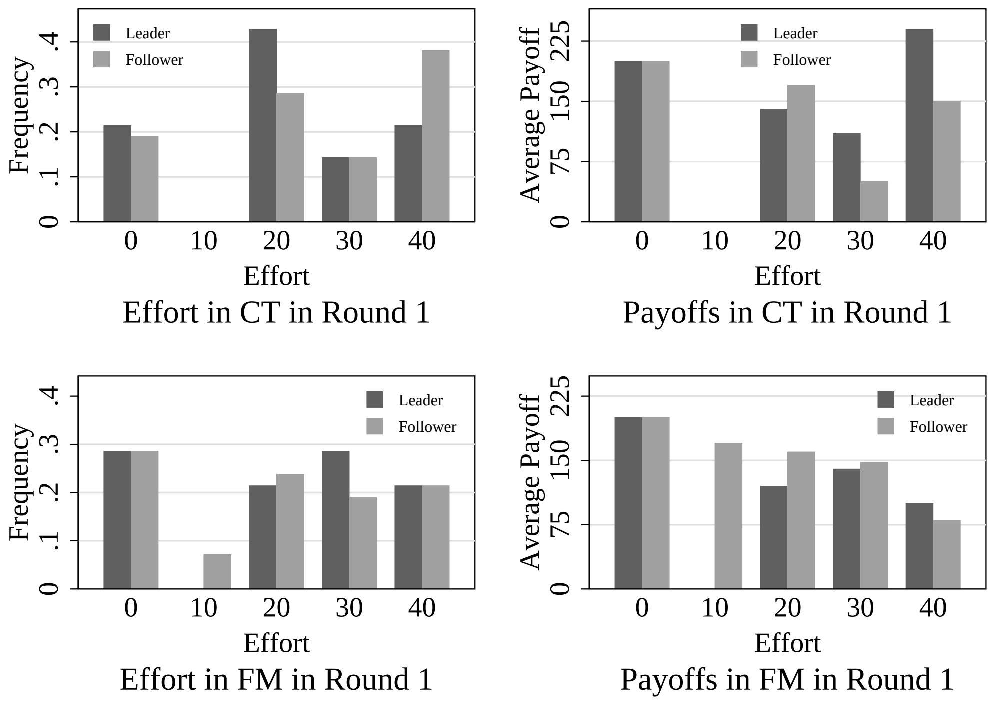


Figure 3.5: Effort distributions and average payoffs in round 1 of Prevent sessions

Note that the average effort of the followers in round 1 is higher in CT than in FM, while the average effort of the leaders is the same in both treatments. Recall that in CT treatments leaders could choose an effort different from the number they suggested; in fact, the average suggestion in CT in round 1 (which was 30.00) was higher than the average effort by the leaders (21.43). Since followers mostly matched the suggestion, this resulted in a higher average effort by followers in CT. The "deceptive" behavior of leaders is, of course, likely to lead to a decrease of effort in the future in their group.

The evolution of average and minimum group efforts over the first 10 rounds in Prevent sessions, separately for CT and FM treatments, can be seen in figure 3.6. There is no significant difference between the mechanisms (minimum p-value of the two-sided rank-sum tests on average or minimum group effort is 0.180 in round-by-round comparisons). As in Restore sessions, average effort declines over time, although average minimum effort increases in some rounds. By round 10, there are 9 out of 28 groups with a positive minimum group

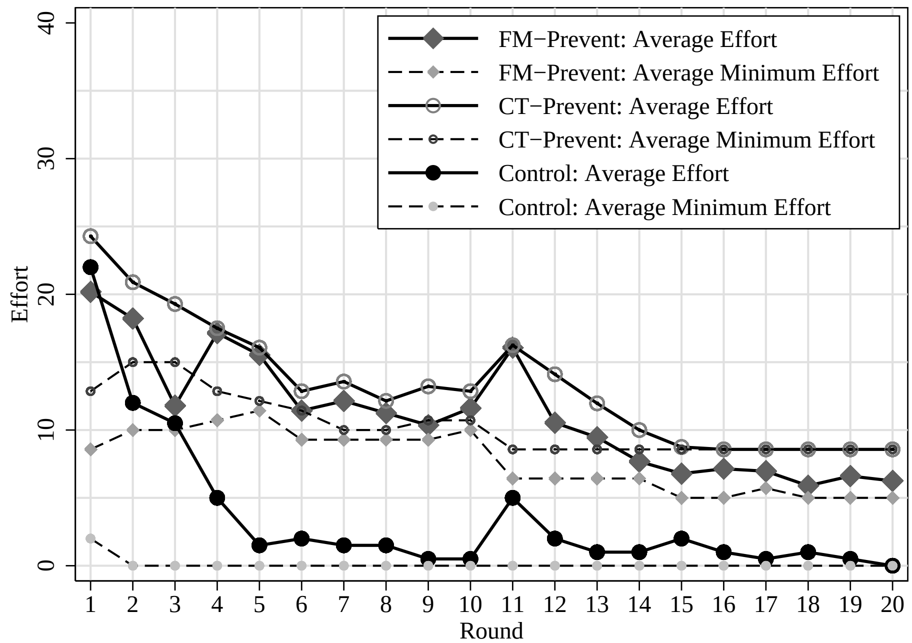


Figure 3.6: Evolution of average and minimum group effort in Prevent and Control sessions

effort in our Prevent sessions (4 out of 14 groups in CT and 5 out of 14 in FM). Although this proportion of groups with non-zero effort is not very high, recall that only 3 out of 35 groups in Control and Restore sessions had a positive minimum effort in round 10. Comparing pooled CT and FM data for rounds 1 to 10 with the simultaneous game, there is a significant difference in average group effort in each round after round 4 (all p-values for the one-sided rank-sum tests < 0.05). The average minimum effort in Prevent sessions is stable around 10 and is significantly higher than in Control and Restore sessions for each round after round 2 (for all these rounds $p < 0.05$). As Cartwright et al. (2013) and Sahin et al. (2015) found in more favorable parametrizations of the minimum effort game, we also observe that CT and FM mechanisms have some ability to raise average and minimum effort.

Do the effects of the mechanisms persist after the mechanism is removed? One can expect that because of an equilibrium lock-in, most subjects would choose the same effort in round 11 as in round 10. Nevertheless, some subjects may increase their effort due to the restart effect mentioned earlier; other subjects may reduce their effort due to beliefs being affected by the removal of the mechanism. In our experiment, more subjects increased their effort than reduced it; the overall effect was that the average effort increased from round 10 to round 11 but the average group minimum effort went down. Of the 9 groups that achieved a non-zero minimum effort in round 10, only 6 groups still have a positive minimum effort in round 11. Thus the removal of the correlation device (leader's suggestion/choice) has an immediate effect on the ability to avoid zero minimum effort in some groups. By round 20, only 5 groups still maintain a minimum effort above zero. Efforts in rounds 12-20 of Prevent sessions are not significantly different from those of rounds 2-10 in Control and Restore (i.e. comparing round 2 in Restore with round 12 in Prevent etc.; the smallest p-value of two-sided rank-sum tests is 0.136).

Result 2 *The leadership mechanisms have some ability to prevent coordination failure but there is no lasting effect after the mechanisms are removed.*

Timing of the mechanisms and welfare

The rules of the second block of the Restore sessions are the same as those of the first block of the Prevent sessions; the only difference is the history of coordination failure in Restore sessions (although not all groups experienced it). Pooling the two mechanisms (CT and FM) together, both average and minimum effort levels are noticeably higher in the first block of Prevent sessions compared with the second block of Restore sessions, as figure 3.7 shows. Non-parametric tests confirm that both average and minimum efforts are significantly higher in the first block of Prevent sessions compared with the second block of Restore sessions (i.e. comparing round 2 in Prevent with round 12 in Restore etc.) after round 2 (p-values of the one-sided rank-sum round-by-round tests are < 0.05). This confirms our hypothesis 2.

Result 3 *The leadership mechanisms are more effective if introduced early.*

For the comparison between rounds 1 in Prevent and 11 in Restore, the difference between group average and minimum efforts is less noticeable on the figure, and non-parametric tests are only marginally significant ($p = 0.055$ for the one-sided test on the minimum group effort).

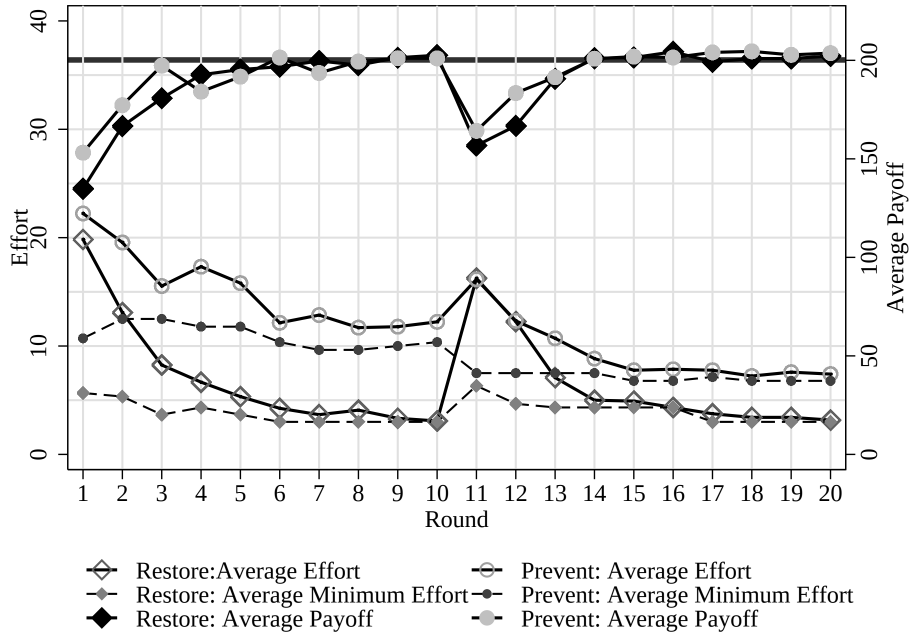


Figure 3.7: Average efforts and payoffs for Restore and Prevent sessions

The analysis of individual strategies presented in the next section will help us understand the dynamics that makes this difference significant in later rounds.

As we have seen in the previous subsections, the mechanisms have a positive effect on individual effort. However, since the average minimum effort remains relatively flat in each treatment, within a treatment a higher average effort means that there is more mis-coordination. Because of the high cost of mis-coordination in our environment, a higher average effort resulted in a lower average payoff. This can be seen in figure 3.7, which, along with the average and minimum effort levels, shows the average payoff (on a different scale) in each treatment over time.

Across treatments, the group payoffs, averaged over all twenty rounds, are not significantly different between Prevent and Restore sessions (p-value of the two-sided rank-sum test is 0.797). These payoffs, pooled over Prevent and Restore sessions, also do not differ significantly from those of groups in the Control session (in Control, groups had an average payoff 182.9 across all rounds; in the other groups the average payoff was 191.2). Thus the mechanisms have no significant effect on group average payoffs. Note also that the average payoffs are below 200, the payoff that any player could guarantee by choosing effort 0; in fact, the difference of average payoff from 200 is significant (p-value of the two-sided sign-rank test based on all 63 groups is < 0.001).

3.4.2 Individual behavior

One innovative aspect of our design is the use of the strategy method to elicit followers' contingent strategies. Followers were asked to state an effort level for each possible choice (suggestion in CT treatment and actual effort in FM treatment) of the leader. In this section we analyse the choices of the leaders, the strategies of the followers, and perform a counterfactual analysis to address the question of whether the responsibility for coordination failure lies with the leader or with the followers.

Leaders' choices

In our FM treatment leaders simply choose effort; in CT treatment leaders also suggest a number that is seen by their followers but they could choose an effort different from the suggested number. In all treatments, leaders also state their beliefs about what they expect the minimum effort of the followers to be. Figure 3.8 shows average leaders' effort choices, suggestions and beliefs in each round, together with the average group minimum effort in the round.

Recall that from our discussion in section 3.2 we could not make an unambiguous prediction about whether leaders would choose a higher effort in CT or in FM. The two-sided rank-sum tests on leader's effort choices find no differences between CT and FM treatments, either in round 1 of Prevent, round 11 of Restore, or averaging each leader's choices over all ten rounds of a mechanism. Pooling CT and FM together and comparing the averages of leaders' effort choices in the ten rounds of mechanisms, we find that leaders in Prevent

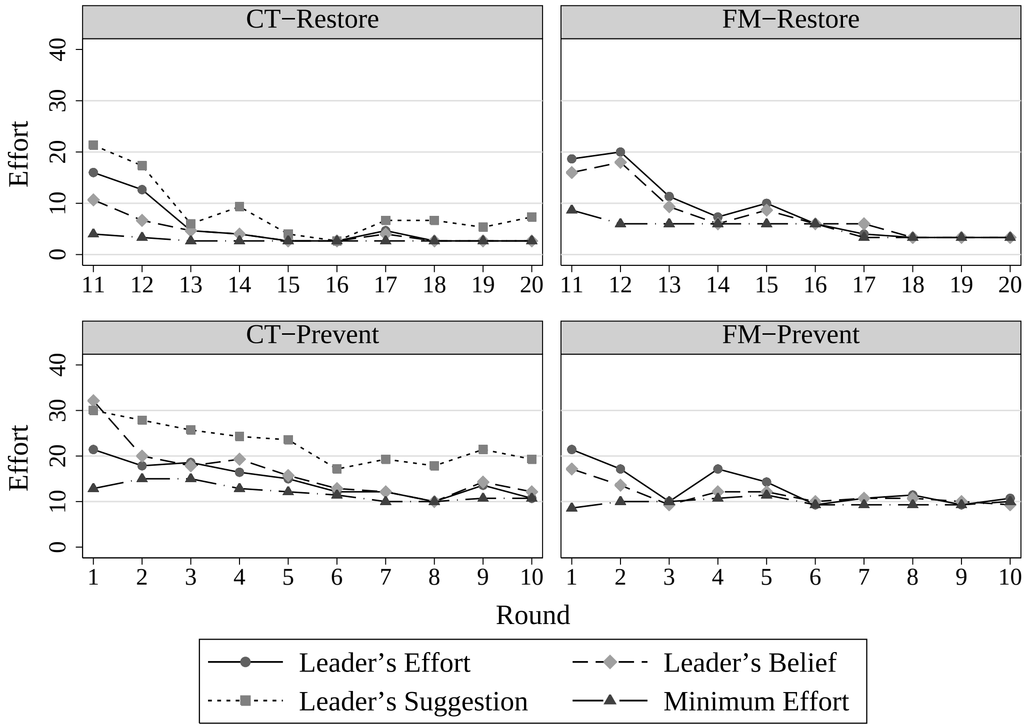


Figure 3.8: Leaders' average effort choices, suggestions and beliefs

choose a significantly higher effort than in Restore (p-value of the one-sided rank-sum test is 0.042).

According to hypothesis 4, we expect leaders' effort to be higher than the effort of players in the simultaneous game. Although leaders' efforts in round 1 of Prevent sessions are not significantly above those in round 1 of the simultaneous game (Restore and Control sessions), the one-sided rank-sum test on the average efforts over ten rounds (averaging all players in a group in the simultaneous game) finds that efforts by leaders are marginally significantly higher (p-value is 0.075). In Restore sessions, leaders' effort in round 11 in groups with a history of coordination failure is significantly higher than efforts in the simultaneous game in the Control treatment (p-value of the one-sided rank-sum test is 0.014). This provides some evidence in support of the hypothesis.

Figure 3.8 shows that, while beliefs, actions and minimum effort become very close in all treatments after a few rounds, average suggestions in CT treatments are higher than average actions for almost all rounds, especially in Prevent sessions. While a majority of leader-communicators' decisions coincide with the suggestion, a sizable minority of effort choices by a leader when the suggestion was not zero is below the suggested number (around 44% in both Restore and Prevent). In Prevent sessions, the suggestions of leaders in CT are significantly higher than the choices of leaders in FM (p-values of the one-sided rank-sum tests are 0.066 for round 1 and 0.022 for averages over rounds 1-10); in Restore sessions, there is no significant difference. Interestingly, the average actual effort of leaders in CT-Prevent is lower than that of the followers (14.79 vs 16.76), implying that the leaders followed their own suggestion (on average 22.64) even less than their followers did, though this difference in effort is not significant.

To get more insight into leaders' decisions, we use regression analysis. Unlike followers, leaders did not have a suggestion or choice of another player to base their decisions on; the amount of information they have available is similar to that of players in the simultaneous game. We therefore combine leaders' effort choices with those of players that did not experience a leadership mechanism (rounds 1-10 in Restore and Control sessions and rounds 11-20 in Control session in our experiment). In the first two columns of table 3.3 we report the results of random-effects regressions of effort choices on treatment dummies, group history and a time trend.[18]

The regressions confirm that there is little difference in leaders' efforts across treatments; they also do not find a significant difference in efforts between the first ten rounds of the simultaneous game and the leaders' efforts in Prevent. The signs of the coefficients show that leaders' efforts were not below the choices in the simultaneous games. The only significant difference is that the efforts in the second ten rounds of the simultaneous game are lower than the efforts of the leaders. The history of the group, summarized by the minimum effort in the previous round, plays a role in the effort choice of the leader, and there is a downward trend.

[18]An ordered probit specification produces similar significance results. We also tried including interaction terms and lagged effort in the regression; the results stay similar.

Table 3.3: Regressions for leader's efforts and suggestions

Dependent variable:	Effort Rounds 1 and 11	Effort Rounds 2-10 and 12-20	Suggestion/choice Rounds 1 and 11	Suggestion/choice Rounds 2-10 and 12-20
Simultaneous Part 1	-1.186 (4.290)	-0.641 (0.746)		
FM-Prevent	(Base)	(Base)	(Base)	(Base)
CT-Prevent	< 0.001 (5.493)	-0.508 (1.040)	8.571 (6.240)	7.869*** (2.500)
Simultaneous Part 2 (Control session)	-16.963*** (4.698)	-2.443*** (0.864)		
FM-Restore	-3.680 (6.209)	-0.213 (1.185)	-2.762 (6.135)	-1.524 (1.655)
CT-Restore	-4.429 (5.450)	-0.997 (0.887)	-0.095 (6.135)	-0.119 (2.151)
Group minimum effort in the previous round		0.948*** (0.020)		0.705*** (0.072)
Round		-0.720*** (0.073)		-0.842*** (0.209)
Constant	21.429*** (4.082)	7.281*** (0.911)	21.429*** (4.412)	10.393*** (1.884)
N	218	1962	58	522
Clusters (by subject)	168	168	58	58

Notes: standard errors adjusted for clusters in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The last two columns in table 3.3 regress leaders' suggestions (in CT) and choices (in FM) on treatment dummies and the other variables. Although in rounds 1 and 11 we are not able to detect significant differences between suggestions and efforts, over all ten rounds of the mechanisms the suggestions of the leaders in CT-Prevent are found to be significantly higher than the leaders' efforts in FM-Prevent, while there is little difference for the other treatments. Since the efforts of the leaders are not significantly different across treatments, this confirms the previous evidence that in CT leaders often put a higher suggestion than the effort they choose.[19]

[19]Pogrebna et al. (2011) find a similar result in a voluntary contribution game setting. Actual contributions of cheap-talk leaders and first movers are similar, but announcements by cheap-talk leaders are significantly above their actual contribution.

Result 4 *Efforts of leaders are similar in the two leadership mechanisms, and only marginally higher than the efforts of players in the simultaneous game. In CT-Prevent treatment, leaders do not follow their own suggestion to the full extent.*

Followers' strategies

Figure 3.9 shows the followers' strategies in each of the mechanisms for each round of the leader-follower block (rounds 1-10 in Prevent and rounds 11-20 in Restore). Each bar represents the frequency of a certain choice by the followers in a certain round, given the leaders' suggestion or choice. For example, the leftmost bar in the bottom-left corner of panel (a) of the figure indicates that about 30% of the followers would choose 0 effort in round 11 (round 1 of the leader-follower setup in Restore) if the leader suggested 40; the rightmost bar in the same corner shows that about 75% of the followers would choose 0 effort in round 20 if the leader suggested 40.

As can be seen in the figure, the most common strategies were either to match the leader's suggestion or choice (the bars on the diagonal of each panel) or to choose zero effort irrespective of the leader's suggestion or choice (the bars in the left column of each panel).[20] We define the *Match+* strategy as a strategy in which a follower chooses at least[21] L for all effort levels L that the leader might suggest or choose. *Always-zero (All0)* is a strategy where a follower chooses zero regardless of what the leader might suggest or choose.

Figure 3.10 condenses the information in figure 3.9 to show the evolution of these two strategies. Initially, Match+ is more frequent than All0. However, in all treatments, the play of the All0 strategy increases over time. The play of the Match+ strategy generally decreases over time except in the FM-Prevent treatment where it stays roughly constant.

[20]Note that followers do not choose zero more often after a higher suggestion/choice of the leader and their non-zero choices match the leader's choice. Thus their efforts are (stochastically) higher after a higher suggestion/choice by the leader, as conjectured in section 3.2.

[21]Choosing an effort above what the leader chooses or suggests might seem irrational but may be done either in expectation that the leader *will* actually choose a high rather than a low effort (thus what the follower chooses for a low effort of the leader is irrelevant), or in order to "teach" the leader the virtue of choosing a high effort.

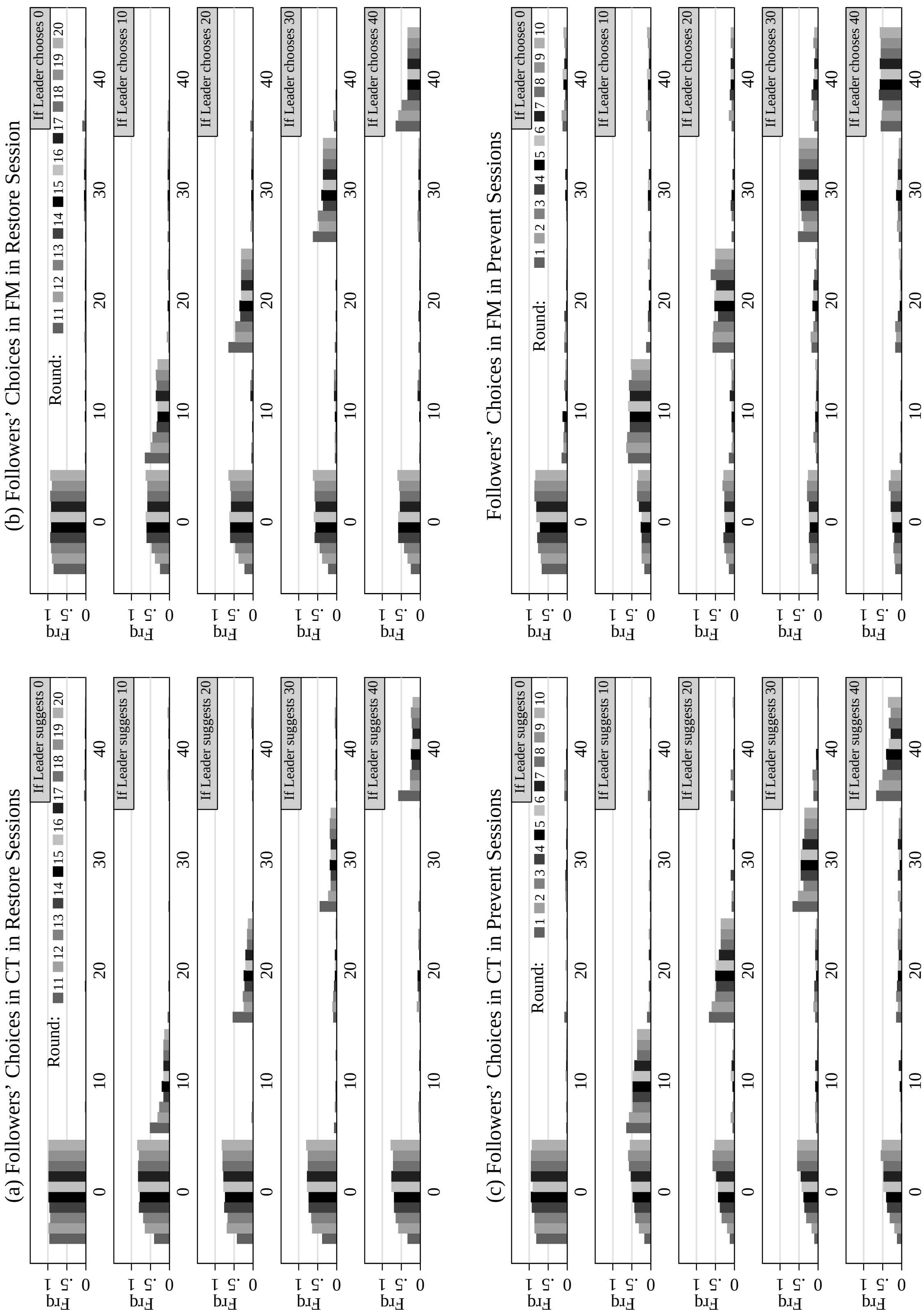


Figure 3.9: Followers' choice in response to leader's suggestion/choice

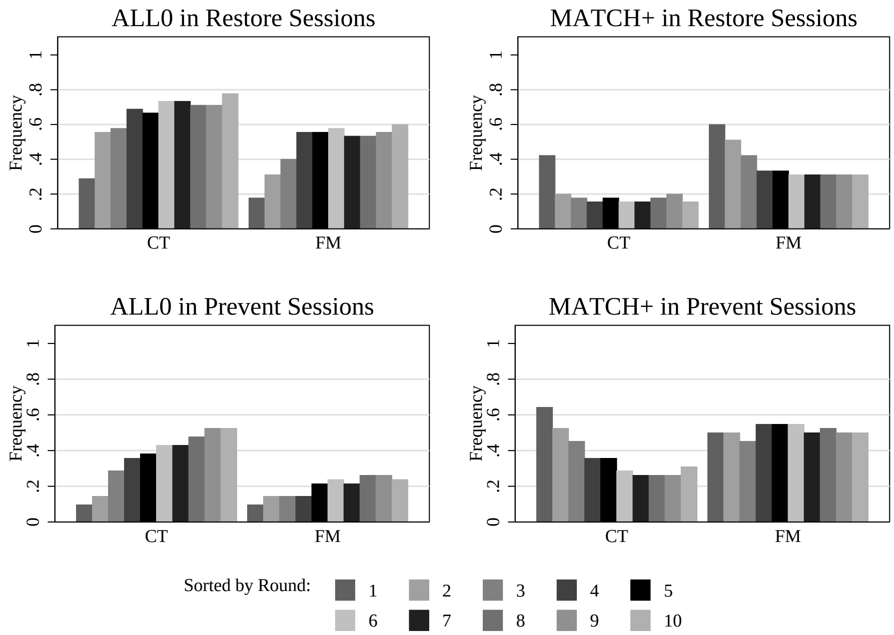


Figure 3.10: Evolution of All0 and Match+ strategies

The reason behind this change in the use of the All0 and Match+ strategies is that the Match+ strategy is effective only if all three followers adopt it (if at least one other follower uses the All0 strategy and the leader suggests or chooses a non-zero effort level, the Match+ strategy hurts the follower who uses it).

From our discussion in section 3.2, followers are expected to shift their effort towards the leader's suggestion/choice, compared with the distribution of choices in the simultaneous game, and more so in FM than in CT. For any given suggestion/choice of the leader, we find no significant difference in followers' choices between CT and FM in round 1 of Prevent sessions, but the pooled distribution of follower's choices in CT and FM is significantly higher than the distribution of choices in the simultaneous game.[22] Similarly, for Restore sessions, in groups with coordination failure in round 10, there is no significant difference between CT and FM followers' choices in round 11, but these (pooled) choices are significantly higher than

[22] If the leader's suggestion/choice is 40, the distributions can be directly compared. For other suggestions/choices (for example, $L = 30$), we look only at the distribution of choices in the simultaneous game that do not exceed L.

the choices in round 11 of the Control session. Thus the followers' strategies are consistent with the mechanisms having a focality effect in the first round.

Followers are also expected to be more responsive to the leader's suggestion/choice in Prevent sessions compared with Restore sessions. In order to include this comparison, since the choices of the followers in a group are not independent after round 1, we take, for a given suggestion/choice of the leader, the average choice of the followers in the same group over all ten rounds as a measure of responsiveness of the followers in this group. This gives us, for each treatment (CT-Restore, CT-Prevent, FM-Restore, FM-Prevent), as many independent observations as there are groups in the treatment. With this measure, for each possible suggestion/choice of the leader, we are able to reject the hypothesis that there are no differences between the four treatments (maximum p-value of the Kruskal-Wallis tests is 0.022). When we pool CT and FM treatments and compare Restore with Prevent sessions, we find a significantly higher responsiveness in Prevent sessions (the largest p-value of the one-sided tests is 0.006). When we pool Restore and Prevent sessions and compare CT with FM, the responsiveness in FM is also significantly higher than in CT (the largest p-value of the one-sided tests is 0.038). Thus we find support for our hypothesis 3.

Table 3.4 reports regressions of followers' choices on leader's suggestion/choice, including treatment dummies, separately for period 1 in Prevent (and period 11 in Restore), and for all other periods. In the regressions we also include the history of the group represented by the minimum effort in the previous round and a time trend.[23]

The leader's suggestion/choice variable is highly significant, as expected, and the coefficients in the random-effects linear regressions show that, for a unit increase in leader's effort, followers increase their effort on average only by 0.58 in round 1 and by 0.39 in rounds 2-10, confirming that they do not match the leader's suggestion/choice perfectly. The regressions also confirm that there are significant differences between CT-Restore and CT-Prevent in

[23]We also ran regressions including interaction terms of the variables with treatment dummies. Most of those interaction terms were insignificant and the coefficients in the table and their significance remained mostly unaffected.

Table 3.4: Regressions for followers' choices

Dependent variable:	Effort (Ordered probit)	Effort (Random-effects)	Effort (Ordered probit)	Effort (Random-effects)
	Rounds 1 and 11		Rounds 2-10 and 12-20	
CT-Restore	-0.557*** (0.195)	-6.286*** (2.095)	-0.341 (0.257)	-3.667* (2.045)
CT-Prevent	(Base)	(Base)	(Base)	(Base)
FM-Restore	-0.210 (0.179)	-2.463 (2.036)	0.088 (0.221)	-0.271 (2.127)
FM-Prevent	-0.077 (0.149)	-1.095 (1.744)	0.592*** (0.193)	5.355*** (1.958)
Leader's suggestion/choice	0.051*** (0.005)	0.582*** (0.039)	0.042*** (0.004)	0.394*** (0.041)
Group minimum effort in the previous round			0.037*** (0.005)	0.226*** (0.059)
Round			-0.045*** (0.011)	-0.426*** (0.084)
Constant		7.309*** (1.551)		3.161** (1.533)
N	870	870	7830	7830

Notes: standard errors clustered by 58 groups in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

round 1 (round 11 in Restore). In the other rounds there is a significant difference between CT-Restore and CT-Prevent, and CT-Prevent and FM-Prevent treatments, again confirming that in Restore sessions followers' choices are lower than in Prevent sessions, as well as that in CT treatment choices are lower than in FM treatment. In addition to the leader's suggestion/choice and treatment dummies, group history is important, and there is also a significant downward trend not explained by the other variables.

Result 4 *On average, followers match a leader's increase in effort only partially. For a given suggestion/choice of the leader, the effort choices of the followers are higher in FM than in CT, and they are higher in Prevent than in Restore.*

The regressions reported in Table 3.4 find little difference between FM-Restore and FM-Prevent treatments in rounds 1 and 11, and indeed from figure 3.10 the proportion of Match+ strategy in FM-Restore in round 11 is actually higher than in FM-Prevent in round 1. How

Table 3.5: Determinants of strategies in FM treatment

Dependent variable:	All0	Match+	All0	Match+	All0	Match+
	Rounds 1 and 11		Rounds 2 and 12		Rounds 2-10 and 12-20	
Prevent	-0.385	-0.253	-0.374	0.109	-0.466***	0.288*
(Base: Restore)	(0.332)	(0.236)	(0.351)	(0.266)	(0.175)	(0.168)
Number of zeros			0.371***	-0.189*	0.248***	-0.205***
of others$_{t-1}$			(0.128)	(0.100)	(0.056)	(0.052)
All0$_{t-1}$			2.145***		2.754***	
			(0.536)		(0.246)	
Match+$_{t-1}$				1.870***		2.664***
				(0.351)		(0.192)
Constant	-0.924***	0.253	-1.475***	-0.884***	-1.639***	-1.289***
	(0.242)	(0.189)	(0.304)	(0.279)	(0.224)	(0.192)
N	87	87	87	87	783	783

Notes: probit regressions with standard errors clustered by 29 groups in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

did the significant difference between FM-Restore and FM-Prevent nevertheless developed? To understand this, we introduce a variable measuring the number of other subjects in the group whose effort was observed to be 0. This variable can take values between 0 (if the leader chose a positive effort and the other followers made a positive effort in response to this) and 3 (if all three others, including the leader, chose 0 effort). The idea is that subjects may become discouraged from choosing positive effort, and thus start playing All0 strategy, if they see that many in their group chose zero effort.

Table 3.5 presents the results of regressions for the FM treatment in which the dependent variables are the indicator variables whether a strategy employed by a follower was All0 or Match+. For rounds 1 and 11, the Restore/Prevent dummy by itself does not have explanatory power. The variable measuring the observed number of zero-effort choices of others is statisticially significant explanation of the probability of choosing strategies All0 (positively) and Match+ (negatively), when also controlling for the choice of these strategies in the previous period, both in rounds 2 and 12 only, and for all rounds between 2-10 and 12-20.

Recall from the previous subsection that leaders' choices of effort were not significantly different between FM-Restore and FM-Prevent in rounds 1 and 11. The regressions thus show that the small initial difference in the frequency of All0 strategy, and thus in observing how many others in the group chose zero effort, is amplified over time leading to the differences between FM-Restore and FM-Prevent observed in later rounds.

Coordination failure: leader's or followers' responsibility?

Knowing followers' strategies, we can see if it would have been possible for leaders to achieve a higher group effort by unilaterally changing their choice. We find that if the leader had chosen a different effort level (and corresponding suggestion in CT), the minimum effort in 22 out of 58 groups would have increased in the first round of the leader-follower setup (i.e. round 11 in Restore sessions and round 1 in Prevent sessions). There are, however, also many cases where the leader's effort is higher than the minimum effort of the followers (29 out of 58 groups).[24]

Given the distribution of the followers' choices, we ask what the expected payoff for leaders would be from choosing various effort levels (in FM) or suggesting various numbers and following them (in CT). The leader's expected payoff is calculated as follows: using followers' choices collected by the strategy method, we calculate the distribution of the minimum effort of three randomly selected followers for each possible choice of the leader, and use this distribution to find the leader's expected payoff for each choice. We also do this for the followers, calculating expected payoffs a follower would get from following various possible choices or suggestions of the leader. For this, we take into account the probability distribution for the choices of the other two followers in the group, randomly chosen from the observed population of followers. Figure 3.11 shows the leader's and a follower's expected payoffs calculated in this way.

[24]Note that a low minimum group effort could be both leader's and followers' fault. For example, suppose that the minimum of the followers' effort is zero except in the case in which the leader chooses 40. If the leader chooses 20, the group minimum is zero while at the same time the leader could have chosen 40 and the whole group would coordinate on the efficient effort of 40.

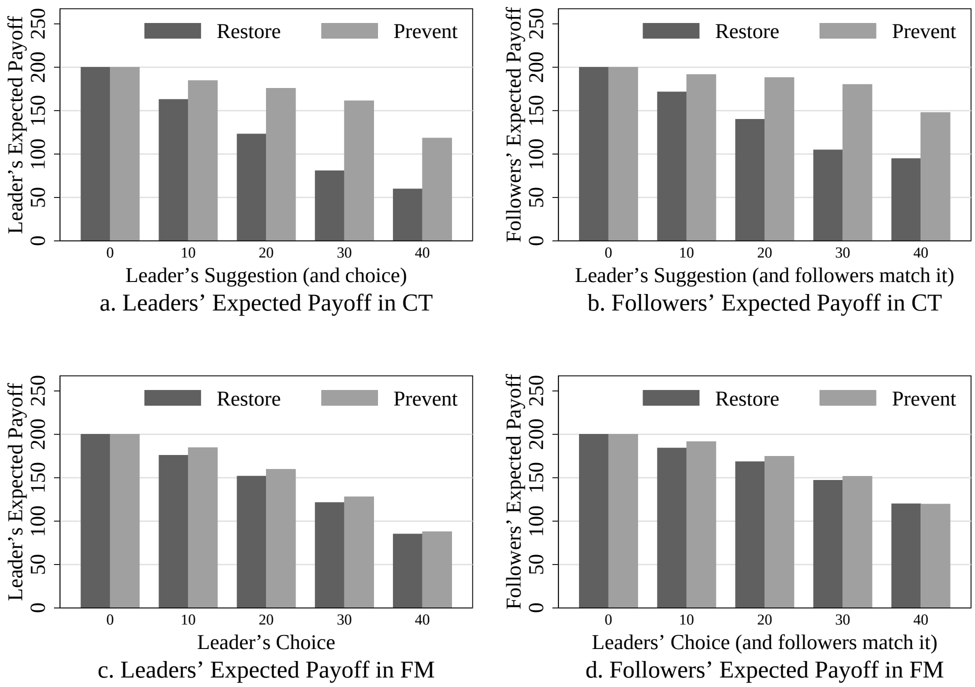


Figure 3.11: Leader's and follower's expected payoffs given followers' choices in strategy method

The two left panels in the figure are for the leaders. Even though there are some treatment differences, zero effort is the optimal choice in all treatments. The two right panels are for the followers. The payoffs of followers are calculated for cases in which the leader would choose (in FM) or suggest and choose (in CT) the given effort level and the follower would follow that leader's choice. For all effort levels higher than 0, the expected payoffs are lower than the payoff 200 that a player could guarantee by always choosing 0. What the figure thus shows is that fully following the leader's suggestion/choice is not optimal even for a risk-neutral follower (and even if CT leaders always followed their own suggestion).[25] The uncertainty arising from the decisions of only two (rather than three as for the leader) other followers in a group is still sufficiently high, so that the expected payoff of a follower is lower than 200. The payoffs in figure 3.11 are based on the first round of the leader-follower setup.

[25]We also calculated expected payoffs of leaders in CT from choosing efforts different from the suggested one and of followers (in all treatments) from partially following the leader (e.g. choosing 10 after the leader suggests/chooses 30). All expected payoffs were below 200.

Given that the strategies of the followers become less responsive over time, in subsequent rounds effort 0 remains the optimal choice.

Result 6 *Given the population distribution of followers' choices, neither the leader nor a follower would individually be better off in expected terms in the first round of the mechanisms by choosing an effort other than 0.*

The main blame for this observation lies with followers: the proportion of them playing the All0 strategy is too high for any positive effort to be profitable. Leaders are also partially to blame though: their persistent failure to follow their own suggestion in CT may be a reason why not all followers follow the leader's suggestion, and in many groups a different leader's choice could have increased the minimum effort. Overall, it is a collective failure: players could not unilaterally have increased their expected payoff by choosing a higher effort, thus it was individually rational to choose the safe option of zero effort.

3.5 Conclusion

We analyzed the effects of two leadership mechanisms (pre-play communication and leading-by-example) in a tough parametrization of the minimum effort game. Unlike most of the literature (e.g. Blume and Ortmann, 2007; Cartwright et al., 2013; Sahin et al., 2015, in different environments), we found that in this challenging setting the mechanisms failed to overcome coordination failure and had only limited effectiveness in preventing it. The mechanisms had some effect in the short run as some players attempted to choose a higher effort but in the long run most players fell back to the lowest possible effort. These results therefore delineate the limits of the mechanisms for preventing and overcoming coordination failure. Our mechanisms involve a rather minimal implementation: our leaders are randomly chosen and communication consists of a single number (interpreted as a suggestion of effort); thus it appears necessary to have more complicated mechanisms to enable players to avoid coordination failure in this game.

In both leadership mechanisms a substantial proportion of followers chose the effort level corresponding to the leader's suggestion or choice. However, in each treatment, there was a considerable number of followers who, instead of following the leader, always chose zero effort, irrespective of the suggestion or choice of the leader. Since the outcome depends on the minimum effort in the group, the presence of even one such player often led to the group effort falling back to the lowest level in the long run. Given the non-negligible proportion of such players, the expected payoff of both leaders and followers would be maximized by choosing zero effort. Thus the mechanisms' failure can be attributed to a large extent to non-responsive followers in our environment.

Notwithstanding the non-responsiveness of some players, the data from the strategy method show that followers followed the leader more in the first-mover treatment than in the cheap-talk treatment. Moving first seems to bestow a greater legitimacy on a leader than simply making a suggestion; indeed, even the leaders themselves did not always follow their own suggestion. However, committing to a high effort is risky in our game and the efforts of first-mover leaders were lower than the suggestions of cheap-talk leaders. The signals sent by leaders were different in the two mechanisms, and followers reacted to them differently, but the combination of leaders' suggestions or choices and followers' reaction to them led on aggregate to similar results in both mechanisms.

Afterword

My goal in this thesis is to contribute to the understanding of behavioural mechanisms in human cooperation and coordination. The three chapters presented show the major output of my research in the pursuit of the Ph.D. degree.

Chapter 1 proposed a mechanism where peers can decide on payoff divisions. By incorporating an equity principle where individual's reward is proportional to their reward, I made theoretical predictions that high contributions can be achieved as a result. I then conducted an experimental investigation to test the effectiveness of the mechanism. In the treatments with the mechanism, I found that even the group composition reshuffles each round in the experiment, more than 80 percent of the participants allocate according to the equity principle. Moreover, the experimental participants achieved a remarkably high contribution rates.

This chapter contributes to the economic design literature by demonstrating that a small intrinsic concern for the distributive justice, when utilised by an appropriate social institution, has significant success in overcoming the free-rider problem in team production and improving social efficiency. It thereby highlights the usefulness of richer behavioural assumptions such as equity and other moral standards in the design of effective institutions.

The equity principle, which featured prominently in chapter 1's mechanism can be challenged by the presence of costly monitoring and heterogeneous social identities. The purpose of chapter 2 was to understand how these two factors would affect the equity principle and the performance of the mechanism. When the monitoring involved a personal cost but no benefit, the traditional economic theory would predict zero monitoring rate and zero contribution rate. The experiment results contradicted the prediction; in almost half of the situations, participants chose to sacrifice their own resources to enforce the equity principle. Likewise, when heterogeneous social identities were present in the group, I found although a few participants gave more to their in-group member, the majority of them still follow the equity principle to allocate. Furthermore, the mechanism had shown very high contribution rates under both circumstances.

The contributions of this chapter are two-folds. First, it offers an example demonstrating people's willingness to incur personal costs to maintain social norm. Such willingness, or "strong reciprocity" (Fehr et al., 2002), plays a crucial role in human cooperation. Second, it shows that the equity principle stands as a robust way in allocation even with the presence of social identity. This chapter thus strengthens the conclusions reached in Chapter 1 and further emphasises the potential of testing the mechanism in the field.

In Chapter 3, I analysed the effects of two leadership mechanisms (pre-play communication and leading-by-example) in a minimum-effort coordination game. Both mechanisms had the potential to help the group members to an efficient coordination outcome. However, unlike previous studies, I found this is not the case; the mechanisms failed to overcome coordination failure and had only limited effectiveness in preventing it.

The main contribution of this last chapter is to suggest that the success of mechanisms such as communication and leadership in achieving efficient outcomes depends on its implementations. In our experiment leaders are randomly chosen participants, and communication only consists of a single number. Such minimal implementations of the leadership mechanism are not able to help achieving efficient coordination.

Overall, this thesis has investigated several simple mechanisms that take into account behavioural regularities and evaluated their effectiveness using experimental methods. There is much that is exciting about this research approach. The emergence of increasingly deeper understandings of individual's social preferences generated by behavioural studies, and the awakening of mechanism design literature to incorporate such preferences, provide for a growing reach of behavioural mechanism. Experimental methods prove to be valuable in economists' toolbox, especially when we are fine-tuning the mechanisms to be used in the real world. Though we need to be cautious when applying the results we get from the laboratories to its practical use, it also means we need more experiments or field experiments to assess the possible successes and failures under different conditions. Beyond the studies presented in this thesis, there are still much to be learned about the mechanisms, for example, how network structure would affect the structure of the mechanism, how learning dynamics would evolve the steady state, or how other behavioural elements would alter people's incentives. It therefore provides an enticing and extremely rewarding agenda for the future research.

Appendices

A. Appendices for Chapter 1

Appendix A1. Proofs of Propositions

Appendix A1.1. Proof of Proposition 1

Proof. In the first stage, $e_i = \bar{e}$ is a dominant strategy if and only if $\pi_i(\bar{e}, e_{-i}) > \pi_i(e_i, e_{-i})$, $\forall e_{-i}$. It suffices to prove $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$,$\forall e_{-i}$ where $\Delta e_i > 0$. We consider the following three cases.

1. Suppose $e_j = e_k = 0$. If player i chooses to contribute, $e_i > 0$, both player j and player k will give player i 1 in the allocation stage. Therefore, $a_{ji} + a_{ki} = 2$ and $\pi_i(e_i, e_{-i}) = \bar{e} - e_i + \frac{2}{3}\beta e_i$. If player i chooses not to contribute, then $\pi_i(0, e_{-i}) = \bar{e}$. Note, $\beta > \frac{3}{2}$ is a sufficient condition to make $\pi_i(e_i, e_{-i}) > \pi_i(0, e_{-i})$. Furthermore, when $e_i > 0$, we have $\pi_i(e_i + \Delta e_i, e_{-i}) = \bar{e} + (\frac{2}{3}\beta - 1)(e_i + \Delta e_i)$. Therefore, $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$ if and only if $\beta > \frac{3}{2}$.

2. Suppose $e_j = 0$ and $e_k > 0$, or, $e_j > 0$ and $e_k = 0$. That is, except for player i, there is only one player who contributes. We only consider the case where player k contributes and player j does not; the other case would be similar. Now if player i chooses to contribute, $e_i > 0$, player k would give player i $a_{ki} = 1$ and player j would give player i $\frac{e_i}{e_i+e_k}$. Therefore $a_{ki} + a_{ji} = 1 + \frac{e_i}{e_i+e_k}$ and $\pi_i(e_i, e_{-i}) = \bar{e} - e_i + \frac{\beta}{3}(e_i + e_k)(1 + \frac{e_i}{e_i+e_k})$.

Next, suppose that player i does not contribute, that is, $e_i = 0$. Then only player k will give him $\frac{1}{2}$ and player j will give him zero. Therefore, $a_{ki} + a_{ji} = 1/2$ and $\pi_i(0, e_{-i}) = \bar{e} + \frac{\beta}{6} e_k$. Note, $\beta > \frac{3}{2}$ is a sufficient condition to make $\pi_i(e_i, e_{-i}) > \pi_i(0, e_{-i})$. Furthermore, when $e_i > 0$, we have $\pi_i(e_i + \Delta e_i, e_{-i}) - \pi_i(e_i, e_{-i}) = (\frac{2}{3}\beta - 1)\Delta e_i$. Therefore, $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$ if and only if $\beta > \frac{3}{2}$.

3. Suppose $e_j > 0$ and $e_k > 0$. That is, both player j and player k contribute. If player i chooses to contribute, $e_i > 0$, player j will give him $\frac{e_i}{e_i+e_k}$ and player k will give him $\frac{e_i}{e_i+e_j}$. Therefore, $a_{ji} + a_{ki} = \frac{e_i}{e_i+e_k} + \frac{e_i}{e_i+e_j}$ and $\pi_i(e_i, e_{-i}) = \bar{e} - e_i + \frac{\beta}{3}(e_i + e_j + e_k)(\frac{e_i}{e_i+e_k} + \frac{e_i}{e_i+e_j})$. If player i chooses not to contribute, then $\pi_i(0, e_{-i}) = \bar{e}$. We next re-write player i's payoff function $\pi_i(e_i, e_{-i})$ for ease of calculation, and then prove $\pi_i(e_i + \Delta e_i, e_{-i}) - \pi_i(e_i, e_{-i}) > 0$:

$$\begin{aligned}
\pi_i(e_i, e_{-i}) &= \bar{e} - e_i + \frac{\beta}{3}(e_i + e_j + e_k)(\frac{e_i}{e_i + e_j} + \frac{e_i}{e_i + e_k}) \\
&= \bar{e} - e_i + \frac{\beta}{3}(e_i + \frac{e_i e_k}{e_i + e_j} + e_i + \frac{e_i e_j}{e_i + e_k}) \\
&= \bar{e} - e_i + \frac{2\beta}{3} e_i + \frac{\beta}{3} e_i (\frac{e_k}{e_i + e_j} + \frac{e_j}{e_i + e_k}) \\
&= \bar{e} + (\frac{2\beta}{3} - 1) e_i + \frac{\beta}{3}(\frac{e_k}{1 + \frac{e_j}{e_i}} + \frac{e_j}{1 + \frac{e_k}{e_i}})
\end{aligned}$$

Thus,

$$\begin{aligned}
&\pi_i(e_i + \Delta e_i, e_{-i}) - \pi_i(e_i, e_{-i}) \\
&= (\frac{2\beta}{3} - 1)\Delta e_i + \frac{\beta}{3}(\frac{e_k}{1 + \frac{e_j}{e_i + \Delta e_i}} + \frac{e_j}{1 + \frac{e_k}{e_i + \Delta e_i}}) - \frac{\beta}{3}(\frac{e_k}{1 + \frac{e_j}{e_i}} + \frac{e_j}{1 + \frac{e_k}{e_i}}) \\
&= (\frac{2\beta}{3} - 1)\Delta e_i + \frac{\beta}{3}(\frac{e_k}{1 + \frac{e_j}{e_i + \Delta e_i}} - \frac{e_k}{1 + \frac{e_j}{e_i}} + \frac{e_j}{1 + \frac{e_k}{e_i + \Delta e_i}} - \frac{e_j}{1 + \frac{e_k}{e_i}})
\end{aligned}$$

The first bracket is greater than zero if and only if $\beta > \frac{3}{2}$; the second bracket is always greater than zero when $\Delta e_i > 0$.

$\square$

N-player case

We can extend Proposition 1 to N players, where $N > 3$. The game can be described as follows. In the first stage, each player, indexed i, has an initial endowment of $\bar{e}$ and takes an observable action $e_i \in E_i = (0, ..., \bar{e})$. The players' actions determine a joint monetary outcome $\Pi = \beta \sum_{i=1}^{n} e_i$, which must be allocated among the players. Let q_i stand for player i's share of the outcome Π, and each player i's payoff functrion is $\pi_i = \bar{e} - e_i + q_i \Pi$.

In the second stage, each player i will propose a fraction, a_{ij}, to each player j such that $a_{ii} = 0$, $a_{ij} \in [0, 1] \forall i \neq j$ and $\sum_{j \neq i} a_{ij} = 1$. The final share q_i that each player i receives is $q_i = \frac{\sum_{j \neq i} a_{ji}}{n}$. We construct a fair allocation rule in the context of the Galbraith Mechanism:

$$a_{ij}^* = \begin{cases} \frac{e_j}{\sum_{i=1}^{n} e_i - e_i} & \text{if } \sum_{i=1}^{n} e_i - e_i \neq 0 \\ \frac{1}{n-1} & \text{if } \sum_{i=1}^{n} e_i - e_i = 0 \end{cases} \tag{1}$$

Proposition 4 (n-player Galbraith Mechanism with Fair Allocation)**.** *For the n player case, supplose each player i allocates using the proportional rule outlined in Equation 1 in the allocation stage, the strategy profile in which $e_i = \bar{e}$ for each i is the dominant strategy Nash equilibrium in the first stage if and only if $\beta > \frac{n}{n-1}$.*

Proof. In the first stage, $e_i = \bar{e}$ is a dominant strategy if and only if $\pi_i(\bar{e}, e_{-i}) > \pi_i(e_i, e_{-i})$, $\forall e_{-i}$. It suffices to prove $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$,$\forall e_{-i}$ where $\Delta e_i > 0$. We consider the following three cases.

1. Suppose $\sum_{j \in N \setminus \{i\}} e_j = 0$. If player i chooses to contribute, $\forall j \in N \setminus \{i\}$ will give player i 1 in the allocation stage. Therefore, $\sum_{j \in N \setminus \{i\}} a_{ji} = n - 1$ and $\pi_i(e_i, e_{-i}) = \bar{e} - e_i + \frac{\beta e_i}{n}(n-1)$. If player i chooses not to contribute, then $\pi_i(0, e_{-i}) = \bar{e}$. Note, $\beta > \frac{n}{n-1}$ is a

sufficient condition to make $\pi_i(e_i, e_{-i}) > \pi_i(0, e_{-i})$. Furthermore, when $e_i > 0$, we have $\pi_i(e_i + \Delta e_i, e_{-i}) = \bar{e} + (\frac{(n-1)\beta}{n} - 1)(e_i + \Delta e_i)$. Therefore, $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$ if and only if $\beta > \frac{n}{n-1}$.

2. Suppose $\forall k \in N \backslash \{i\}, \exists e_k \neq 0$ and $\forall j \in N \backslash \{i, k\}, e_j = 0$. That is, there is only one player who contributes, say player k. Now if player i chooses to contribute, player k would give player i $a_{ki} = 1$ and the remaining $(n-2)$ players will give player i $\frac{e_i}{e_i+e_k}$. Therefore, $\sum_{j \in N \backslash \{i\}} a_{ji} = 1 + \frac{e_i}{e_i+e_k}(n-2)$ and $\pi_i(e_i, e_{-i}) = \bar{e} - e_i + \frac{\beta}{n}(e_i + e_k)\left(1 + \frac{e_i}{e_i+e_k}(n-2)\right)$. Next suppose that player i does not contribute, that is, $e_i = 0$. Then only player k will give him $\frac{1}{n-1}$ and the remaining players will give him zero. Therefore, $\sum_{j \in N \backslash \{i\}} a_{ji} = \frac{1}{n-1}$ and $\pi_i(0, e_{-i}) = \bar{e} + \frac{\beta e_k}{n(n-1)}$. Note, $\beta > \frac{n}{n-1}$ would be a sufficient condition to make player i contributes, i.e., $\pi_i(e_i, e_{-i}) > \pi_i(0, e_{-i})$. Furthermore, when $e_i > 0$, we have $\pi_i(e_i + \Delta e_i, e_{-i}) - \pi_i(e_i, e_{-i}) = \left(\frac{(n-1)\beta}{n} - 1\right) \Delta e_i$. Therefore, $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$ if and only if $\beta > \frac{n}{n-1}$.

3. Suppse $\forall k \in N \backslash \{i\}, \exists e_k \neq 0$ and $\sum_{j \in N \backslash \{i,k\}} e_j \neq 0$. That is, there are at least two players who contribute, say player j^* and player k, contribute. If player i chooses to contribute, $e_i > 0$, all the other players, $j \in N \backslash \{i\}$ will give player i $\frac{a_{ji}}{\sum_{i=1}^n e_i - e_j}$. Therefore $\sum_{j \in N \backslash \{i\}} a_{ji} = \sum_{j \in N \backslash \{i\}} \left(\frac{a_{ji}}{\sum_{i=1}^n e_i - e_j}\right)$ and $\pi_i(e_i, e_{-i}) = \bar{e} - e_i + \frac{\beta}{n}(\sum_{i=1}^n e_i)(\sum_{j \in N \backslash \{i\}} \frac{e_i}{\sum_{i=1}^n e_i - e_j})$. We next re-write $\pi_i(e_i, e_{-i})$ for the ease of calculation, and then prove $\pi_i(e_i + \Delta e_i, e_{-i}) - \pi_i(e_i, e_{-i}) > 0$:

$$
\begin{aligned}
\pi_i(e_i, e_{-i}) &= \bar{e} - e_i + \frac{\beta}{n}(\sum_{i=1}^{n} e_i)(\sum_{j\in N\setminus\{i\}} \frac{e_i}{\sum_{i=1}^{n} e_i - e_j}) \\
&= \bar{e} - e_i + \frac{\beta}{n}(\sum_{j\in N\setminus\{i\}} \frac{e_i(\sum_{i=1}^{n} e_i)}{\sum_{i=1}^{n} e_i - e_j}) \\
&= \bar{e} - e_i + \frac{\beta}{n}\sum_{j\in N\setminus\{i\}} \left\{\frac{e_i(\sum_{i=1}^{n} e_i - e_j) + e_i e_j}{\sum_{i=1}^{n} e_i - e_j}\right\} \\
&= \bar{e} - e_i + \frac{\beta}{n}\sum_{j\in N\setminus\{i\}} \left\{e_i + \frac{e_i e_j}{\sum_{i=1}^{n} e_i - e_j}\right\} \\
&= \bar{e} - e_i + \frac{\beta e_i}{n}\sum_{j\in N\setminus\{i\}} \left\{1 + \frac{e_j}{\sum_{i=1}^{n} e_i - e_j}\right\} \\
&= \bar{e} - e_i + \frac{\beta e_i}{n}\left\{(n-1) + \sum_{j\in N\setminus\{i\}} \left(\frac{e_j}{\sum_{i=1}^{n} e_i - e_j}\right)\right\} \\
&= \bar{e} + \left(\frac{(n-1)\beta}{n} - 1\right) e_i + \frac{\beta e_i}{n}\sum_{j\in N\setminus\{i\}} \left(\frac{e_j}{\sum_{i=1}^{n} e_i - e_j}\right)
\end{aligned}
$$

Thus,

$$
\begin{aligned}
&\pi_i(e_i + \Delta e_i, e_{-i}) - \pi_i(e_i, e_{-i}) \\
&= \frac{(n-1)\beta - n}{n}\Delta e_i + \frac{\beta(e_i + \Delta e_i)}{n}\sum_{j\neq i}^{n-1}\left(\frac{e_j}{\sum_{i=1}^{n} e_i + \Delta e_i - e_j}\right) - \frac{\beta e_i}{n}\sum_{j\neq i}^{n-1}\left(\frac{e_j}{\sum_{i=1}^{n} e_i - e_j}\right) \\
&= \frac{(n-1)\beta - n}{n}\Delta e_i + \frac{\beta}{n}\sum_{j\neq i}^{n-1}\left(\frac{e_j}{1 + \frac{\sum_{i=1}^{n} e_i - e_i - e_j}{e_i + \Delta e_i}}\right) - \frac{\beta}{n}\sum_{j\neq i}^{n-1}\left(\frac{e_j}{1 + \frac{\sum_{i=1}^{n} e_i - e_i - e_j}{e_i}}\right) \\
&= \frac{(n-1)\beta - n}{n}\Delta e_i + \frac{\beta}{n}\sum_{j\neq i}^{n-1}\left(\frac{e_j}{1 + \frac{\sum_{i=1}^{n} e_i - e_i - e_j}{e_i + \Delta e_i}} - \frac{e_j}{1 + \frac{\sum_{i=1}^{n} e_i - e_i - e_j}{e_i}}\right)
\end{aligned}
$$

The first term of the above equation is greater than zero if and only if $\beta > \frac{n}{n-1}$. The second bracket is always greater than zero when $\Delta e_i > 0$.

□

Appendix A1.2. Proof for Proposition 2

Proof. The proof of Proposition 2 will follow the steps of Proposition 1.

1. Suppose $e_j = e_k = 0$. Then the argument follows exactly as in Proposition 1 for this case and $\pi_i(e_i + \Delta e_i, e_{-i}) > \pi_i(e_i, e_{-i})$ if and only if $\beta > \frac{3}{2}$. If $\beta < \frac{3}{2}$, player i will not contribute.

2. Suppose $e_j = 0$ and $e_k > 0$. There are four cases to consider:

   (a) If $e_i = 0$, then $a^l_{ji} = 0$ and $a^l_{ki} = \frac{1}{2}$, therefore $a^l_{ji} + a^l_{ki} = \frac{1}{2}$ and $\pi_i(0, e_{-i}) = \bar{e} + \frac{\beta}{6} e_k$.

   (b) If $0 < e^1_i < e_k$, then $a^l_{ji} = 0$ and $a^l_{ki} = 1$, therefore $a^l_{ji} + a^l_{ki} = 1$ and $\pi_i(e^1_i, e_{-i}) = \bar{e} - e^1_i + \frac{\beta}{3}(e^1_i + e_k)$

   (c) If $0 < e^2_i = e_k$, then $a^l_{ji} = \frac{1}{2}$ and $a^l_{ki} = 1$, therefore $a^l_{ji} + a^l_{ki} = \frac{3}{2}$ and $\pi_i(e^2_i, e_{-i}) = \bar{e} - e^2_i + \frac{\beta}{2}(e^2_i + e_k)$

   (d) If $0 < e_k < e^3_i$, then $a^l_{ji} = 1$ and $a^l_{ki} = 1$, therefore $a^l_{ji} + a^l_{ki} = 2$ and $\pi_i(e^3_i, e_{-i}) = \bar{e} - e^3_i + \frac{2\beta}{3}(e^3_i + e_k)$

   We have the following:

   - $\pi_i(e^3_i, e_{-i}) - \pi_i(0, e_{-i}) = (\frac{2\beta}{3} - 1)e^3_i + \frac{\beta}{2} e_k$
   - $\pi_i(e^3_i, e_{-i}) - \pi_i(e^1_i, e_{-i}) = (\frac{2\beta}{3} - 1)(e^3_i - e^1_i) + \frac{\beta}{3}(e^1_i + e_k)$
   - $\pi_i(e^3_i, e_{-i}) - \pi_i(e^2_i, e_{-i}) = (\frac{2\beta}{3} - 1)(e^3_i - e^2_i) + \frac{\beta}{6}(e^2_i + e_k)$

   So $\beta > \frac{3}{2}$ is sufficient for e^3_i to be the preferred option and $\pi_i(e^3_i + \Delta e_i, e_{-i}) - \pi_i(e^3_i, e_{-i}) > 0$. That is $\beta > \frac{3}{2}$ is sufficient for the maximum contribution to be the best response.

   What if $\frac{12}{11} < \beta < \frac{3}{2}$? We set $e^*_i = e_k + \Delta e_i$, we have the following:

   - $\pi_i(e^*_i, e_{-i}) - \pi_i(0, e_{-i}) = (\frac{7\beta}{6} - 1)e_k + (\frac{2\beta}{3} - 1)\Delta e_i$
   - $\pi_i(e^*_i, e_{-i}) - \pi_i(e^1_i, e_{-i}) = (\beta - 1)e_k + (1 - \frac{\beta}{3})e^1_i + (\frac{2\beta}{3} - 1)\Delta e_i$

- $\pi_i(e_i^*, e_{-i}) - \pi_i(e_i^2, e_{-i}) = (\frac{5\beta}{6} - 1)e_k + (1 - \frac{\beta}{2})e_i^2 + (\frac{2\beta}{3} - 1)\Delta e_i = \frac{\beta}{3}e_k + (\frac{2\beta}{3} - 1)\Delta e_i$

When $\frac{12}{11} < \beta < \frac{3}{2}$, because $e_k \geq 1$ and $\Delta e_i = 1$ in our model, we find $\pi_i(e_i^*, e_{-i})$ is the preferred option and $\pi_i(e_i^* + \Delta e_i, e_{-i}) - \pi_i(e_i^*, e_{-i}) < 0$. That is, contributing more than the second largest contributor by the minimal amount, in our case $\Delta e_i = 1$, is the best response.

3. Suppose $0 < e_j < e_k \leq \bar{e}$. There are five cases to consider:

   (a) If $0 \leq e_i^1 < e_j \leq e_k$, then $a_{ji}^l = 0$ and $a_{ki}^l = 0$, therefore $a_{ji}^l + a_{ki}^l = 0$ and $\pi_i(e_i^1, e_{-i}) = \bar{e} - e_i^1 < \pi_i(0, e_{-i}) = \bar{e}$.

   (b) If $0 < e_i^2 = e_j < e_k$, then $a_{ji}^l = 0$ and $a_{ki}^l = \frac{1}{2}$, therefore $a_{ji}^l + a_{ki}^l = \frac{1}{2}$ and $\pi_i(e_i^2, e_{-i}) = \bar{e} - e_i^2 + \frac{\beta}{6}(e_i^2 + e_k + e_j)$.

   (c) If $0 < e_j < e_i^3 < e_k$, then $a_{ji}^l = 0$ and $a_{ki}^l = 1$, therefore $a_{ji}^l + a_{ki}^l = 1$ and $\pi_i(e_i^3, e_{-i}) = \bar{e} - e_i^3 + \frac{\beta}{3}(e_i^3 + e_k + e_j)$.

   (d) If $0 < e_j < e_i^4 = e_k$, then $a_{ji}^l = \frac{1}{2}$ and $a_{ki}^l = 1$, therefore $a_{ji}^l + a_{ki}^l = \frac{3}{2}$ and $\pi_i(e_i^4, e_{-i}) = \bar{e} - e_i^4 + \frac{\beta}{2}(e_i^4 + e_k + e_j)$.

   (e) If $0 < e_j \leq e_k < e_i^5$, then $a_{ji}^l = 1$ and $a_{ki}^l = 1$, therefore $a_{ji}^l + a_{ki}^l = 2$ and $\pi_i(e_i^5, e_{-i}) = \bar{e} - e_i^5 + \frac{2\beta}{3}(e_i^5 + e_j + e_k)$.

We have the following:

- $\pi_i(e_i^5, e_{-i}) - \pi_i(0, e_{-i}) = (\frac{2\beta}{3} - 1)e_i^5 + \frac{2\beta}{3}(e_j + e_k)$
- $\pi_i(e_i^5, e_{-i}) - \pi_i(e_i^2, e_{-i}) = (\frac{2\beta}{3} - 1)(e_i^5 - e_i^2) + \frac{1}{2}(e_i^2 + e_j + e_k)$
- $\pi_i(e_i^5, e_{-i}) - \pi_i(e_i^3, e_{-i}) = (\frac{2\beta}{3} - 1)(e_i^5 - e_i^3) + \frac{1}{3}(e_i^3 + e_j + e_k)$
- $\pi_i(e_i^5, e_{-i}) - \pi_i(e_i^4, e_{-i}) = (\frac{2\beta}{3} - 1)(e_i^5 - e_i^4) + \frac{1}{6}(e_i^4 + e_j + e_k)$

So $\beta > \frac{3}{2}$ is sufficient for e_i^5 to be the preferred option and $\pi_i(e_i^5 + \Delta e_i, e_{-i}) - \pi_i(e_i^5, e_{-i}) > 0$. That is $\beta > \frac{3}{2}$ is sufficient for the maximum contribution to be the best response.

What if $\frac{12}{11} < \beta < \frac{3}{2}$? We set $e_i^* = e_k + \Delta e_i$, we have the following:

- $\pi_i(e_i^*, e_{-i}) - \pi_i(0, e_{-i}) = (\frac{4}{3}\beta - 1)e_k + (\frac{2\beta}{3} - 1)\Delta e_i$
- $\pi_i(e_i^*, e_{-i}) - \pi_i(e_i^2, e_{-i}) = (\frac{7\beta}{6} - 1)e_k + \frac{\beta}{2}e_j + (1 - \frac{\beta}{6})e_i^2 + (\frac{2\beta}{3} - 1)\Delta e_i$
  $= (\frac{7\beta}{6} - 1)e_k + (\frac{\beta}{3} + 1)e_j + (\frac{2\beta}{3} - 1)\Delta e_i$
- $\pi_i(e_i^*, e_{-i}) - \pi_i(e_i^3, e_{-i}) = (\beta - 1)e_k + \frac{\beta}{3}e_j + (1 - \frac{\beta}{3})e_i^3 + (\frac{2\beta}{3} - 1)\Delta e_i$
- $\pi_i(e_i^*, e_{-i}) - \pi_i(e_i^4, e_{-i}) = (\frac{5\beta}{6} - 1)e_k + \frac{\beta}{6}e_j + (1 - \frac{\beta}{2})e_i^2 + (\frac{2\beta}{3} - 1)\Delta e_i$
  $= \frac{\beta}{3}e_k + \frac{\beta}{6}e_j + (\frac{2\beta}{3} - 1)\Delta e_i$

When $\frac{12}{11} < \beta < \frac{3}{2}$, because $e_j \geq 1$, $e_k \geq 1$ and $\Delta e_i = 1$ in our model, we find $e_i^* = e_k + \Delta e_i$ is the preferred option and $\pi_i(e_i^* + \Delta e_i, e_{-i}) - \pi_i(e_i^*, e_{-i}) < 0$. That is, contributing more than the second largest contributor by the minimal amount, in our case $\Delta e_i = 1$, is the best response.

4. Suppose $0 < e_j = e_k < \bar{e}$. There are two cases to consider:

   (a) If $0 < e_j = e_k = e_i^1 < \bar{e}$, then $a_{ji}^l = \frac{1}{2}$ and $a_{ki}^l = \frac{1}{2}$, therefore $a_{ji}^l + a_{ki}^l = 1$ and $\pi_i(e_i^1, e_{-i}) = \bar{e} - e_i^1 + \beta e_i^1$.

   (b) If $0 < e_j = e_k < e_i^2 \leq \bar{e}$, then $a_{ji}^l = 1$ and $a_{ki}^l = 1$, therefore $a_{ji}^l + a_{ki}^l = 2$ and $\pi_i(e_i^2, e_{-i}) = \bar{e} - e_i^2 + \frac{2\beta}{3}(e_i^2 + e_k + e_j)$.

   We set $e_i^2 = e_i^1 + \Delta e_i = e_k + \Delta e_i$, then $\pi_i(e_i^2, e_{-i}) = \bar{e} - e_i^1 + 2\beta e_i^1 + (\frac{2\beta}{3} - 1)\Delta e_i$ and $\pi_i(e_i^2, e_{-i}) - \pi_i(e_i^1, e_{-i}) = \beta e_i^1 + (\frac{2\beta}{3} - 1)\Delta e_i$.

   So $\beta > \frac{3}{2}$ is sufficient for the maximum contribution to be the best response.

   When $\frac{12}{11} < \beta < \frac{3}{2}$, because $e_i^1 \geq 1$ and $\Delta e_i = 1$ in our model, we find $\pi_i(e_i^2, e_{-i}) > \pi_i(e_i^1, e_{-i})$ and $\pi_i(e_i^2 + \Delta e_i, e_{-i}) - \pi_i(e_i^2, e_{-i}) < 0$. That is, contributing more than the second largest contributor by the minimal amount, in our case $\Delta e_i = 1$, is the best response.

5. Suppose $e_j = e_k = \bar{e}$. If i contributes less than $\bar{e}$, then $a_{ji}^l + a_{ki}^l = 0$ and i is better off not contributing at all. If i contributes $\bar{e}$, then $a_{ji}^l + a_{ki}^l = 1$ and $\pi_i(\bar{e}, 2\bar{e}) = \beta\bar{e} > \pi_i(0, 2\bar{e}) = \bar{e}$. The best response for i is to also contribute $\bar{e}$.

$\square$

Appendix A1.3. Proof for Proposition 3

Proof. First observe that $b_{ji}(\bar{e},\bar{e},\bar{e}) = \frac{1}{2} < 1$. Thus $a_{ji} = b_{ji}$ for all i and $j \neq i$. $\pi_i(\bar{e},\bar{e},\bar{e}) = \beta\bar{e}$. Now consider some $e_i < \bar{e}$. It can be shown that $b_{ji}(e_i,\bar{e},\bar{e}) < 1$ since $\frac{\gamma e_j + (1-\gamma)\bar{e}}{e_j+\bar{e}} < 1$. If e_i is such that $b_{ji}(e_i,\bar{e},\bar{e}) \leq 0$, we have $a_{ji} = 0$ for all i and $j \neq i$ and hence, $\pi_i(e_i,\bar{e},\bar{e}) = \bar{e} - e_i < \pi_i(\bar{e},\bar{e},\bar{e})$. The only other case to consider is when $a_{ji} = b_{ji}(e_i,\bar{e},\bar{e})$. Then, we have

$$\pi_i(e_i,\bar{e},\bar{e}) = (\bar{e} - e_i) + \frac{2}{3}\beta\left[\frac{\gamma e_i + (1-\gamma)\bar{e}}{e_i + \bar{e}}\right](e_i + 2\bar{e})$$

Thus, $\pi_i(e_i,\bar{e},\bar{e}) < \pi_i(\bar{e},\bar{e},\bar{e})$ iff

$$\frac{2}{3}\beta\gamma e_i^2 + \frac{2}{3}\beta(1-\gamma)\bar{e}e_i + \frac{4}{3}\beta\gamma\bar{e}e_i + \frac{4}{3}\beta(1-\gamma)\bar{e}^2 - e_i^2 - \bar{e}e_i < (\beta-1)\bar{e}e_i + (\beta-1)\bar{e}^2$$

Therefore, $\pi_i(e_i,\bar{e},\bar{e}) < \pi_i(\bar{e},\bar{e},\bar{e})$ iff

$$(\frac{2}{3}\beta\gamma - 1)e_i^2 + (\frac{2}{3}\beta\gamma + \frac{1}{3}\beta)e_i\bar{e} < (\frac{4}{3}\beta\gamma - \frac{1}{3}\beta - 1)\bar{e}^2$$

Now since $e_i < \bar{e}$ and $\beta > \frac{3}{2\gamma}$, there exists some $\epsilon > 0$ such that $\pi_i(e_i,\bar{e},\bar{e}) < \pi_i(\bar{e},\bar{e},\bar{e})$ iff

$$(\frac{4}{3}\beta\gamma + \frac{1}{3}\beta - 1)\bar{e}^2 - \epsilon < (\frac{4}{3}\beta\gamma - \frac{1}{3}\beta - 1)\bar{e}^2$$

This completes the proof. $\square$

Appendix A2. Experimental Instructions

We present the experimental instructions for the experiment treatment (Sequence 1 is for the equal sharing rule and sequence 2 is for the Galbraith Mechanism). Participants receive printed copies of the instructions and the experimenter read it aloud in each session. Sequence 2 instruction is distributed only after the completion of sequence 1 decisions. Samples of screenshots are also included. The accompanied quiz questions and z-Tree program are available upon request.

SEQUENCE 1 (Decision round 1-10)

Welcome! You are taking part in a decision making experiment. Now that the experiment has begun, we ask that you do not talk. The instructions are simple. If you follow them carefully and make good decisions, you can earn a considerable amount of money. If you have questions after we finish reading the instructions, please raise your hand and one of the experimenters will approach you and answer your questions in private. This experiment consists of two sequences of decision rounds. Each sequence contains ten rounds. In each round, you will be in a group with two other people, but you will not know which of the other two people in this room are in your group. The people in your group will change from round to round, and in particular you will never be matched with the same set of two other participants twice during the whole experiment.

The decisions made by you and the other people in your group will determine your earnings in that round. Your earnings in this experiment are expressed in experimental currency units, which we will refer to as ECUs. At the end of the experiment you will be paid in cash using a conversion rate of Âč1 of every 30 ECUs of earnings from the experiment. Under no circumstance will we expose your identity. In other words, your decisions and earnings will remain anonymous with us. This set of instructions details Sequence 1. An additional set of instructions detailing sequence 2 will be provided after sequence 1 is completed.

Sequence 1 consists of ten decision rounds. At the beginning of each round, you will be randomly allocated a participant identification letter, either A, B, or C. (Thus, your identification letter may change from round to round).

Decision Task in Each Round Each individual begins each round with an endowment of 10 tokens in their Individual Fund. Tokens in Individual Fund worth 1 ECU each. Each three-person group begins with a Group Fund of 0 ECUs each round. Each person will decide independently and privately whether or not to contribute any of his/her tokens from his/her own Individual Fund into the Group Fund. Tokens in the Group Fund worth 1.8 ECU each. Each person can contribute up to a maximum of 10 tokens to the Group Fund. Decisions must be made in whole tokens. That is, each person can add 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10 tokens to the Group Fund.

Feedback and Earnings After all participants have made their decisions for the round, the computer will tabulate the results. ECUs in Group Fund = 1.8 $\times$ (Sum of tokens in the Group Fund). ECUs in the Group Fund will be divided equally among all individuals in the group. That is, each group member will receive one-third of ECUs in the Group Fund. Your earning in one round equals ECUs in your Individual Fund plus one-third of ECUs in the Group Fund. Your Earnings =ECUs in Individual Fund + $\frac{1}{3}$ ECUs in Group Fund. At the end of each round, you will receive information on your Group Fund earnings and your total earnings for that round. You will also be informed of all group members' contribution to the Group Fund and their earnings in ECUs. Total Earnings for the experiment will be the sum of the earnings in all rounds of the experiment. This completes the instructions for Sequence 1.Before we begin the experiment, to make sure that every participant understands the instructions, please answer several review questions on your screen.

SEQUENCE 2 (Decision round 11-20)

Sequence 2 consists of ten decision rounds. In each round, you will be in a group with two other people, but you will not know which of the other two people in this room are in your group. The people in your group will change from round to round, and in particular you will never be matched with the same set of two other participants twice during the whole experiment. At the beginning of each round, you will be randomly allocated a participant identification letter, either A, B, or C. (Thus, your identification letter may change from round to round).

<u>Decision Task in Each Round</u> Each individual begins each round with an endowment of 10 tokens in their Individual Fund. Tokens in Individual Fund worth 1 ECU each. Each decision round will have two phases.

<u>Phase 1: Decision Choice</u> Decision choice will be the same as in Sequence 1. Each three-person group begins with a Group Fund of 0 ECUs each round. Each person will decide independently and privately whether or not to contribute any of his/her tokens from his/her own Individual Fund into the Group Fund. Tokens in Group Fund worth 1.8 ECU each. Each person can contribute up to a maximum of 10 tokens to the Group Fund. Decisions must be made in whole tokens. That is, each person can add 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10 tokens to the Group Fund.

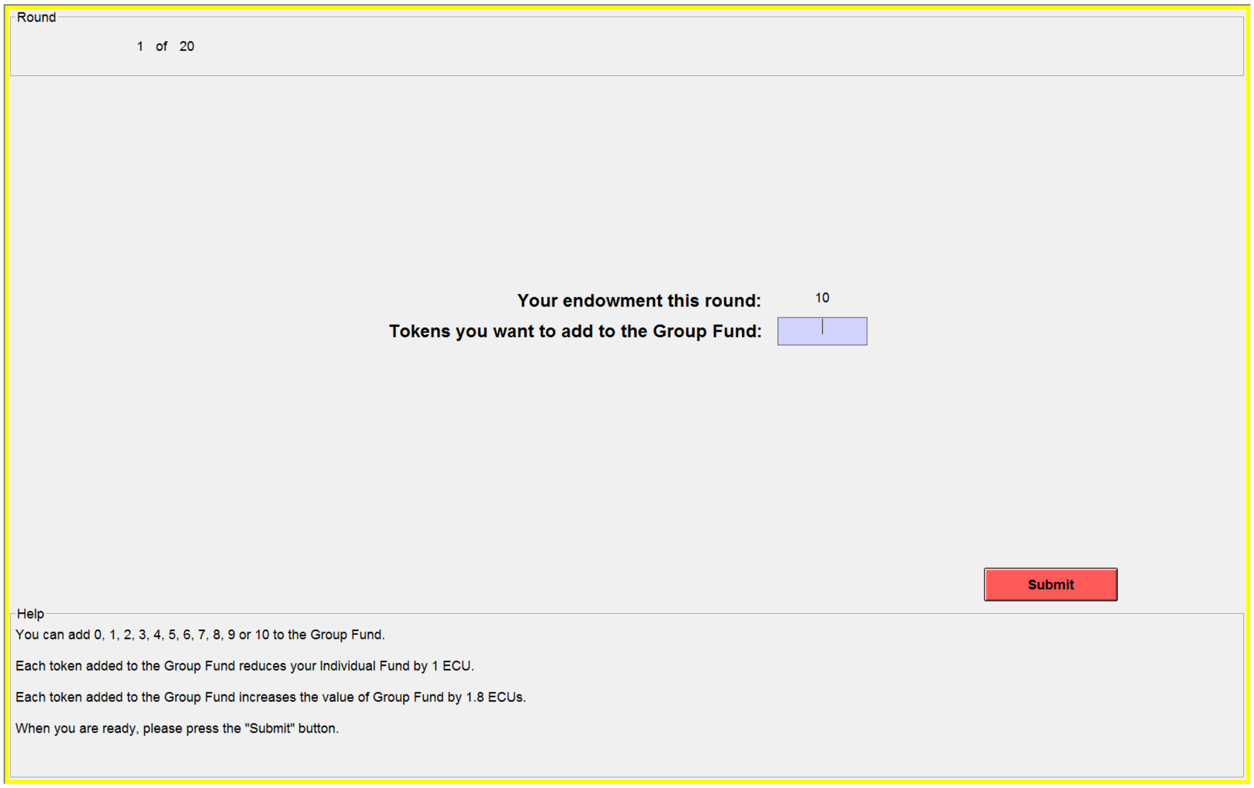
<u>Phase 2: Allocation Choice</u> After all individuals have made their decisions in Phase 1, you will be informed of the other two group members' contribution to the Group Fund, the total number of tokens and ECUs in the Group Fund. ECUs in Group Fund = 1.8 $\times$ (Sum of tokens in the Group Fund). You decide how to allocate one-third of the ECUs in the Group Fund between the other two group members.In other words, the sum of your allocation between the other two group members will be one-third of ECUs in the Group Fund. Each person can only divide one-third of ECUs in the Group Fund for the other two group members, their own share of the Group Fund will be determined by the allocation decisions of the other two group members. Specifically, 1) Person A will divide one-third of ECUs in the Group Fund between Person B and Person C. 2) Person B will divide one-third of ECUs in the Group Fund between Person A and Person C. 3) Person C will divide one-

third of ECUs in the Group Fund between Person A and Person B. You may change your choice as often as you like. But once you click Submit, the decision will be final. Click the calculator button on the lower-right corner if you need the assistance of calculation.

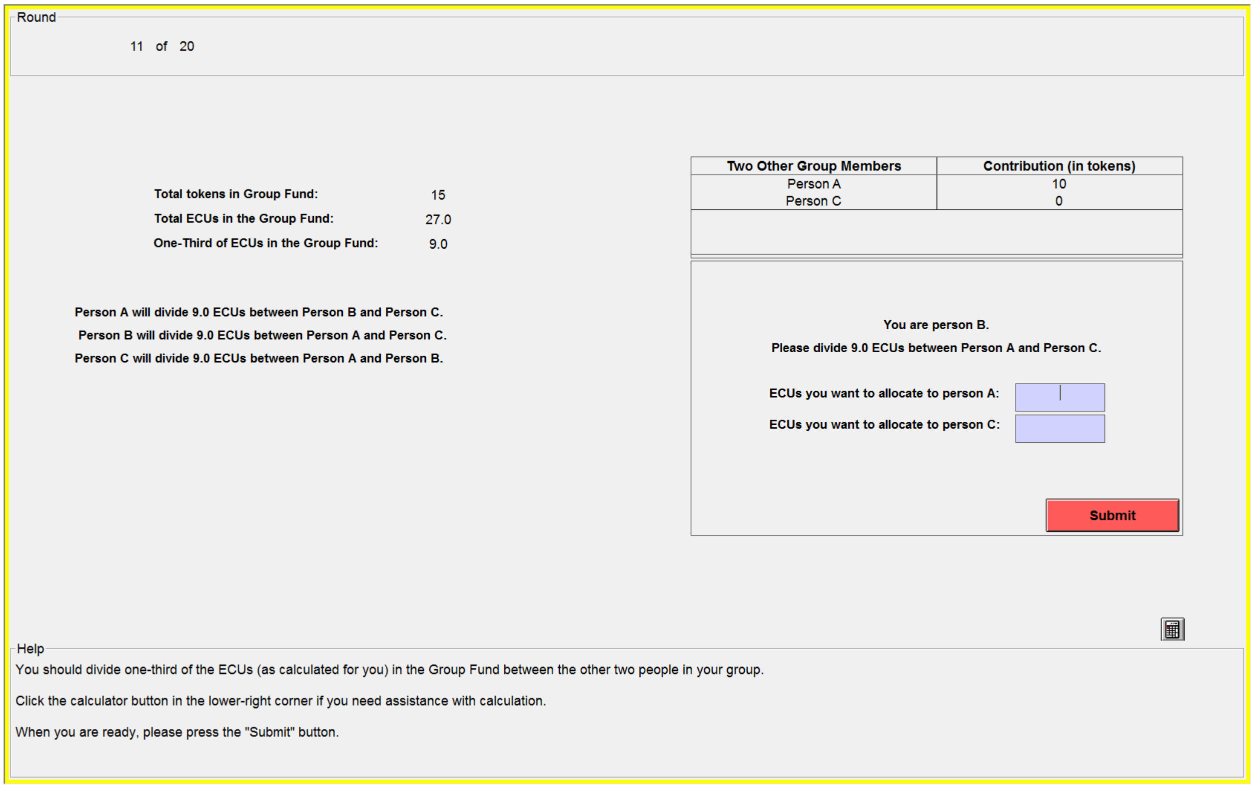
Feedback and Earnings After all individuals have made their decisions for the round, the computer will tabulate the results. A person's share of the Group Fund will be determined at the end of phase 2. His/her earnings from Group Fund will be the sum of ECUs that the other two group members allocate towards him/her. Your earnings in a round will equal ECUs in your Individual Fund plus ECUs the other two group members allocated to you (i.e., your share of ECUs in the Group Fund). At the end of each round, you will receive information on your Group Fund earnings and your total earnings for that round. You will also be informed of all group members' contribution to the Group Fund, their allocation decisions in phase 2 and their earnings in ECUs for that round. Total Earnings for the experiment will be the sum of the earnings in all rounds of the experiment. This completes the instructions. Before we resume the experiment, to make sure that every participant understands the instructions, please answer several review questions on your screen.

Screenshots

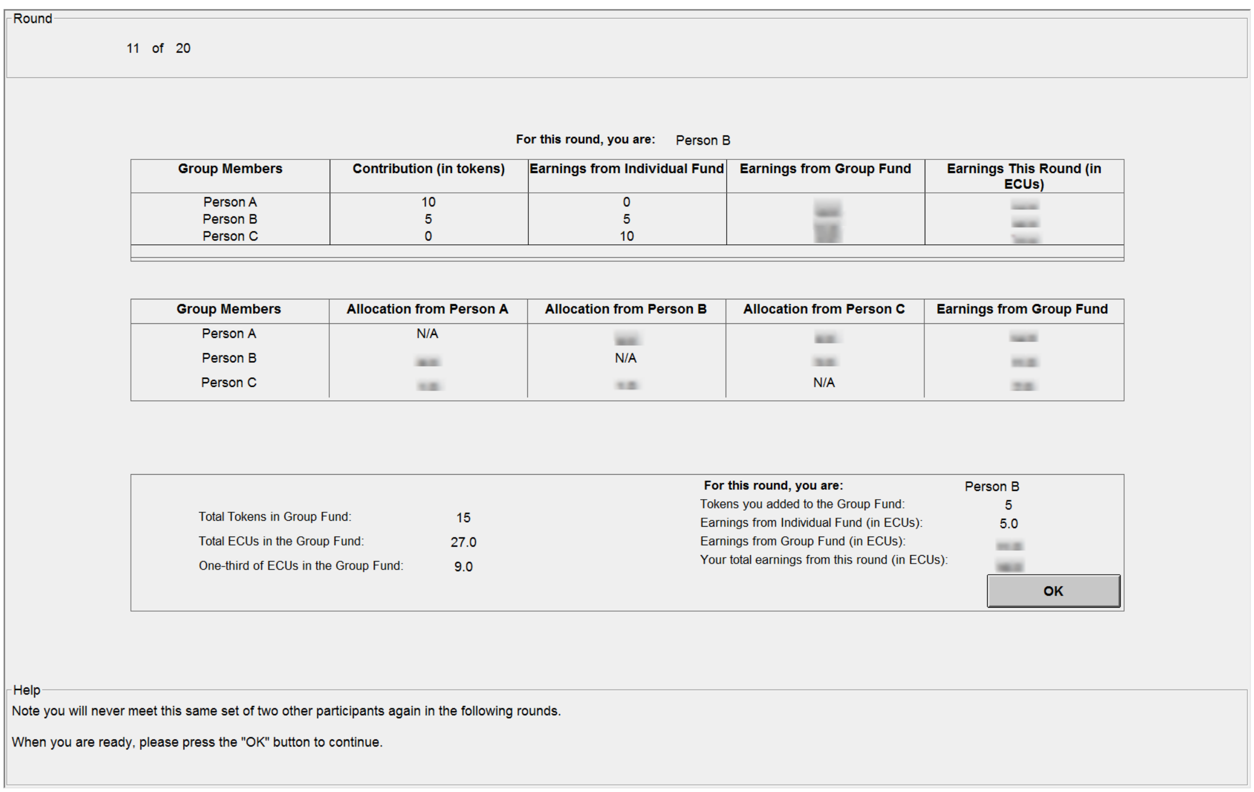
Contribution Decision



Allocation Decision



Feedback screen



Appendix A3. Further statistical analysis

This appendix provides additional statistics on players' contribution decisions. Figure A3.1 outlines the average contributions for each independent group from round 11 to 20.

	GM1.8		GM1.2		Equal Share	
Round	Mean	S.D.	Mean	S.D.	Mean	S.D.
11	5.17	3.56	4.03	3.45	1.33	2.98
12	6.18	3.22	4.68	3.13	0.44	1.18
13	7.29	2.98	5.39	3.45	0.14	0.54
14	8.06	2.75	5.69	3.49	0.14	0.68
15	8.59	2.28	6.16	3.44	0.22	1.05
16	8.94	2.14	6.11	3.36	0.33	1.69
17	8.97	2.45	6.24	3.42	0.03	0.17
18	8.93	2.44	6.13	3.54	0.00	0.00
19	9.11	2.23	6.38	3.51	0.00	0.00
20	9.17	2.20	6.42	3.60	0.06	0.33

	Alternative Hypotheses					
	Between treatments			Within treatments:		
Round	GM1.8> GM1.2	GM1.8> ES1.8	GM1.2> ES1.8	GM1.8> ES1.8	GM1.2> ES1.2	ES1.8 $\neq$ ES1.8
11	0.05	0.00	0.00	0.02	0.04	0.01
12	0.04	0.00	0.00	0.00	0.00	0.06
13	0.04	0.00	0.00	0.00	0.00	0.01
14	0.03	0.00	0.00	0.00	0.00	0.02
15	0.02	0.00	0.00	0.00	0.00	0.04
16	0.02	0.00	0.00	0.00	0.00	0.04
17	0.01	0.00	0.00	0.00	0.00	0.01
18	0.01	0.00	0.00	0.00	0.00	0.02
19	0.03	0.00	0.00	0.00	0.00	0.01
20	0.01	0.00	0.00	0.00	0.00	0.08

Notes: The upper panel of the table lists the average contributions and their standard deviations across three treatments from round 11 to 20, and the lower panel reports the hypotheses tests clustered at the session (independent group) level with the null hypothesis of equal contributions. The lower left panel shows the p-values of the ranksum tests between treatments comparison from round 11 to 20, while the lower right panel shows p-values of the signrank tests for the comparison within each treatment, for example round 11 is compared with round 1 in session T1.

A3.1: Summary of contributions in round 11-20

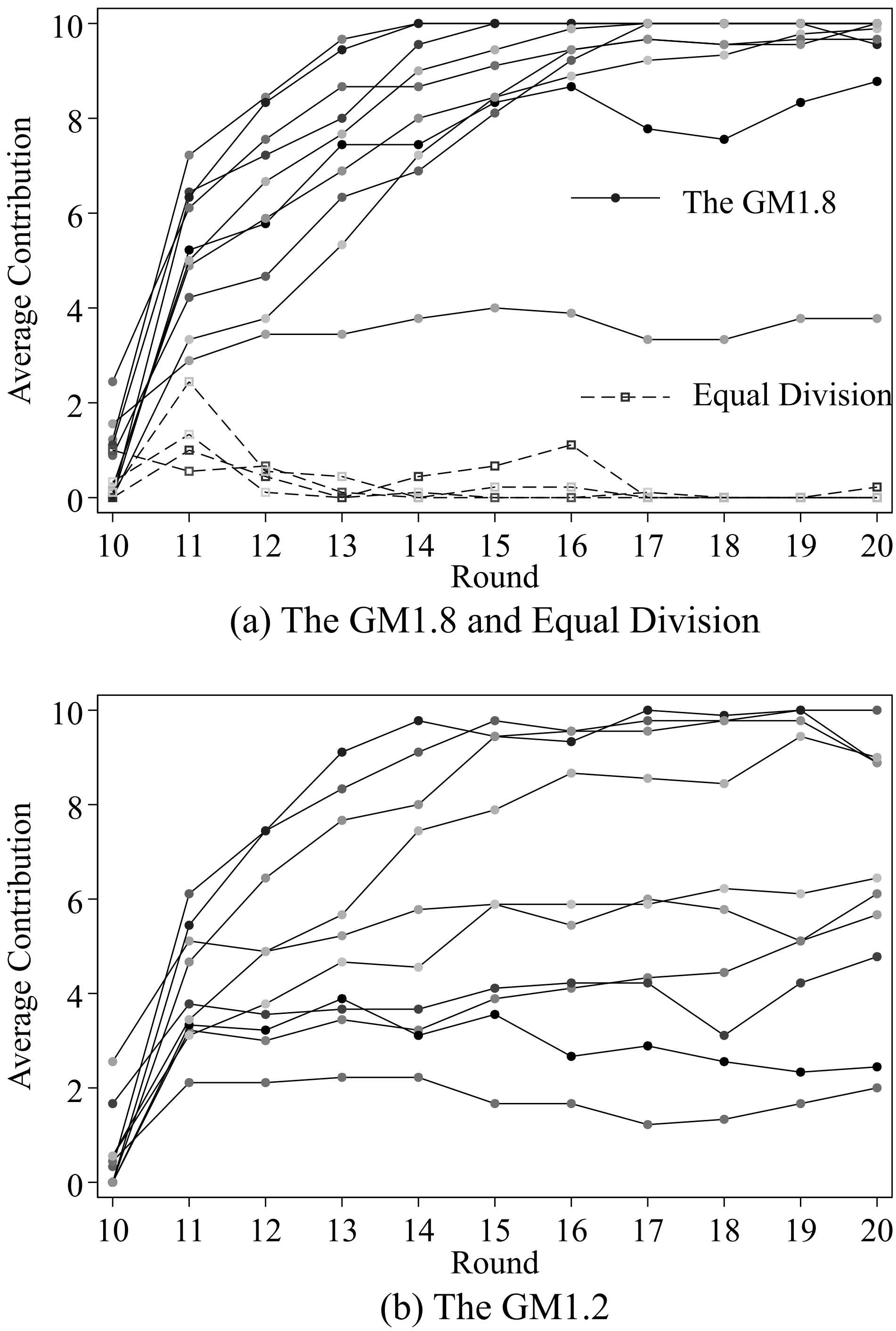


Figure A3.1 Average contributions in each nine-participant experimental session (independent group)

Appendix A4. Postexperimental Survey

Please answer the following survey questions. Your answers will be used for this study only. Individual data will not be exposed. (*summary statistics in italics*)

1. What is your age? (*Mean 20.67, Std Dev 1.99, Median 20, Min 18, Max 29*)

2. What is your gender? (*Male 35.65%, Female 64.35%*)

3. What is your faculty in the university? (*Arts 18.06%, Engineering 12.5%, Medicine and Health Sciences 11.11%, Science 19.44%, Social science 38.89%*)

4. In SEQUENCE 1, which of the following best describe your CONTRIBUTION decisions?

   - I minimize my contribution to the Group Fund to guarantee as much ECUs as possible in my Individual Fund. (*64.81%*)
   - I contribute to the Group Fund so that others can benefit from my contributions. (*21.30%*)
   - None of the above. (*13.89%*)

5. In SEQUENCE 2, which of the following best describe your CONTRIBUTION decisions?

   - I minimize my contribution to the Group Fund to guarantee as much ECUs as possible in my Individual Fund. (*8.33%*)
   - I contribute to the Group Fund with the belief that others will allocate a fair proportion of ECUs to me. (*86.11%*)
   - None of the above. (*5.56%*)

6. In SEQUENCE 2, which of the following best describe your ALLOCATION decisions?

- I allocate ECUs RANDOMLY between the other two group members. I am not concerned about the monetary payoff of my group members. (*5.00%*)
- I allocate ECUs EQUALLY between the other two group members. (*6.11%*)
- I allocate ECUs PROPORTIONALLY between the other two group members according to their contribution decisions. (*46.67%*)
- I allocate MOST ECUs to the one who contribute more and EQUALLY if they contribute equally. (*40.56%*)
- None of the above. (*1.67%*)

7. Bob and John are identical in terms of physical and mental abilities. They become shipwrecked on an uninhabited island where the only food is bananas. They can collect as many bananas as they want by climbing up a tree, picking them before they fall into the ocean and throwing them into a pile. In this way Bob picks 15 bananas per day and John picks 5 per day.

   - Bob takes from the pile the 15 bananas he picked leaving John with the 5 which John picked. (*Unfair 35.19%, Fair 64.81%*)
   - John takes 10 bananas from the pile leaving 10 for Bob (*Unfair 46.76%, Fair 53.24%*)

A4.1 summarizes the contribution decisions between genders and across faculties, and we find no evidence of significant difference.

Table A4.1 Contribution decisions between genders and across faculties

Gender	Round 1-10	Round 11-20	*Faculties*	Round 1-10	Round 11-20
Male (77)	1.54	5.56	Arts (39)	1.69	5.87
Female (139)	1.51	6.18	Engineering (27)	1.81	6.53
			Medicine (24)	1.50	5.08
			Science (42)	1.45	5.35
			Social science (84)	1.39	5.91

Alternative Hypotheses	p-value(Round 1-10)	p-value(Round 11-20)
Contribution (Male≠Female)	0.19	0.75
Contribution (Different across faculties)	0.32	0.24

Notes: Numbers of observations are in the parentheses. Data include all sessions of the experiment. The null hypotheses of the hypotheses tests are of equal contributions between genders or across faculties. We use ranksum tests for gender comparisons and Kruskal-Wallis equality tests for comparisons across faculties. Individuals' average contributions over 10 rounds of each sequence are used as independent observations.

B. Appendices for Chapter 2

Appendix B1. Experimental Instructions

We present the experimental instructions for the CostHet *treatment (Part 1 is the painting recognition task to form heterogeneous groups and part 2 is the Galbraith Mechanism). Part 2 is distributed only after the completion of part 1 decisions. Samples of screenshots are also included. Instructions for the other treatments are similar with certain modifications (the* HetHom *eliminates phase 2 in part 2; the* CostHom *part 1; the* FreeHom *eliminates part 1 and phase 2 in part 2). The accompanied quiz questions and z-Tree program are available upon request.*

Part 1

Welcome! You are taking part in a decision-making experiment.The amount of money you earn will depend upon the decisions you make and on the decisions other people make. Your earnings in this experiment are expressed in experimental currency units, which we will refer to as ECUs. This experiment has 2 parts and your total earnings will be the sum of your payoffs in each part. At the end of the experiment you will be paid in cash using a conversion rate of Âč1 for every 25 ECUs of earnings from the experiment. Everyone will be paid in private and you are under no obligation to tell others how much you earn. Please do not communicate with each other during the experiment. If you have a question, feel free to raise your hand, and an experimenter will come to help you. In Part 1 everyone will be shown 5 pairs of paintings by two artists (on the screen and on the prints). You will be asked to choose which painting in each pair you prefer. You will then be classified into groups of six people, based on your choice relative to other people's choice in this room. Then you will be asked to answer questions on two other paintings. Each correct answer will bring you 15 ECUs. The earnings will be shown at the end of this experiment.

An built-in chatting program will be available to you to get help from or help other members in your own group while answering the questions. All group members will be randomly assigned a group ID that will be only used in this chatting-box. Except for the following restrictions, you can type whatever you want in the lower box of the chat program. Messages will be shared only among all the members of your own group. You will not be able to see the messages exchanged among other groups. People in other groups will not be able to see the messages from your own group either. You will be given 10 minutes to communicate with your group members. Restrictions on messages:1) Please do not identify yourself or send any information that could be used to identify you (e.g., age, race, professional background, etc.) 2) Please refrain from using obscene or offensive language. After Part 1 has finished, we will give you instructions for part 2 of the experiment.

Part 2

Part 2 consists of 12 decision rounds. In each round, you will be in a group with two other people. You will not be able to identify which of the other people in this room are in your group, but you will know which painting groups they came from while you are making the decisions. The people in your group will change from round to round, and in particular you will never be matched with the same set of two other participants twice for the rest of the experiment. At the beginning of each round, you will be randomly allocated a participant identification letter, either A, B, or C (Thus, your identification letter may change from round to round). Each decision round has three phases.

<u>Phase 1: Decision Choice</u> Each individual begins each round with an endowment of 10 tokens in their Individual Fund. Tokens in Individual Fund are worth 1 ECU each. Each three-person group begins with a Group Fund of 0 ECUs each round. You decide independently and privately whether or not to contribute any of your tokens from your Individual Fund into the Group Fund. Tokens in Group Fund are worth 1.8 ECU each. In other words, each token that a person adds to the Group Fund reduces the value of his/her Individual Fund by 1 ECU. Each token added to the Group Fund by a group member increases the value of the Group Fund by 1.8 ECUs. Each person can contribute up to a maximum of 10 tokens to the Group Fund. Decisions must be made in whole tokens. That is, each person can add 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10 tokens to the Group Fund. Note that when you are making the contribution decision, you will know your group composition.

<u>Phase 2: Information Choice</u> After all participants have made their decisions for the round, the computer will tabulate the results: ECUs in Group Fund = 1.8 $\times$ (Sum of tokens in the Group Fund). You will be asked of whether or not to spend 0.5 ECU to reveal other two group members' individual contribution to the Group Fund. If you choose "Yes":1) 0.5 ECU will be deducted from your earnings; 2) Information about the other two group members' individual contributions to the group fund will available while making the allocation choice in Phase 3. If you choose "no": 1) no ECU will be deducted; 2) no information while making the allocation choice in Phase 3.

<u>Phase 3: Allocation Choice</u> You then decide how to allocate one-third of the ECUs in the Group Fund between the other two group members. The sum of your allocation between the other two group members will be one-third of ECUs in the Group Fund. In other words, each person can only divide one-third of ECUs in the Group Fund for the other two group members, and their own share of the Group Fund will be determined by the allocation decisions of the other two group members. Specifically, 1) Person A will divide one-third of ECUs in the Group Fund between Person B and Person C. 2) Person B will divide one-third of ECUs in the Group Fund between Person A and Person C. 3) Person C will divide one-third of ECUs in the Group Fund between Person A and Person B. If you chose "Yes" in Phase 2, the other two group members' individual contributions to the group fund and their painting groups will be shown on the upper right table when you are making allocation choices. If you chose "No" in Phase 2, the other two group members' individual contributions to the group fund remain unknown to you, but their painting groups will be shown on the upper right table. Click the calculator button on the lower-right corner if you need assistance with calculation.

Feedback and Earnings After all individuals have made their decisions for the round, the computer will tabulate the results. A person's share of the Group Fund will be determined at the end of phase 3. His/her earnings from Group Fund will be the sum of ECUs that the other two group members allocate towards him/her. Your earnings in a round will equal ECUs in your Individual Fund plus ECUs the other two group members allocated to you (i.e., your share of ECUs in the Group Fund). At the end of each round, you will receive information on your Group Fund earnings and your total earnings for that round. You will be informed of all group members' allocation decisions in phase 3. You will also see all group members' contribution to the group fund and their painting groups if you chose "Yes" in phase 2; if you chose "No" in phase 2, other group members' individual contributions to the group fund remain unknown to you. Total Earnings for the experiment will be the sum of the earnings in all rounds in part 2 plus your earnings from part 1. This completes the instructions. Before we begin the experiment, to make sure that every participant understands the instructions, please answer several review questions on your screen.

Screenshots

After reading the part 2 of the instructions, the subjects had to solve eight quiz questions on the screen. The questions included hypothetical combinations of group members' contribution and allocation decisions and the participants had to calculate the resulting payoffs. There were also True/False questions to check participants' understanding of the instructions. Af-

ter all participants completed the quiz questions the experiment began. At the beginning of each round, participants learn their group composition in the FreeHet *and the* CostHet. *Figure B1.1 shows an example of the screen for the contribution phase in round 1. After the contribution decisions, subjects were informed of each group members' painting group identity (in the* FreeHet *and the* CostHet*) and the total group fund to be allocated. They decided whether or not to spend money to reveal others' contribution decisions. Figure B1.2 shows an example of the information phase (in the* CostHom *and the* CostHet*). If the subject chose to buy the information, they would be informed about other two group members' contribution decisions. If they chose not to buy, they would never learn others' contribution decisions. Figure B1.3 shows an example of the allocation phase with other participants' contribution information. At the end of each round, participants were informed about each of their group members' contribution decisions, payoffs (only if they bought the information in the* CostHom *and the* CostHet*) and their allocation decisions. Figure B1.4 shows an example of the feedback screen.*

Figure B1.1 Contribution Phase

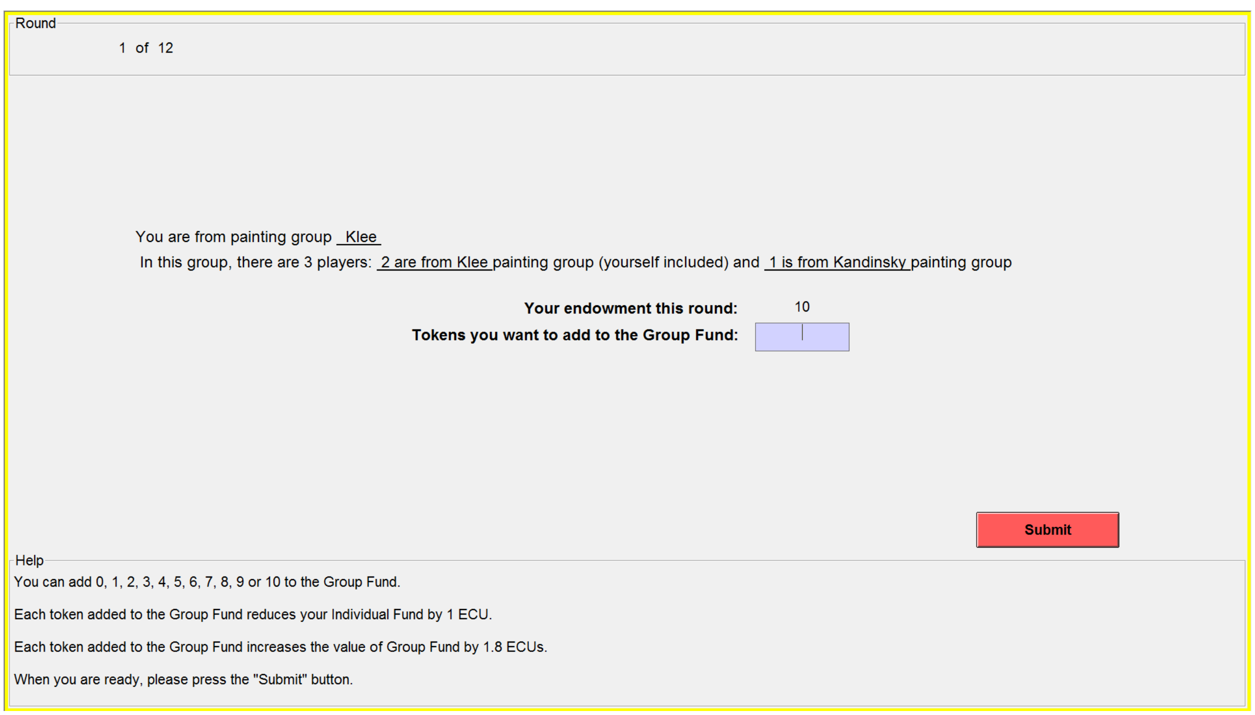


Figure B1.2 Information Phase

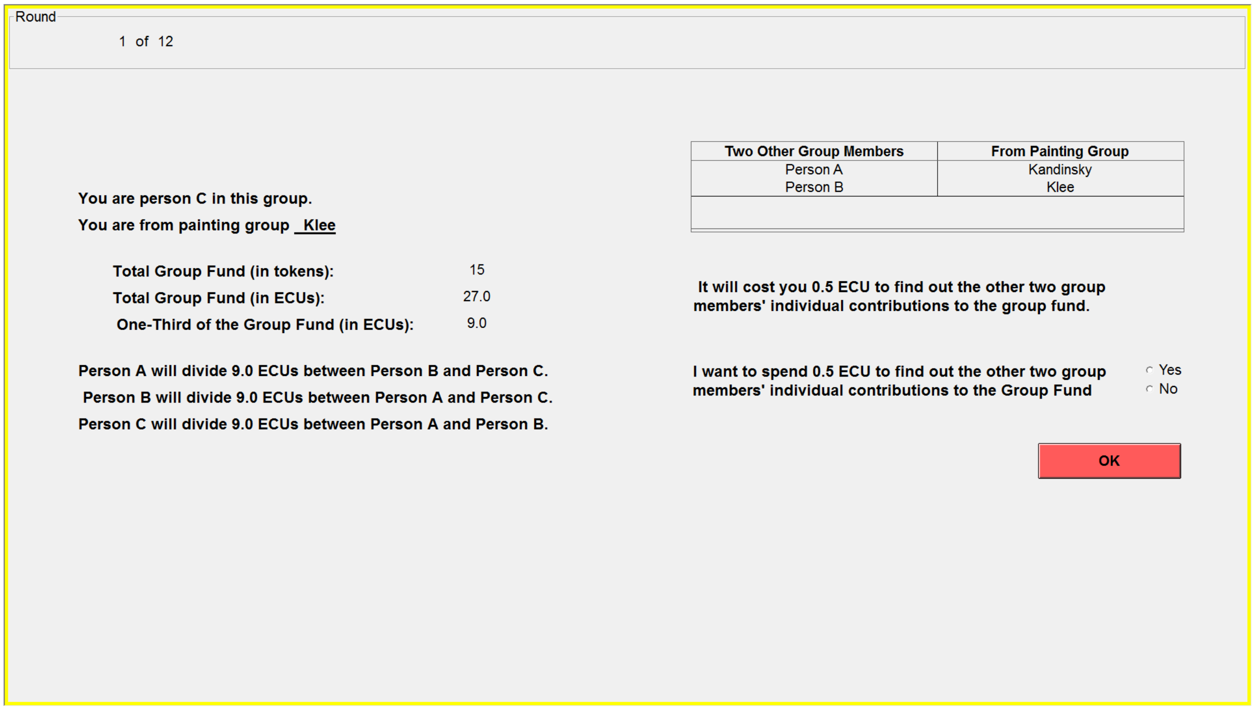


Figure B1.3 Allocation Phase

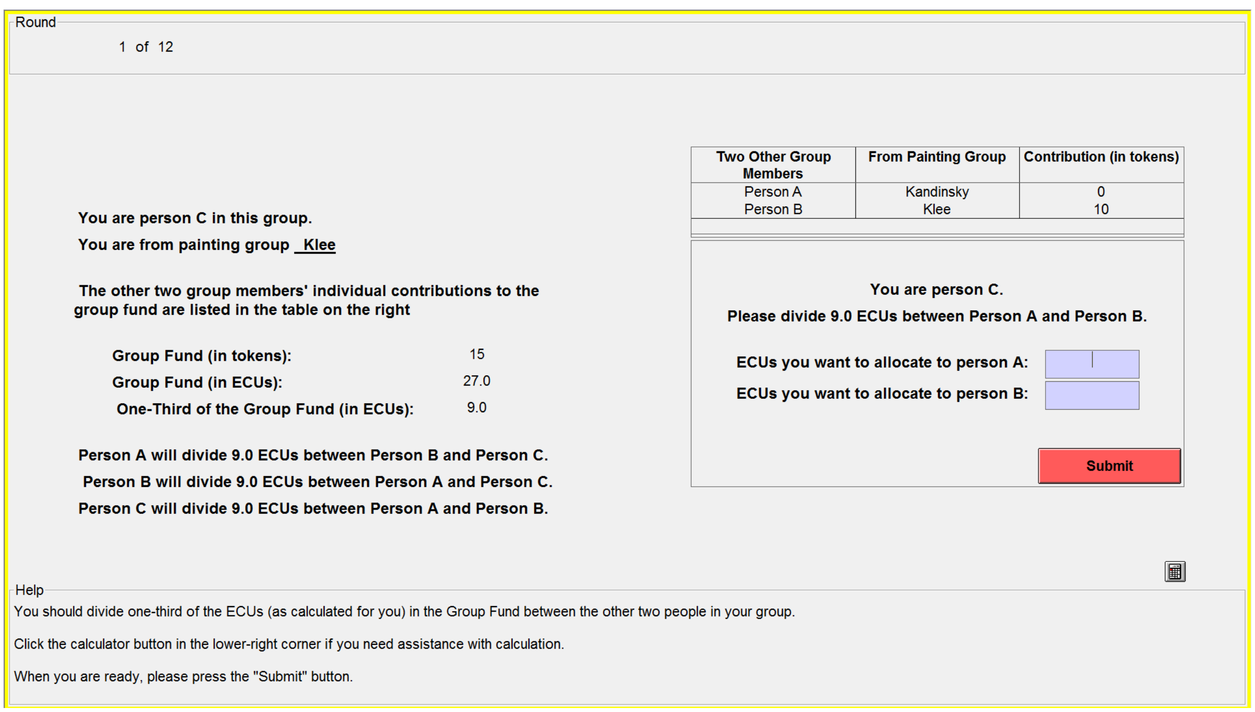


Figure B1.4 Feedbacks

Round

1 of 12

For this round, you are: Person C

Group Members	From Painting Group	Contribution (in tokens)	Earnings from Individual Fund	Earnings from Group Fund
Person A	Kandinsky	0	10	8.5
Person B	Klee	10	0	11.0
Person C	Klee	5	5	7.5

Group Members	Allocation from Person A	Allocation from Person B	Allocation from Person C	Earnings from Group Fund
Person A	N/A	4.5	4.0	8.5
Person B	6.0	N/A	5.0	11.0
Person C	3.0	4.5	N/A	7.5

You are from painting group:	Klee
Tokens you added to the Group Fund:	5
Total Tokens in the Group Fund:	15
Total ECUs in the Group Fund:	27.0
One-third of ECUs in the Group Fund:	9.0

For this round, you are:	Person C
Earnings from Individual Fund (in ECUs):	5.0
Earnings from Group Fund (in ECUs):	7.5
Your information expenditure (in ECUs):	-0.5
Your total earnings from this round (in ECUs):	12.0

Note you will never meet this same set of two other participants again in the following rounds.

OK

Appendix B2. Post-experimental Survey

Please answer the following survey questions. Your answers will be used for this study only. Individual data will not be exposed. (*summary statistics in italics*)

1. What is your age? (*Mean 21.76, Std Dev 2.65, Median 21, Min 18, Max 35*)

2. What is your gender? (*Male 40.28%, Female 59.72%*)

3. What is your faculty in the university? (*Arts 10.42%, Engineering 12.5%, Medicine and Health Sciences 26.74%, Science 22.57%, Social science 27.78%*)

4. Which of the following best describe your contribution decisions?

   - I minimized my contribution to the group fund to guarantee as much ECU as possible in my individual fund. (*7.29%*)
   - I contributed to the group fund so that others can benefit from my contributions. (*13.19%*)
   - I contributed to the group fund with the belief that others would allocate a fair proportion of ECUs to me. (*76.74%*)
   - None of the above. (*2.78%*)

5. Which of the following best describe your buying information decisions?

   - I spent 0.5 ECUs in most of the rounds. Because I wanted to make allocation decisions based on this information (*59.72% in CostHom and 43.06% in CostHet*)
   - I did not spend 0.5 ECUs in most of the rounds. Because it was too costly, if the cost was smaller, I would choose to reveal others' contribution decisions (*11.11% in CostHom and 13.89% in CostHet*)
   - I did not spend 0.5 ECUs in most of the rounds. Because this expenditure had no impact on my profit. So even if the cost was zero, I won't bother to know (*22.22% in CostHom and 29.17% in CostHet*)

- I did not spend 0.5 ECUs in most of the rounds. Because I believed the person who were in my painting group must be the person who contribute more (*5.56% in CostHet*)
- None of the above (*6.94% in CostHom and 8.33% in CostHet*)

6. Which of the following best describe your allocation decisions?

   - I allocated PROPORTIONALLY to the other two players (*73.61% in CostHom and 59.72% in CostHet*)
   - I allocated EQUALLY to the other two players (*14.58% in CostHom and 22.92% in CostHet*)
   - I allocated RANDOMLY to the other two players (*6.94% in CostHom and 6.94% in CostHet*)
   - I allocated MORE to the person who shares the same painting group with me (*6.94% in CostHet*)
   - None of the above (*4.86% in CostHom and 3.47% in CostHet*)

7. Bob and John are identical in terms of physical and mental abilities. They become shipwrecked on an uninhabited island where the only food is bananas. They can collect as many bananas as they want by climbing up a tree, picking them before they fall into the ocean and throwing them into a pile. In this way Bob picks 15 bananas per day and John picks 5 per day.

   - Bob takes from the pile the 15 bananas he picked leaving John with the 5 which John picked. (*Unfair 27.78%, Fair 72.22%*)
   - John takes 10 bananas from the pile leaving 10 for Bob (*Unfair 51.39%, Fair 48.61%*)

Table B2.1 summarizes the contribution and buying decisions between genders and across faculties, and we find no evidence of significant difference.

Table B2.1: Contribution and buying decisions between genders and across faculties

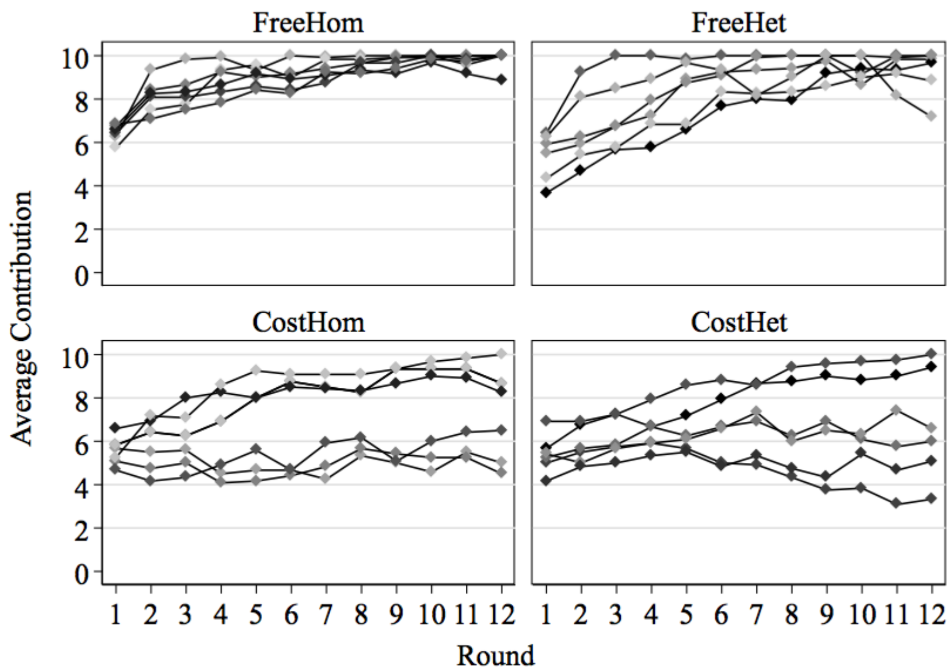
Gender	Contributions	Buying Rate	*Faculties*	Contributions	Buying Rate
Male (116)	7.51	44.71%	Arts (30)	7.52	49.35%
Female (172)	7.67	43.11%	Engineering (36)	7.58	39.28%
			Medicine (77)	7.72	46.07%
			Science (65)	7.95	42.81%
			Social science (80)	7.24	43.26%

Alternative Hypotheses	*p-value*
Contribution (Male≠Female)	0.66
Contribution (Different across faculties)	0.44
Buying Rate (Male≠Female)	0.77
Buying Rate (Different across faculties)	0.88

Notes: Numbers of observations are in the parentheses. The null hypotheses of the hypotheses tests are of equal contributions or buying rates between genders or across faculties. We use ranksum tests for gender comparisons and Kruskal-Wallis equality tests for comparisons across faculties. Individuals' average contribution and buying rate over 12 rounds of the experiment are used as independent observations.

Appendix B3. Descriptive Data and Further Analysis

This Appendix contains additional statistics for contribution, monitoring and allocation decisions. Figure B3.1 presents the average contribution sorted by each session in four treatments and Figure B3.2 presents the mean contribution in each treatment with 95-percent confidence intervals. Table B3.1 provides a robustness check for Table 2.7.



Notes: Each treatment has 6 sessions and each session has 12 participants.

Figure B3.1: Time-path of the Average Contribution by Sessions and Treatments

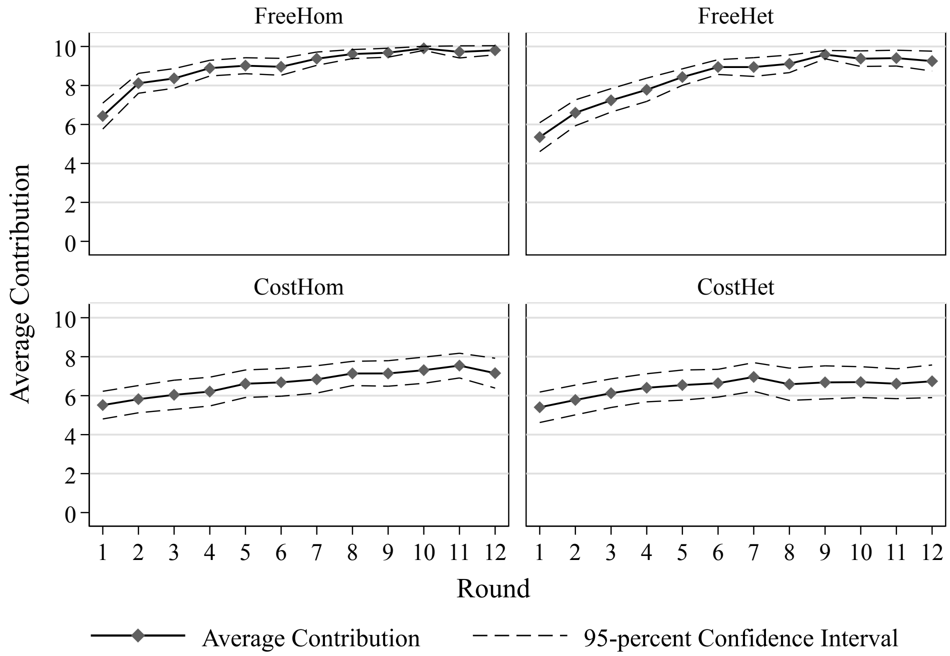


Figure B3.2: Time-path of the Average Contribution and 95-percent Confidence Interval

Table B3.1: Robustness (Table 2.7): Including Minority Players and Round One Decisions

	Dep. Variable: Fraction Player i Allocate to Player j					
	(1)	(2)	(3)	(4)	(5)	(6)
j's relative contributions: β_1	1.016***	0.878***	0.828***	0.841***	0.726***	0.575***
	(0.029)	(0.028)	(0.048)	(0.061)	(0.161)	(0.181)
j is In-group: β_2	0.038	-0.069*	-0.026	-0.044	-0.184	-0.082
	(0.023)	(0.034)	(0.022)	(0.054)	(0.129)	(0.095)
j's relative contributions $\times$ j is In-group: β_3	-0.036	0.082*	0.186***	0.111	0.267	0.349
	(0.037)	(0.042)	(0.054)	(0.088)	(0.185)	(0.211)
Costly Monitoring : β_4	-0.007	0.022	-0.026**	0.022	0.038	0.007
	(0.014)	(0.023)	(0.019)	(0.023)	(0.032)	(0.041)
Intercept: β_0	-0.007	0.094***	0.048**	0.081*	0.172	0.153*
	(0.014)	(0.023)	(0.019)	(0.035)	(0.115)	(0.063)
R-square	0.698	0.676	0.759	0.708	0.681	0.712
#Data Used when	All	$e_j > e_k$	$e_j < e_k$	All	$e_j > e_k$	$e_j < e_k$
#Rounds	1-12	1-12	1-12	1	1	1
#Observations	2485	737	740	185	84	79
#Cluster	24	24	24			
Hausman test for random vs fixed effects	1.44	0.62	1.01			
	($p = 0.487$)	($p = 0.734$)	($p = 0.602$)			
$H_0 : \beta_1 = 1$	0.30	18.86	12.99	6.73	2.88	5.49
	($p = 0.584$)	($p = 0.000$)	($p = 0.000$)	($p = 0.010$)	($p = 0.094$)	($p = 0.022$)
$H_0 : \beta_1 + \beta_3 = 1$	0.72	1.70	0.31	0.54	0.01	0.56
	($p = 0.396$)	($p = 0.192$)	($p = 0.578$)	($p = 0.463$)	($p = 0.937$)	($p = 0.457$)

Notes: (i) $\frac{a^t_{ij}}{a^t_{ij}+a^t_{ik}} = \beta_0 + \beta_1 \times \frac{e^t_j}{e^t_j+e^t_k} + \beta_2 \times \text{Ingrp}_j + \beta_3 \times \frac{e^t_j}{e^t_j+e^t_k} \times \text{Ingrp}_j + \beta_4 \times \text{CostMonitor}^t_i + u_i + \varepsilon^t_i$ (ii) Comparing to table 2.7, column 1-3 also include minority players' allocation decisions in heterogeneous treatment, and column 4-6 are only focused on the first round behaviour using OLS regressions. (iii) Column 2 and 5 are based on the decisions where player j contributes more than player k. Column 3 and 6 are based on the decisions where player j contributes less than player k. (iv) An interaction term between the variables CostMonitor and Ingrp has tried to be added. The effect is not significant. We therefore omit this interaction term in the regression. (v)*, ** and *** denote, respectively, significance at the 10-percent, 5-percent and 1-percent levels. Standard errors clustered on the session level are in the brackets. (vi) We report the test statistics for the hypotheses tests and 2-sided p values are in the brackets.

C. Appendix for Chapter 3

Experimental Instruction

In this appendix, I present the experimental instructions for the CT-Restore treatment (Part 1 is a minimum effort game without mechanisms and part 2 introduces cheap-talk leaders). Participants receive printed copies of the instructions and the experimenter read it aloud in each session. Part 2 instruction is distributed only after the completion of Part 1 decisions. Instructions for other treatments are similar and are available upon request.

Part 1 (Decision Round 1-10)

The purpose of this experiment is to study how people make decisions in a particular situation. From now until the end of the experiment, any communication with other participants or use of mobile phones is not permitted. If you have a question, please raise your hand and one of us will come to your desk to answer it. This experiment will have several *parts*. In each part there will be several *rounds*. You will earn some points each round during the experiment. Upon completion of the experiment the total amount of points will be converted into pounds, and will be paid to you in cash. The conversion rate is 400 Points = 1 Pound**.** Payments will be confidential, i.e., no other participant will be told the amount you make. To ensure your anonymity, your actions in this experiment are only linked to your participant

ID number contained in the white envelope. Now, please enter your participant ID number on the screen.

In Part I there will be ten *rounds.* After these ten rounds have finished, we will give you instructions for the next part of the experiment. In each round you will be in a *group* with three other participants. The participants you are grouped with will be the same in all rounds of Part 1.

You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. In each week, each of the employees in the firm spends 40 hours at the firm. You have to choose how to allocate your time between two activities, Activity A and Activity B. Specifically, you will be asked to choose how much time to devote to Activity A. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, and 40 hours. Your remaining hours will be put toward Activity B. For example, if you devote 30 hours to Activity A, this means that 10 hours will be put toward Activity B.

The payoff that an employee receives in a round depends on the number of hours he/she chooses to spend on Activity A and the number of hours chosen by the others in his/her firm to spend on Activity A. The payoff (in points) for the i^{th} employee of the firm, π_i, is given by the formula below where H_i is the number of hours spent by the i^{th} employee of the firm on Activity A, and $\min(H_A)$ is the smallest number of hours an employee of the firm spends on Activity A. You do not need to memorize this formula — the computer program will give you payoff tables at any point where you need to make a decision:

$$\pi_i = 6 \cdot \min(H_A) - 5 \cdot H_i + 200$$

For each round of the experiment, the computer will display a screen like the one shown below (see Screenshot A1) representing your possible payoffs calculated from the formula above. Each employee will choose a number of hours to spend on Activity A using the buttons on the right-hand side of the screen. You will be given 1 minute for each round and you may change your choice as often as you like, but once you click "OK", the choice is final. Note that when you make your decision you will *not* know what the other employees in your firm are doing in the round. At no point in time will we identify the other employees in your firm. In other words, the actions you take in this experiment will remain confidential.

After each round you will be informed about the number of hours you have spent on Activity A, the lowest number chosen by all of the employees in your firm, your payoff for the latest round, and your accumulated payoffs through the current round. You will also be shown your decisions and the decisions of all the other employees of your group for the current round. *These decisions will be sorted from lowest to highest, and will not include any identifying information about which employee was responsible for which choice* .

Quiz for Part 1

Before we begin the experiment, please answer the following questions. The payoff table is shown below. We will go through the answers to a sample problem before you do the rest of the quiz.

Sample Question: Suppose you choose to spend 10 hours on Activity A. The other employees choose to spend 30, 20, and 40 on Activity A. The minimum number of hours an employee of the firm spends on Activity A is **<u>10</u>**. Your payoff is **<u>210</u>** points. Now, please do the following quiz. If you have trouble answering any of the questions or have finished the quiz, please raise your hand.

1. Suppose you choose to spend 20 hours on Activity A. The other employees choose to spend 30, 0, and 10 hours on Activity A. The smallest number of hours an employee of the firm spends on A is ___. Your payoff is ___ points.

2. Suppose you choose to spend 30 hours on Activity A. The other employees choose to spend 20, 30, and 40 hours on Activity A. The smallest number of hours an employee of the firm spends on A is ___. Your payoff is ___ points.

3. At the end of each round, the decisions of each employee in my group will be displayed in the upper left corner of the information screen, without revealing the identity of each employee. (True/False)

4. I am grouped with the same three individuals for the entire Part 1 of the experiment. (True/False)

5. My actions and payoffs will be confidential. (True/False)

Part 2 (Decision Round 11-20)

In Part 2, there will be *ten* rounds. In all rounds, you will *still* be grouped with the same three individuals as in Part 1 of the experiment. However, *one* of you will be randomly chosen to play the role of Employee X and the other THREE group members will play the role of Employee Y. You will learn whether your role is Employee X or Employee Y at the start of Part 2. These roles will remain *fixed* during the entire Part 2. The profit table will be the same as in Part 1.

First, *Employee X suggests a number* for the group each round. This suggested number will be available to the other group members.

Employee X will also *make an estimate* about "the minimum number of hours that the other three employees will choose to spend on activity A in response to his/her suggested number" (see Screenshot A2). There will be 20 extra points for each correct estimate. Those points will be added up at the final payment stage.

After Employee X suggests the number, each Employee Y will choose how many hours to spend on activity A. While in principle each Employee Y decides after Employee X, Employee Y will be asked to decide before learning the actual suggested number of Employee X. Specifically, Employee Y will fill in a table where he/she can indicate how many hours he/she wants to spend on activity A for each possible number Employee X might suggest (see Screenshot A3).

- In the first box: how many hours you want to spend on activity A if Employee X suggests 0,
- In the second box: how many hours you want to spend on activity A if Employee X suggests 10,
- In the third box: how many hours you want to spend on activity A if Employee X suggests 20, etc.

After every employee in the group makes their decision, Employee X's suggested number will be revealed to all group members. The relevant decision of Employee Y will be determined by Employee X's actual suggested number. For example, if employee X suggested 10, the only relevant decision for Employee Y is the number entered in the second box.

At the end of each round, you will receive the same information as in Part 1. That is, you will be informed about the number of hours you have spent on activity A, the lowest number chosen by all of the employees in your firm, your payoff for the latest round, and your accumulated payoffs through the current round. *You will also be shown all decisions in your group sorted from lowest to highest.*

Quiz for Part 2

Before we begin Part 2 of the experiment, please answer the following questions. The payoff table is shown below. We will go through the answers to a sample problem before you do the rest of the quiz. Please raise your hand if you are having trouble answering any of the questions.

Sample Question: Suppose you are employee X, you suggest 10 and choose to spend 10 hours on Activity A**.** The three employee Y's choices are **10,** **0,** and **40** on Activity A (numbers are provided for explanatory purpose only). The minimum number of hours an employee of the firm spends on Activity A is **0** . Your payoff is **150** points. Now, please do the following quiz. If you have trouble answering any of the questions or have finished the quiz, please raise your hand.

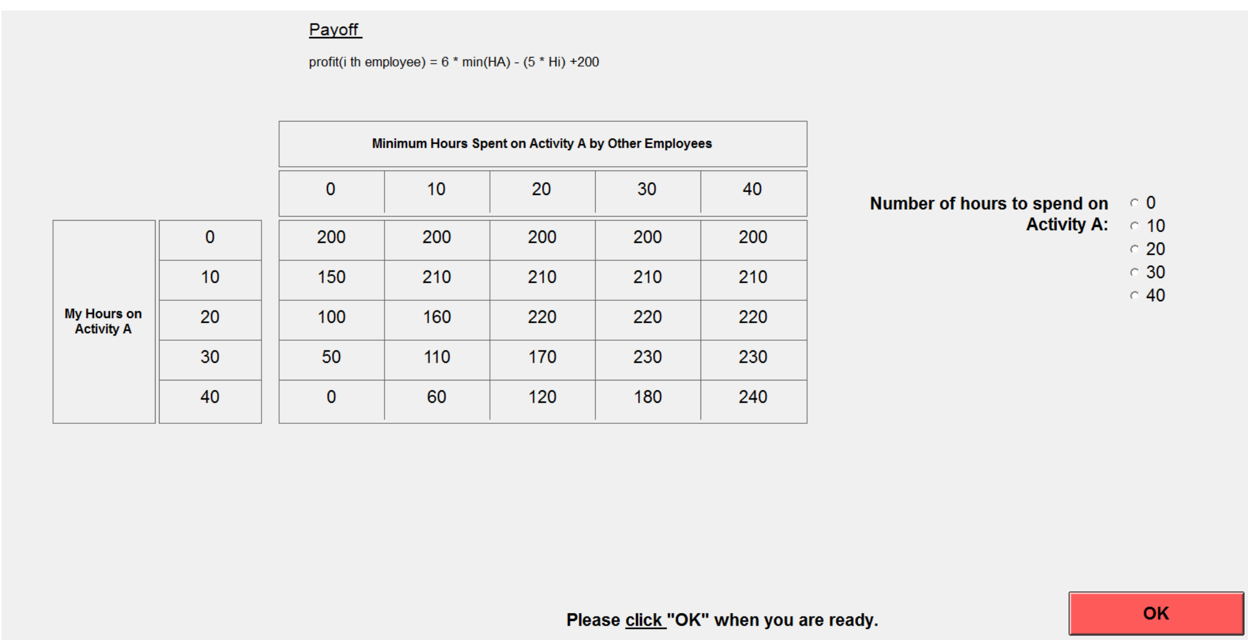
1. Employee Y will learn Employee X's suggested number each round. (True/False)

2. Suppose you are employee X, and you suggest 30 and choose to spend 20 hours on Activity A. The three employee Y's relevant decisions are 20, 40, and 40 on Activity A. The minimum number of hours an employee of the firm spends on Activity A is ___. Your payoff is ___ points.

3. Suppose you are one of the employee Ys, Employee X suggested 30 and you choose to spend 40 on Activity A. Employee X's choice is 20 and the other two Employee Ys' choices are 20 and 40. The minimum number of hours an employee of the firm spends on Activity A is ___. Your payoff is ___ points.

4. I am grouped with the same three individuals for Part 1 and 2 of the experiment. (True/False)

Screenshots



Payoff

profit(i th employee) = 6 * min(HA) - (5 * Hi) +200

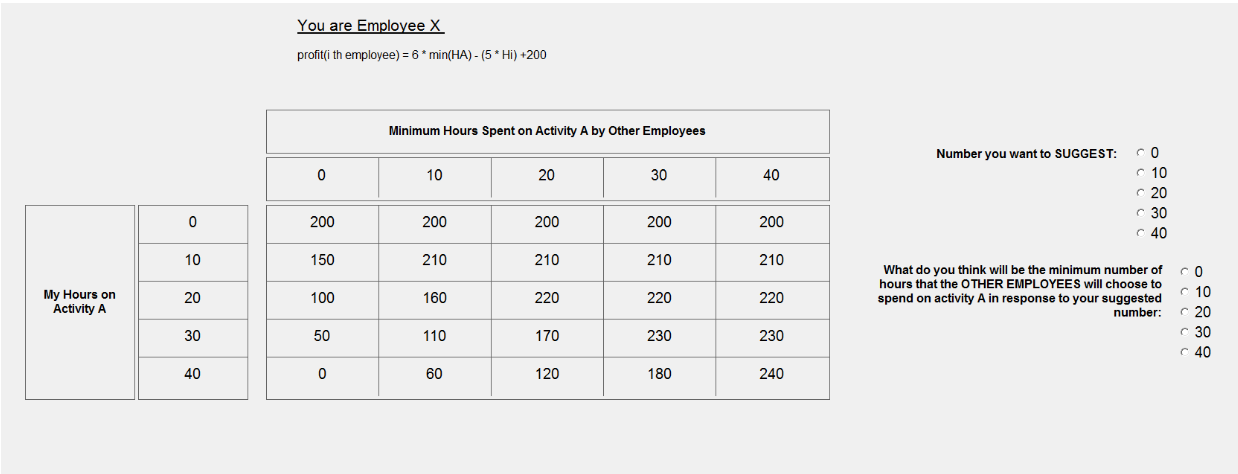
		Minimum Hours Spent on Activity A by Other Employees				
		0	10	20	30	40
My Hours on Activity A	0	200	200	200	200	200
	10	150	210	210	210	210
	20	100	160	220	220	220
	30	50	110	170	230	230
	40	0	60	120	180	240

Number of hours to spend on Activity A: 0 10 20 30 40

Please click "OK" when you are ready.

OK

Screenshot A2: Decision Screen for Employee X in Part 2


You are Employee X

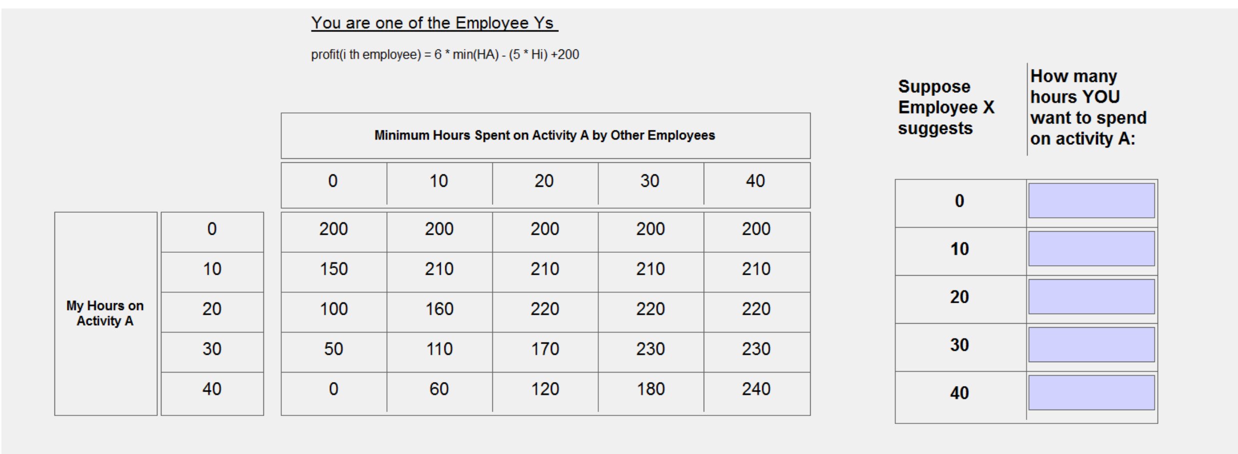
profit(i th employee) = 6 * min(HA) - (5 * Hi) +200

		Minimum Hours Spent on Activity A by Other Employees				
		0	10	20	30	40
My Hours on Activity A	0	200	200	200	200	200
	10	150	210	210	210	210
	20	100	160	220	220	220
	30	50	110	170	230	230
	40	0	60	120	180	240

Number you want to SUGGEST: 0 10 20 30 40

What do you think will be the minimum number of hours that the OTHER EMPLOYEES will choose to spend on activity A in response to your suggested number: 0 10 20 30 40

Screenshot A3: Decision Screen for Employee Y in Part 2


You are one of the Employee Ys

profit(i th employee) = 6 * min(HA) - (5 * Hi) +200

		Minimum Hours Spent on Activity A by Other Employees				
		0	10	20	30	40
My Hours on Activity A	0	200	200	200	200	200
	10	150	210	210	210	210
	20	100	160	220	220	220
	30	50	110	170	230	230
	40	0	60	120	180	240

Suppose Employee X suggests	How many hours YOU want to spend on activity A:
0	
10	
20	
30	
40	

Bibliography

Adams, J. S. (1965). Inequity in Social Exchange. In Berkowitz, L., editor, *Advances in Experimental Social Psychology*, pages 267–99. New York: Academic Press.

Akerlof, G. A. and Kranton, R. E. (2000). Economics and Identity. *Quarterly Journal of Economics*, 115(3):715–753.

Andreoni, J., Harbaugh, W., and Vesterlund, L. (2003). The Carrot or the Stick: Rewards, Punishments, and Cooperation. *American Economic Review*, 93(3):893–902.

Andreoni, J. and Varian, H. R. (1999). Preplay Contracting in the Prisoners' Dilemma. *Proceedings of the National Academy of Sciences*, 96(19):10933–10938.

Aristotle (1566). *Nicomachean Ethics.* V, 1131(a), Roger, C., translator, Cambridge University Press 2000.

Baranski, A. (2016). Voluntary Contributions and Collective Redistribution. *American Economic Journal: Microeconomics*, 8(4):149–173.

Baron, D. P. and Ferejohn, J. A. (1989). Bargaining in Legislatures. *American Political Science Review*, 83(4):1181–1206.

Becker, G. (1971). *The Economics of Discrimination.* Chicago: University of Chicago Press.

Benabou, R. and Tirole, J. (2011). Identity, Morals, and Taboos: Beliefs as Assets. *Quarterly Journal of Economics*, 126(2):805–855.

Bernhard, H., Fehr, E., and Fischbacher, U. (2006). Group Affiliation and Altruistic Norm Enforcement. *American Economic Review*, 96(2):217–221.

Blume, A. and Ortmann, A. (2007). The Effects of Costless Pre-play Communication: Experimental Evidence from Games with Pareto-ranked Equilibria. *Journal of Economic Theory*, 132(1):274–290.

Boyd, R., Gintis, H., Bowles, S., and Richerson, P. J. (2003). The Evolution of Altruistic Punishment. *Proceedings of the National Academy of Sciences*, 100(6):3531–3535.

Bracht, J., Figuières, C., and Ratto, M. (2008). Relative Performance of Two Simple Incentive Mechanisms in A Public Goods Experiment. *Journal of Public Economics*, 92(1-2):54–90.

Brandts, J. and Charness, G. (2011). The Strategy versus the Direct-response Method: A First Survey of Experimental Comparisons. *Experimental Economics*, 14(3):375–398.

Brandts, J. and Cooper, D. J. (2006). A Change Would Do You good ... An Experimental Study on How to Overcome Coordination Failure in Organizations. *American Economic Review*, 96(3):669–693.

Brandts, J. and Cooper, D. J. (2007). It's What You Say, Not What You Pay: An Experimental Study of Manager Employee Relationships in Overcoming Coordination Failure. *Journal of the European Economic Association*, 5(6):1223–1268.

Brandts, J., Cooper, D. J., and Weber, R. A. (2015). Legitimacy, Communication and Leadership in the Turnaround Game. *Management Science*, 61(11):2627–2645.

Brick, I. E., Palmon, O., and Wald, J. K. (2006). CEO Compensation, Director Compensation, and Firm Performance: Evidence of Cryonism? *Journal of Corporate Finance*, 12(3):403–423.

Camerer, C. F. (2003). *Behavioral Game Theory: Experiments in Strategic Interaction*. Princeton University Press.

Cappelen, A. W., Eichele, T., Hugdahl, K., Specht, K., Sørensen, E. Ø., and Tungodden, B. (2014). Equity Theory and Fair Inequality: A Neuroeconomic Study. *Proceedings of the National Academy of Sciences*, 111(43):15368–15372.

Cappelen, A. W., Hole, A. D., Sørensen, E. Ø., and Tungodden, B. (2007). The Pluralism of Fairness Ideals: An Experimental Approach. *American Economic Review*, 97(3):818–827.

Cappelen, A. W., Konow, J., Sørensen, E. Ø., and Tungodden, B. (2013). Just Luck: An Experimental Study of Risk-Taking and Fairness. *American Economic Review*, 103(4):1398–1413.

Cartwright, E., Gillet, J., and Van Vugt, M. (2013). Leadership by Example in the Weak-link Game. *Economic Inquiry*, 51(4):2028–2043.

Charness, G. and Rabin, M. (2002). Understanding Social Preferences with Simple Tests. *Quarterly Journal of Economics*, 117(3):817–869.

Charness, G., Rigotti, L., and Rustichini, A. (2007). Individual Behavior and Group Membership. *American Economic Review*, 97(4):1340–1352.

Chaudhuri, A. (2010). Sustaining Cooperation in Laboratory Public Goods Experiments: A Selective Survey of the Literature. *Experimental Economics*, 14(1):47–83.

Chaudhuri, A., Schotter, A., and Sopher, B. (2009). Talking Ourselves to Efficiency: Coordination in Inter-generational Minimum Effort Games with Private, Almost Common and Common Knowledge of Advice. *Economic Journal*, 119(534):91–122.

Chen, R. and Chen, Y. (2011). The Potential of Social Identity for Equilibrium Selection. *American Economic Review*, 101(6):2562–2589.

Chen, Y. (2008). Incentive Compatible Mechanisms for Pure Public Goods: A Survey of Experimental Research. In Smith, V. L. and Plott, C. R., editors, *The Handbook of Experimental Economics Results*, volume 1.

Chen, Y. and Li, S. (2009). Group Identity and Social Preferences. *American Economic Review*, 2005(1):431–457.

Chen, Y. and Plott, C. R. (1996). The Groves-Ledyard mechanism: An experimental study of institutional design. *Journal of Public Economics*, 59(3):335–364.

Chhatre, A. and Agrawal, A. (2008). Forest Commons and Local Enforcement. *Proceedings of the National Academy of Sciences of the United States of America*, 105(36):13286–13291.

Clarke, E. H. (1971). Multipart Pricing of Public Goods. *Public Choice*, 11(1):17–33.

Cox, J. C., Friedman, D., and Gjerstad, S. (2007). A Tractable Model of Reciprocity and Fairness. *Games and Economic Behavior*, 59(1):17–45.

Croson, R. (1996). Partners and Strangers Revisited. *Economics Letters*, 53(1):25–32.

Dana, J., Weber, R. A., and Kuang, J. X. (2007). Exploiting Moral Wiggle Room: Experiments Demonstrating An Illusory Preference for fairness. *Economic Theory*, 33(1):67–80.

Devetag, G. and Ortmann, A. (2007). When and Why? A Critical Survey on Coordination Failure in the Laboratory. *Experimental Economics*, 10(3):331–344.

Dong, L., Montero, M., and Possajennikov, A. (2015). Communication, Leadership and Coordination Failure. *Available at: https://ssrn.com/abstract=26558.*

Dugar, S. (2010). Nonmonetary Sanctions and Rewards in An Experimental Coordination Game. *Journal of Economic Behavior & Organization*, (3):377–386.

Eckel, C. C. and Grossman, P. J. (2005). Managing Diversity by Creating Team identity. *Journal of Economic Behavior & Organization*, 58(3):371–392.

Falkinger, J., Fehr, E., Gächter, S., and Winter-Ebmer, R. (2000). A Simple Mechanism for the Efficient Provision of Public Goods: Experimental Evidence. *American Economic Review*, 90(1):247–264.

Fehr, D. (2016). Costly Communication and Learning from Failure in Organizational Coordination. *Working Paper*.

Fehr, E. and Fischbacher, U. (2004). Third-party Punishment and Social Norms. *Evolution and Human Behavior*, 25(2):63–87.

Fehr, E., Fischbacher, U., and Gächter, S. (2002). Strong Reciprocity, Human Cooperation, and the Enforcement of Social Norms. *Human Nature*, 13(1):1–25.

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

Fehr, E. and Schmidt, K. M. (1999). A Theory of Fairness, Competition, and Cooperation. *Quarterly Journal of Economics*, 114(3):817–868.

Feld, J., Salamanca, N., and Hamermesh, D. S. (2016). Endophilia or Exophobia: Beyond Discrimination. *Economic Journal*, 126(594):1503–1527.

Fershtman, C. and Gneezy, U. (2001). Discrimination in A Segmented Society: An Experimental Approach. *Quarterly Journal of Economics*, 116(1):351–377.

Fischbacher, U. (2007). z-Tree: Zurich Toolbox for Ready-made Economic Experiments. *Experimental economics*, 10(2):171–178.

Fischbacher, U. and Gächter, S. (2010). Social Preferences, Beliefs, and the Dynamics of Free Riding in Public Goods Experiments. *American Economic Review*, 100(1):541–556.

Fischbacher, U., Gächter, S., and Fehr, E. (2001). Are People Conditionally Cooperative? Evidence from A Public Goods Experiment. *Economics Letters*, 71:397–404.

Fischbacher, U., Gächter, S., and Quercia, S. (2012). The Behavioral Validity of the Strategy Method in Public Good Experiments. *Journal of Economic Psychology*, 33(4):897–913.

Fliessbach, K., Weber, B., Trautner, P., Dohmen, T., Sunde, U., Elger, C. E., and Falk, A. (2007). Social Comparison Affects Reward-Related Brain Activity in the Human Ventral Striatum. *Science*, 318(5854):1305–1308.

Freeman, R. (2008). When Workers Share in Profits: Effort and Responses to Shirking. *CEP Discussion Paper No.882*.

Frohlich, N., Oppenheimer, J., and Kurki, A. (2004). Modeling Other-Regarding Preferences and an Experimental Test. *Public Choice*, 119(1/2):91–117.

Gächter, S., Renner, E., and Sefton, M. (2008). The Long-run Benefits of Punishment. *Science*, 322(5907):1510.

Gächter, S. and Riedl, A. (2006). Dividing Justly in Bargaining Problems with Claims. *Social Choice and Welfare*, 27(3):571–594.

Galbraith, J. K. (1963). *The Great Crash 1929*. Penguin Books, Harmondsworth, Middlesex.

Gibson, C. C., Williams, J. T., and Ostrom, E. (2005). Local Enforcement and Better Forests. *World Development*, 33(2):273–284.

Gintis, H., Bowles, S., Boyd, R., and Fehr, E. (2003). Explaining Altruistic Behavior in Humans. *Evolution and Human Behavior*, 24(3):153–172.

Goeree, J. K. and Holt, C. A. (2005). An Experimental Study of Costly Coordination. *Games and Economic Behavior*, 51(2):349–364.

Goette, L., Huffman, D., and Meier, S. (2006). The Impact of Group Membership on Cooperation and Norm Enforcement: Evidence Using Random Assignment to Real Social Groups. *American Economic Review*, 96(2):212–216.

Goldberg, M. S. (1982). Discrimination, Nepotism, and Long-Run Wage Differentials. *Quarterly Journal of Economics*, 97(2):307.

Greiner, B. (2015). Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE. *Journal of the Economic Science Association*, 1(1):114–125.

Groves, T. (1973). Incentives in Teams. *Econometrica*, 41(4):617.

Groves, T. and Ledyard, J. O. (1977). Optimal Allocation of Public Goods: A Solution to the Free-rider Problem. *Econometrica*, 45(4):783–809.

Hamman, J. R., Rick, S., and Weber, R. A. (2007). Solving Coordination Failure With “All-or-none" Group-level Incentives. *Experimental Economics*, 10(3):285–303.

Hardin, G. (1968). The Tragedy of the Commons. *Sciene*, 12(January 2015):1243–1248.

Hargreaves Heap, S. P. and Zizzo, D. J. (2009). The Value of Groups. *American Economic Review*, 99(1):295–323.

Harsanyi, J. C. and Selten, R. (1988). *A General Theory of Equilibrium Selection in Games.* MIT Press, Cambridge MA.

Herrmann, B., Thöni, C., and Gächter, S. (2008). Antisocial Punishment Across Societies. *Science*, 319(5868):1362–1367.

Heywood, J. S. and Jirjahn, U. (2009). Profit Sharing and Firm Size: The Role of Team Production. *Journal of Economic Behavior & Organization*, 71(2):246–258.

Homans, G. (1958). Social Behavior as Exchange. *American Journal of Sociology,*, 63(6):597–606.

Knez, M. and Simester, D. (2001). FirmâĂŘWide Incentives and Mutual Monitoring at Continental Airlines. *Journal of Labor Economics*, 19(4):743–772.

Konow, J. (1996). A Positive Theory of Economic Fairness. *Journal of Economic Behavior & Organization*, 31:13–35.

Konow, J. (2000). Fair Shares : Accountability and Cognitive Dissonance in Allocation Decisions. *American Economic Review*, 90(4):1072–1091.

Konow, J. (2009). Is Fairness in the Eye of the Beholder? An Impartial Spectator Analysis of Justice. *Social Choice and Welfare*, 33(1):101–127.

Kriss, P. H., Blume, A., and Weber, R. A. (2016). Coordination with Decentralized Costly Communication. *Journal of Economic Behavior & Organization*, 130:225–241.

Laband, D. N. and Piette, M. J. (1994). Favoritism versus Search for Good Papers: Empirical Evidence Regarding the Behavior of Journal Editors. *Journal of Political Economy*, 102(1):194.

Laffont, J.-J. (1987). Incentives and the allocation of public goods. In Auerbach, A. J. and Feldstein, M., editors, *Handbook of Public Economics*, volume 2, pages 537–569. Amsterdam: North Holland.

Laroze, D., Hugh-jones, D., and Leininger, A. (2016). The Impact of Group Identity on Coalition Building. *Working Paper*.

Ledyard, J. O. (1995). Public Goods: A Survey of Experimental Research. In Kagel, J. H. and Roth, A. E., editors, *Handbook of Experimental Economics*, pages 111–94. Princeton University Press, Princeton.

Levine, D. K. (1998). Modeling Altruism and Spitefulness in Experiments. *Review of Economic Dynamics*, 1(3):593–622.

Manzini, P., Sadrieh, A., and Vriend, N. J. (2009). On smiles, Winks and Handshakes as Coordination Devices. *Economic Journal*, 119(537):826–854.

Masuda, T., Okano, Y., and Saijo, T. (2014). The Minimum Approval Mechanism Implements the Efficient Public Good Allocation Theoretically and Experimentally. *Games and Economic Behavior*, 83:73–85.

Monderer, D. and Shapley, L. S. (1996). Potential Games. *Games and Economic Behavior*, 14(1):124–143.

Nikiforakis, N. (2008). Punishment and Counter-punishment in Public Good Games: Can We Really Govern Ourselves? *Journal of Public Economics*, 92(1-2):91–112.

Nosenzo, D. and Sefton, M. (2012). Promoting Cooperation: The Distribution of Reward and Punishment Power. In Van Lange, P. A., Rockenbach, B., and Yamagishi, T., editors, *Social Dilemmas: New Perspectives on Reward and Punishment*. Oxford University Press.

Pogrebna, G., Krantz, D. H., Schade, C., and Keser, C. (2011). Words versus Actions As A Means to Influence Cooperation in Social Dilemma Situations. *Theory and Decision*, 71(4):473–502.

Rivkin, S. G., Hanushek, E. A., and Kain, J. F. (2005). Teachers, Schools, and Academic Achievement. *Econometrica*, 73(2):417–458.

Rousseau, J. J. (1755). *A Discourse on Inequality*. Marc-Michel Rey, Holland.

Ruffle, B. J. (1998). More Is Better, But Fair Is Fair: Tipping in Dictator and Ultimatum Games. *Games and Economic Behavior*, 23(2):247–265.

Rustagi, D., Engel, S., and Kosfeld, M. (2010). Conditional Cooperation and Costly Monitoring Explain Success in Forest Commons Management. *Science*, 330(6006):961–965.

Sahin, S. G., Eckel, C., and Komai, M. (2015). An Experimental Study of Leadership Institutions in Collective Action Games. *Journal of the Economic Science Association*, 1(1):100–113.

Sefton, M., Shupp, R., and Walker, J. M. (2007). The Effect of Rewards and Sanctions in Provision of Public Goods. *Economic Inquiry*, 45(4):671–690.

Selten, R. (1978). The Equity Principle in Economic Behavior. In Gottinger, H. and Leinfellner, W., editors, *Decision Theory and Social Ethics*, pages 289–301. Reidel Publishing, Dordrecht, Holland.

Shaw, A. (2013). Beyond “to Share or Not to Share": The Impartiality Account of Fairness. *Current Directions in Psychological Science*, 22(5):413–417.

Smith, A. (1776). *An Inquiry into the Nature and Causes of the Wealth of Nations*. Modern Library, New York, 1937.

Smith, V. L. (1982). Microeconomic Systems as an Experimental Science. *American Economic Review*, 72(5):923–955.

Stoddard, B., Walker, J. M., and Williams, A. (2014). Allocating A Voluntarily Provided Common-Property Resource: An Experimental Examination. *Journal of Economic Behavior & Organization*, 101:141–155.

Tajfel, H. (1982). Social Psychology of Intergroup Relations. *Annual Review of Psychology*, 33(1):1–39.

Tajfel, H., Billig, M. G., Bundy, R. P., and Flament, C. (1971). Social Categorization and Intergroup Behaviour. *European Journal of Social Psychology*, 1(2):149–178.

Tajfel, H. and Turner, J. C. (1986). The Social Identity Theory of Intergroup Behavior. In Worchel, S. and Austin, W., editors, *Psychology of Intergroup Relations*, pages 7–24. Chicago: Nelson Hall.

Tatsuya, T., Takezawa, M., and Hastie, R. (2003). The Logic of Social Sharing: An Evolutionary Game Analysis of Adaptive Norm Development. *Personality and Social Psychology Review*, 7(1):2–19.

Tremewan, J. (2010). Group Identity and Coalition Formation : Experiments in A Three-âĂŘplayer Divide the Dollar Game. *Working Paper*.

Van Huyck, J. B., Battalio, R. C., and Beil, R. O. (1990). Tacit Coordination Games, Strategic Uncertainty, and Coordination Failure. *American Economic Review*, 80(1):234–248.

Weber, R. A., Camerer, C. F., Rottenstreich, Y., and Knez, M. (2001). The Illusion of Leadership: Misattribution of Cause in Coordination Games. *Organization Science*, 12(5):582–598.

Weitzman, M. L. and Kruse, D. L. (1990). Profit Sharing and Productivity. In Blinder, A. S., editor, *Paying for Productivity: A Look At the Evidence*, pages 95–140. Brooking Institution, Washington, D.C.