SOCIAL PREFERENCES AND SOCIAL COMPARISONS

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ABSTRACT

Chapter 1 introduces the thesis providing an overview of the common themes and methods underlying this research.

Chapter 2 reports an experiment that examines the characteristics of effective leaders in a leader-follower voluntary contributions game. We focus on two factors: leaders' cooperativeness and their beliefs about followers' cooperativeness. We find that groups perform best when led by cooperatively inclined leaders, partly because they are intrinsically motivated to contribute more than non-cooperative leaders, partly because they are more optimistic about followers' cooperativeness.

Chapter 3 reports an experiment comparing sequential and simultaneous contributions to a public good in a quasi-linear two-person setting. As predicted, we find that overall provision may be lower under sequential than simultaneous contributions. However, we also find that the distribution of contributions is more equitable than predicted when the first-mover is predicted to free-ride, but not when the second-mover is predicted to free-ride. These results can be explained by second-movers' willingness to punish free-riders, and unwillingness to reward first-movers who contribute.

Chapter 4 investigates the impact of social comparisons on reciprocal relationships. Using a three-person gift-exchange game we study how employees' reciprocity towards an employer is affected by pay comparison information (information about what co-workers earn) and effort comparison information (information about how co-workers perform). We find that pay

comparison information does not affect reciprocity, while effort comparison information can influence reciprocal relationships in important ways.

Chapter 5 also examines the impact of pay comparisons on effort behaviour. We compare effort in a treatment where co-workers' wages are secret with effort in two 'public wages' treatments differing in whether co-workers' wages are chosen by an employer, or are fixed exogenously by the experimenter. We find that pay comparisons are detrimental for effort, particularly when co-workers' wages are exogenous.

Chapter 6 summarises the findings of this research and concludes.

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CHAPTER 1

INTRODUCTION

1.1 General Introduction

This thesis is a collection of four chapters reporting studies that contribute to different research areas in the field of behavioural and experimental economics. While each chapter is self-contained and can be read independently from the others, there are common themes underlying the research questions examined in these studies, as well as commonalities in the methodology and research strategies used to address these questions.

The research questions addressed in the four chapters of this thesis are inspired by considerations about the importance of 'social preferences' for understanding the outcomes of economic interactions among individuals. While standard economic theory relies on the simplifying assumption that all economic agents behave in accordance with their narrowly-defined self-interest, a large body of evidence gathered through suitably designed laboratory and field experiments have shown that a substantial fraction of individuals is systematically willing to act against their narrow self-interest in order to influence the well-being of others, to comply with principles of fairness and reciprocity, and to promote the wider interests of the group. A variety of models of social preferences have in turn been proposed to organise the mounting experimental evidence within the framework of rational choice (for recent overviews of the experimental evidence and of the theoretical approaches flourished to organise it, see, e.g., Camerer, 2003; Fehr and Schmidt, 2006). While differing in the characterization of the 'social' motives driving the behaviour of the not exclusively self-interested individuals, all these approaches emphasise the importance of taking into account the existence of social preferences for understanding how the interaction of economic agents can give rise to outcomes that are sometimes substantially different from those predicted by standard economic theory. The four studies reported in this thesis build on this lesson and set out to investigate human behaviour in decision settings where the presence of socially motivated individuals may challenge theoretical predictions about economic outcomes, or may make theoretically irrelevant features of the environment (e.g. payoff irrelevant information) become an important determinant of behaviour in practice.

All four chapters examine the effects of social preferences in settings where actors move in sequence. In one chapter we focus on first-movers' behaviour in a setting where collective interest and self-interest are in conflict, and explore the importance of first-movers' social motivation for successfully promoting socially efficient outcomes. In another chapter we examine a setting where standard economic theory predicts that sequential move orderings can be detrimental for efficiency. Since theory also predicts that an inequitable distribution of earnings among individuals arises in equilibrium, it is not clear that the predicted outcomes will be observed in practice in the presence of agents motivated by more than their narrow self-interest. In the remaining two chapters we focus on second-movers' behaviour in settings where they can observe how a first-mover treats other second-movers that are in a similar position, and how these other second-movers respond to the first-mover. We investigate how individuals' social motivation is affected by the availability of such pieces of comparison information.

All four chapters use novel laboratory experiments to address the proposed research questions. The use of the experimental method in empirical research is now well-established in economics (see Kagel and Roth, 1995, and Plott and Smith, 2008 for overviews of economic research using the experimental method). The general advantage of using laboratory data rather than happenstance data to answer the research questions proposed in our studies lies in the greater control over the data generating process which is guaranteed by the experimental method. In some of our studies (e.g. those examining how individuals' social motivation is influenced by comparison information about similar others) inference based on happenstance data would hardly be possible because of measurement problems that limit the identification of these effects in natural, uncontrolled situations (see, e.g., Manski, 1993). Laboratory experiments are often criticised on the ground that behaviour is studied in highly stylised, abstract environments which lack several important elements of natural social situations. While it is clear that the set of controlled conditions defining an experimental environment poses limits to the generalizability of the research findings to different environments, this should not be seen as a drawback of the methodology itself (see, e.g., Falk and Heckman, 2009). In fact, we see these considerations as a warrant for further experimentation to establish how research results obtained under a given set of experimental conditions extend to different conditions. Our two studies on the effects of comparison information on individuals' social motivation represent a case in point: while previous experiments have provided ample evidence of the importance of social preferences in bilateral interactions between individuals (see, e.g., Camerer, 2003 for a review), there is little research on how these findings extend to situations where subjects do not interact in isolated pairs and receive information about the behaviour or treatment of other parties in similar circumstances. The two laboratory experiments reported in this thesis are precisely motivated by these considerations and set out to examine how individuals' social motivation is affected in settings where the treatment and behaviour of others is observable, as it is in many natural social situations.

Chapter 2 and chapter 3 contribute to the experimental literature on the private provision of public goods in settings where agents contribute in sequence. Most of the previous experimental literature on public goods provision has focused on linear public goods settings where group members make simultaneous contributions to a public good (for an overview of the early literature see Ledyard, 1995; the more recent literature is surveyed, e.g., in Gächter and Herrmann, 2009). Building on the usual linear setup a recent literature in experimental economics has started examining whether the presence of a first-mover who can commit to an initial contribution level may successfully lead the group to higher degrees of cooperation relative to the simultaneous case (this literature is surveyed at some length in Koukoumelis et al., 2009). In chapter 2 we also use this type of setting, and focus on the importance of first-movers' cooperativeness for successfully fostering cooperation within their group. In chapter 3 we use a different setup that builds on Varian's (1994) theoretical model of sequential contributions to public goods. Differently from the usual setup, in this setting the availability of commitment opportunities is predicted to have a detrimental effect on public goods provision. Our laboratory experiment examines under which conditions we should expect the model predictions to hold in practice.

In chapter 4 and chapter 5 we contribute to the economic literature on the effects of social comparison processes, i.e. processes through which individuals use information about referent others to draw judgements about the self. Social comparison processes are a prominent notion in the social sciences. A seminal theoretical contribution is the study by social psychologist Leon Festinger (1954). A recent review of the current state of research on social comparisons in social psychology is provided by Buunk and Gibbons (2007). In economics social comparison processes are central in the theoretical and empirical literature stressing the importance of relative income as a determinant of individuals' utility (for a review see Clark et al., 2008). Social comparisons also feature prominently in recent models of social preferences in which the distribution of outcomes between the individual and relevant referent others is an important determinant of behaviour – e.g., Fehr and Schmidt (1999); Bolton and Ockenfels (2000); Falk and Fischbacher (2006). The focus in this thesis is on how the presence of social comparison information (specifically, information about how similar others are treated or behave in similar circumstances) affects individuals' willingness to forgo material well-being to respond favourably or unfavourably to more or less generous actions by others. While there exists abundant experimental evidence showing that a substantial fraction of individuals are willing to engage in costly rewarding / punishment of favourable / unfavourable actions by others (see, e.g., Camerer, 2003), little is known about whether - and if so how - the presence of social comparison information affects such socially motivated behaviours in these settings.

1.2 Outline of the Thesis

Chapter 2, entitled "*Who Makes a Good Leader? Cooperativeness, Optimism and Leading-by-Example*", uses a laboratory experiment to study a leader-follower contribution game where the interest of the individual is in contrast with the interest of the group: in fact, group earnings are maximized when players contribute their whole endowment to a public good, whereas narrowly-defined self-interest pulls each player to contribute nothing. Previous experiments with this type of setting have shown that individuals on average contribute positive amounts to the public good (although not sufficiently to maximize joint earnings), and that second-movers' contributions are typically positively related to first-movers' contributions: the higher is the leader's propensity to contribute to the public good, the higher is the follower's propensity to contribute as well. In this context an important question is then which characteristics should a leader possess in order to better promote the collective interest of the group. In this study we focus on two characteristics: the leader's cooperativeness, and the leader's optimism about the cooperativeness of the follower. We find that leaders who are cooperatively inclined are most successful in serving the interest of the group: they make the highest contributions to the public good and thus set a better example to followers than less cooperative leaders. Partly this is because they are intrinsically motivated to contribute higher amounts than less cooperative leaders, and partly because they are more optimistic that followers will follow their example.

Chapter 3, entitled "*Sequential versus Simultaneous Contributions to Public Goods: Experimental Evidence*", also studies a two-person contribution game where players make voluntary contributions to a public good. We use a setup based on Varian's (1994) contribution model in which players have quasi-linear and asymmetric returns from public/private good consumption. We study different versions of the contribution game where we vary the order of moves and the degree of asymmetry in players' returns from public/private good consumption. In all versions of the game standard theory predicts that one person free-rides off the other person's contributions in the unique (subgame perfect) equilibrium, and that this results in an inequitable distribution of earnings between players. In one version of the game standard theory predicts that a sequential contribution mechanism yields lower public good provision than a simultaneous mechanism: this is because in the sequential game a free-rider can exploit a first-mover advantage by committing to a low initial contribution and letting the other agent provide the public good on her own. Given the importance of social preferences in such settings (e.g. reciprocity), it is not clear that these theoretical predictions will hold in practice: will individuals accept equilibria characterised by inequitable distributions of earnings and contributions, or will they rather be willing to forgo their private interest to resist unfair outcomes? In our laboratory experiment we observe that outcomes are generally more equitable than predicted, especially when first-movers are predicted to free-ride: in this case, second-movers are often willing to forgo part of their material interest to punish free-riders. Outcomes are instead more in line with theoretical predictions when second-movers are predicted to free-ride. Here, equity requires a rewarding type of behaviour, and it appears that this is a much less effective mechanism for moving outcomes away from (inequitable) equilibrium predictions. The theoretical prediction that sequential mechanisms may damage public good provision is borne out in the data. However, we also find that first-movers do not have any advantage in committing to free-ride because of the punishments inflicted by second-movers: thus, it is questionable that (detrimental) sequential move orderings will emerge in practice in settings where individuals can choose whether to commit or not to initial contributions.

Chapter 4, entitled "*The Impact of Social Comparisons on Reciprocity*", reports a laboratory experiment that uses a three-person gift-exchange game where a first-mover (the 'employer') chooses the size of the gift ('wage') she wishes to make to two other players (the 'employees'). Employees learn the two wages that the employer has chosen, and then decide in sequence whether to engage in costly actions ('effort') that reward the employer. In our setting the second employee in the sequence (labelled 'Employee 2' in our laboratory experiment) can observe the effort chosen by the first employee (labelled 'Employee 1') as well as the wages paid by the employer, while Employee 1 only observes the two wages but not the effort of Employee 2. This setup allows us to study how receiving information about how similar others are treated (i.e. the wage the employer pays to the co-worker) and how similar others behave (i.e. the effort decisions of Employee 1) affects the extent to which employees are willing to engage in favourable actions towards the employer. We find that information about how others are treated has no effect on employees' willingness to expend effort for the employer. On the other hand, observing favourable behaviours on the part of the co-worker can strongly affect employees' willingness to reward the employer: when both employees receive a high wage, Employee 2 is willing to behave favourably towards the employer only if Employee 1 is also willing to do so.

Chapter 5, entitled "*The Impact of Pay Comparisons on Effort Behaviour*", reports a laboratory experiment which further explores whether information about how others are treated has an impact on individuals' behaviour. We use a three-person gift-exchange game similar to the one used in chapter 4, but where employees choose efforts simultaneously. Also differently from chapter 4, in this study we identify the effects of social comparison information by comparing a treatment where employees receive no information about the wage paid to their co-worker with treatments where the co-worker's wage is publicly observable. We find evidence that social comparison information can be harmful for effort provision: employees respond less favourably to a given own wage offer when they learn to be paired with a highly-paid co-worker relative to the case where co-workers' wages are not observable. The detrimental effects of social comparison information are amplified when the co-worker's wage is *not* chosen by the employer, but is fixed exogenously by the rules of the experiment, suggesting that exogenous interventions into groups dynamics (e.g. labour market regulations shaping firms' wage policies) may strengthen the importance of horizontal comparisons and their relevance for feelings of entitlement.

Chapter 6 concludes by summarizing the results of chapters 2 to 5, pointing out their limitations and suggesting directions for further research.

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CHAPTER 2

WHO MAKES A GOOD LEADER? COOPERATIVENESS, OPTIMISM AND LEADING-BY-EXAMPLE*

2.1 Introduction

One of the challenges facing leaders is how to get followers to do something they otherwise would not do. In settings where followers are tempted to free-ride on the contributions of others, the challenge is for leaders to somehow induce followers to eschew their narrowly-defined personal interests in order to promote the wider interests of the group. Such settings are commonplace in the workplace, and also in political and military organizations. One mechanism by which a leader may influence her followers is through leading-by-example. Recent experimental research has shown that followers respond strongly to the example set by a leader (Gächter and Renner (2003); Gächter and Renner (2007), Moxnes and van der Heijden (2003); Arbak and Villeval

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* This chapter is based on joint work with Simon Gächter, Elke Renner and Martin Sefton. A research paper companion to this chapter is forthcoming in *Economic Inquiry*.

(2007); Güth, et al. (2007); Levati, Sutter and van der Heijden (2007); Potters, Sefton and Vesterlund (2007); Kumru and Vesterlund (2008); Rivas and Sutter (2008); Pogrebna, et al. (2009)).

In this chapter we report an experiment on a simple leader-follower game in which efficiency and self-interested behaviour are in conflict. More specifically, we study a sequential voluntary contributions game where each player has an endowment and can choose how much of this to contribute to a project. Joint earnings are maximized when each player contributes their full endowment, but if subjects maximize own-earnings they will contribute nothing. We focus on the question of who makes the best leader, in terms of promoting efficient outcomes. We focus on two factors: the individual's cooperativeness, as measured by her willingness to contribute to the project when others do so, and the individual's beliefs about the cooperativeness of others.[1]

Previous experiments with this type of game show that subjects do make positive contributions, but, at the same time, contributions fall short of efficient levels. Moreover, there is substantial heterogeneity in decisions across subjects in both roles. Among followers, some maximize own earnings but others contribute substantial amounts. Moreover, followers' contributions are heavily influenced by leaders' contributions. In experiments with sequential prisoner's dilemmas second movers often cooperate if the first mover cooperates, but

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[1] Of course there are many other aspects of leadership that we do not address in this chapter. See Yukl (1989) for a comprehensive treatment. In natural settings the role of a leader may encompass a broad range of activities – coordinating and organizing efficient allocation of individual tasks, mediating conflicts, designing incentive schemes, disciplining deviators, maintaining group relations, etc. – and these activities may require different (psychological) qualities. See Van Vugt and De Cremer (2002) for a social psychological perspective on aspects of leadership in social dilemma situations.

hardly ever if the first mover defects (Clark and Sefton (2001)), and in experiments with sequential contributions to a public good followers' contributions tend to increase with leaders' contributions (Gächter and Renner (2003); Gächter and Renner (2007)). Thus, cooperative behaviour by followers is often described as evidence of reciprocation or conditional cooperation (Keser and van Winden (2000); Fischbacher, Gächter and Fehr (2001); Frey and Meier (2004); Croson (2007); Glöckner, et al. (*forthcoming*)).

The experiments also reveal variability in leaders' decisions. Some leaders contribute nothing, almost certainly leading the group toward the lowest possible joint earnings. Other leaders contribute large amounts. If matched with a conditional cooperator this leads to high joint earnings, but there is also the possibility of being suckered when matched with a self-interested player. Compared with followers' decisions, it is more difficult to interpret leaders' decisions. If a person contributes a lot in the role of leader is it because they are somehow cooperatively inclined, or simply because they are self-interested but optimistic about the prospects of meeting a cooperator? If a person contributes nothing is it because they are selfish, or are they cooperators who are pessimistic about the prospects of meeting another cooperator? And, what type of player is likely to set a better example as a leader?

To answer these questions we present a new experiment based on a leader-follower game in which contribution decisions were elicited using the strategy method and subjects played in both roles. Using decisions in the role of follower we are able to classify players according to their degree of conditional cooperativeness. Correlating these measures with their own decisions in the role of leader allows a within-subject cross-tabulation of leader-contribution against follower-type. Thus we are able to answer whether cooperators make better or worse leaders. In order to answer whether differences between leaders' decisions reflect differing underlying social motivations or differing expectations about the follower we also have subjects predict what their opponent will do in the role of a follower. These predictions are used to gauge how optimistic subjects are about the chances of meeting a cooperator, and we then ask whether leaders' decisions vary across followers' types controlling for this degree of optimism. That is, we ask whether optimistic cooperators or optimistic self-interested non-cooperators make better leaders.

From their decisions as follower we classify about half of our subjects as non-cooperative and about half as cooperative to some degree. In the role of leader we find that cooperators contribute substantially more than non-cooperators. Although several variables help predict leaders' decisions – for example economists contribute less than non-economists – the most useful variable for explaining leaders' contributions is their degree of (conditional) cooperativeness. We find that part (roughly, a quarter) of the difference between the leader-contributions of "Non-Cooperators" and "Strong Cooperators" can be attributed to their differing beliefs. These differences in beliefs are consistent with a *false consensus effect* (Ross, Greene and House (1977)). Non-Cooperators tend to expect they will be paired with another Non-Cooperator and thus contribute little, whereas Strong Cooperators are more optimistic about the prospect of being paired with another Strong Cooperator and so contribute more. Even so, after controlling for optimistic beliefs Strong Cooperator leaders still contribute substantially more than Non-Cooperator leaders. Thus, we conclude that differing leader-contributions by differing types of leader must in large part reflect social motivations. Groups perform best when led by those who are cooperatively inclined.

Our study is related to a number of experimental papers that explore how social preferences affect play in social dilemmas by examining the correlations between decisions in the role of first mover, decisions in the role of second mover, and beliefs about opponents, in games where subjects play both roles (Fischbacher, et al. (2001); Vyrastekova and Garikipati (2005); Chaudhuri and Gangadharan (2007); Blanco, Engelmann and Normann (2008); Altmann, Dohmen and Wibral (2008); Blanco, et al. (2009); Bruttel and Eisenkopf (2009)). We discuss this related literature in the conclusions.

The remainder of the chapter is organized as follows. In the next Section we describe our experimental design and procedures. Section 2.3 presents our results. We offer concluding comments in Section 2.4.

2.2 Experimental Design & Procedures

2.2.1 The Experimental Game

Our experiment is based on a simple two-player leader-follower game. Each player is endowed with 5 tokens, and must decide how many to contribute to a joint project. The Leader moves first and her contribution decision is revealed to the Follower before the Follower chooses her own contribution. After the Follower's choice, the game ends and players' earnings are determined. For each token contributed to the project both players receive £1, and for each token a player does not contribute to the project that player receives £1.50. Thus, player *i*'s earnings are given by:

$$\pi_i = 1.5 \times (5 - c_i) + c_i + c_j$$

where c_i, $c_j \in \{0, 1, 2, 3, 4, 5\}$ represent the contribution decisions of player i and j, for $i, j \in \{Leader, Follower\}$ and $i \neq j$.

In our experiment we implemented a one-shot version of this game and had subjects make decisions both in the role of Leader and Follower. Follower's decisions were elicited using the strategy method (Selten (1967)), i.e. they had to specify complete strategies in the game-theoretic sense. Thus, participants in our experiments were asked to make in total seven contribution decisions: one contribution decision in the role of Leader and six contribution decisions in the role of Follower, one for each possible contribution by the Leader. Only after all decisions had been made were subjects assigned a role in the experiment and paid according to the decisions they made in that role: with probability one half they were assigned the role of Leader and with probability one half the role of Follower. Hence, all seven contribution decisions were elicited using monetary incentives.

Subjects also had to complete a "Prediction Task". In this task subjects were asked to predict the contribution decisions that the other person in their group had made in the role of Follower. Thus subjects made six point predictions, one for each contribution decision their opponent made in the role of Follower. Subjects earned £0.50 for each correct prediction.

Immediately after having submitted their decisions, subjects were asked to complete a short post-experimental questionnaire asking for basic demographic and attitudinal information. This included a self-assessment of subjects' risk attitudes, which were elicited using the question suggested by Dohmen, et al. (*forthcoming*). The question reads: "*Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?*", and subjects answered on a scale from 1 (unwilling to take risks) to 10 (fully prepared to take risk).[2] To measure and control for inherent predisposition to self-interested behaviour we also employed the "*Machiavellian instrument*" (Christie and Geis (1970)), a psychometric test consisting of 20 statements – such as "*anyone who completely trusts anyone else is asking for trouble*" or "*it is hard to get ahead without cutting corners here and there*" – to which subjects were asked to agree or disagree using a 7-level Likert scale. Those who tend to agree with the statements score higher on the Machiavellian instrument, signalling a combination of selfishness, cynicism about human nature and manipulativeness.[3]

2.2.2 Discussion of the Design

Our main interest lies in exploring the relation between subjects' contributions in the role of Leader, their own cooperation preferences and their expectations about others' cooperation preferences. We measure subjects' cooperation preferences by the extent to which they are conditionally cooperative in their follower-contribution response to the Leader's contribution decisions. Note that the Follower's decision directly determines the distribution of earnings and thus provides a cleaner measure of cooperation preferences than the Leader's decision. It is possible that a Leader may contribute not because they are inherently cooperative, but rather because they expect a cooperative response

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[2] The average response was 6.05 (s.d. 2.10).

[3] Higher Machiavellian scores are generally associated with less generous offers in dictator games, but not in ultimatum games (see, e.g. Carpenter, Burks and Verhoogen (2005); Carpenter, Verhoogen and Burks (2005); Spitzer, et al. (2007)). Meyer (1992) shows that subjects scoring high on the Machiavellian instrument are less likely to reject unfair offers in a one-shot ultimatum game with hypothetical payoffs. Burks, Carpenter and Verhoogen (2003) find that subjects with higher Machiavellian scores send less in trust games, while Gunnthorsdottir, McCabe and Smith (2002) find that subjects with higher Machiavellian scores are less likely to reciprocate trust.

that makes contributing pay. In order to measure a Follower's degree of conditional cooperation we need to observe a follower's response to different possible contribution decisions by the Leader.[4] The use of the strategy method allows us to observe Followers' contribution responses conditional on *each* possible Leader's contribution decision without either resorting to repeated play (which might induce strategic confounds) or using deception. Thus, from each subject we elicit in an incentive compatible way a complete vector of conditional contribution decisions that we then use to classify subjects into "cooperation types" according to their revealed (conditional) cooperativeness.

Letting subjects play in both roles of the game allows us to correlate subjects' cooperativeness (measured, as explained, by their conditional contribution decisions) with their (unconditional) contribution decision in the role of Leader. Thus, we can observe a within-subject cross-tabulation of Leader's contribution against Follower's cooperation types that allows us to explore whether more cooperative types make better or worse Leaders.

Since we are also interested in how subjects' beliefs about others' cooperation preferences relate to their leader-contributions and their cooperation types, we also have subjects predict what their opponent will do in the role of Follower. That is, from each subject we elicit a vector of *predicted* conditional contribution decisions. This allows us to measure how optimistic subjects are about the cooperativeness of the players they are matched with. Subjects were given monetary incentives for correctly predicting others'

[4] For example, observing a Follower that contributes zero tokens in response to a Leader's contribution of zero tokens does not reveal whether the subject is a conditional cooperator (and hence responds with low contributions to low leader-contributions) or whether he or she is motivated by own-profit maximization. What we need to observe is the Follower's contributions in different subgames.

contributions and could earn up to £3 from the prediction task. Note that this gives subjects an incentive to predict the most likely response to each possible leader-contribution, rather than report their subjective probability distributions over possible responses. In order to elicit subjective probability distributions over possible responses in an incentive compatible manner subjects would have had to complete a 6x6 matrix, and we would have had to use a different scoring rule. This of course would only be incentive-compatible to the extent that subjects understand the mechanism. Our simpler Prediction Task has the advantage that it yields an operational measure of optimism while keeping the task manageable for subjects.

Decisions in the role of Leader and Follower and beliefs were elicited using a single computer screen so that subjects could make and revise their choices in any order they liked.[5] This design choice was motivated by a desire to avoid potential ordering effects which could have arisen had we prompted subjects to complete the three experimental tasks in a predetermined order.

2.2.3 Experimental Procedures

The experiment was conducted at the University of Nottingham in autumn 2008 using subjects recruited from a university-wide pool of students who had previously indicated their willingness to be paid volunteers in decision-making experiments.[6] Six sessions were conducted, four with 18 participants, one with 16 participants and one with 14 participants: thus, 102 subjects participated in total.

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[5] A screenshot of the computer screen used to elicit subjects' decisions and beliefs was included in the instructions that were given to subjects, which are reproduced in Appendix A at the end of the chapter.

[6] Subjects were recruited through the online recruitment system ORSEE (Greiner (2004)). The experiment was programmed and conducted with the software z-Tree (Fischbacher (2007)).

The average age was 19.7 years and 55% were male. No subject took part in more than one session.

All sessions used an identical protocol. Upon arrival, subjects were welcomed and randomly seated at visually separated computer terminals. Subjects were then given a written set of instructions that the experimenter read aloud. The instructions included a set of control questions about how choices translated into earnings. Subjects had to answer all the questions correctly before the experiment could continue. The instructions also included a screenshot of the screen on which subjects entered their decisions. The instructions did not use the labels "Leader" and "Follower", but rather referred to "First Movers" and "Second Movers".

The decision-making phase of the session consisted of three tasks: two decision tasks and the prediction task. In the two decision tasks subjects were asked to make contribution decisions both in the role of Leader and in the role of Follower. Subjects were informed at the beginning of the experiment that they would have had to make contribution decisions in both roles and that only after all decisions had been made would they have been informed of their actual role. All decisions were made anonymously, and neither during nor after the experiment were subjects informed about the identity of the other person in their group. Once everyone in the room had completed the three tasks subjects were informed of their role. Decisions and predictions were then implemented and subjects paid accordingly.

With our design players' earnings can range from a maximum of £15.50 (£12.50 if a player contributes 0 tokens to the joint project while her opponent contributes 5 tokens, plus £3 if she reports 6 correct predictions) to a minimum of £5. In the experiment, subjects' earnings ranged from £6.00 to £15.00, averaging £9.39 (at the time of the experiment £1 ≈ $1.50). On average the experimental sessions lasted about 50 minutes, including the completion of a post-experimental questionnaire and the payments.

2.3 Results

The following analysis of data is structured around our main research questions: What type of player makes the best Leader? And, do different leader-contributions reflect differing underlying social motivations, or differing expectations about the Follower? To explore these questions:

i. We first classify subjects' cooperativeness according to the degree of conditional cooperativeness exhibited by their contribution responses in the role of Follower.

ii. We then explore the relation between subjects' cooperativeness and their (unconditional) contribution decisions in the role of Leader. Thus we will be able to answer whether more cooperative types make better or worse Leaders.

iii. In a third step, we ask whether cooperativeness is systematically related to beliefs about the cooperativeness of others and whether differences in beliefs are related to differences in Leaders' unconditional contribution decisions.

2.3.1 Expressed Cooperation Preferences and Cooperation Types

We measure subjects' cooperativeness using their conditional contribution decisions in the role of Follower. Subjects are classified as **Non-Cooperators (NC)** if they contribute nothing in the role of Follower irrespective of the

Leader's contribution. Forty-six percent of our 102 subjects fall into the NC category. The remaining subjects are classified into three different cooperation types according to the following criterion. For each subject we computed how a (hypothetical) self-interested Leader would best-respond to the vector of conditional contribution decisions submitted by this subject.[7] If even a self-interested Leader would contribute her entire endowment as a best-response to the subject's vector of conditional contributions, we conclude that the subject must exhibit a strong degree of conditional cooperativeness. We classify such a subject as a **Strong Cooperator (SC)**. Twenty-six percent of our subjects fall into this category. A subject is classified as a **Weak Cooperator (WC)** if, when matched with her, a self-interested Leader would find it optimal to contribute some, but not all, of her tokens. Twenty-two percent of the subjects can be classified as WC. Finally, if a subject submitted a vector of contributions that contains positive contributions in response to some of the Leader's contributions, but does not give an incentive to a self-interested Leader to contribute any token to the project, we classify the subject in the category **Other**. Only six percent of subjects fall into this category.[8]

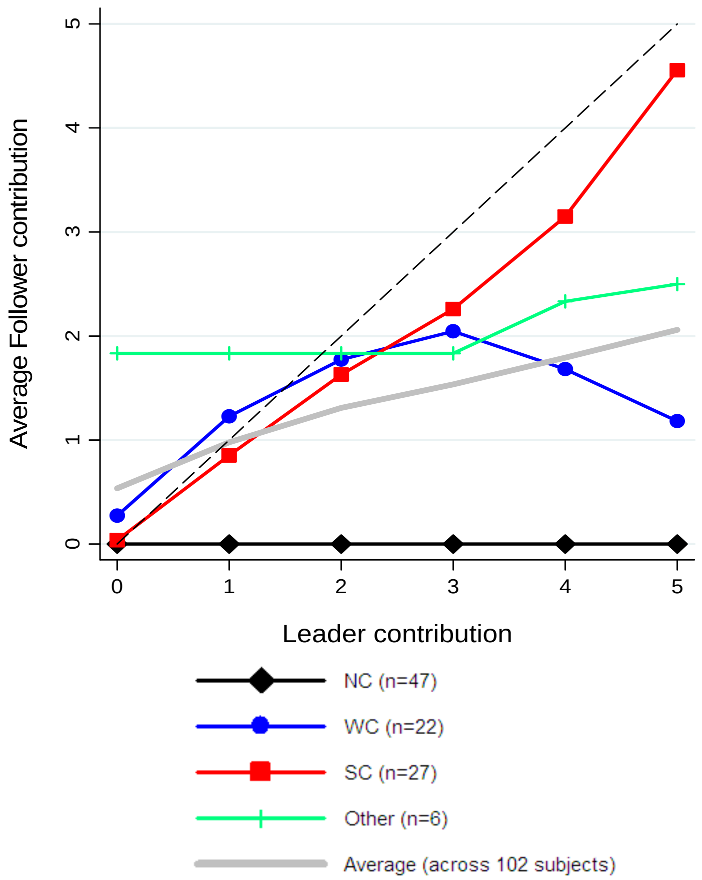
Figure 2.1 depicts – both separately for each preference type and aggregated across types – the average contribution decisions subjects made in the role of Follower as a function of the contribution level by the Leader.[9]

[7] Should the Leader be indifferent between two or more contribution decisions, the largest contribution is used for computing the Leader's best-response.

[8] Half of these subjects are "unconditional co-operators" who contribute the same (non-zero) amount irrespective of the Leader's contribution. The other half contributes 1 or 2 tokens only if the Leader contributes 4 or 5 tokens.

[9] Note that the patterns of contribution decisions of NC, SC, WC and Other closely resemble the average contribution patterns typically found in linear public goods games for "Free Rider", "Conditional Cooperator", "Hump-shaped" and "Other" according to the classification system introduced by Fischbacher, et al. (2001). In fact the two classification systems are highly

Figure 2.1 Average Follower Contribution



2.3.2 Which Cooperation Type Makes a Better Leader?

We next move to the analysis of the relation between subjects' cooperativeness and their contribution decisions as Leaders. Figure 2.2 plots the average leader-contribution decisions separately for the three major preference types.[10] SC Leaders are those who contribute most to the public good (about 2.8 tokens on average), while NC Leaders contribute on average least (slightly more than 0.5 tokens on average). WC Leaders' contribution decisions fall midway between the contributions of NC and SC Leaders.

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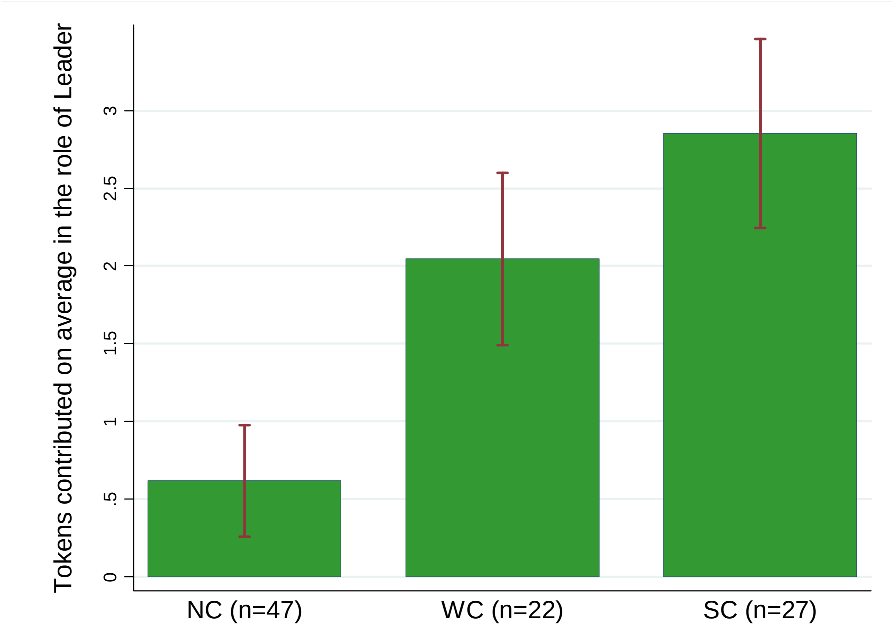
consistent with one another: all NC and Other would be classified as Free Riders and Other respectively, 85% of SC subjects as Conditional Cooperators, and 64% of WC subjects as Hump-shaped Contributors.

[10] In the remainder of the chapter we will focus our analysis on the three major groups and ignore the subjects we classified as Other. With only six subjects in the Other category we would not be able to draw any valid inference from their behaviour and their inclusion in the analysis would only unnecessarily complicate the exposition of our results. All our findings are robust to whether we include or exclude these six subjects.

Figure 2.2 Average Contribution in the Role of Leader*



* Lines represent 95% confidence intervals.

We can strongly reject the hypothesis that types contribute similar amounts (Kruskal-Wallis test: $\chi^2(2\,d.f.) = 38.65,\ \ p < 0.001$). Pair-wise two-sided Mann-Whitney-U-tests reveal that NC Leaders' contributions differ significantly from WC Leaders' contributions ($z = 4.575, p < 0.001$), which in turn differ significantly from SC Leaders' contributions ($z = 2.065, p = 0.039$).

Regression analysis of Leaders' contributions on a set of dummy variables identifying the three Leader's types shows that the results are substantially robust to a set of controls for individual and session effects (Table 2.1).[11] Models I to III build incrementally including personal characteristics (Models II and III) and controls for session effects (Model III). SC Leaders'

[11] Long (1997) (pp. 115-119) discusses the costs and benefits of using a linear regression model (LRM) instead of ordered regression models (ORM) when using ordinal dependent variables and concludes that in general "*…the results of the LRM only correspond to those of the ORM if* [the cut-points of an ORM] *are all about the same distance apart*" (p. 119), i.e. if the intervals between adjacent categories of the dependent variable are equal, which is in fact the case for the variable "Leader's contribution". Given their simpler interpretation, OLS estimates are reported hereafter. Any inference based on such estimates can be also derived using ORM estimation.

contributions exceed NC Leaders' contributions by about two tokens in all regression models and the difference is always significant at the 1% level. WC Leaders also contribute about 1 token more than NC Leaders and the difference is highly significant in all models. Differences between the contributions of WC and SC Leaders are significant either at the 5% or 10% level depending on the regression model specification.[12]

Table 2.1. Determinants of Leader Contributions

	I	II	III
1 if SC	2.235^{***} *(0.345)*	2.078^{***} *(0.364)*	1.928^{***} *(0.384)*
1 if WC	1.428^{***} *(0.320)*	1.400^{***} *(0.286)*	1.187^{***} *(0.324)*
1 if Male		0.287 *(0.284)*	0.401 *(0.301)*
1 if Area of Study is Economics		-0.841^{***} *(0.263)*	-0.876^{***} *(0.287)*
Willingness to Take Risks		-0.010 *(0.077)*	0.001 *(0.081)*
Machiavellian score		-0.021^{**} *(0.010)*	-0.021^{*} *(0.010)*
Number of Known Others in Session			-0.507^{**} *(0.237)*
Constant	0.617^{***} *(0.180)*	2.867^{***} *(1.323)*	2.882^{**} *(1.386)*
Session dummies	*No*	*No*	*Yes*
N.	*96*	*96*	*96*
F-statistic	*F(2,93) = 24.55*	*F(6,89) = 16.16*	*F(12,83) = 10.60*
Prob > F	*0.000*	*0.000*	*0.000*
R^2:	*0.360*	*0.447*	*0.489*

OLS regressions. Dependent variable is Leader's contribution. Robust standard errors in parentheses. * $.05 \le p \le .10$; ** $.01 \le p < .05$; *** $p < .01$.

Among the variables controlling for individual characteristics, the dummy identifying subjects studying Economics is highly significant in both the regression models where it is included: Leaders who study Economics appear to contribute significantly less than others. This result is consistent with findings from other laboratory experiments (e.g. Marwell and Ames (1981);

[12] The p-values from the F-test for equality of coefficients on SC and WC are 0.045 (Model I), 0.069 (Model II), and 0.051 (Model III).

Frank, Gilovich and Regan (1993)), although there is an ongoing debate about the reasons for these differences in other-regarding behaviour (see, e.g., Frey and Meier (2003)).

Also important is the 'Machiavellianism' of the subject. The coefficient of the Machiavellian score (Christie and Geis (1970)) – a psychometric test where higher scores signal a combination of selfishness and opportunism – is negative and statistically significant in both models: Leaders with high Machiavellian scores tend to contribute less than those who score low in Machiavellianism. This result is consistent with Burks, et al. (2003) who also find that first movers with a high Machiavellian score send less in a trust game where subjects played both roles.[13]

We do not observe a clear gender effect. The regressions show that, after controlling for cooperativeness, males contribute more than females, although the difference is insignificant. Arbak and Villeval (2007) report a similar finding. This result compares also with findings on first-mover's behaviour in trust games where men are sometimes found to send larger amounts than women (e.g. Buchan, Croson and Solnick (2008)) but the effect is often not significant (e.g. Croson and Buchan (1999)).[14]

As commitment to a leadership contribution is a risky decision we may expect that Leaders who are more willing to take risks contribute more than those who are less prepared to make risky decisions. Our measurement of

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[13] Across the whole sample scores ranged from 67 to 136. The average score was 98.92 with a standard deviation of 14.11,which is similar to that reported in other experimental studies (e.g. Gunnthorsdottir, et al. (2002); Burks, et al. (2003); Carpenter, et al. (2005); Flues and Gächter (2008)).

[14] See Bohnet (2007) for a discussion of gender effects in trusting behaviour. Croson and Gneezy (2009) provide a general review of gender effects in experiments.

subjects' willingness to take risks is instead negatively correlated with leader-contributions in Model II, while it enters with a positive coefficient in Model III. In both cases the effect is not statistically significant ($p = 0.896$ in Model II; $p = 0.992$ in Model III), suggesting that risk considerations are unimportant for leader decisions in our experiment.

Model III includes session dummies (which are jointly insignificant) and the variable "Number of Known Others in Session" measuring the number of other participants in the session known to the subject. Although the overwhelming majority of participants were strangers to one another (on average a participant only knew 0.12 other participants), knowing other participants in the session reduces the amount a Leader is willing to contribute.

To get a sense of the importance of assigning given cooperation types to the role of Leader we conducted a simple accounting exercise. For every possible pairing of subjects we calculated total contributions for both possible role assignments. We present the average of these by cooperation type combination in Table 2.2. For example, on average an SC Leader and NC Follower make a total contribution of 2.85 tokens. For a given Follower type contributions increase with the cooperativeness of the Leader, and for a given Leader type contributions increase with the cooperativeness of the Follower (with one exception: when an NC Leader is paired with a WC Follower contributions are higher than when paired with an SC Follower). Note also that when types differ, contributions are always higher when the more cooperative type occupies the role of Leader. Using the observed distribution of cooperation types we also compute the expected total contribution for each

Leader cooperation type. SC Leaders generate more than 4 times as many contributions as NC Leaders.

Table 2.2. Total Contributions by Cooperation Types

Type of Leader	Type of Follower			
	NC (n = 47)	WC (n = 22)	SC (n = 27)	Expected*
NC	0.63	1.24	1.17	0.92
WC	2.05	3.66	3.70	2.87
SC	2.85	4.40	5.32	3.89

* The expected total contribution takes into account that a subject cannot be matched with oneself. For instance, for an NC Leader the expected total contribution is calculated as $(0.63 \times 46/95) + (1.24 \times 22/95) + (1.17 \times 27/95)$.

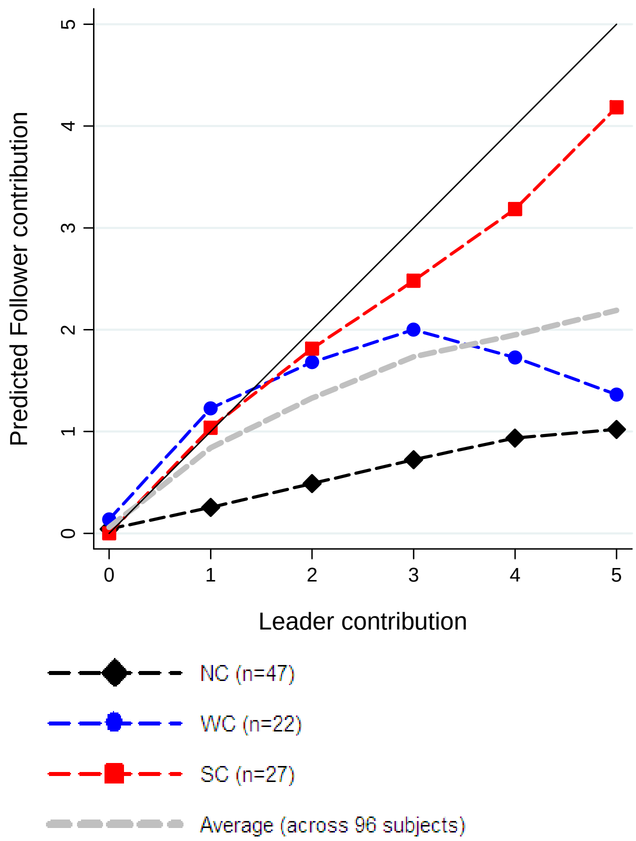
2.3.3 Are Strong Cooperators Better Leaders Because They Are More Optimistic About Followers?

So far we have shown that cooperation preferences, as measured by conditional contribution decisions, strongly correlate with leader-contributions: cooperative Leaders contribute significantly more than non-cooperative Leaders.

However, the large difference in leader-contribution decisions between SC, WC and NC subjects observed in our experiment may not necessarily be due to differences in the underlying social motivations of these three types. SC, WC and NC subjects may instead hold different expectations about the Follower's behaviour, which may in turn drive their contribution decisions. For example, NC subjects may believe that Followers are more likely to behave as a free-riding "NC type", while SC subjects may believe that free-riding behaviour is relatively less common and hence may expect a positive return from contributing to the project. Such a systematic bias in beliefs (and in particular the tendency to estimate one's own behaviour to be more common than it is estimated by those who engage in alternative behaviours) is called the *false consensus effect* (Ross, et al. (1977)).[15]

To verify whether a false consensus effect might be driving our results, we start by exploring the relation between subjects' own preferences and their expectations about the cooperation preferences of their opponents, as elicited in the prediction task. As a first step in Figure 2.3 we draw – both separately for each preference type and aggregated across types – the average conditional contribution decisions that subjects *predicted* the other person in their group would have made in the role of Follower.

Figure 2.3 Average Predicted Follower Contribution



[15]In the context of cooperation a seminal paper is Kelley and Stahelski (1970). Recent experimental studies finding evidence of a false consensus effect are Selten and Ockenfels (1998), Charness and Grosskopf (2001) and van der Heijden, Nelissen and Potters (2007). Engelmann and Strobel (2000) and Engelmann and Strobel (2007) discuss whether the consensus effect is "truly" false and show that the bias mitigates with the presentation of representative information (see also Offerman, Sonnemans and Schram (1996)).

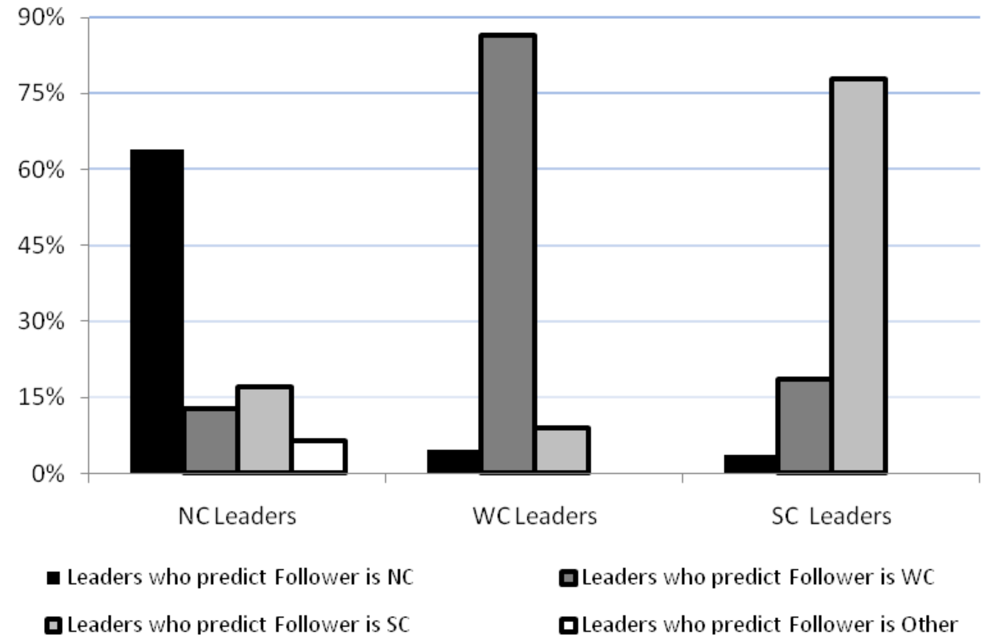
The most remarkable feature of Figure 2.3 is its similarity with Figure 2.1, where we depicted subjects' average own contribution decisions by cooperation type. SC and WC contribution decisions are almost identical to their beliefs about others' contribution decisions. NC subjects' predictions of others' contribution decisions differ instead from their own contribution decisions, as these subjects seem to believe that others' contributions increase in the Leader's contribution decisions whereas they always choose to contribute nothing irrespective of the Leader's decision.

Overall, Figure 2.3 suggests that different cooperation types hold different beliefs about others' cooperation types. To explore this issue further, we use subjects' predictions about their opponent's conditional contributions to classify subjects according to their predicted Follower type. Our classification method parallels the one we used to classify cooperation types. If a subject predicts that the opponent will contribute nothing to the project irrespective of the Leader's contribution we classify that subject as having a predicted Non-Cooperator Follower.[16] If a subject predicts that the Follower will contribute something in response to some contribution by the Leader, we classify the subject as having a predicted Other, Weak Cooperator, or Strong Cooperator Follower depending on whether a risk-neutral selfish Leader's optimal choice

[16] One might worry that subjects may report biased beliefs in the Prediction Task in order to hedge against risk. For example, a Leader who contributes five tokens will receive a low payoff if the Follower contributes zero. Even if the Leader expects the Follower to reciprocate he may predict the Follower will contribute zero in order to insure against the worst possible case. If this were indeed the case, Leaders who contribute more would report more pessimistic beliefs about the Follower's cooperativeness. In fact, Figure 2.3 suggests the opposite: more cooperative Leaders predict higher contributions by the Follower. More generally, we note that there is very limited evidence of hedging in sequential prisoner's dilemma experiments when first-movers predict second-mover's choices (see, e.g., Blanco, et al. (2010)).

would be to contribute zero, some, or all of her endowment to the project.[17]. Figure 2.4 shows – separately for each preference type – the proportion of Leaders who predict an NC, WC, SC or Other Follower.

Figure 2.4 Proportions of Predicted Follower Type by each Type of Leader



Clearly, subjects' predictions about others' preferences are strongly biased towards their own preference type: more than 60% of NC Leaders predict that they are matched with a NC Follower, more than 80% of WC Leaders predict they are matched with a WC Follower, whereas almost 80% of SC Leaders predict that the person they are matched with is also a SC type. We can strongly reject the hypothesis that the distribution of predicted cooperation

[17] We thought it natural to convert the vector of predictions into a type using the same method as that used to convert the vector of Follower choices into a cooperation type. However, the predicted Follower type labels should interpreted with caution. The optimal contribution of a risk neutral selfish Leader depends on the *expected* responses to the six possible leader-contributions and, as noted previously, our belief elicitation procedure gives subjects an incentive to reveal the *most likely* response to each leader-contribution. Thus a risk-neutral selfish Leader may not necessarily find it optimal to contribute five tokens against a predicted SC Follower type.

types is the same across the three Leader types: $\chi^2(6\,d.f.) = 81.11, p < 0.001$. Pair-wise Fisher exact tests performed separately for each preference type are all significant at the 1% level. Thus, subjects' predictions about others' preference types are consistent with a false consensus effect.[18]

Our next step is to explore whether such a false consensus effect is actually driving our results about differences in leader-contributions across preference types. It may be that differences in cooperation preferences are not the reason why SC subjects contribute more than NC subjects in the role of leader. Instead, SC subjects may choose to make larger leader-contributions than NC subjects because they are more optimistic that Followers will respond with positive contributions. If this is in fact the case, we would then expect that, for a *given* belief about the opponent's type, leader-contributions would not be significantly different across Leader's preference types.

To explore this issue we augment our regression analysis of Leaders' contributions developed in Models I to III and reported in Table 2.1 with the variable "Degree of Optimism". This variable measures the Leader's best-response to his or her own predictions about the opponent's conditional contribution decisions. The higher the Leader's best-response to his or her own beliefs, the more optimistic he or she is about the cooperativeness of their Follower: the most optimistic Leaders are those whose best-response is to

[18] One may argue that such a strong bias towards the own preference type may be due to the fact that subjects do not report their beliefs truthfully but rather in a way that satisfies the need to see oneself behaving "as others do" and hence behaving appropriately. In fact, one potential explanation for the false consensus effect is based on such a "motivational" mechanism. However, if this is the case, one would also expect the bias to disappear or to be mitigated in the presence of financial incentives and to be stronger for answers to questions about socially desirable/undesirable activities. However, the false consensus effect has also been reproduced in the presence of monetary incentives, as in the present experiment. Moreover, a false consensus effect has been found also in studies employing morally neutral questions (see, e.g., Engelmann and Strobel (2000); Engelmann and Strobel (2007)).

contribute 5 tokens to the joint project because, as explained above, these Leaders predict that the Follower is a Strong Cooperator. The least optimistic Leaders are those whose best-response is to contribute nothing: these subjects predict that they are matched either with a Non-Cooperator or with a Follower that belongs to the category Other. Leaders whose best-response range from 1 to 4 predict that they are matched with a Weak Cooperator.[19]

The results of the regressions are shown in Table 2.3.[20] The variable "Degree of Optimism" is significant and positive in all three models: consistent with a belief-based explanation of differing leader-contributions across types Leaders who are more optimistic about the cooperativeness of their Follower make higher contributions. Nevertheless, for a *given* degree of optimism, WC Leaders still contribute about one token more than NC Leaders, and SC Leaders contribute about 1 ½ tokens more than NC Leaders (both coefficients are significantly different from zero at the 1% or 5% level). Hence, Leaders with the same degree of optimism do make different contributions depending on their preference type.

Our controls for individual characteristics and session effects substantially reproduce the same pattern of results observed in the regressions reported in Table 2.1: Leaders studying Economics contribute significantly less than others, as do Leaders with high Machiavellian scores, as do Leaders who know more other participants in the session. Session dummies, included in

[19] Again, the caveat noted in footnote 17 applies.

[20] Because Leaders' type and degree of optimism are correlated it may be difficult to identify the contribution of each factor to leader-contributions if there is a collinearity problem. However, checks for multicollinearity (based on variance inflation factor values) suggest that this is not the case for the Models reported in Table 2.3.

Model III, are jointly insignificant, as is the dummy controlling for gender and our measure of subjects' risk attitudes.

Overall, these results show that Leaders' expectations about their opponent's preference type are systematically biased towards their own preference type (i.e. they are influenced by a false consensus effect). However, the large differences in leader-contribution decisions between SC, WC and NC subjects that we observed in our experiment cannot be entirely explained in terms of systematic differences in expectations about others' cooperation preferences, because for a *given* belief about the Follower's cooperativeness, leader-contributions are still significantly different across Leader's preference types.

Table 2.3. Leader Contribution and Degree of Optimism

	I	II	III
1 if SC	1.712***	1.503***	1.427***
	(0.497)	*(0.462)*	*(0.524)*
1 if WC	1.228***	1.193***	0.966**
	(0.344)	*(0.295)*	*(0.375)*
Degree of Optimism	0.167*	0.175**	0.159*
	(0.097)	*(0.084)*	*(0.095)*
1 if Male		0.265	0.349
		(0.266)	*(0.280)*
1 if Area of Study is Economics		-0.913***	-0.925***
		(0.265)	*(0.286)*
Willingness to take risks		-0.023	-0.011
		(0.078)	*(0.084)*
Machiavellian score		-0.019*	-0.019*
		(0.010)	*(0.011)*
Number of Known Others in Session			-0.417*
			(0.224)
Constant	0.414**	2.594*	2.523*
	(0.175)	*(1.345)*	*(1.465)*
Session dummies	*No*	*No*	*Yes*
N.	*96*	*96*	*96*
F-statistic	*F(3,92) = 20.16*	*F(7,88) = 14.30*	*F(13,82) = 10.54*
Prob > F	*0.000*	*0.000*	*0.000*
R^2:	*0.390*	*0.479*	*0.510*

OLS regressions. Dependent variable is Leader's contribution. Robust standard errors in parentheses. * $.05 \le p \le .10$; ** $.01 \le p < .05$; *** $p < .01$.

2.4 Discussion & Conclusions

We examine how cooperativeness and beliefs about the cooperativeness of others affect leadership contributions in a simple leader-follower game. The game uses the same type of earnings functions used in experiments examining voluntary contributions to a public good. Thus, a Follower's contribution increases group earnings at the expense of the Follower's narrow personal interests. Our experiment allows Leaders to attempt to induce such group-oriented behaviour through "leading-by-example": by contributing Leaders might, if the Follower is sufficiently conditionally cooperative, induce the Follower to contribute as well. Our focus is on the extent to which the Leader's willingness to lead-by-example depends on her own cooperation preferences, her beliefs about the cooperation preferences of her Follower, and other personal characteristics.

As in previous experiments we find that many Followers are conditionally cooperative and are willing to reciprocate the Leader's contribution. About half of our subjects exhibit a degree of conditional cooperativeness such that it pays for a self-interested Leader to contribute something, and about half of these cooperators are classified as "Strong Cooperators", as they are conditionally cooperative to the extent that a self-interested Leader should contribute her entire endowment. These cooperation preferences are strongly correlated with (unconditional) Leaders' decisions. For example Strong Cooperator Leaders contributed around 57% of their endowments, significantly more than Non-Cooperator Leaders who contributed only 12% of their endowments. Part of this effect can be explained by subjects' personal characteristics. Economists contribute less as Leaders, as do those who are more ‘Machiavellian’. However, even after controlling for these personal characteristics, Strong Cooperator Leaders contribute 40% more of their endowments than Non-Cooperator Leaders.

Our finding that Strong Cooperators make higher leader-contributions than Non-Cooperators is in line with recent studies from trust and sequential social dilemma games where subjects play both roles. For example, Altmann, et al. (2008) and Chaudhuri and Gangadharan (2007) both find that trustees who reciprocate more are more trusting in trust games. We see two main differences between trust games and our leader-follower game. First, our focus on leading-by-example has guided our choice of a game where the leader and follower have identical choice sets and earnings functions, and so the leader’s decision can be easily viewed as an “example” to the follower. In the trust game there is an asymmetry between roles that goes beyond the sequential structuring of choices, and this asymmetry makes it less clear that the trustor can “lead-by-example”. Second, in our game the follower’s decision affects group earnings, whereas in a trust game the second-mover’s decision is a pure transfer, only affecting the distribution of group earnings.

Altmann, et al. (2008) speculate that a false consensus effect, whereby selfish subjects believe others are selfish and reciprocal subjects believe others are reciprocal, could explain why reciprocal trustees trust more in their experiment. The same effect may also explain why Strong Cooperators make higher leader-contributions in our experiment – they may be more optimistic about the cooperativeness of Followers. Similarly, this could explain the positive correlation between decisions as first-mover and second-mover reported in sequential social dilemma game experiments where subjects play both roles (see, e.g., Fischbacher, et al. (2001); Blanco, Engelmann and Normann (2008); Bruttel and Eisenkopf (2009)). Differently from these studies, our design allows us to control for differences in beliefs, and we do indeed find a strong correlation between own cooperation preferences and beliefs about the cooperation preferences of others: Strong Cooperators are more optimistic than Non-Cooperators about the chances of being paired with another Strong Cooperator. However, even after controlling for optimism, Strong Cooperators contribute about 30% more of their endowment than Non-Cooperator Leaders. Thus, differing degrees of optimism can only explain part of the difference between the leadership contributions of Non-Cooperators and Strong Cooperators and most of the difference reflects their differing social motivations.

Our findings are comparable with those reported in a recent experiment by Blanco, et al. (2009). Their extensive design uses several treatments to examine the relationship between cooperativeness and beliefs in a sequential prisoner's dilemma game experiment where subjects play both roles. In their Baseline treatment they do not elicit beliefs about second-movers' cooperativeness, and find, like the studies cited above, that a majority of subjects make the same choice as a first-mover and as a second-mover. The correlation between first- and second-mover's decisions persists in a second treatment where, as in our experiment, they elicit subjects' beliefs about second-movers' cooperativeness. They find that beliefs about second movers' cooperativeness are positively related to subjects' own cooperativeness, which is consistent with a consensus effect. In a third treatment, Blanco, et al. (2009) provide subjects with feedback about the true distribution of second-movers' cooperativeness before eliciting their first-mover decisions. They find that the correlation between first-mover's and second-mover's decisions persists even when accurate feedback about second-movers' cooperativeness is provided, suggesting that a consensus effect can only provide a partial explanation for the positive correlation between first-mover's and second-mover's decisions observed in their experiments.[21] Our findings are also consistent with those of Vyrastekova and Garikipati (2005): using a trust game, they correlate trustors' decisions with their beliefs about the trustee's decision and with their distributional preferences as measured using the "*Decomposed Game* technique", an instrument developed by sociologists and social psychologists to assess individual value orientations (see, e.g., Liebrand, et al. (1986)). They find a strong relation between distributional preferences and trustors' decisions even after controlling for beliefs. They also find that beliefs are strongly correlated with distributional preferences.

We only address a narrow aspect of leadership: leading-by-example. Nevertheless our results are suggestive that effective leadership will depend on the Leader's cooperative preferences and beliefs. To the extent that a large part of the variation in Leaders' contributions can be explained by cooperation preferences, even after controlling for optimism, this suggests that groups may perform better when led by individuals who are willing to sacrifice personal benefit for the greater good. Further, since beliefs are highly correlated with cooperation preferences, such individuals are more likely to have optimistic views about Followers that will reinforce their propensity to contribute. While

[21] Note that their subjects are classified as cooperative or not according to whether, as second-mover, they cooperate or defect in response to cooperate. Our second-movers fill in a contribution schedule indicating how may tokens they contribute (up to 5) for each possible contribution decision by the first-mover (again, a number from 0 to 5). We measure a subject's degree of cooperativeness based on the slope of this contribution schedule.

non-cooperative Leaders could, in principle, do anything that an optimistic cooperator does, their cooperation preferences and expectations about others may make them less likely to provide effective leadership.

A natural question that follows from our findings and that may be particularly relevant in settings that involve repeated interactions is whether it is more beneficial for the group that the most cooperative individuals set an example by committing to an initial contribution, or whether it can be better to have other, less cooperative individuals move first and let strong cooperators observe their contributions and discipline them. Rivas and Sutter (2008) report on an experiment where they let leaders move after other subjects have made a contribution and find that this does not affect cooperation rates in a simple public good setting. However, they do not selectively choose the most cooperative types as leaders in their experiment. Moreover, leaders in their study can only discipline first-movers through their own contribution decisions. An interesting development, which we leave for further investigation, would be to assess how cooperativeness is affected when second-movers are given some form of sanctioning or rewarding power such that they can effectively discipline early contributors' behaviour.

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Appendix A: Experimental Instructions

Instructions

General

Welcome! You are about to take part in a decision-making experiment. It is important that you do not talk to any of the other participants until the experiment is over. If you have a question at any time, raise your hand and a monitor will come to your desk to answer it.

In this experiment you will be randomly paired with another person in this room. The amount you earn will depend on your decisions and the decisions of the person you are paired with. You will not learn the identity of the person you are paired with, neither during nor after today's session. You will be paid in private and in cash at the end of the experiment.

The Basic Decision Situation

The basic decision situation is simple. In each pair one person is designated as the "FIRST MOVER" and the other as the "SECOND MOVER". Each person is endowed with five tokens. The FIRST MOVER first decides how many of his or her tokens to contribute to a joint project. The SECOND MOVER is then informed of the FIRST MOVER's decision. The SECOND MOVER then decides how many of his or her tokens to contribute to the project.

Earnings depend on the decisions as follows.

For each token contributed to the project, the FIRST MOVER and the SECOND MOVER get £1 each.

For each token a person does not contribute to the project, that person gets £1.50.

So that everyone understands how choices translate into earnings we will give an example and a test. (The allocations of tokens used for the example and test are simply for illustrative purposes. In the experiment the allocations will depend on the actual choices of the participants.)

Example: Suppose the FIRST MOVER contributes 2 tokens to the project, and the SECOND MOVER contributes 4 tokens to the project. Thus in total 6 tokens are contributed to the project.

The FIRST MOVER will earn 6 x £1 = £6 from the project and 3 x £1.50 = £4.50 from the 3 tokens he or she has not contributed to the project. Thus, the FIRST MOVER's total earnings will be £6 + £4.50 = £10.50.

The SECOND MOVER will earn 6 x £1 = £6 from the project and 1 x £1.50 = £1.50 from the 1 token he or she has not contributed to the project. Thus, the SECOND MOVER's total earnings will be £6 + £1.50 = £7.50.

Test: Before we continue with the instructions we want to make sure that everyone understands how their earnings are determined. Please answer the questions below. After a few minutes a monitor will check your answers. When everyone has answered the questions correctly we will continue with the instructions. Raise your hand if you have a question.

1. Suppose the FIRST MOVER contributes 5 tokens to the project, and the SECOND MOVER contributes 5 tokens to the project. Thus, in total 10 tokens are contributed to the project.

    * How much will the FIRST MOVER earn from the project? _______
    * How much will the FIRST MOVER earn from the tokens he or she does not contribute to the project? _______
    * How much will the FIRST MOVER earn in total? _______
    * How much will the SECOND MOVER earn from the project? _______
    * How much will the SECOND MOVER earn from the tokens he or she does not contribute to the project? _______
    * How much will the SECOND MOVER earn in total? _______

2. Suppose the FIRST MOVER contributes 0 tokens to the project, and the SECOND MOVER contributes 0 tokens to the project. Thus, in total 0 tokens are contributed to the project.

    * How much will the FIRST MOVER earn from the project? _______
    * How much will the FIRST MOVER earn from the tokens he or she does not contribute to the project? _______
    * How much will the FIRST MOVER earn in total? _______
    * How much will the SECOND MOVER earn from the project? _______
    * How much will the SECOND MOVER earn from the tokens he or she does not contribute to the project? _______
    * How much will the SECOND MOVER earn in total? _______

3. Suppose the FIRST MOVER contributes 1 token to the project, and the SECOND MOVER contributes 4 tokens to the project. Thus, in total 5 tokens are contributed to the project.

* How much will the FIRST MOVER earn from the project? ______
* How much will the FIRST MOVER earn from the tokens he or she does not contribute to the project? ______
* How much will the FIRST MOVER earn in total? ______
* How much will the SECOND MOVER earn from the project? ______
* How much will the SECOND MOVER earn from the tokens he or she does not contribute to the project? ______
* How much will the SECOND MOVER earn in total? ______

How You Make Decisions

You will make decisions on the computer by completing a screen. The attached sheet shows what the screen will look like. We want to know what you would do in the role of the FIRST MOVER and what you would do in the role of the SECOND MOVER. Thus you will be prompted to make decisions in **both roles**. Only after you have made your decisions will the computer inform you of your actual role, “FIRST MOVER” or “SECOND MOVER”, and this will determine your relevant decisions for calculating earnings. The computer will select roles randomly: there is a 50% chance you will be the FIRST MOVER and the person you are paired with will be the SECOND MOVER, and a 50% chance you will be the SECOND MOVER and the person you are paired with will be the FIRST MOVER.

DECISION TASK 1: In the first blank field you must enter your contribution decision in the role of the FIRST MOVER. You simply indicate how many tokens to contribute to the project. You can enter any whole number from 0 to 5 inclusive.

DECISION TASK 2: The next set of blank fields is for your contribution decision in the role of the SECOND MOVER. We want to know what you as SECOND MOVER would do for any contribution that the FIRST MOVER might make. That is, we want to know:

- what you would do if the FIRST MOVER contributed zero tokens to the project,
- what you would do if the FIRST MOVER contributed one token,
- what you would do if the FIRST MOVER contributed two tokens, etc.

Thus the SECOND MOVER will be prompted to make a decision for every possible contribution by the FIRST MOVER. The relevant decision will be determined by the

FIRST MOVER's actual contribution. If the FIRST MOVER contributed zero tokens to the project, the SECOND MOVER's contribution will be the number he or she types in the first box. If the FIRST MOVER contributed one token to the project, the SECOND MOVER's contribution will be the number he or she types in the second box, and so on. In each box you can enter any whole number from 0 to 5 inclusive.

The screen also has a final set of blank fields for a PREDICTION TASK. Here you must enter a prediction about what the other person enters for DECISION TASK 2.

Once you have completed the decision and prediction tasks you should click on the "Submit" button. You will then be prompted to either change or confirm your decisions and predictions. At this point, if you want to you will be able to go back and change your entries. Once you confirm your decisions and predictions you cannot change them. When everyone in the room has submitted and confirmed their decisions and predictions earnings will be calculated.

How Your Cash Earnings Are Determined

First you will be informed of whether you are the FIRST MOVER or the SECOND MOVER. The computer then determines contributions from decisions as follows.

If you are the FIRST MOVER your contribution is determined by what you entered in DECISION TASK 1. The other person's contribution is determined by what they entered in DECISION TASK 2 in the box corresponding to your contribution.

If you are the SECOND MOVER the other person's contribution is determined by what they entered in DECISION TASK 1. Your contribution is determined by what you entered in DECISION TASK 2 in the box corresponding to the other person's contribution.

From these contributions earnings are calculated. For each token you do not contribute to the project you get £1.50 and for each token contributed to the project the FIRST MOVER and the SECOND MOVER get £1 each.

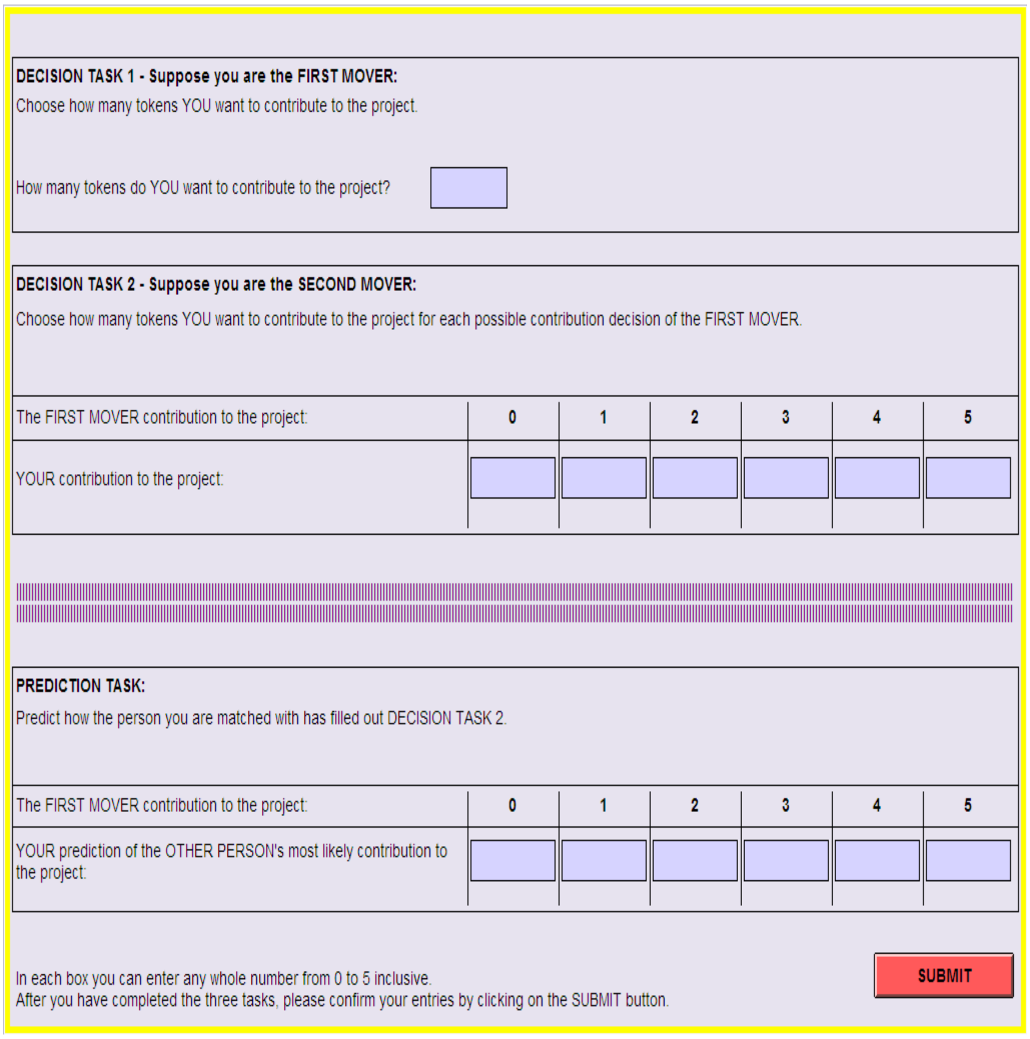
Bonus Earnings

In addition, you can earn money from correctly predicting what the other person enters for DECISION TASK 2. Your predictions in the PREDICTION TASK will be compared with what the person you are matched with actually did in DECISION TASK 2. For each correct prediction you will receive 50p.

Beginning the Experiment

Note: in this experiment you will complete ONLY ONE screen. After you submit your entries you will be prompted to confirm them. At this point, if you want to you will be able to change your entries. Once you confirm your entries you cannot change them, and these will be used for determining earnings. If you have a question at any time please raise your hand and a monitor will come to your desk to answer it. Now, please look at your computer screen and begin making your decisions and predictions.

The Decision Screen



DECISION TASK 1 - Suppose you are the FIRST MOVER:
Choose how many tokens YOU want to contribute to the project.

How many tokens do YOU want to contribute to the project? [ ]

DECISION TASK 2 - Suppose you are the SECOND MOVER:
Choose how many tokens YOU want to contribute to the project for each possible contribution decision of the FIRST MOVER.

The FIRST MOVER contribution to the project:	0	1	2	3	4	5
YOUR contribution to the project:						

PREDICTION TASK:
Predict how the person you are matched with has filled out DECISION TASK 2.

The FIRST MOVER contribution to the project:	0	1	2	3	4	5
YOUR prediction of the OTHER PERSON's most likely contribution to the project:						

In each box you can enter any whole number from 0 to 5 inclusive.
After you have completed the three tasks, please confirm your entries by clicking on the SUBMIT button.

SUBMIT

CHAPTER 3

SEQUENTIAL VERSUS SIMULTANEOUS CONTRIBUTIONS TO PUBLIC GOODS: EXPERIMENTAL EVIDENCE*

3.1 Introduction

In an important theoretical contribution to the literature on the voluntary provision of public goods Varian (1994) shows that, under appropriate assumptions, a sequential contribution mechanism elicits lower contributions than a simultaneous contribution mechanism. Key to this result is that under sequential moves a first-mover may enjoy a first-mover advantage by contributing zero, relying on other contributors to provide the public good on their own. We examine this and related predictions using a laboratory experiment. There is now a large body of evidence from a variety of experimental studies documenting the importance of concerns for fairness and reciprocity. Specifically, numerous public goods experiments have shown that many people are "conditional cooperators", that is, they are willing to contribute to the public good but only if others do the same.[22] Moreover,

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* This chapter is based on joint work with Simon Gächter, Elke Renner and Martin Sefton. A research paper companion to this chapter is forthcoming in *Journal of Public Economics*.

[22] See, e.g., Andreoni (1995); Keser and van Winden (2000); Fischbacher, et al. (2001); Croson (2007); Muller, et al. (2008); Fischbacher and Gächter (forthcoming).

numerous experiments have also shown that people are prepared to punish decisions that lead to unfair outcomes (Fehr and Gächter (2000)). Since first-movers may not want to exploit their first-mover advantage if they care about more than their narrow self-interest, and they may not be able to exploit their first-mover advantage if others are willing to eschew their private interests in order to resist unfair outcomes, it is unclear whether Varian's theoretical comparative static results will hold in a laboratory setting.

Our experiment focuses on the simplest version of Varian's model with two players, quasi-linear returns from public/private good consumption, and complete information about returns from public/private good consumption. This differs from most previous experimental work on voluntary contributions in three important respects. First, we use a setup more aligned with the theoretical literature, where a selfish second-mover's contribution is decreasing in the first-mover's contribution, rather than the usual setup where predicted contributions are independent of others' contributions. Second, we use a setup where the returns from the public good vary across players, whereas the usual setup studies symmetric games. Third, whereas the usual setup has participants make simultaneous contributions, we also study sequential contribution mechanisms.[23]

Previously Andreoni, et al. (2002) (ABV hereafter) studied a similar environment. They also compared simultaneous and sequential contribution games based on Varian's model and concluded: "*...while the pull of equilibrium is evident in early rounds – with the first-movers attempting to*

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[23] There are some studies which look at the role of move structure for the provision of step-level public goods (Erev and Rapoport (1990); Coats, et al. (2009)). For a general discussion of the importance of move structures in public good and other dilemma games and an overview of experimental findings, see Au and Budescu (1999).

exploit their advantaged position – the pull of fairness eventually dominates – simultaneous and sequential play are very similar by the end of the experiment." (p. 19). Our experimental design (described in detail in the next Section) studies the robustness of these findings by examining two different parameterizations of Varian's model.

In one parameterization one player gets much lower returns from the public good than the other. ABV introduced minimal asymmetry between players, with the consequences that equal contributions resulted in roughly equal earnings, and predicted aggregate contributions varied by just one token across move orderings.[24] By introducing a greater degree of asymmetry we increase the predicted effect of move order on aggregate contributions, and so the theoretical comparative static result may have a better chance of being observed in the data. By increasing the degree of asymmetry we also reduce the saliency of fairness: equal contributions generally lead to inequitable earnings and it is more difficult for players to identify equitable allocations. Thus, with this parameterization we can test whether the 'pull of fairness' still dominates the 'pull of equilibrium' in environments where there is no prominent contribution combination that can enforce an equitable distribution of earnings.

Our second parameterization features an even greater degree of asymmetry in returns from the public good and extends ABV's study to a setting where the existence of commitment opportunities does *not* affect equilibrium outcomes: regardless of the move ordering, equilibrium predicts that the person with lowest returns will contribute nothing and all contributions

[24] ABV's main focus is on comparing behaviour across games with similar equilibrium predictions in order to identify factors that may cause equilibrium predictions to work well or to fail, rather than on testing Varian's theoretical comparative static results.

will be made by the person with highest returns. Under this parameterization we can study behaviour in a sequential game where in theory it is the second-mover who free rides off the first-mover, and it is the first-mover who earns less in equilibrium. As a consequence, attaining fair distributions of earnings in this game requires second-movers, and not first-movers, to contribute more than predicted. Thus, this game illustrates a case where the 'pull of fairness' relies on the use of rewards by second-movers, and not on first-movers' generosity or fear of punishment.

We report our results in Section 3.3. In our first parameterization we find that, consistent with comparative static predictions, aggregate contributions are lowest when the person with highest returns moves first. However, as in ABV, the extreme prediction that the first-mover free rides completely off the second-mover is not supported and the distribution of contributions is more compressed than predicted. A consequence of this is that we do not observe a predicted first-mover advantage. In our second parameterization we find that, contrary to Varian's model predictions, move order matters. In the game where the pull of fairness relies on the use of rewards the equilibrium prediction is a very good approximation of actual behaviour, and the first-mover suffers low earnings as predicted. In the other move orderings we observe more equitable distributions of contributions and earnings than predicted. As a consequence we observe an unpredicted first-mover *dis*advantage. In Section 3.4 we discuss our results and conclude.

3.2 Experimental Design & Procedures

3.2.1 The Experimental Game

Our experiment is based on the following two-player game. Each player is endowed with 17 tokens, and must decide how many to place in a Private Account and how many to place in a Shared Account. For each token a player places in the Private Account that player receives 50 points. For each token placed in the Shared Account both players receive an additional amount of points, as shown in Table 3.1.

In all treatments the 'LOW' player receives a lower return from the Shared Account than the 'HIGH' player. In our FMA (for "First-Mover Advantage") treatments we use a set of parameters where theory predicts that each player prefers moving first to moving second in a sequential move game. In the sequential move game where LOW moves first (our LOW-FMA treatment) the unique subgame perfect equilibrium involves LOW contributing 0 tokens and HIGH contributing 15 tokens, so that LOW earns 1555 and HIGH earns 1150. In the game where HIGH moves first (HIGH-FMA treatment) the unique subgame perfect equilibrium has HIGH contributing 0 tokens and LOW contributing 6 tokens, so that HIGH earns 1340 and LOW earns 890. The HIGH-FMA treatment illustrates a case where sequential moves yield lower overall contributions (and earnings) than simultaneous moves – the unique Nash equilibrium of the simultaneous move game (SIM-FMA treatment) is for HIGH to contribute 15 tokens and LOW to contribute 0 tokens (this is the same predicted outcome as LOW-FMA).[25]

[25] For a full derivation of theoretical predictions see Appendix A at the end of the chapter.

Table 3.1. Returns from Shared Account*

Tokens in the Shared Account	HIGH PLAYER		LOW PLAYER (*FMA treatments*)		LOW PLAYER (*NOFMA treatments*)	
	Earnings from the Shared Account	Marginal return from the Shared Account	Earnings from the Shared Account	Marginal return from the Shared Account	Earnings from the Shared Account	Marginal return from the Shared Account
0	0	-	0	-	0	-
1	90	90	60	60	55	55
2	180	90	120	60	110	55
3	260	80	175	55	155	45
4	340	80	230	55	200	45
5	415	75	285	55	245	45
6	490	75	340	55	290	45
7	565	75	385	45	330	40
8	635	70	430	45	370	40
9	700	65	475	45	410	40
10	765	65	520	45	450	40
11	825	60	560	40	485	35
12	885	60	600	40	520	35
13	940	55	635	35	555	35
14	995	55	670	35	590	35
15	1050	55	705	35	620	30
16	1095	45	740	35	650	30
17	1140	45	770	30	675	25
18	1180	40	800	30	700	25
19	1220	40	830	30	725	25
20	1260	40	855	25	750	25
21	1295	35	880	25	770	20
22	1330	35	900	20	790	20
23	1360	30	920	20	805	15
24	1385	25	940	20	820	15
25	1410	25	960	20	835	15
26	1435	25	975	15	850	15
27	1455	20	990	15	860	10
28	1470	15	1000	10	870	10
29	1485	15	1010	10	880	10
30	1500	15	1020	10	890	10
31	1510	10	1025	5	895	5
32	1515	5	1030	5	900	5
33	1520	5	1035	5	905	5
34	1525	5	1040	5	910	5

*The earnings were derived from a quadratic utility function of the form $\pi_i = 50(17 - g_i) + t_i(68G - G^2)$, where g_i represents *i*'s contribution to the Shared Account, G represents aggregate contributions, $t_{HIGH} = 1.32$ and $t_{LOW} = 0.89$ (FMA treatments) or 0.78 (NOFMA treatments). Earnings were then rounded to a multiple of 5 points.

The other parameter set increases the asymmetry between players by reducing LOW's returns from the Shared Account. In this parameterization there is no predicted first-mover advantage and so we refer to these as our NOFMA treatments. For any move ordering the unique (subgame perfect) equilibrium involves HIGH contributing 15 tokens and LOW contributing 0 tokens, yielding HIGH 1150 and LOW 1470. Note that the HIGH-NOFMA game differs from the other three sequential move games in that, in equilibrium, it is the first-mover, HIGH, who supplies the public good, while the second mover, LOW, free rides. Table 3.2 summarizes our design.

Table 3.2.Overview of Treatments

Treatment	Subgame Perfect Equilibrium Contributions {HIGH, LOW}	 Payoffs {HIGH, LOW}
LOW-FMA	{15, 0}	{1150, 1555}
HIGH-FMA	{0, 6}	{1340, 890}
SIM-FMA	{15, 0}	{1150, 1555}
LOW-NOFMA	{15, 0}	{1150, 1470}
HIGH-NOFMA	{15, 0}	{1150, 1470}
SIM-NOFMA	{15, 0}	{1150, 1470}

3.2.2 Experimental Procedures

The experiment was conducted at the University of Nottingham using 192 subjects recruited from a university-wide pool of students who had previously indicated their willingness to be paid volunteers in decision-making experiments.[26] Twelve sessions were conducted (two per treatment) with 16 participants per session. No subject took part in more than one session. Upon arrival, subjects were welcomed and randomly seated at visually separated

[26] Subjects were recruited through the online recruitment system ORSEE (Greiner (2004)). The experiment was programmed and conducted with the software z-Tree (Fischbacher (2007)). Experimental instructions and earnings tables are reproduced in Appendix B and C respectively.

computer terminals. Subjects were then given a written set of instructions that the experimenter read aloud. The instructions included a set of control questions about how choices translated into earnings. Subjects had to answer all the questions correctly before the experiment could continue.

The session then consisted of 15 rounds of the game described above, where in each round subjects were randomly matched with another participant. Subjects were not informed of the identities of the other people in the room they were matched with, neither during nor after the experiment. Moreover, we did not make use of subject IDs, and so subjects' decisions were not associated with identification numbers which could be used to establish reputations. The matching procedure worked as follows. At the beginning of each session the participants were randomly allocated to one of two eight-person matching groups. The computer then randomly allocated the role of HIGH to four subjects and the role of LOW to the other four subjects in each matching group. Subjects were informed of their role at the beginning of the first round and kept this role throughout the 15 rounds. At the beginning of each round the computer randomly formed pairs consisting of one HIGH and one LOW participant within each matching group. To ensure comparability among sessions and treatments, we randomly formed pairings within each matching group prior to the first session and used the same pairings for all sessions. Because no information passed across the two matching groups, we treat data from each matching group as independent. Thus our design generates four independent observations per treatment. Repetition of the task was used because we expected that subjects might learn from experience. However, our desire to test predictions based on a one-shot model led us to use the random re-matching design in order to reduce repeated game effects.[27]

Subjects were paid based on their choices in one randomly-determined round. At the end of round fifteen a poker chip was drawn from a bag containing chips numbered from 1 to 15. The number on the chip determined the round that was used for determining all participants' cash earnings. At the end of the experiment subjects were asked to complete a short questionnaire asking for basic demographic information and were then privately paid according to their point earnings in the round which had been randomly selected at the end of round fifteen. Point earnings were converted into British Pounds at a rate of £0.01 per point. Subject earnings ranged from £8.50 to £17.50, averaging £12.69 (at the time of the experiment £1 ≈ $1.61), and sessions lasted about 75 minutes on average.

3.3 Results

3.3.1 Aggregate Contributions

Figure 3.1 displays aggregate contributions in the six treatments. In all treatments contributions fall in the first five rounds before stabilizing from round six onwards. In the SIM-FMA and LOW-FMA treatments equilibrium aggregate contributions are predicted to be 15 tokens. On average, pairs contributed 14.3 tokens per game in SIM-FMA compared with 13.3 in LOW-FMA – this difference is not significant at conventional levels ($p = 0.457$).[28] In

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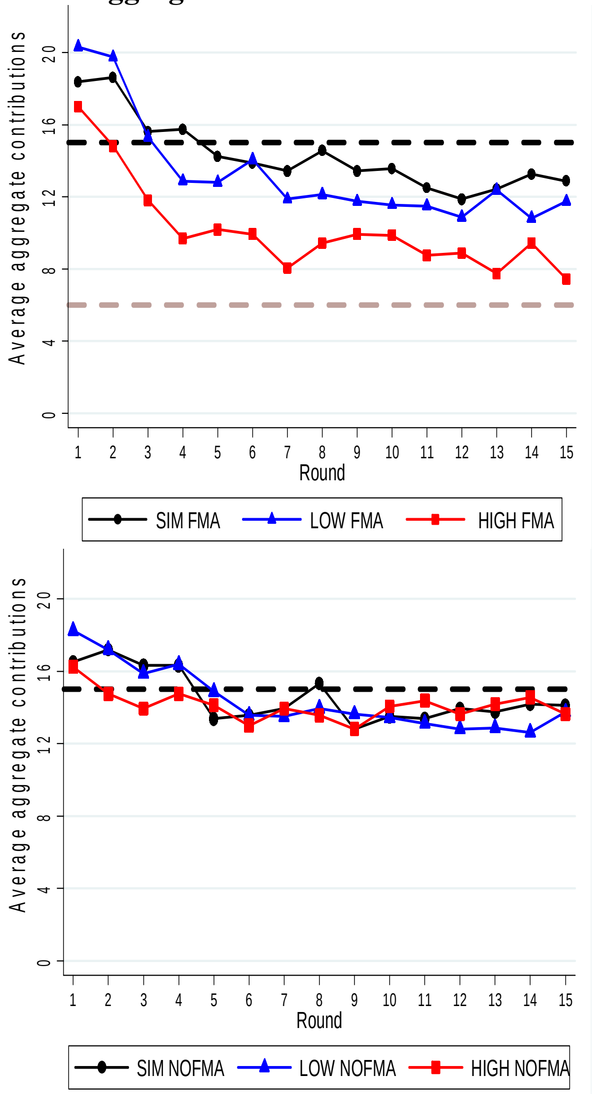
[27] Subjects were informed that they would be randomly matched with another person in the room in each round, but the details of the matching procedure were not specified. For details see the instructions reproduced in Appendix B.

[28] All p-values are based on two-sided randomization tests applied to 4 independent observations per treatment, unless otherwise stated. Summary data on individual and group contributions are given in Appendix E.

HIGH-FMA contributions are predicted to be lower, 6 tokens. Although on average pairs contribute more than this, 10.2 tokens per game, this is significantly lower than in the other FMA treatments (HIGH-FMA vs. SIM-FMA: p = 0.029; HIGH-FMA vs. LOW-FMA: p = 0.029). Similar results are obtained if we focus on the last five rounds: contributions in SIM-FMA and LOW-FMA are not significantly different (p = 0.457), but contributions in HIGH-FMA are significantly lower than in SIM-FMA (p = 0.029) or LOW-FMA (p = 0.086).

Figure 3.1. Aggregate Contributions across Rounds*



*Equilibrium aggregate contributions shown by dashed lines. HIGH-FMA light dash, all other treatments dark dash.

In the NOFMA treatments, consistent with equilibrium predictions, aggregate contributions do not differ significantly across move orderings ($p > 0.457$ in all pair-wise comparisons, whether we focus on all rounds or the last five rounds). All three treatments track the prediction quite well: average contributions across all three treatments are 14.3 tokens per game compared with the predicted 15 tokens per game.

In summary, our data are consistent with comparative static predictions regarding aggregate contributions. In particular, in the FMA treatments aggregate contributions are lower when the person with highest returns from the public good moves first. By comparison, ABV found that aggregate contributions were slightly lower in their sequential treatment, and noted that while players of a given role should behave differently across simultaneous and sequential move games, by the end of the experiment they are behaving, on average, similarly in the two games.[29] It is interesting that in their experiment differences across treatments disappeared with repetition, whereas our treatment effect is robust across rounds. First, it may be that our predicted effect is sufficiently large that fairness considerations can lead to deviations from equilibrium outcomes without overcoming the comparative static result. Second, fairness considerations may be less relevant in our experiment simply because the degree of asymmetry makes it difficult to identify fair allocations.

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[29] ABV do not report formal statistical comparisons of aggregate contributions across move orderings, but using their data we found that aggregate contributions were significantly different at the 10% level when one looks at all rounds ($p = 0.100$), but not significantly different in the last five rounds ($p = 0.800$). These p-values are based on randomization tests treating aggregate contributions in a session as the unit of observation, and so are based on comparisons of two sets of three observations. One can also use a less conservative approach and treat each game in a round as an independent observation. Doing this we found that contributions were often significantly different in early rounds, but not significant in any of the last five rounds ($p > 0.204$ for the last five rounds). We thank the authors for making their data available at http://econlab.ucsd.edu/getdata/.

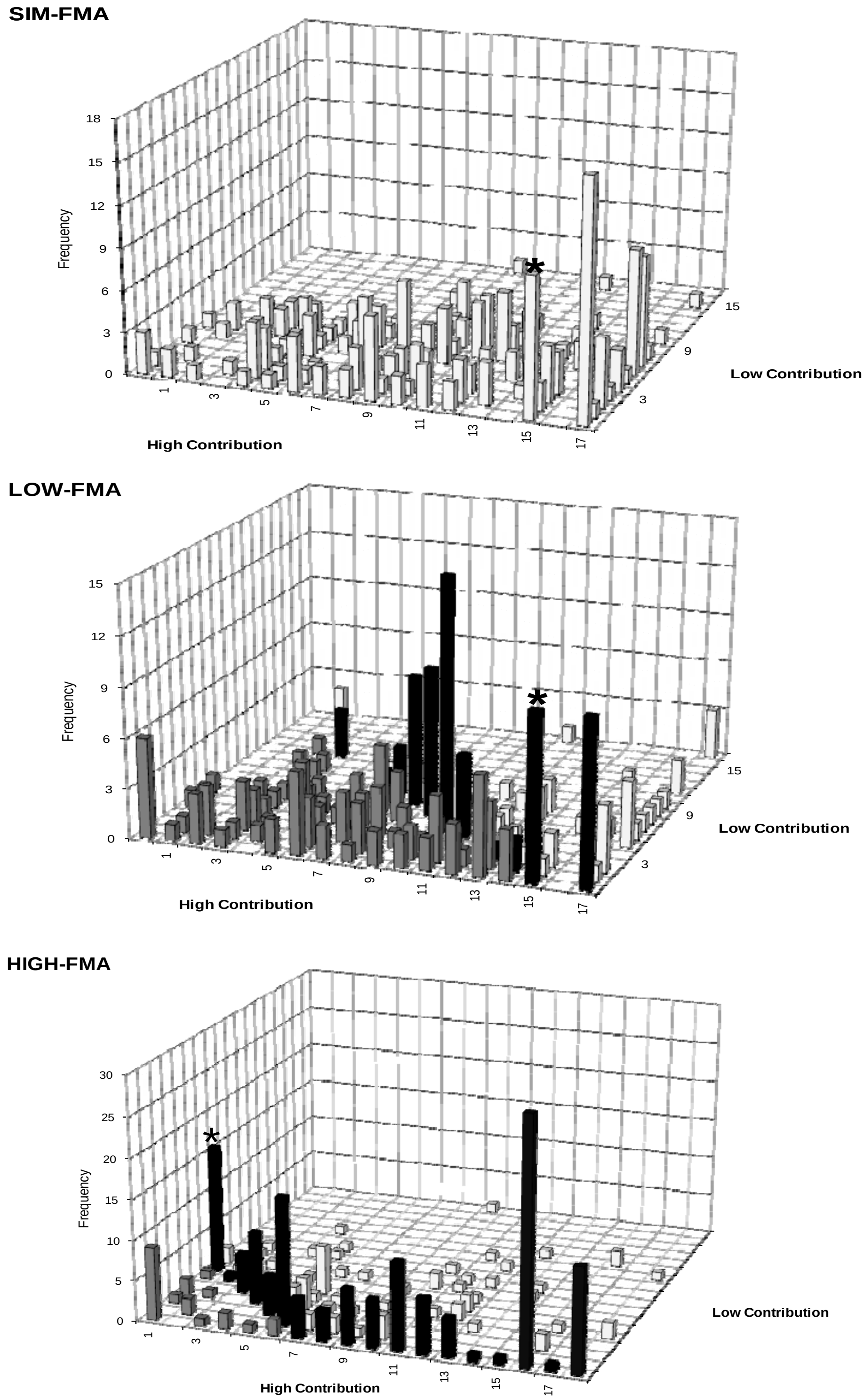
Further evidence on this comes from examining the distribution of contributions.

3.3.2 Individual Contributions

ABV found that players contributed almost equal amounts (especially in their simultaneous treatment), in contrast to the extreme theoretical prediction that one player would free ride completely off the other. Figure 3.2 displays individual contributions in our FMA treatments. For the sequential game treatments black bars indicate second-mover contributions consistent with a best response, light grey bars contributions in excess of the best response, and dark grey bars contributions below the best response. The results are qualitatively similar to ABV (c.f. their Figures 3a and 3b). In SIM-FMA only 4% of games correspond to the equilibrium prediction and the data are relatively disorganized. The major difference from ABV is that contributions are more asymmetric in our experiment: HIGH contributes 10.5 tokens on average compared with LOW's 3.8 tokens.

In the sequential move treatments, first note that although few games result in the subgame perfect equilibrium (7% in HIGH-FMA and 4% in LOW-FMA), a substantial portion lies along the diagonals corresponding to the predicted aggregate (24% in HIGH-FMA and 21% in LOW-FMA). Thus, when the predicted aggregate is observed it usually involves both players sharing the burden of providing the public good, rather than the predicted allocation where one player free rides off the other.

Figure 3.2 Individual contributions (FMA treatments)*



*Based on all 240 games in each treatment. (Subgame perfect) equilibrium outcome is marked with a star. In the sequential game treatments, games lying on the second-mover's best-response are shown in black, while games resulting in deviations below (above) the best-response are shown in dark (light) grey.

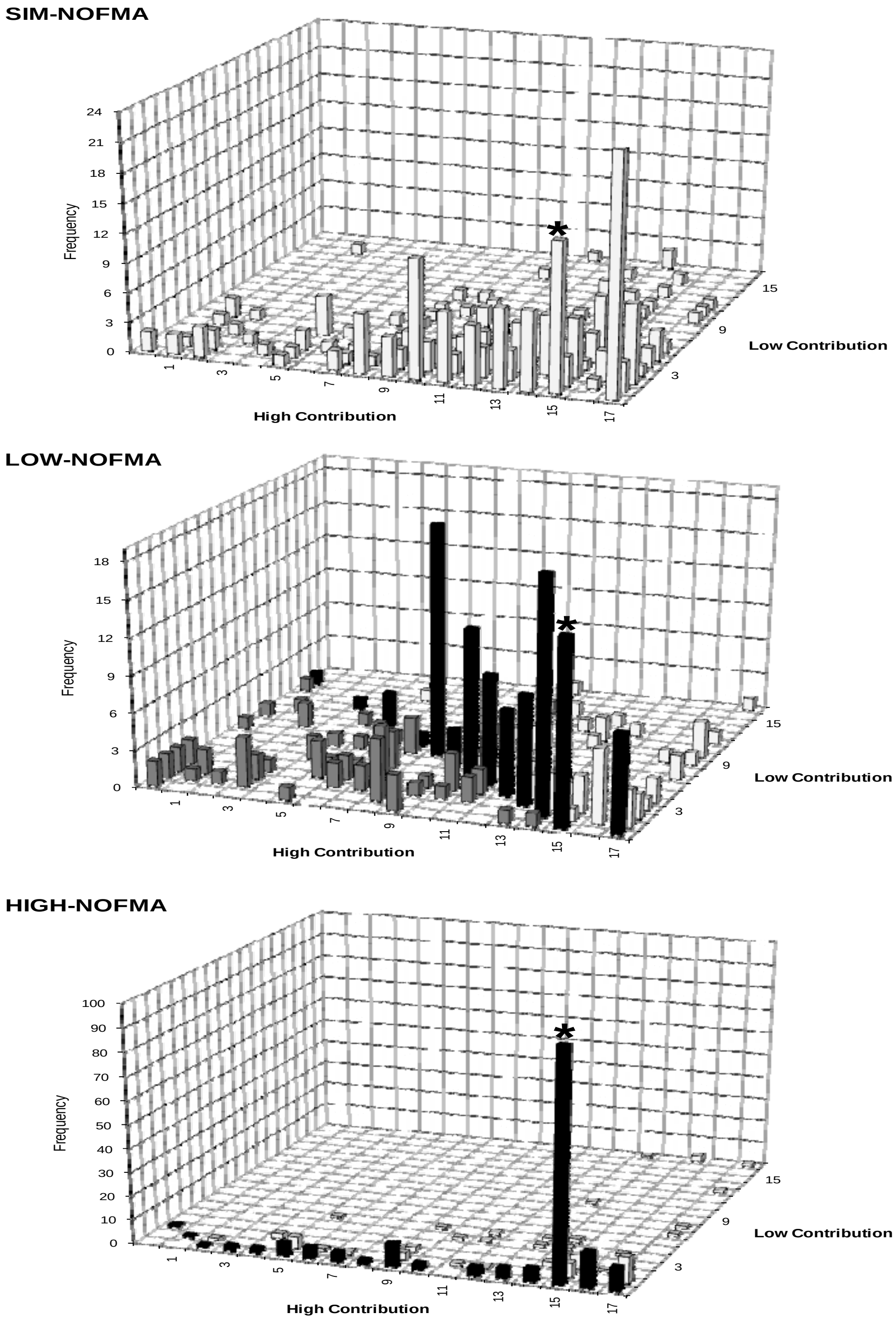
Moreover, while second-movers often play a best-response to the first-mover's contribution (60% in HIGH-FMA, 25% in LOW-FMA), in a large number of games second-movers choose to reward first-movers by contributing above the best response (in 31% of games in HIGH-FMA, in 26% of games in LOW-FMA), or to punish them by contributing below (in 9% of games in HIGH-FMA, in 49% of games in LOW-FMA).[30] Notably, 65% of the games involving punishment occur when the first-mover contributes between 0 and 2 tokens, but games involving rewards are not clustered at any specific interval of the first-mover's contributions. Moreover, the pattern of rewards and punishment changes across rounds: focusing on the last five rounds, deviations from best-responses are just as frequent as in earlier rounds, but they are more likely to be deviations below the best-response function. Thus, as subjects gain experience with the experimental setting the incidence of punishment increases, while rewarding behaviour tends to diminish.

Figure 3.3 displays individual contributions in our NOFMA treatments. Again, only 6% of simultaneous move games result in the equilibrium outcome, and there is considerable dispersion in outcomes. As in the sequential FMA treatments there is a clustering of data in LOW-NOFMA where many games result in the predicted aggregate contribution of 15 tokens (41%). Again, however, only 6% of games correspond to the predicted extreme allocation. As in the sequential FMA treatments we observe both punishing (32% of all games and increasing over time) and rewarding behaviour (27% of all games and decreasing over time).

[30] For expositional purposes we refer to a second-mover contribution below the best response as a punishment (since, relative to the best response, it reduces the first-mover's payoff at a cost to oneself) and a contribution above the best response as a reward (since it raises the first-mover's payoff at a cost to oneself). Of course a variety of other motives, or even error, could account for deviations from best responses. We describe later temporal patterns in punishment that suggest limited scope for interpreting punishments as due to error. See also Gächter, et al. (2008) who observe punishment even after 50 rounds of experience with a public goods game.

Thus the general patterns in these two treatments are similar to those observed in the FMA treatments.

Figure 3.3 Individual contributions (NOFMA treatments)*



*Based on all 240 games in each treatment. (Subgame perfect) equilibrium outcome is marked with a star. In the sequential game treatments, games lying on the second-mover's best-response are shown in black, while games resulting in deviations below (above) the best-response are shown in dark (light) grey.

Taken together these five treatments provide little support for the extreme theoretical prediction, and suggest that fairness considerations are relevant in our experiment. Contributions are less asymmetric than predicted and in the sequential games fairer allocations are supported by “punishment strategies” whereby second-movers react to low first-mover contributions by contributing less than the best-response. All of this is qualitatively similar to the results in ABV.

Finally, the last panel of Figure 3.3 shows the HIGH-NOFMA treatment. The picture is remarkably similar to the outcomes of a third “best shot” ABV treatment, where the only point of any significance is the subgame perfect equilibrium. They attribute the difference between their sequential and best shot treatments to the difference in payoff possibilities: equilibrium works well in the best shot game because players cannot reduce inequality and at the same time increase the payoff of the disadvantaged party. While we do not doubt that this is an important factor in determining when an equilibrium prediction works well, this cannot account for the difference we observe across NOFMA treatments. In all move orderings, there are deviations from equilibrium that enable players to attain more equal payoffs and at the same time increase the payoff to the disadvantaged party. Thus it is unclear why fairness considerations that are relevant in the other treatments appear less important in HIGH-NOFMA.

This treatment differs from the other sequential treatments in that theory predicts the first-mover is the disadvantaged party. Reducing inequality and increasing the disadvantaged party’s payoff requires second-movers to contribute more than predicted. In particular, attaining the same distribution of payoffs as observed in LOW-NOFMA requires the first-mover to contribute less than 15 tokens and the second-mover to reward. In contrast, in the other sequential treatments the second-mover is disadvantaged in equilibrium, and for inequality to be reduced while increasing the second-mover's payoff the first-mover must contribute more than predicted. Even a selfish first-mover might be willing to do so if they anticipated that selfish behaviour would be punished (as in fact it is). Thus, in the other sequential treatments the anticipation of punishment is sufficient to reduce inequality and benefit the disadvantaged party.[31] The ineffectiveness of rewards relative to punishment for moving first-mover behaviour from the theoretical prediction is reminiscent of results from the proposer-responder games reported in Andreoni, et al. (2003).[32]

3.3.3 Earnings

Table 3.3 shows how the compression of contributions in the FMA treatments leads to compression of earnings. Although the model's comparative static prediction about aggregate earnings is borne out – earnings in HIGH-FMA are significantly lower than in other treatments (focusing on all rounds: HIGH-FMA vs. SIM-FMA: p = 0.029; HIGH-FMA vs. LOW-FMA: p = 0.029: focusing on the last five rounds; HIGH-FMA vs. SIM-FMA: p = 0.029; HIGH-FMA vs. LOW-FMA: p = 0.086) – there are some important deviations from

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[31] Another structural feature of the HIGH-NOFMA treatment that distinguishes it from the other sequential treatments is that the subgame perfect equilibrium outcome is also the unique Nash equilibrium outcome, whereas in the other treatments there are (imperfect) Nash equilibria where aggregate contributions are the same as in the subgame perfect equilibrium, but the first-mover makes positive contributions. For example, a second-mover might threaten to contribute 0 tokens if the first-mover contributes less than a threshold value $\hat{g}$ and to best-respond if and only if $g \geq. \hat{g}$ Given this threat the first-mover may find it optimal to choose $\hat{g}$.

[32] Sefton, et al. (2007) also find that the opportunity to reward by itself is insufficient to sustain contributions in a public goods game, whereas the opportunity to punish is a more effective mechanism for sustaining cooperation. In addition, a wide range of experiments find positive reciprocity to be weak relative to negative reciprocity (see, e.g., Abbink, et al. (2000); or Offerman (2002)).

comparative static predictions. In particular HIGH earns *less* in HIGH-FMA than in the other move orderings.[33]

Table 3.3. Earnings*

	HIGH			LOW			AGGREGATE		
	Predicted	All Rounds	Last 5 Rounds	Predicted	All Rounds	Last 5 Rounds	Predicted	All Rounds	Last 5 Rounds
SIM-FMA	1150	1289 *(169.8)*	1235 *(172.0)*	1555	1311 *(193.7)*	1291 *(214.6)*	2705	2601 *(244.9)*	2525 *(306.3)*
LOW-FMA	1150	1293 *(202.6)*	1220 *(174.3)*	1555	1269 *(202.4)*	1289 *(197.4)*	2705	2562 *(262.3)*	2509 *(272.3)*
HIGH-FMA	1340	1203 *(168.8)*	1166 *(154.1)*	890	1228 *(234.2)*	1172 *(259.2)*	2230	2431 *(270.9)*	2338 *(301.9)*
SIM-NOFMA	1150	1230 *(162.1)*	1197 *(122.4)*	1470	1302 *(178.7)*	1336 *(154.3)*	2620	2532 *(203.1)*	2533 *(182.3)*
LOW-NOFMA	1150	1321 *(189.1)*	1232 *(138.5)*	1470	1219 *(194.9)*	1277 *(182.8)*	2620	2540 *(203.9)*	2509 *(233.0)*
HIGH-NOFMA	1150	1164 *(104.2)*	1151 *(53.4)*	1470	1373 *(162.3)*	1416 *(109.2)*	2620	2537 *(184.8)*	2568 *(144.0)*

* The table shows average point earnings per game. Standard deviations in parentheses.

Likewise, in the NOFMA treatment aggregate earnings are, as predicted, invariant to move ordering ($p > 0.371$ in all pair-wise comparisons, whether we focus on all rounds or the last five rounds), but, in contrast to predictions, the distribution of earnings varies across treatments. Here we observe a first-mover *dis*advantage: both players earn most when they move second and least when they move first. The differences in earnings between LOW-NOFMA and HIGH-NOFMA are significant for both types of player ($p = 0.029$ in both comparisons).[34]

[33] An inspection of ABV's data reveals that their HIGH player earned slightly more when they moved first than moving simultaneously, although the difference is insignificant. Randomization tests applied to the two sets of three observations, where each observation is average HIGH player earnings within a session yields $p = 0.800$. Restricting attention to the last five rounds yields $p = 0.400$. A less conservative approach treating each HIGH player as an independent observation yields $p = 0.949$ (last five rounds $p = 0.206$).
[34] The result holds for LOW even in the last five rounds ($p = 0.029$). HIGH earnings are still higher in LOW-NOFMA than in HIGH-NOFMA in the last five rounds, but the difference is just insignificant ($p = 0.114$).

3.4 Discussion & Conclusions

This chapter reports an experiment examining the effects of move structure in a quasi-linear public good setting. Previously Andreoni, et al. (2002) (ABV) studied a similar setting, and we extend their experimental analysis by considering two different sets of parameters. In our FMA treatments the asymmetry between players is more pronounced than in ABV, and in terms of equilibrium incentives our design creates greater separation between equilibrium aggregate contributions across move structures. At the same time, while in ABV's design equal contributions lead to almost-equal earnings, our design makes it more difficult for players to identify fair allocations. Our results are qualitatively similar to ABV's in that individual contributions are not as asymmetric as predicted, and when first-movers free ride second-movers often punish them by contributing less than their best response. However, whereas in their experiment any differences between aggregate contributions in simultaneous and sequential games disappeared by the end of their experiment, in ours we find robust support for the theoretical comparative static prediction that aggregate contributions are lower in a sequential move ordering when the person who values the public good most moves first. On the other hand, we do not observe the first-mover advantage predicted by the model.

In a second parameter set (our NOFMA treatments) differences in returns from the public good are so large that, in theory, the player with the highest returns from the public good supplies the public good regardless of move ordering. Thus, we study three games that differ only in terms of move orderings: in each game the players' action sets are the same, the players' payoff functions are the same, and the equilibrium allocations are the same.

Here we find the distribution of contributions varies across move structures and in fact equilibrium predictions work remarkably well in one game but not in the others. This allows us to refine ABV's explanation for why equilibrium provides a good approximation to behaviour in some games but not in others. Move orderings matter because they determine what mechanism is required in order to achieve more equitable allocations. Consistent with findings from experiments in other settings, punishment, or merely the anticipation of punishment, can be an effective mechanism for moving outcomes away from equilibrium predictions toward more equitable outcomes, whereas rewards are much less effective. In the NOFMA treatments earnings are predicted to be independent of move ordering, but we observe a first-mover *dis*advantage.

Our results on the distribution of contributions and earnings have important policy implications. First, if a fundraiser is choosing between a sequential and simultaneous solicitation mechanism the optimal choice may depend on the distribution of contributions as well as the level of overall contributions. Although aggregate contributions follow theoretically predicted directions, the distribution of contributions does not. When the person with lowest returns from the public good moves first aggregate contributions are never lower and the distribution of contributions is also more even. Thus, this sequential move ordering may be quite acceptable on many normative criteria, and may even be preferred to a simultaneous move structure. An implication of our results on earnings is that there is not much of an advantage to committing to being a free-rider, and this in turn may have important implications for endogenous move structures. In naturally occurring settings the move structure is not exogenously imposed, but rather emerges endogenously, and this process typically reflects how alternative move structures reward participants. Since no first-mover advantage is actually attained it is unclear whether the detrimental move ordering would emerge in practice.

Taken together these results suggest that commitment opportunities may be less damaging than previously thought. ABV show that when there is limited asymmetry in players' preferences the existence of commitment opportunities does not exacerbate the free rider problem as the 'pull of fairness' ends up dominating the 'pull of equilibrium', and, as a consequence, sequential and simultaneous mechanisms do not lead to dramatically different levels of public good provision. When the asymmetry in players' preferences is very large aggregate contributions are predicted to be the same in sequential and simultaneous move games and the data from our NOFMA treatments confirm this prediction. Thus, only when players' preferences are sufficiently different, but not too different, does Varian's theoretical result that sequential mechanisms yield lower provision than simultaneous mechanisms seem to hold, as confirmed by our FMA treatments. However, even in this case, the absence of a first mover advantage makes it questionable whether the sequential move ordering would emerge naturally.

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Appendix A: Theoretical Background

This appendix outlines the theoretical background underlying the payoff functions used in the experiment. For further details and discussion of the model refer to Varian (1994, *Journal of Public Economics* 53, 165-186). The payoff functions are based on a simple two-player quasi-linear model. The two players have different preferences over the public good. We refer to the player who enjoys a higher return from the public good as the 'HIGH' player, and the player who enjoys a lower return as the 'LOW' player.

Player i, $i \in$ {HIGH, LOW}, is endowed with wealth w_i and contributes an amount $0 \leq g_i \leq w_i$ to a public good. The remainder is allocated to private good consumption. The total amount of the public good provided is $G = g_{\text{HIGH}} + g_{\text{LOW}}$. Player i's payoff is given by:

$$\pi_i = w_i - g_i + f_i(G)$$

where individual i's return from the public good, $f_i(G)$, is increasing and strictly concave.

If the other agent contributes zero, player i's best response is her 'stand-alone contribution' $\hat{g}_i$. We assume that $w_i > \hat{g}_i$ so that the first order condition for an interior optimum is satisfied:

$$f'_i(\hat{g}_i) = 1.$$

If player j contributes $g_j > \hat{g}_i$ then i's marginal return from contributing g_i is $f'_i(g_j + g_i) - 1 < 0$. Thus i's best response is $g_i = 0$. If player j contributes $g_j \leq \hat{g}_i$, then i's best response satisfies:

$$f'_i(g_i + g_j) = 1.$$

Comparing this with the first-order condition for an interior optimum we have:

$$f'_i\,(g_i + g_j) = f'_i\,(\hat{g}_i)$$

or

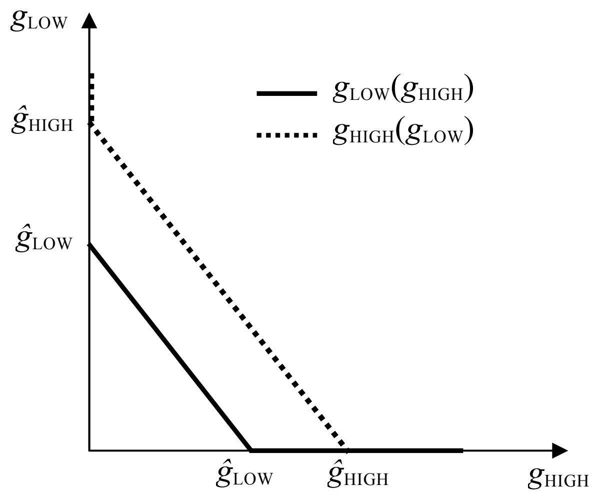
$$g_i = \hat{g}_i - g_j.$$

Thus, i's best response function is:

$$g_i = \max\{\hat{g}_i - g_j, 0\}.$$

Figure AA.1 shows the best response functions. With simultaneous moves, the unique Nash Equilibrium is the intersection of the best response functions: $g_{HIGH} = \hat{g}_{HIGH}$, $g_{LOW} = 0$. Thus LOW contributes zero and HIGH makes her stand-alone contribution.

Figure AA.1 Best-response functions



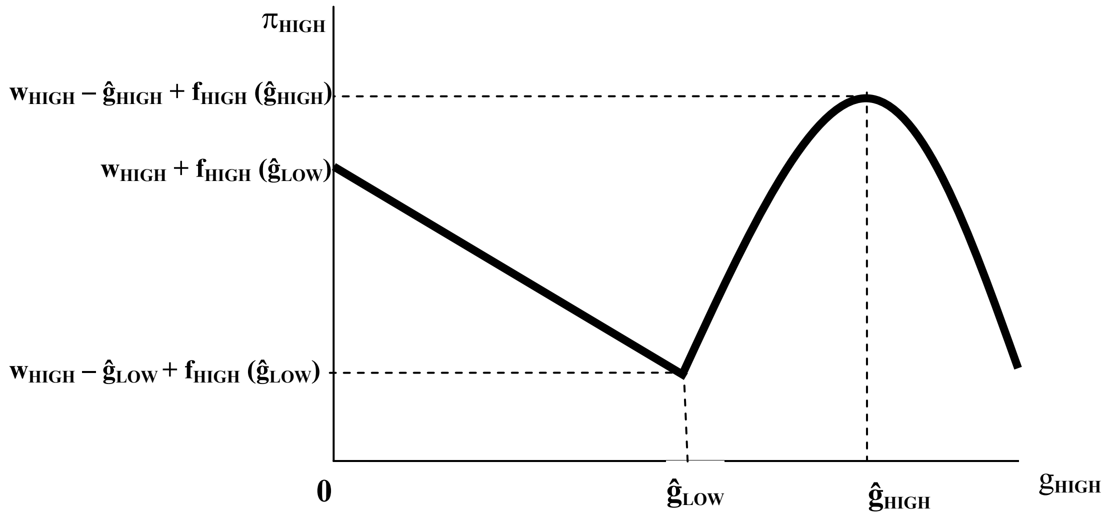
Next, suppose LOW moves first. In a subgame perfect equilibrium the second-mover's strategy is given by her best response function: $g_{HIGH} = \max\{\hat{g}_{HIGH} - g_{LOW}, 0\}$. LOW's subgame perfect equilibrium strategy results in her most preferred point on the HIGH's best response function. Suppose $g_{LOW} > \hat{g}_{HIGH}$ so that HIGH then contributes zero. LOW could reduce g_{LOW} so that HIGH still contributes zero, but LOW moves closer to her stand-alone contribution (which is her optimal contribution given that $g_{HIGH} = 0$). Thus, LOW's payoff increases as she moves down the vertical part of HIGH's best response function. Now suppose that $0 < g_{LOW} \leq \hat{g}_{HIGH}$ so that HIGH responds by ensuring that $G = \hat{g}_{HIGH}$. LOW player can reduce her first-mover contribution and HIGH will compensate by increasing her second-mover contribution so that overall provision remains at $G = \hat{g}_{HIGH}$. Thus LOW's payoff continues to increase as she moves down HIGH's best response function. Her most preferred point is where $g_{LOW} = 0$ and HIGH responds by choosing $g_{HIGH} = \hat{g}_{HIGH}$. Thus, when LOW moves first she contributes zero, free riding off the second mover's stand-alone contribution. This outcome is the same as with simultaneous moves.

If HIGH moves first she could also commit to contributing zero and rely on LOW to contribute $\hat{g}_{LOW}$, giving her a payoff of $w_{HIGH} + f_{HIGH}(\hat{g}_{LOW})$. If she were to contribute a small amount LOW would reduce her contribution to the public good so that total provision remains at $\hat{g}_{LOW}$. Thus HIGH's payoff would decrease, as she would enjoy a lower level of private good consumption and the same level of public good consumption. If HIGH contributes more than $\hat{g}_{LOW}$ then LOW contributes zero and HIGH's payoff will be $w_{HIGH} - g_{HIGH} + f_{HIGH}(g_{HIGH})$. In this range her payoff is maximized by her stand-alone contribution, $\hat{g}_{HIGH}$, leading to a payoff of $w_{HIGH} - \hat{g}_{HIGH} + f_{HIGH}(\hat{g}_{HIGH})$. HIGH's optimal first-mover contribution depends on the comparison between her payoff when she contributes zero, $w_{HIGH} + f_{HIGH}(\hat{g}_{LOW})$, and her payoff when she makes her stand-alone contribution, $w_{HIGH} - \hat{g}_{HIGH} + f_{HIGH}(\hat{g}_{HIGH})$.

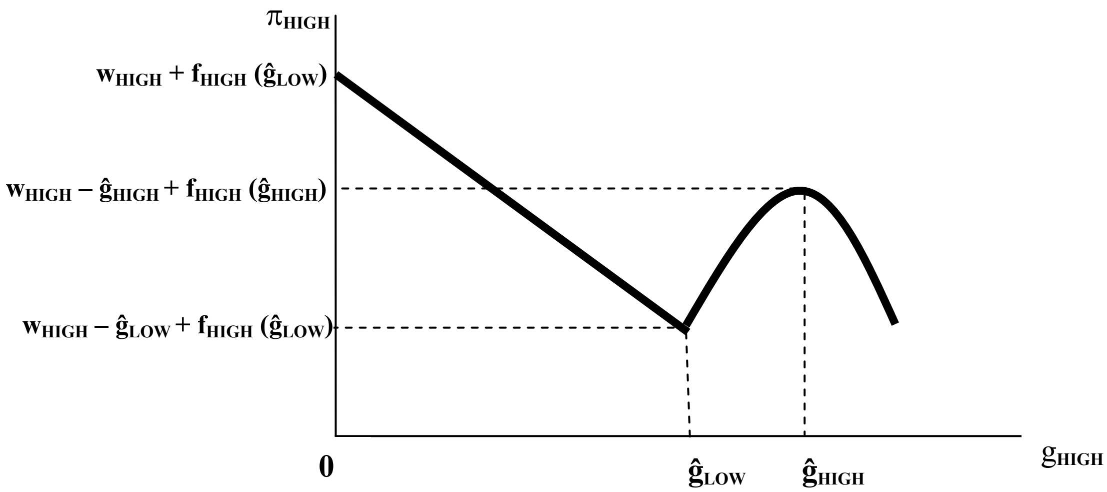
Figure AA.2 displays HIGH's payoff as a function of her contribution for each of the two cases. In the "No First Mover Advantage" case (NOFMA), with $f_{HIGH}(\hat{g}_{HIGH}) - f_{HIGH}(\hat{g}_{LOW}) > \hat{g}_{HIGH}$, the subgame perfect equilibrium is for HIGH to choose $g_{HIGH} = \hat{g}_{HIGH}$ and the LOW player responds with $g_{LOW} = 0$. Again, the outcome is the same as with simultaneous moves. However, in the "First Mover Advantage" case (FMA) where $f_{HIGH}(\hat{g}_{HIGH}) - f_{HIGH}(\hat{g}_{LOW}) < \hat{g}_{HIGH}$, the subgame perfect equilibrium is for HIGH to choose $g_{HIGH} = 0$ and LOW responds with $g_{LOW} = \hat{g}_{LOW}$. Here, since $\hat{g}_{LOW} < \hat{g}_{HIGH}$ public good provision is lower than with simultaneous moves. If the players have similar preferences $\hat{g}_{HIGH}$ will be similar to $\hat{g}_{LOW}$ and so $f_{HIGH}(\hat{g}_{HIGH}) - f_{HIGH}(\hat{g}_{LOW})$ will be close to zero, and we are in the FMA case. Thus when preferences are not too different and HIGH moves first, contributions are lower than with simultaneous moves.

The earnings tables used in the experiment were derived from a quadratic utility function of the form $\pi_i = 50(17 - g_i) + t_i(68G - G^2)$, where $t_{HIGH} = 1.32$ and $t_{LOW} = 0.89$ (FMA treatments) or 0.78 (NOFMA treatments), and earnings were then rounded to a multiple of 5 points. The rounding preserved the key features of the Varian model predictions. HIGH's stand-alone contribution is $\hat{g}_{HIGH} = 15$, and her best response function is $g_{HIGH} = \max\{15 - g_{LOW}, 0\}$. LOW's stand-alone contribution is $\hat{g}_{LOW} = 2$ (NOFMA) or $\hat{g}_{LOW} = 6$ (FMA). In the simultaneous move games the unique Nash equilibrium is $\hat{g}_{HIGH} = 15$, $g_{LOW} = 0$. In the sequential games the unique subgame perfect equilibrium is for the first-mover to contribute zero and the second-mover to best respond, except for HIGH-NOFMA, where the first-mover makes her stand-alone contribution, $\hat{g}_{HIGH} = 15$, and the second-mover best responds (and in equilibrium contributes zero).

Figure AA.2. HIGH player's payoff as a function of her first-mover contribution



No first mover advantage: $f_{HIGH}(\hat{g}_{HIGH}) - f_{HIGH}(\hat{g}_{LOW}) > \hat{g}_{HIGH}$



First mover advantage: $f_{HIGH}(\hat{g}_{HIGH}) - f_{HIGH}(\hat{g}_{LOW}) < \hat{g}_{HIGH}$

Appendix B: Experimental Instructions

Instructions

General

Welcome! You are about to take part in an experiment in the economics of decision making. You will be paid in private and in cash at the end of the experiment. The amount you earn will depend on your decisions, so please follow the instructions carefully. It is important that you do not talk to any of the other participants until the experiment is over. If you have a question at any time, raise your hand and a monitor will come to your desk to answer it.

The experiment will consist of fifteen rounds. There are sixteen participants in this room. Before the first round begins the computer will randomly assign the role of "RED" to eight participants and the role of "BLUE" to eight participants. You will be informed of your role, either RED or BLUE, at the beginning of round one and you will keep this role throughout the fifteen rounds. In each round the computer will randomly form eight pairs consisting of one RED and one BLUE participant. Thus, you will be randomly matched with another person in this room in each round, but this may be a different person from round to round. You will not learn who is matched with you in any round, neither during nor after today's session.

Each round is identical. In each round you and the person you are matched with will make choices and earn points. The point earnings will depend on the choices as we will explain below. At the end of the experiment one of the fifteen rounds will be selected at random. Your earnings from the experiment will depend on your point earnings in this randomly selected round. These point earnings will be converted into cash at a rate of 1p per point.

How You Earn Points

At the beginning of the round you will be given an endowment of 17 tokens. You have to decide how many of these tokens to place in a Private Account and how many to place in a Shared Account.

For each token you place in your Private Account you will earn 50 points, as shown in Table 1.

For each token placed in the Shared Account you will earn an additional amount, regardless of whether the token was placed by you or the person you are matched with. Likewise, for each token placed in the Shared Account the person you are matched with will earn an additional amount, regardless of whether the token was placed by you or them. Earnings from the Shared Account are shown in Table 2.

Your point earnings for the round will be the sum of your earnings from your Private Account and your earnings from the Shared Account.

So that everyone understands how choices translate into point earnings we will give an example and a test. Please note that the allocations of tokens used for the example and test are simply for illustrative purposes. In the experiment the allocations will depend on the actual choices of the participants.

[NOFMA treatments:

Example: Suppose RED places 9 tokens in his Private Account and 8 tokens in the Shared Account, and BLUE places 10 tokens in his Private Account and 7 tokens in the Shared Account. In this example there are a total of 15 tokens in the Shared Account. RED will earn 450 points from his Private Account, plus 1050 points from the Shared Account, for a total of 1500 points. BLUE will earn 500 points from his Private Account, plus 620 points from the Shared Account, for a total of 1120 points. **]**

[FMA treatments:

Example: Suppose RED places 9 tokens in his Private Account and 8 tokens in the Shared Account, and BLUE places 10 tokens in his Private Account and 7 tokens in the Shared Account. In this example there are a total of 15 tokens in the Shared Account. RED will earn 450 points from his Private Account, plus 1050 points from the Shared Account, for a total of 1500 points. BLUE will earn 500 points from his Private Account, plus 705 points from the Shared Account, for a total of 1205 points.**]**

Test: Before we continue with the instructions we want to make sure that everyone understands how their earnings are determined. Please answer the questions below. Raise your hand if you have a question. After a few minutes a monitor will check your answers. When everyone has answered the questions correctly we will continue with the instructions.

> Suppose RED allocates 11 tokens to his Private Account and 6 tokens to the Shared Account, and BLUE allocates 5 tokens to his Private Account and 12 tokens to the Shared Account.
>
> 1. What will be RED's point earnings from his private account? ___________
> 2. What will be RED's point earnings from the shared account? ___________
> 3. What will be RED's point earnings for the round? ___________
> 4. What will be BLUE's point earnings from his private account? ___________

5. What will be BLUE’s point earnings from the shared account? __________
6. What will be BLUE’s point earnings for the round? __________

How You Make Decisions

[Sequential treatments:

At the beginning of a round BLUE will make a decision about how to allocate his or her endowment by typing in a number of tokens to place in the Shared Account. BLUE can enter any whole number between 0 and 17 inclusive. The computer will then automatically place the remainder of BLUE’s endowment in BLUE’s Private Account.

The computer will then inform RED of BLUE’s decision.

After RED has seen how many tokens BLUE has allocated to the Shared Account, RED will decide how to allocate his or her endowment. RED will do this by typing in a number of tokens to place in the Shared Account. RED can enter any whole number between 0 and 17 inclusive. The computer will then automatically place the remainder of RED’s endowment in RED’s Private Account.

After RED has made his or her decision the computer will then show an information screen to both RED and BLUE. This screen will display the total number of tokens placed in the Shared Account and the earnings of each person for that round.**]**

[Simultaneous treatments:

At the beginning of a round you will make a decision about how to allocate your endowment by typing in a number of tokens to place in the Shared Account. You can enter any whole number between 0 and 17 inclusive. The computer will then automatically place the remainder of your endowment in your Private Account.

At the same time, the person with whom you are matched will be deciding how many tokens to place in the Shared Account by entering a number between 0 and 17 inclusive.

After you and the person you are matched with have both made your decisions the computer will then show an information screen to both RED and BLUE. This screen will display the total number of tokens placed in the Shared Account and the earnings of each person for that round.**]**

After you have read the information screen, you must click on the continue button to go on to the next round.

How Your Cash Earnings Are Determined

At the end of round fifteen there will be a random draw to select the round for which you will be paid. A poker chip will be drawn from a bag containing chips numbered from 1 to 15. The number on the chip will determine the round that is used for determining all participants' cash earnings. Your point earnings in this randomly selected round will be converted into cash at a rate of 1p per point. You will be paid in private and in cash.

Beginning the Experiment

Now, please look at your computer screen and begin making your decisions. If you have a question at any time please raise your hand and a monitor will come to your desk to answer it.

Appendix C: Earnings Tables

This appendix contains the earnings tables given to subjects.

FMA treatments:

EARNINGS TABLES

Table 1. Earnings from Your Private Account

Tokens in Your Private Account	Your Point Earnings from the Private Account
0	0
1	50
2	100
3	150
4	200
5	250
6	300
7	350
8	400
9	450
10	500
11	550
12	600
13	650
14	700
15	750
16	800
17	850

Table 2. Earnings from the Shared Account

Tokens in the Shared Accoun t	Red's Point Earnings from the Shared Account	Blue's Point Earnings from the Shared Account
0	0	0
1	90	60
2	180	120
3	260	175
4	340	230
5	415	285
6	490	340
7	565	385
8	635	430
9	700	475
10	765	520
11	825	560
12	885	600
13	940	635
14	995	670
15	1050	705
16	1095	740
17	1140	770
18	1180	800
19	1220	830
20	1260	855
21	1295	880
22	1330	900
23	1360	920
24	1385	940
25	1410	960
26	1435	975
27	1455	990
28	1470	1000
29	1485	1010
30	1500	1020
31	1510	1025
32	1515	1030
33	1520	1035
34	1525	1040

NOFMA treatments

EARNINGS TABLES

Table 1. Earnings from Your Private Account

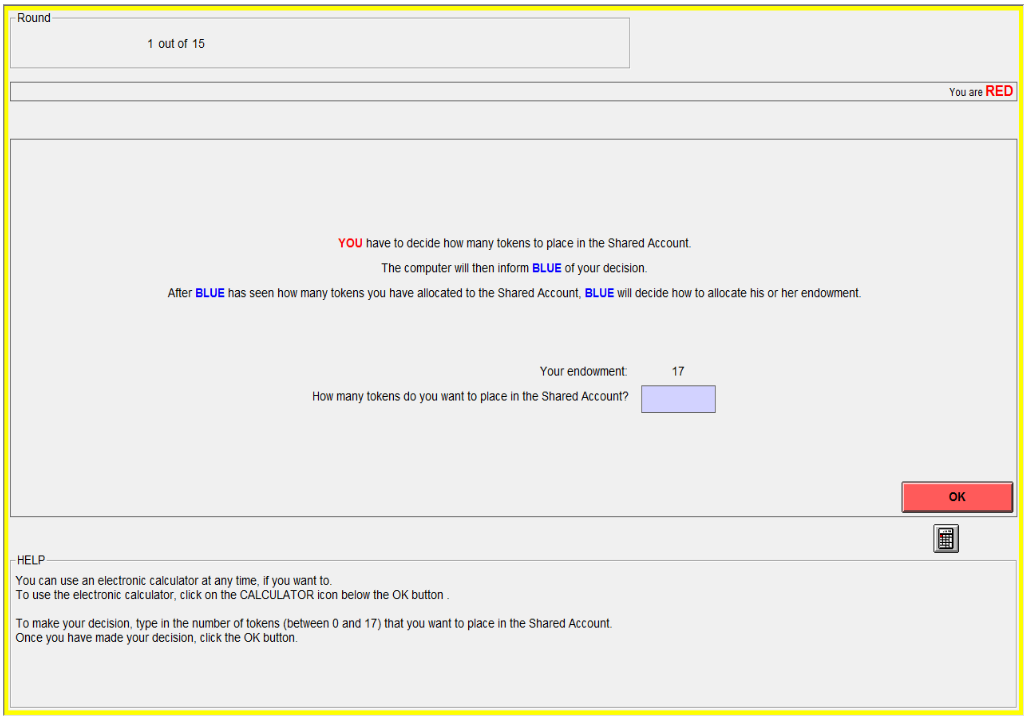
Tokens in Your Private Account	Your point Earnings from the Private Account
0	0
1	50
2	100
3	150
4	200
5	250
6	300
7	350
8	400
9	450
10	500
11	550
12	600
13	650
14	700
15	750
16	800
17	850

Table 2. Earnings from the Shared Account

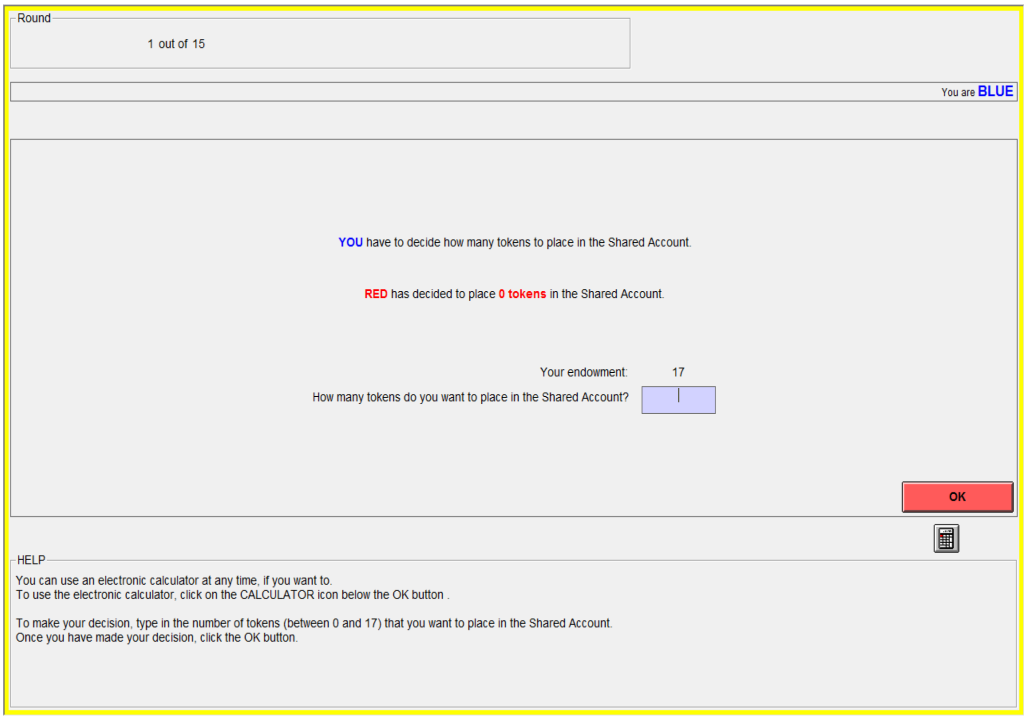
Tokens in the shared Accoun t	Red's point earnings from the shared Account	Blue's point Earnings from the shared Account
0	0	0
1	90	55
2	180	110
3	260	155
4	340	200
5	415	245
6	490	290
7	565	330
8	635	370
9	700	410
10	765	450
11	825	485
12	885	520
13	940	555
14	995	590
15	1050	620
16	1095	650
17	1140	675
18	1180	700
19	1220	725
20	1260	750
21	1295	770
22	1330	790
23	1360	805
24	1385	820
25	1410	835
26	1435	850
27	1455	860
28	1470	870
29	1485	880
30	1500	890
31	1510	895
32	1515	900
33	1520	905
34	1525	910

Appendix D: Screenshots of the decision screens used by subjects in the experiment

Screenshot of the decision screen used by a first-mover in a sequential treatment.



Screenshot of the decision screen used by a second-mover in a sequential treatment.



Appendix E: Average Contributions per treatment

Table AE.1. Average Contributions in SIM-FMA

	AGGREGATE		HIGH			LOW		
	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds
Session 1, Matching Group 1	12	10.2						
			5	5	4	*1*	4	1
			6	5.1	3.2	*2*	11.3	4
			7	12.2	12.8	*3*	0.3	0.6
			8	9.3	9.4	*4*	0.8	6
Session 1, Matching Group 2	15.4	13.7						
			13	12.3	13.8	*9*	1.1	0
			14	6.3	1.2	*10*	0.3	6.4
			15	17	17	*11*	1	0
			16	12.7	13.6	*12*	2.8	3
Session 2, Matching Group 1	13.7	12.8						
			5	15.7	13	*1*	2	4.4
			6	9.9	8.4	*2*	2.7	2.4
			7	8.8	8.2	*3*	1.9	0
			8	11.1	14.4	*4*	0.4	0.4
Session 2, Matching Group 2	16.1	13.5						
			13	7.9	3.2	*9*	4.7	6
			14	6.3	5	*10*	5.8	6
			15	10.9	10.2	*11*	0.3	5
			16	17	17	*12*	1.3	1.8
MEAN	14.3	12.6						
MEDIAN	15	13						

Table AE.2. Average Contributions in LOW-FMA

	AGGREGATE		HIGH			LOW		
	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds
Session 1, Matching Group 1	14.1	12.2						
			5	6.1	6	*1*	7.3	7
			6	11.6	12.4	*2*	5.5	1
			7	8.7	8.8	*3*	3.3	0.4
			8	9.3	8.4	*4*	4.6	5
Session 1, Matching Group 2	15	13						
			13	10.2	12.4	*9*	3.6	0.2
			14	12.1	10.6	*10*	4.5	4.2
			15	8.5	10.8	*11*	6.7	4.8
			16	8.7	5.6	*12*	5.8	3.6
Session 2, Matching Group 1	12.5	11.8						
			5	15.5	15	*1*	4.6	3.6
			6	6.5	5.2	*2*	2.3	2.4
			7	11.6	13.2	*3*	0.6	0
			8	7.2	7.2	*4*	1.8	0.6
Session 2, Matching Group 2	11.7	8.7						
			13	8.3	7.4	*9*	4.3	1
			14	9.7	11.2	*10*	4.6	3
			15	4.1	1.2	*11*	0.7	0.2
			16	11.9	10	*12*	3.1	1
MEAN	13.3	11.5						
MEDIAN	15	12.5						

Table AE.3. Average Contributions in HIGH-FMA

	AGGREGATE		HIGH			LOW		
	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds
Session 1, Matching Group 1	10.7	9.2						
			5	4.3	0.8	*1*	3.1	0.6
			6	16.4	17	*2*	3.4	4.2
			7	7.3	7.8	*3*	2.2	1.2
			8	3.6	3.4	*4*	2.6	2
Session 1, Matching Group 2	8.7	7.8						
			13	4.8	4	*9*	1.1	1
			14	6.5	5.2	*10*	3.3	3.2
			15	6.5	5	*11*	3.6	2.8
			16	7.9	8.2	*12*	1.1	1.8
Session 2, Matching Group 1	10.6	7.3						
			5	2.9	0	*1*	4.4	1.8
			6	9.1	5.4	*2*	3.6	4.2
			7	6.9	5.2	*3*	3.1	3
			8	10.3	8.8	*4*	2.1	0.8
Session 2, Matching Group 2	10.8	9.4						
			13	12	15	*9*	2.2	4.8
			14	4.3	0	*10*	0.5	0
			15	6.5	0	*11*	3.5	3
			16	14.1	15	*12*	0.1	0
MEAN	10.2	8.4						
MEDIAN	9	6						

Table AE.4.Average Contributions in SIM-NOFMA

	AGGREGATE		HIGH			LOW		
	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds
Session 1, Matching Group 1	16.2	15.3						
			5	14.8	15	*1*	4	4
			6	12.3	10.4	*2*	11.3	9.2
			7	7.1	8.2	*3*	0.3	0
			8	14.3	14.4	*4*	0.8	0.2
Session 1, Matching Group 2	15	15						
			13	13	14	*9*	1.1	0
			14	17	17	*10*	0.3	0
			15	13.1	16.6	*11*	1	0
			16	11.6	10.8	*12*	2.8	1.6
Session 2, Matching Group 1	13.5	12.5						
			5	11.3	11.2	*1*	2	2
			6	8.2	7.8	*2*	2.7	0
			7	12.2	13.2	*3*	1.9	1.2
			8	15.3	14.6	*4*	0.4	0
Session 2, Matching Group 2	13.4	12.6						
			13	10.7	11.2	*9*	4.7	4.6
			14	15.3	15	*10*	5.8	2.8
			15	3.7	1.6	*11*	0.3	0
			16	12.1	15	*12*	1.3	0.4
MEAN	14.5	13.9						
MEDIAN	15	14						

Table AE.5. Average Contributions in LOW-NOFMA

	AGGREGATE		HIGH			LOW		
	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds
Session 1, Matching Group 1	13.4	10.3						
			5	11.1	13	*1*	5.3	1.6
			6	10.8	6.4	*2*	5.4	0.2
			7	12.6	14	*3*	3	2.8
			8	3.6	1.6	*4*	1.9	1.6
Session 1, Matching Group 2	14.9	12.9						
			13	10.9	13.6	*9*	7.7	2.6
			14	7.8	7.2	*10*	2.9	2
			15	13.1	14	*11*	2.9	1
			16	8.3	10.4	*12*	5.8	1
Session 2, Matching Group 1	14.9	14.4						
			5	12.1	11.6	*1*	2.3	1
			6	12.6	12.8	*2*	0	0
			7	7.2	8.4	*3*	6.7	6.4
			8	16.3	16.4	*4*	2.3	1
Session 2, Matching Group 2	14.3	14.5						
			13	7.6	8.2	*9*	4.8	0.6
			14	9.3	11.2	*10*	3.7	3.2
			15	10.2	11.6	*11*	7	7
			16	9.2	11.2	*12*	5.4	5
MEAN	14.4	13						
MEDIAN	15	15						

Table AE.6. Average Contributions in HIGH-NOFMA

	AGGREGATE		HIGH			LOW		
	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds	*Subject ID*	All Rounds	Last 5 Rounds
Session 1, Matching Group 1	16.3	14.3						
			5	15	15	*1*	0.3	0
			6	11.8	11.4	*2*	1.9	1.8
			7	13.5	14.4	*3*	5.8	0
			8	14.2	13.6	*4*	2.5	1.2
Session 1, Matching Group 2	13	13.9						
			13	14.8	16	*9*	1.4	1.6
			14	6.1	7.4	*10*	0.1	0
			15	14.3	15	*11*	0.3	0
			16	14.9	15	*12*	0.3	0.8
Session 2, Matching Group 1	12.2	12.9						
			5	14.7	15	*1*	0.1	0
			6	13.1	15	*2*	0.3	0
			7	8.5	12.8	*3*	0.7	0
			8	10.3	9	*4*	1.2	0.8
Session 2, Matching Group 2	14.9	15						
			13	14.5	15	*9*	0.3	0
			14	15.5	15	*10*	0	0
			15	15.3	15	*11*	0	0
			16	13.1	15	*12*	0.9	0.2
MEAN	14.1	14.1						
MEDIAN	15	15						

CHAPTER 4

THE IMPACT OF SOCIAL COMPARISONS ON RECIPROCITY*

4.1 Introduction

Reciprocity can have an important influence on economic behaviour, as demonstrated in a wide range of experiments where many individuals forgo some of their own earnings to reward the generosity of others.[35] Much of this evidence, however, comes from stylized social environments which lack important elements of naturally occurring social situations. In natural settings interactions between individuals are often non-anonymous and contextualized, and the treatment and behaviour of similar others in similar circumstances is often observable. This chapter investigates how exposure to *social comparison information about referent others* (i.e. learning what similar others do and how

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* This chapter is based on joint work with Simon Gächter and Martin Sefton.

[35] For reviews and discussions of the relevance of reciprocity in economics, see Fehr and Gächter (2000) and Fehr and Fischbacher (2002). For general overviews of the importance of reciprocity in social interactions, see Cialdini (2001) and Gintis et al. (2005).

they are treated) influences the extent to which individuals comply with norms of positive reciprocity.

The next section discusses the related literatures in detail. We note that evidence of reciprocity is largely based on bilateral interactions as represented by gift-exchange, investment and sequential dilemma games. Naturally occurring interactions, on the other hand, take place in complex social environments where individuals are typically exposed to considerable amounts of social information about similar others, which can ultimately affect their attitudes and behaviour. For example, in natural social situations individuals can often observe how similar others behave in similar circumstances, and this may in turn provide valuable guidance in understanding how one is expected to behave. Moreover, in natural social environments information about others' material well-being is also sometimes available and may influence the way individuals evaluate their own well-being (Fliessbach et al. (2007)), and hence their behaviour. Such pieces of social comparison information may be particularly salient in the workplace. In natural organizations a worker can observe the relationship between other workers and the employer, i.e. workers can observe how similar others are rewarded by the employer (*pay comparison information*) and how they behave and perform (*effort comparison information*). The availability of such pieces of social information may in turn affect a worker's attitude towards the employer and the extent to which he or she complies with norms of vertical reciprocity.

Section 4.3 describes our experimental design. Our paradigm for studying how social comparisons influence reciprocal behaviour is the gift-exchange game. In a standard bilateral gift-exchange game a first-mover decides on the size of the gift she sends to a second-mover, who can in turn reciprocate by choosing costly actions that reward the first-mover. In standard gift-exchange game experiments many second-movers are willing to eschew their private interests to reciprocate first-movers' gifts. In particular, the larger the gift, the stronger is the second-movers' reciprocation. In this chapter we extend the standard bilateral setting to a three-person gift-exchange game with a labour market frame. In our experiment subjects are grouped in experimental "firms" composed of three members, labelled "Employer", "Employee 1" and "Employee 2". The game begins with the Employer choosing wages (gifts) for each employee, which are then publicly observed. A key feature of the game is that the two employees then choose costly efforts sequentially: thus, while both have full information about relative wages at the time they make effort choices, only Employee 2, who moves last, has access to information about the co-worker's effort. Hence, Employee 1's behaviour reveals how pay comparison information in isolation from effort comparison information influences effort choices and reciprocity, while from Employee 2's decisions we can study the effects of the concurrent availability of pay and effort comparison information.

We report our results in Section 4.4. The four main findings from our study are as follows. *1)* As in many other gift-exchange game experiments we also observe a strong, positive own wage-effort relationship: employees reciprocate higher own wages with higher effort. *2)* Exposure to pay comparison information has little impact on the observed wage-effort relationship: while own wage is a powerful determinant of own effort, co-workers' wages have virtually no effect. *3)* When the wage for both employees is high, the effort level for Employee 2 is sensitive to the effort provided by Employee 1: if Employees 2 receive a high wage but observe that their co-worker who is also paid generously chooses minimal effort, they tend to expend low effort. If Employees 2 receive a high wage and observe that the highly paid co-worker expends high effort, they also strongly reciprocate generous wage offers. When Employee 2's wage is high and Employee 1's wage is low, there is a slight negative relationship between own effort and the co-worker's effort. When Employee 2's wage is low, the co-worker's effort does not affect own effort. *4)* On average the Employer does worse when both pay and effort information are provided, suggesting that social comparisons have an overall detrimental impact on reciprocity.

We discuss these results in Section 4.5. We believe that our findings extend beyond the labour market context used in our experiment. Because reciprocity has been observed in many games (see below), our results suggest that social comparisons may play an important role in shaping reciprocal relationships whenever reciprocity is relevant in natural social environments. Our finding that, on average, social comparison information undermines reciprocity is related to other recent findings which suggest that individuals tend to evaluate the social information available in the environment in a way beneficial to their self-interest. However, we also find that social information sometimes has beneficial effects, and we suggest that group composition may be an important tool for harnessing the positive effects of social comparisons.

4.2. Related Literature

Numerous studies have shown the importance of positive reciprocity for economic behaviour. Using simple bilateral games such as investment (Berg, Dickhaut and McCabe (1995)), sequential prisoner's dilemma (Clark and Sefton (2001)) and gift-exchange games (Fehr, Kirchsteiger and Riedl (1993)), numerous experimental studies have shown that people are willing to incur costs to reward kind actions, even in non-repeated and anonymous interactions where there are no positive future consequences associated with reciprocal behaviour.

A particularly suitable framework for our research question is the gift-exchange game (GEG). This game reproduces a contractually incomplete labour relation where an "employer" makes a wage offer to an "employee" who, upon acceptance, chooses how much costly effort to supply. GEG experiments have been extensively used to examine the "fair wage-effort hypothesis" formulated in the seminal work by Akerlof and Yellen (1990). According to this hypothesis labour relations can be described as a reciprocal "gift exchange": employees are willing to "gift" harder work effort to their employers in exchange for a fair wage. Consistent with the fair wage-effort hypothesis, many GEG laboratory experiments, including some with one-shot situations where decisions are made anonymously and pure self-interest would lead employees to shirk, have shown that employees systematically choose to reciprocate generous wage offers with higher effort (e.g., Fehr, Kirchsteiger and Riedl (1993); Fehr, Gächter and Kirchsteiger (1997); Gächter and Falk (2002); Brandts and Charness (2004); Charness (2004); Maximiano, Sloof and Sonnemans (2007) - for a recent review of the experimental literature see Fehr et al., 2009)). Thus, positive reciprocity has been extensively documented in many simple GEG studies, providing support for the relevance of the norm of reciprocity in these social situations.[36]

[36] Evidence of varying degrees of reciprocal gift-exchange has been also found in numerous field studies and real effort experiments, see, e.g., Fehr and List (2004), Gneezy and List (2006); Falk (2007); Maréchal and Thöni (2007); Cohn, Goette and Fehr (2008); Bellemare and Shearer (2009); Kube, Maréchal and Puppe (2010); Hennig-Schmidt, Rockenbach and Sadrieh (forthcoming).

However, we note that settings where reciprocity may be important are often complex social systems, hardly resembling the stylized environments studied in standard GEG experiments where subjects interact in isolated pairs. For example, in workplaces employers typically interact with many employees at the same time and individuals are typically exposed to considerable amounts of social information about how similar others are treated by the employer (*pay comparison information*) and about how they behave (*effort comparison information*). The availability of such pieces of social information may in turn influence individuals' compliance with norms of vertical reciprocity systematically.

A number of recent studies suggest in fact that information about the behaviour and treatment of others can systematically affect individuals' behaviour in a variety of settings. For example, a number of dictator game experiments have shown that dictators tend to behave more (less) generously towards recipients when they are informed about generous (selfish) choices made by other participants (Cason and Mui (1998); Bicchieri and Xiao (2009); Krupka and Weber (2009)). Analogous evidence on the importance of social information comes from ultimatum game experiments (Knez and Camerer (1995); Bohnet and Zeckhauser (2004)) and public goods game experiments (Carpenter (2004); Bardsley and Sausgruber (2005); Sausgruber (2009)).[37]

The relevance of pay comparison information in the workplace is suggested by abundant survey and case-study evidence pointing to the importance of horizontal fairness concerns (i.e. fairness between employees) in

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[37] An exception appears to be Brandts and Fatás (2004), who do not find any significant impact of social information on behaviour in a two-person public goods game.

labour relations (e.g., Campbell III and Kamlani (1997); Bewley (1999)). Furthermore, pay comparisons constitute a central component in a number of theoretical approaches which build on equity theory (Adams (1965)) to improve the understanding of labour relations. In fact, the "fair wage" in Akerlof and Yellen's model is defined in relative terms, as employees compare their own pay with their peers' to judge how fairly they are being treated by the employer. Nevertheless, empirical support for the notion that pay comparisons systematically influence employees' behaviour remains weak at best, both in the field and in the lab. Field studies exploring the relation between pay dispersion within an organization and its performance have produced mixed results (see, e.g., Leonard (1990); Cowherd and Levine (1992); Main, O'Reilly and Wade (1993); Eriksson (1999); Winter-Ebmer and Zweimuller (1999); Hibbs and Locking (2000); Martins (2008)). The empirical evidence from laboratory experiments is scarce and equally inconclusive, as some experiments report that horizontal fairness concerns can have a negative impact on work effort (Gächter and Thöni (2009); Abeler et al. (forthcoming); Clark, Masclet and Villeval (forthcoming)), while in others such effects are weak or absent (Güth et al. (2001); Charness and Kuhn (2007)).

A possible explanation for these mixed findings is that the prominence of horizontal fairness considerations may crucially depend on the *concurrent* availability of pay and effort comparison information. Konow (1996, p. 22) makes the general point that "*…information plays an important role in determining the extent to which, indeed whether, a situation will be judged fair or unfair…*", and in the context of multi-worker firms individuals may struggle to develop clear judgments of what constitutes a fair distribution of earnings when they know what their co-workers are paid but have incomplete information about how they perform.

Moreover, effort comparison information may have an important influence on behaviour *per se*, independently of pay comparison processes: the ability to observe how similar others behave in a given situation may in fact provide informative cues about how one is expected to behave (see, e.g., Cialdini, Reno and Kallgren (1990), and Keizer, Lindenberg and Steg (2008)). A number of recent field studies and real effort experiments have shown that individual effort behaviour can be systematically affected by information about the effort behaviour of others (see, e.g., Falk and Ichino (2006); Mas and Moretti (2009); Bellemare, Lepage and Shearer (2010)).

In summary, while there is large body of evidence on the importance of reciprocal considerations in a variety of settings and social situations, there is also some evidence that individuals' behaviour can be systematically affected by social information about the behaviour and treatment of others. We argue that exposure to social comparison information is typical in natural environments, and hence we believe that studying the behavioural effects of social comparisons can add in important ways to the understanding of reciprocal relationships in natural social situations. In this study our focus is inspired by behaviour in workplaces, where two pieces of social information that may be particularly salient are pay and effort comparison information. We note that the weak empirical evidence for pay comparison effects may be because clear judgments of what constitutes a fair distribution of earnings fail to emerge when individuals know what others are paid but have incomplete information about how they perform. To gauge the plausibility of this argument, in our experiment we study the impact of pay comparison information on effort behaviour both when effort comparison information is concurrently available, and when instead it is not available. Effort comparison information may also have important effects on behaviour independently of pay comparison processes: a number of field and real effort studies have shown that individual effort behaviour is systematically affected by the effort behaviour of similar others. A potential difficulty with the use of field or real effort data is that it is generally hard to strip down the mechanics of effort comparison processes because a number of different forces (e.g. social pressure, learning, social preferences, self-motivation considerations) may simultaneously operate in these settings, potentially confounding the interpretation of results. Our laboratory experiment is based on a tightly controlled abstract setting where subjects interact once and anonymously, and make decisions that lead to quantifiable payoff consequences. Thus, our design allows us to isolate a pure, preference-based form of effort comparisons, separately from the (potentially confounding) effects of social pressure, self-motivation considerations and knowledge spillovers.

4.3. Experimental Design & Procedures

4.3.1. The Experimental Game

Our aim is to set up a GEG where we can study how the combination of pay and effort comparison information affects employees' reciprocal behaviour. To achieve this aim, we adapt the payoff structure from the experiment by Fehr, Gächter and Kirchsteiger (1997) and modify the GEG used in Fehr et al. (1998). In our experiment each firm is composed of three members: Employer, Employee 1 and Employee 2. All players move sequentially: the Employer moves first and chooses a wage $w_i \in \{16, 32\}$ for each Employee $i \in \{1, 2\}$. The

Employer can (but does not have to) choose different wages for different employees. Employee 1 observes both wages and then chooses an effort level $e_1 \in \{1,2,3,4\}$. Employee 2 observes both wages *and* the effort chosen by Employee 1 and then chooses an effort level $e_2 \in \{1,2,3,4\}$. After Employee 2's choice, the game ends and the Employer's earnings are computed as:

$$\pi_{ER} = 10 \cdot (e_1 + e_2) - w_1 - w_2$$

and employee *i*'s earnings are computed as:

$$\pi_i = w_i - 5 \cdot (e_i - 1).$$

Our implementation of the game used the strategy method (Selten (1967)), i.e. subjects had to specify complete strategies in the game-theoretic sense. Participants in the role of Employee 1 specified four effort choices, one for each wage combination that could possibly be chosen by the Employer. Participants in the role of Employee 2 specified sixteen effort choices, one for each of the sixteen possible combinations of wages and effort chosen by the Employer and Employee 1 (four possible wage combinations times four different possible effort choices by Employee 1).

We implemented a one-shot version of this experimental game. The game was described to subjects using the same labour market frame that we use throughout the text.

4.3.2. Discussion of the Design

There are a number of reasons why we use the GEG to address the questions we are interested in. First, as discussed in the previous section, positive reciprocity has been documented extensively in many GEG experiments, providing support for the relevance of the norm of reciprocity in these social situations. Second, also as argued earlier, individuals are typically exposed to considerable amounts of social information about similar others in naturally occurring organizations, and hence our three-person GEG provides the minimal environment to study how social comparisons influence behaviour. Lastly, the particular GEG we use in our experiment, as compared to other experimental settings, provides a cleaner environment for studying the "pure" effects of social comparison information on reciprocity. On this last point, while some environments are simply not suitable to study positive reciprocity (e.g. the dictator game), in others (e.g. public good games) actions of other players have a direct impact on one's own monetary payoffs and hence on reciprocal considerations: thus, reactions to information about others' contributions to a public good may reflect direct reciprocation (i.e., a reaction to a kind or unkind act by another) rather than the effect of social information *per se*.[38] In our three-person GEG, on the other hand, the wage the employer pays to the co-worker and the co-worker's effort do not directly affect an employee's payoff and hence cannot be used to develop pure reciprocal considerations.

The fully sequential structure of the constituent game allows us to observe effort decisions in environments which contain different amounts of social information. Subjects in the role of Employee 1 move after the Employer and before subjects in the role of Employee 2: thus they can condition their effort on both the level of the own wage and the level of the co-worker's wage, but not on the co-worker's effort. Hence, they have access to pay comparison information, but not effort comparison information, and their effort choices can be represented by an effort function $e_1 = f(w_1, w_2)$. Subjects in the role of

[38] Brandts and Fatás (2004) and Bardsley and Sausgruber (2005) propose experimental designs that allow to overcome this identification problem in voluntary contribution games.

Employee 2 move last and are fully informed about the co-worker's effort as well as about relative wages, and their effort choices can be represented by an augmented effort function $e_2 = g(w_2, w_1, e_1)$, which describes how social information affects reciprocity when both pay and effort comparison information are available.

We believe that the use of the strategy method in our experiment is necessary because it allows us to observe subjects' behaviour across all the information sets they control in the game without either using deception or resorting to repeated play, which might induce strategic confounds and still does not guarantee the collection of a sufficient number of observations in all the different wage and/or effort combinations for all employees. With the strategy method we are able to elicit from each employee a *complete* effort function, $e_1 = f(w_1, w_2)$ or $e_2 = g(w_2, w_1, e_1)$, which fully specifies how individuals respond to changes in the own wage, and in the co-worker's wage and effort: thus, we are able to measure own wage-effort relations at the individual level, and to investigate how these are affected by social comparison information. Moreover, at the time a player makes decisions, she is not informed of the actual decisions of any other player, and so this feature of the design also preserves the statistical independence of each subject's decisions vis-à-vis those of other subjects.[39]

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[39] The use of the strategy method in experiments has undergone a (still open) methodological debate. The question is whether behavioural responses differ when they are elicited in a "hot" version of the game (i.e., when subjects directly respond to decisions made by other players) rather than in a "cold" version (i.e., when the game is played with the strategy method). The issue has been addressed in a number of experimental studies with mixed results: some authors find evidence for the existence of a "hot-cold empathy gap" (e.g., Güth, Huck and Muller (2001); Brosig, Weimann and Yang (2003); Casari and Cason (2009)), while in other studies behaviour does not seem to be sensitive to the elicitation method (e.g., Cason and Mui (1998); Brandts and

Given that our main interest is to isolate the impact of social information on reciprocal behaviour, we kept the structure of the decision situation as simple as possible. In particular our setting involves no productivity differences or technological interdependences between employees. One could argue that the absence of productivity differences might reduce the scope for observing wage differences between employees, thus posing a threat to the interpretability of subjects' responses to seemingly arbitrary unequal wage offers. Although productivity differences between employees constitute an important reason why employers may want to introduce pay differentials, there also exist other rationales for unequal wage offers. For example, employers may find it optimal to choose unequal wages if they believe workers will supply high effort only if they are paid more than co-workers. We implemented a one-shot version of our game in order to study the mechanics of social comparisons in a stripped-down environment which mutes the (confounding) influence of strategic considerations that may arise due to repeated play.

4.3.3. Experimental Procedures

The experiment was conducted at the University of Nottingham using subjects recruited from a university-wide pool of students who had previously indicated

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Charness (2000); Oxoby and McLeish (2004); Solnick (2007); Muller et al. (2008)). See Brandts and Charness (2009) for a survey of the experimental literature. We believe that the use of the strategy method is unlikely to distort behaviour in our experiment. The study by Cason and Mui (1998) provides support for the behavioural validity of the strategy method in experimental situations where individuals are exposed to social information. Moreover, the literature suggests that the "hot-cold empathy gap" may be particularly relevant for negative reciprocity, while positive reciprocity appears to be less sensitive to the elicitation method (see, e.g., Brosig, Weimann and Yang (2003)). If anything, the use of the strategy method appears to dampen reciprocal responses (e.g, Casari and Cason (2009)); Fischbacher and Gächter (forthcoming)), and thus our experiment can be thought as a tool that defines a *lower* bound to the reciprocal relationships we are interested in.

their willingness to be paid volunteers in decision-making experiments.[40] Six sessions were conducted with a total of 84 participants, 28 in each role. The average age was 20 years and 58 percent were male. No subject took part in more than one session.

Upon arrival, subjects were welcomed and randomly seated at visually separated computer terminals. Subjects were given 15 minutes to read through the instructions, and then the experimenter read aloud a briefer précis outlining the most important points contained in the instructions.[41] Subjects were then randomly assigned to a group and a role. All decisions were made anonymously, and neither during nor after the experiment were subjects informed about the identity of the other members of their firm. Before proceeding to the decision stage, subjects were guided through two role-specific video presentations which carefully illustrated the main features of the decision screens they were going to use during the experiment.[42] The first video presentation explained the functioning of an on-screen electronic calculator (the *What-if-calculator*) that subjects could use to compute their and other players' payoffs. At the end of the first video presentation, subjects were asked to solve a set of control questions and they could not enter the decision stage unless they had solved all the questions correctly. The second video presentation showed subjects how to enter their choices in a Decision Table and explained once again the structure of the game and the strategy method.

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[40] The experiment was programmed and conducted with the software z-Tree (Fischbacher (2007)). Subjects were recruited through the online recruitment system ORSEE (Greiner (2004)).

[41] The experimental instructions and the précis are reproduced in Appendix A and Appendix B at the end of this chapter.

[42] Video presentations were shown to subjects individually in z-Tree. Video presentations and the software are available upon request.

On average the experimental sessions lasted about one hour, including the reading of the instructions and of the précis and the completion of a post-experimental questionnaire. All participants were endowed with an initial amount of 95 points, and earnings from the decision task (which could be negative) were added to this initial amount.[43] At the end of the session, the final point earnings were converted into British Pounds at a rate of £0.10 per point. Subjects were paid in private and in cash at the end of each session. Subject earnings ranged from £5.10 to £12.70, averaging £10.30 (approximately $21 at the time of the experiment).

4.4. Results

In our experiment employers can choose from four possible wage combinations. Of the 28 employers, 6 (21 percent) paid the high wage to both employees, 7 (25 percent) paid different wages, and 15 (54 percent) paid the low wage to both employees. In the rest of this section we examine how employees reacted to the different wage combinations. We start by exploring whether pay comparison information affects effort behaviour among Employees 1, i.e. when employees can access it in isolation from effort comparison information. We then turn to Employees 2 and explore the impact on effort of the simultaneous exposure to pay and effort comparison information. Lastly, we compare behaviour in the two environments to assess the overall impact of social comparisons on reciprocity.

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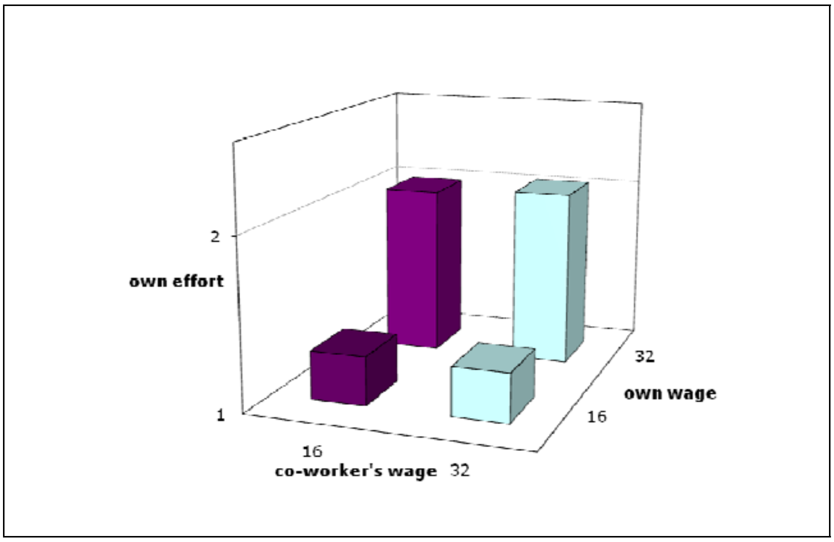
[43] Note that subjects in the role of the Employer could incur losses from the decision task. The initial endowment outweighed any possible losses.

4.4.1. Social Comparisons and Effort among Employees 1

Figure 4.1 displays the average effort expended by the twenty-eight Employees 1 who participated in our experiment. The two bars in the front row represent the wage combinations where the employee gets a low (16 points) own wage, while the bars at the back correspond to the two cases where the own wage is high (32 points). Darker bars represent wage combinations where the co-worker gets a low wage, while lighter bars correspond to the two cases where the co-worker is paid a high wage.

Figure 4.1. Average Effort - Employees 1



An immediately apparent feature of Figure 4.1 is that Employees 1 expend more effort when they are paid a high wage. In fact, employees' average effort when the own wage is high exceeds the effort exerted when the own wage is low by around 0.714 when the co-worker's wage is low, and by 0.750 when the co-worker's wage is high. Both differences are highly significant ($p < 0.001$ in both cases).[44] This pattern reproduces the standard

[44] All p-values are based on two-sided Wilcoxon matched-pairs signed-ranks tests, unless reported otherwise.

"reciprocity result" documented in the GEG literature: employees reciprocate higher wages with higher effort.

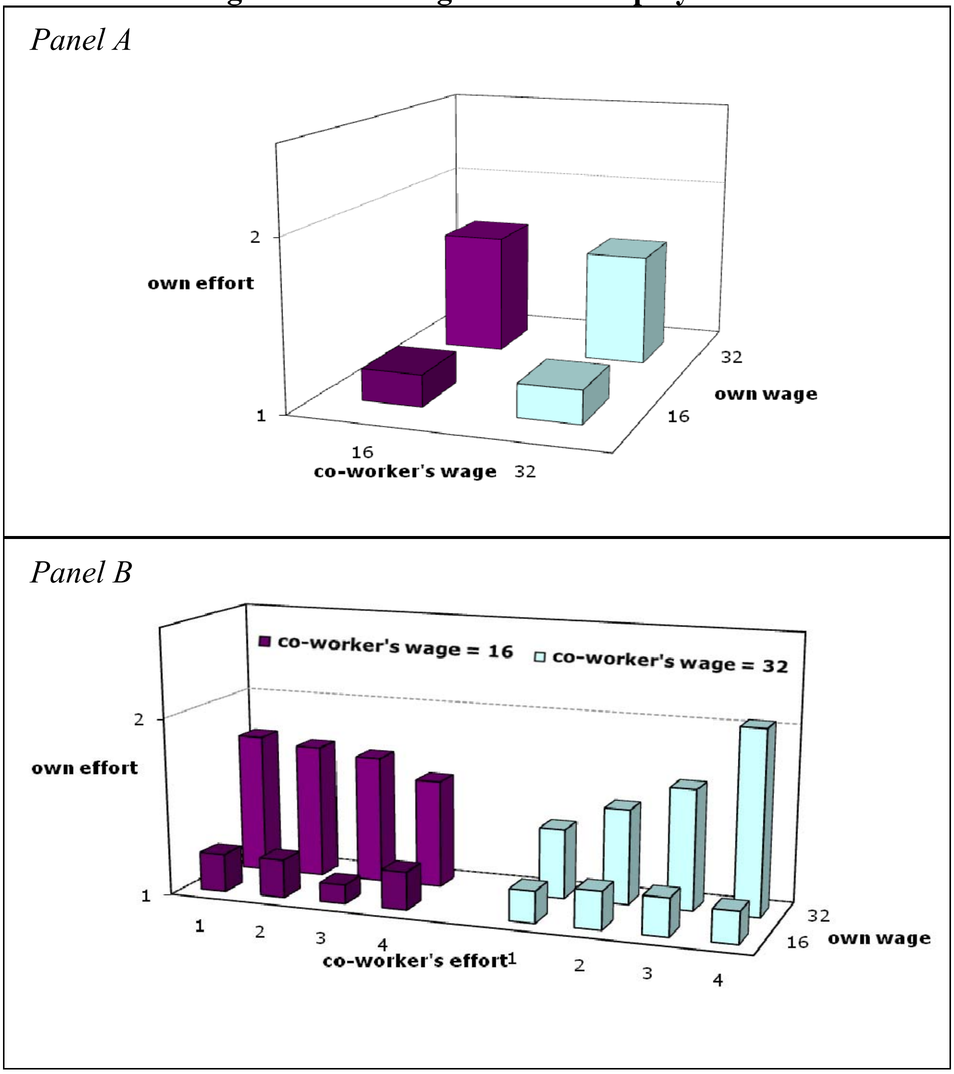
Another noticeable feature of Figure 4.1 is that social information has virtually no effect on Employee 1's effort choices: for a given own wage, they expend roughly the same effort irrespective of the wage the Employer pays to the co-worker ($p = 1.000$ when the own wage is low, and $p = 0.739$ when the own wage is high). We conclude that Employees 1 reciprocate high wages with higher effort, but pay comparison information has no impact on effort decisions.

4.4.2. Social Comparisons and Effort among Employees 2

We now turn the attention to our twenty-eight Employees 2, who were simultaneously exposed to pay and effort comparison information. Figure 4.2 shows Employee 2's average effort and is divided in two panels. Panel A has the same structure of Figure 4.1 above and shows average effort per wage combination, aggregating across different levels of the co-worker's effort. Panel B disaggregates the average efforts of Panel A by the four different levels of the co-worker's effort.

A first feature which is apparent in both panels of Figure 4.2 is that also Employees 2 increase their effort when they are paid the high wage. Ignoring for a moment the effects of social information and averaging across contingencies where the own wage is the same (i.e. across rows in Figure 4.2), we note that Employee 2's effort increases on average by about 0.495 and the effect is highly significant ($p < 0.001$). In fact, for each combination of the co-worker's wage and effort shown in Panel B of Figure 4.2, the mean increase in effort of Employee 2 after an increase in the own wage differs from zero at $p < 0.058$.[45]

Figure 4.2. Average Effort - Employees 2



The figure shows Employee 2’s average effort. Panel A shows average effort per wage combination, aggregating across different levels of the co-worker’s effort. Panel B disaggregates effort choices by different levels of the co-worker’s effort.

A second notable feature of Figure 4.2 is that, as with Employees 1, information about the co-worker’s wage has on average no effect on Employee 2’s effort. This is particularly evident in Panel A of Figure 4.2: for a given own wage, Employees 2 expend roughly the same effort irrespective of the wage the

[45] The p-values range from 0.0578 for the case where the co-worker is paid a high wage and supplies minimal effort to 0.0004 for the case where the co-worker is paid a high wage and supplies maximal effort.

Employer pays to the co-worker ($p = 0.740$ when the own wage is low, and $p = 0.639$ when the own wage is high). Thus, pay comparisons do not seem to affect effort decisions even when effort comparison information is concurrently available.

The most noteworthy feature of Figure 4.2 can be found in Panel B, which clearly shows that effort comparison information can substantially affect Employee 2's effort decisions. In the wage combination where the Employer pays a high wage to both employees the magnitude of Employees 2's effort crucially depends on the co-worker's effort: employees strongly increase their effort as the co-workers expend higher effort. When Employee 2 is paid a high wage and the co-worker's wage is low information about the co-worker's effort seems to have a slightly negative effect on Employee 2's effort. When Employee 2's wage is low there is no clear relation between Employee 2's effort and the co-worker's effort.

We examined the impact of effort comparison information on Employee 2's effort decisions using the regression model

$$e_2 = \alpha_0 + \alpha_1 * high\ wage + \alpha_2 * e_1 + \alpha_3 * high\ wage * e_1 + \varepsilon$$

where the explanatory variable "*high wage*" is a dummy variable which assumes the value 1 when the own wage is high and 0 otherwise. Note that this models e_2 as a linear function of e_1, where the marginal impact of e_1 on e_2 is α_2 (when own wage is low) or $\alpha_2 + \alpha_3$ (when own wage is high). We used OLS to estimate separate models for the cases where the co-worker's wage is low or high and report the results in Table 4.1.

In both models the estimate of α_2 is very close to, and not significantly different from, zero, showing that no relation between employees' effort exists when the Employer pays a low wage to Employee 2. When the co-worker is paid a low wage the estimate of $\alpha_2 + \alpha_3$ is negative. This reflects the decline in effort that is apparent in Panel B of Figure 4.2 (bars corresponding to own wage = 32, co-worker's wage = 16). However, the estimate is low and not significantly different from zero ($F(1,27) = 0.73$; $p = 0.401$). In contrast, when the co-worker is paid a high wage the estimate of $\alpha_2 + \alpha_3$ is positive and statistically significant ($F(1,27) = 7.24$; $p = 0.012$). Thus, when the Employer pays a high wage to both workers, Employees 2 systematically increase their effort when the co-worker also does so. The impact of this effect on effort is remarkable. When the co-worker receives a high wage and chooses minimal effort, the Employer elicits only a small amount of extra effort (about 0.15) from Employees 2 by increasing their wage from 16 to 32. On the other hand, if the co-worker chooses maximum effort Employees 2's reciprocal response is about five times this (about 0.77). Thus, the own wage-effort relationship is significantly affected by effort comparison information.

Table 4.1. Employee 2's Effort Regressions

	co-worker's wage is LOW	co-worker's wage is HIGH
high wage	0.643***	-0.054
	(0.169)	*(0.147)*
e_1	-0.011	0.000
	(0.052)	*(0.037)*
e_1 * high wage	-0.046	0.207***
	(0.049)	*(0.067)*
constant	1.214***	1.196***
	(0.145)	*(0.122)*
N.	*224*	*224*
F-statistic	*F(3,27) = 5.48*	*F(3,27) = 6.69*
Prob > F	*0.004*	*0.002*
R^2:	*0.110*	*0.134*

Dependent variable is Employee 2's effort. Robust standard errors in parentheses adjusted for intragroup correlation (subjects are used as independent clustering units). *** $p < .01$.

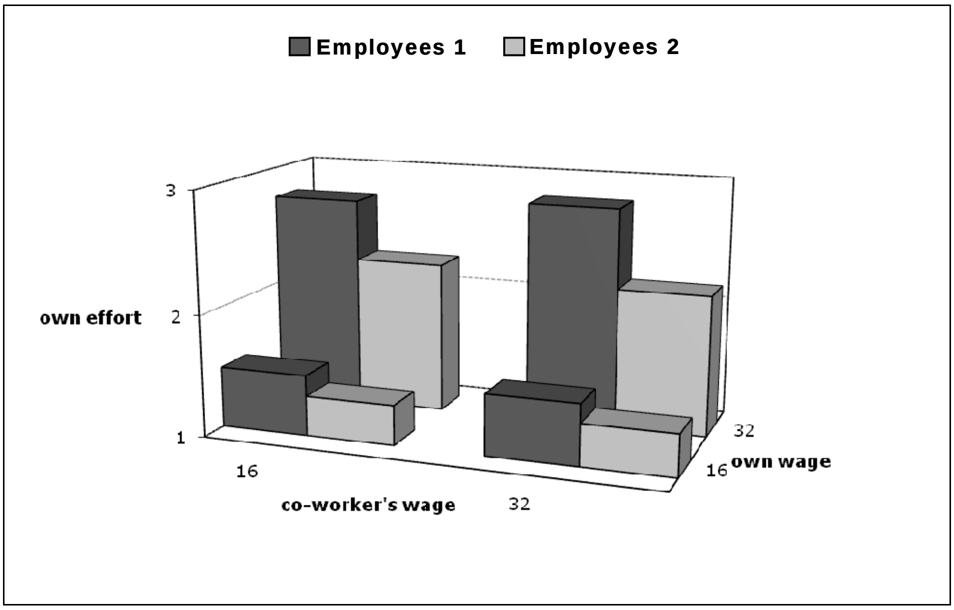
4.4.3. The Impact of Social Comparisons on Reciprocity

Our results show that social comparisons can shape the intensity of employees' reciprocal responses towards the employer in important ways. In particular, whereas information about the co-worker's wage has little impact on effort choices, we find that the availability of information about the co-worker's effort can be a crucial piece of social information in our setting: when the Employer offers a high wage to both Employees, Employees 2's reciprocal behaviour depends on the reciprocal behaviour of the co-worker.

A natural question is then whether an employer could in principle exploit this dependency in employees' reciprocal responses, by adopting a policy of paying equally generous wages *and* making information on co-workers' efforts available. Would such a policy make the employer better off on average, strengthening the reciprocity of those employees who observe reciprocal behaviour on the part of others? Or would any positive effect of social information be rather outweighed by the negative effects which may occur when reciprocally motivated employees observe that others do not comply with norms of reciprocity?

To answer these questions, we investigate whether the availability of effort comparison information had on average a beneficial or detrimental effect on employees' reciprocity in our experiment. We do so by comparing the average reciprocal response of Employees 1, who were just exposed to pay comparison information, with the reciprocal response of Employees 2, who had access to both pay and effort comparison information. As we are interested in the impact of effort comparison information on reciprocity, we focus on "non-selfish" employees, i.e. those employees who made at least one non-minimal effort choice in the experiment. Out of our 56 employees, 32 (57 percent) are non-selfish: 16 Employees 1 and 16 Employees 2. Figure 4.3 and Table 4.2 restrict attention to these employees. The figure shows the effort expended on average by non-selfish Employees 1 (dark bars) and Employees 2 (light bars) in the four wage combinations possibly chosen by the Employer.[46]

On average, Employees 1's effort exceeds Employees 2's effort in each given wage combination. Moreover, Employees 1 appear to be decidedly more responsive to own wage increases than Employees 2, irrespective of the co-worker's wage. Thus, on average the availability of effort comparison information seems to have a *detrimental* effect on effort behaviour and employees' reciprocal response.[47]



The figure shows average effort by non-selfish Employees 1 (n=16, dark bars) and non-selfish Employees 2 (n=16, light bars).

[46] As in Panel A of Figure 4.2, an Employee 2's average effort in each wage combination is simply the average effort choice across the four co-worker's effort contingencies.

[47] It should be noted that another difference between Employee 1's and Employee 2's decision environments is that Employees 1 know that their behaviour will be observed by Employees 2. In contrast, Employees 2's behaviour is not known to Employees 1. Thus an alternative explanation for the differences in effort between Employees 1 and Employees 2 could be that Employees 1 expend higher effort because of an *audience effect*.

How much "reciprocal effort" does the Employer lose on average when effort comparison information is made available? Table 4.2 answers this question and reports the magnitude of Employee 1's and Employee 2's average reciprocal responses for the various co-worker's wages and (where applicable) effort levels. Reciprocal responses are computed as the change in own effort after an own wage rise *ceteris paribus*, i.e. holding constant the co-worker's wage and (for Employees 2) effort.[48]

Table 4.2. Social Comparisons and Reciprocity

		Average Reciprocal Response	
		co-worker's wage is LOW	co-worker's wage is HIGH
Employees 1		1.250 *(0.856)*	1.312 *(0.704)*
Employees 2	when e_1 is 1	1.000 *(0.816)*	0.375 *(0.719)*
	when e_1 is 2	0.937 *(0.929)*	0.562 *(0.727)*
	when e_1 is 3	1.062 *(0.929)*	0.812 *(0.910)*
	when e_1 is 4	0.687 *(1.138)*	1.500 *(0.966)*
	overall	0.922 *(0.780)*	0.812 *(0.574)*

The table shows average reciprocal responses of Employees 1 and Employees 2 with standard deviations in parentheses.

Aggregating across the four co-worker's effort contingencies, Employees 2's average reciprocal response is substantially lower than Employees 1's, both when the co-worker's wage is high (0.812 vs. 1.312), and when the co-worker's wage is low (0.922 vs. 1.25). A Wilcoxon rank-sum test detects a significant difference in both cases: $p = 0.055$ when the co-worker's wage is

[48] More formally, we compute Employee 1's reciprocal responses as $\Delta e_1 = f(w_1 = 32, w_2) - f(w_1 = 16, w_2)$ and evaluate them for different values (low or high) of the co-worker's wage. Employee 2's reciprocal response is computed as $\Delta e_2 = g(w_2 = 32, w_1, e_1) - g(w_2 = 16, w_1, e_1)$ and evaluated for different values of the co-worker's wage and effort.

high, and $p = 0.025$ when the co-worker's wage is low. While reciprocal responses are generally lower among Employees 2 than among Employees 1 when the co-worker's wage is low, Employees 2's reciprocity in the contingencies where the co-worker's wage is high clearly depends on the co-worker's effort. Employees 2 respond to an own wage increase by expending only 0.375 additional units of effort if they observe that the co-worker expends minimal effort, but they increase effort by 1.5 units when the co-worker expends maximum effort. Thus, the Employer could in principle extract more effort from Employees 2 than from Employees 1, provided that Employees 2 are paired with a sufficiently reciprocal co-worker. Nevertheless, our data suggest that on average reciprocity towards the employer is weakened by exposure to both pay and effort comparison information relative to the case when only pay comparison information is available.

4.5. Discussion & Conclusions

We have designed an experimental situation to study reciprocal behaviour in an environment where subjects can observe the treatment and behaviour of similar others before deciding on their reciprocal response. We argue that exposure to these pieces of social information is typical in naturally occurring social environments (e.g. in the workplace) and hence we believe that studying the behavioural effects of social comparison information can add in important ways to the understanding of individual decisions in social situations.

In our experiment we find strong evidence of reciprocity: employees expend higher effort when they are offered higher wages. However, the strength of the own wage-effort relationship is significantly affected by social comparison information, although a distinction has to be made between the effects of pay and effort comparison information. On the one hand, as in some previous studies (e.g., Charness and Kuhn (2007)), exposure to pay comparison information does not seem to have a significant impact on individual behaviour: effort choices respond strongly to own wage considerations, but not to information about the co-worker's wage. This is true whether or not effort comparison information is concurrently available to employees. On the other hand, we find that the own wage-effort relationship is significantly affected by effort comparison information, in a way which is dependent on relative pay conditions. When the employer pays equal and generous wages to both employees, reciprocal responses are conditional on the reciprocal behaviour of others. Employees are reluctant to increase their effort in response to a generous wage offer if their co-worker does not increase effort as well. On the other hand, employees strongly respond to generous wage offers if they observe reciprocal behaviour on the part of the co-worker. The magnitude of the effect is considerable: relative to the case where the co-worker chooses minimal effort, effort responses are five times stronger when the co-worker chooses maximum effort. Effort behaviour appears instead less sensitive to social information when the employer pays unequal wages to the employees, or when the own wage is low: in these circumstances effort choices do not depend significantly on the effort expended by the co-worker.

Our results show that the strength of reciprocal relations can be substantially eroded, or amplified by the ability to observe reciprocal behaviour on the part of others. What is then the average impact of social comparisons on reciprocity? Our data suggest that, although social information can have a positive effect on reciprocity in some circumstances, this positive impact tends to be outweighed by its negative effects: on average Employee 2's reciprocal responses are less intense that Employee 1's. Such results appear in line with the recent findings by Thöni (2005) and Mittone and Ploner (2009), which also report an overall negative impact of social information on reciprocal behaviour in related laboratory experiments. One explanation for these findings could be that individuals tend to use the social information available in the environment in a way beneficial to their self-interest. Indeed, other studies have argued that when the informational structure of a social situation leaves room for ambiguity about appropriate behaviour, or makes different and perhaps competing norms of conduct salient, individuals tend to comply with the behavioural motivation that best suits their own self-interest (e.g. Dana, Weber and Kuang (2007); Xiao and Bicchieri (*forthcoming*)).[49] In our setting, Employees 2 face a relatively complex social environment where a number of relevant motivational forces are salient. In some cases these motivational tendencies pull in different directions: consider the case of an employee receiving a high wage and observing the equally well-paid co-worker expending little effort. Vertical reciprocity would require that the employee expends high effort to repay the employer's generous wage offer. Nevertheless the employee may dislike earning less than the co-worker and hence may prefer to reduce her reciprocal response in order not to "fall behind". Conformism considerations may also induce the employee to match the co-worker's low effort. What should one do in the face of such competing norms of

[49] Other related studies also point to the existence of a *self-serving* or *egocentric bias* when one's self-interest is at stake. See Konow (2005) for an overview and related literature. For an exploration of this phenomenon in the context of the gift exchange game, see Charness and Haruvy (2000).

behaviour? Under such circumstances our subjects tended to disregard vertical reciprocity and choose low effort, a choice which is also payoff-maximizing.

Nevertheless, our finding that there exist circumstances where social comparisons have beneficial effects on reciprocity points to the importance of devising mechanisms that can reshape the social environment such that social information may end up fostering reciprocal behaviour. We propose that selective group composition may be one such mechanism. We see considerable heterogeneity across players: some appear reciprocally motivated whereas others choose uniformly low effort. An employer choosing employees should avoid low effort providers for two reasons. First, such employees cannot be motivated to supply high levels of effort, since they respond to high wages by shirking. Second, as we have stressed, they also undermine the employer's ability to induce gift exchange from reciprocally motivated employees. In fact, social information within heterogeneous groups tends to undermine performance, as observation of shirkers tends to induce "team players" to adopt more selfish behaviours.[50] Similarly, employers should find reciprocally motivated employees attractive for two reasons. They can be motivated to supply high levels of effort, and as our study shows, they induce higher levels of reciprocity from other employees. Thus homogeneous groups of reciprocally motivated employees may provide the best environment for harnessing the positive effects of social comparisons. This argument is complementary to those made by the business executives interviewed by Bewley (1999, p. 16): in

[50] A similar process has been observed in public goods experiments, where it has also been suggested that selective group composition may foster cooperation, see for example Burlando and Guala (2005); Gächter and Thöni (2005); Gunnthorsdottir, Houser and McCabe (2007); and Ones and Putterman (2007).

their view layoffs do less damage to work morale and performance than pay cuts, because layoffs "get the misery out the door" while pushing the remaining workers to work harder in order to avoid future dismissal, whereas pay cuts have a negative impact on all workers' motivation. In addition, our results suggest that if layoffs target less productive workers this has the further advantage of reducing the heterogeneity of the workforce thus strengthening the effort responses of reciprocally motivated employees.

To conclude, while we have used a labour market context to fix ideas, we believe that our results extend beyond this context because reciprocity is an important social norm in many situations. Our study shows that in settings where social comparison information about referent others is available the extent to which individuals are willing to comply with norms of positive reciprocity depends systematically on the extent to which similar others are willing to comply with these norms as well.

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Appendix A: Experimental Instructions

You are now taking part in an economic experiment on decision-making that has been financed by various foundations for research promotion.

If you read the following instructions carefully, you can – depending on your decisions – earn a considerable amount of money. It is therefore very important to read these instructions with care.

During this experiment you are not allowed to communicate with the other participants in any way. If you have any questions, please raise your hand and the experimenter will come to your desk. If you violate this rule, you will be excluded from the experiment and from all payments.

During the experiment your earnings will be calculated in points. You will receive an initial endowment of 95 points, which will be enough to cover any loss that might occur during the experiment. The points you lose will be subtracted from your endowment.

At the end of the experiment we will convert your point earnings into money at the following rate:

1 Point = 10 Pence

Your total money earnings will be paid out to you in private and **in cash** at the end of the experiment.

1. Introduction

In this experiment you will be randomly matched with two other participants to form a group of three persons. We will refer to **each group as a firm, and to the three group members as Employer, Employee 1 and Employee 2**. You will be assigned to a firm and a role entirely at random, and the computer will inform you of your role before the decision-making part of the experiment begins. You will not be informed about who of the other participants are in your firm, either during or after the experiment. Therefore, all decisions are made anonymously.

2. Decisions within a Firm

The structure of the decision-making within each firm is as follows.

- ✓ First, **the Employer** chooses the wages to pay to Employee 1 (**$Wage_1$**) and Employee 2 (**$Wage_2$**). The Employer can choose between two wage levels, 16 or 32. If he or she wants to, the Employer can choose different wages for different Employees.
- ✓ Next, **Employee 1** learns the wages the Employer pays to each Employee, and then chooses an effort level (**$Effort_1$**), either 1, 2, 3 or 4.
- ✓ Finally, **Employee 2** learns the wages the Employer pays to each Employee, and also the effort decision of Employee 1. Employee 2 then chooses an effort level (**$Effort_2$**), either 1, 2, 3 or 4.

3. Distribution of earnings within a Firm

Earnings within the Firm are determined according to the following rules:

Employer

The Employer receives revenue from the effort chosen by the two Employees, and incurs costs from the wages paid to the two Employees. The revenue produced by each Employee equals 10 times the effort he or she chooses. The costs are simply the sum of the two wages the Employer pays to the Employees. The Employer's earnings are therefore:

$$\textbf{Employer's Earnings} = 10 * (Effort_1 + Effort_2) - Wage_1 - Wage_2$$

The Employer's earnings increase with higher effort levels. The higher the wages the Employer pays to the two Employees, the lower are the Employer's earnings. Note that the Employer's earnings could be negative.

Employee 1

Employee 1 receives the wage from the Employer as revenue, and may incur an effort cost. The minimum effort choice of 1 is costless. Each additional unit of effort costs 5 points to the Employee. Therefore the effort cost is calculated as: 5 * (Effort – 1). The earnings of Employee 1 are therefore:

$$\textbf{Employee 1's Earnings} = Wage_1 - 5 * (Effort_1 - 1)$$

The earnings of Employee 1 only depend on his or her own wage and effort. The higher the wage, the higher are the earnings. The higher the effort he or she chooses, the lower are the earnings.

Employee 2

The earnings of Employee 2 are calculated in the same way as those of Employee 1, except, of course, that Employee 2's earnings depend on his or her own wage ($Wage_2$) and his or her own effort choice ($Effort_2$):

$$\textbf{Employee 2's Earnings} = Wage_2 - 5 * (Effort_2 - 1)$$

HYPOTHETICAL EXAMPLE FOR DEMONSTRATION PURPOSES

ASSUME THAT THE EMPLOYER CHOOSES THE FOLLOWING WAGES FOR HIS OR HER EMPLOYEES:

WAGE FOR EMPLOYEE 1 = 32

WAGE FOR EMPLOYEE 2 = 16

THE EMPLOYEES CHOOSE THE FOLLOWING EFFORT:

EFFORT EMPLOYEE 1 = 2

EFFORT EMPLOYEE 2 = 3

THIS SITUATION RESULTS IN THE FOLLOWING EARNINGS:

EMPLOYER'S EARNINGS: THE EMPLOYER RECEIVES REVENUE FROM THE EFFORT OF THE TWO EMPLOYEES, I.E.: 10*(2 + 3) = 50. THE EMPLOYER PAYS A TOTAL OF 48 POINTS TO THE EMPLOYEES.

THE EARNINGS OF THE EMPLOYER ARE: 50 – 48 = **2**.

EMPLOYEE 1'S EARNINGS: EMPLOYEE 1 RECEIVES A WAGE OF 32. THE EFFORT CHOICE OF 2 HAS A COST OF 5*(2 – 1) = 5.

THE EARNINGS OF EMPLOYEE 1 ARE: 32 – 5 = **27**.

EMPLOYEE 2'S EARNINGS: EMPLOYEE 2 RECEIVES A WAGE OF 16. THE EFFORT CHOICE OF 3 HAS A COST OF 5*(3 – 1) = 10.

THE EARNINGS OF EMPLOYEE 2 ARE: 16 – 10 = **6**.

4. The Decision Task

Although the structure of the decision-making within each firm is the one described above, in this experiment we ask you to take a decision for each possible situation that may arise. Please note that one of these situations will be actually relevant, so make your choices carefully.

The situations you face when making your decisions will depend on your role.

If you are an **Employer you must choose two wage**s, one for each Employee within the Firm. The Employer can choose between:

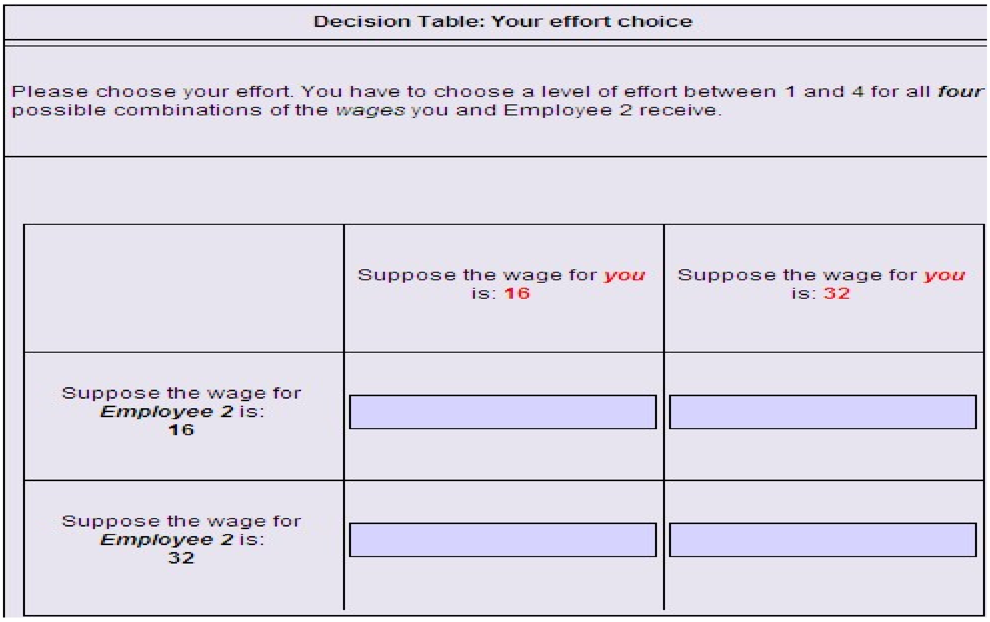
- $Wage_1$ = 16 and $Wage_2$ = 16;
- $Wage_1$ = 16 and $Wage_2$ = 32;
- $Wage_1$ = 32 and $Wage_2$ = 16;
- $Wage_1$ = 32 and $Wage_2$ = 32.

Depending on the choice of the Employer one of four situations will arise:

- Employee 1 and Employee 2 could both have a wage of 16;

- Employee 1 could have a wage of 16 while Employee 2 has a wage of 32;
- Employee 1 could have a wage of 32 while Employee 2 has a wage of 16;
- Employee 1 and Employee 2 could both have a wage of 32.

If you are **Employee 1** you will be in one of these four situations. However, before knowing which of these situations you are actually in, you will be asked to **indicate what you would do for each of the four possible situations** you may be in. You will see a decision screen like the one below:



Decision Table: Your effort choice

Please choose your effort. You have to choose a level of effort between 1 and 4 for all *four* possible combinations of the *wages* you and Employee 2 receive.

	Suppose the wage for *you* is: 16	Suppose the wage for *you* is: 32
Suppose the wage for *Employee 2* is: 16		
Suppose the wage for *Employee 2* is: 32		

Each box represents one of the four possible situations you may be in. In each of these boxes, you must enter an effort choice, either 1, 2, 3 or 4. Your actual effort choice will depend on which of these four possible situations will actually realise, i.e. on the wage combination actually chosen by the Employer.

Depending on the choices of the Employer and Employee 1 one of sixteen situations may arise:

- Employer could choose $Wage_1$= 16 and $Wage_2$ =16 while Employee 1 chooses 1 unit of effort;
- Employer could choose $Wage_1$= 16 and $Wage_2$ =16 while Employee 1 chooses 2 units of effort;
- ….and so on.

If you are **Employee 2** you will be in one of these sixteen situations. However, before knowing which of these situations you are actually in, you will be asked to **indicate what you would do for each of the sixteen possible situations** you may be in. You will see a decision screen like the one below:

Decision Table: Your effort choice

Please choose your effort. You have to choose a level of effort between 1 and 4 for all ***sixteen*** possible combinations of the *wages* you receive and the *wages* and *effort* of Employee 1.

	Suppose the wage for ***you*** is: 16 Suppose the wage for ***Employee 1*** is: 16	Suppose the wage for ***you*** is: 16 Suppose the wage for ***Employee 1*** is: 32	Suppose the wage for ***you*** is: 32 Suppose the wage for ***Employee 1*** is: 16	Suppose the wage for ***you*** is: 32 Suppose the wage for ***Employee 1*** is: 32
Suppose ***Employee 1*** chooses effort: **1**				
Suppose ***Employee 1*** chooses effort: **2**				
Suppose ***Employee 1*** chooses effort: **3**				
Suppose ***Employee 1*** chooses effort: **4**				

Each box represents one of the sixteen possible situations you may be in. In each of these boxes, you must enter an effort choice, either 1, 2, 3 or 4. Your actual effort choice will depend on which of these sixteen possible situations will actually realise, i.e. on the wage combination actually chosen by the Employer and on the effort actually chosen by Employee 1.

More information about how to solve your specific Decision task will be provided to you via computer later on during the experiment, once your role has been determined.

You have to perform this task **only once**.

5. How do we determine your actual earnings?

Although Employee 1 will take four effort decisions, **only one** will be relevant in determining the earnings of members of the Firm. Similarly, only one of the sixteen effort decisions made by Employee 2 will be actually used in the earnings' computation.

Which decision is actually relevant will be determined at the end of the experiment, once everyone in the firm has taken his or her decisions: the actual wage combination chosen by the Employer will determine which of the four possible situations is relevant for Employee 1. Employee 1's choice in this relevant situation will determine which of the sixteen possible situations is relevant for Employee 2.

6. What happens next?

I. When the experiment starts you will be informed about whether you are an Employer or an Employee in this experiment. In case you are an Employee, it will be specified whether you are Employee 1 or Employee 2.

II. When you press the "Continue" button, a screen with a brief video-presentation about the main features of the experiment will appear. In this video-presentation you will receive some information about the "*What-if-calculator*", a tool you can use during the experiment to facilitate your computations. It is important to note that no other participant will be informed about your calculations and that these calculations do not have any effect on your earnings.

III. After this brief video-presentation, you will access a new screen where you will be asked to answer a few questions. You will have to calculate the earnings of all members of your Firm for five hypothetical scenarios, with the help of the "*What-if-calculator*". Press "Check" when you have answered all the questions. You will be informed about whether your answers are correct.

IV. Once you have answered all the questions correctly, you will be guided to a new short video-presentation that will give you specific information about how to enter your decisions into the Decision Table.

V. After that, you will finally enter the Decision Task screen. Depending on whether you are an Employer or an Employee you will have to choose wages or effort levels. In this screen, you will again have the possibility to use the "*What-if-calculator*".

Please, raise your hand if you have any questions.

Appendix B: Experimental Précis

I will now briefly summarize the content of the instructions you have just read.

At the beginning of the experiment you will be randomly matched with two other participants to form a group of three people and you will be randomly assigned a role within this group, which we will call "firm". You will be either the Employer or Employee 1 or Employee 2.

The structure of the decision-making within each firm is as follows.

First, **the Employer** chooses one wage to pay to Employee 1 (**$Wage_1$**) and one wage to pay to Employee 2 (**$Wage_2$**).

Next, **Employee 1** learns the wages the Employer pays to each Employee, and then chooses an effort level (**$Effort_1$**).

Finally, **Employee 2** learns the wages the Employer pays to each Employee and also the effort decision of Employee 1, and then chooses an effort level (**$Effort_2$**).

The Employer's earnings increase with higher effort levels and decrease with higher wages.

The Employees' earnings increase in the wage they receive and decrease with higher effort. The earnings of each Employee only depend on his or her own wage and effort.

Although the structure of the decision-making within each firm is the one I have just described, in this experiment we ask you to take a decision for each possible situation that may arise. This is a crucial point, so make sure you have understood it correctly.

The possible situations you will face when making your decisions will depend on your role.

If you are an **Employer you must choose two wage**s, one for each Employee within the Firm. Thus, depending on the choice of the Employer one of four situations will arise:

- Both Employees could get a wage of 16;
- Both Employees could get a wage of 32;
- And the two situations where one Employee gets a wage of 16 while the other Employee gets a wage of 32;

If you are **Employee 1** you must indicate an effort choice for each of these four possible situations, before knowing which one you are actually in. Remember, one of these four decisions will be the one that is actually relevant, so make your choice carefully.

Depending on the choices of the Employer and Employee 1 one of sixteen situations may arise:

- Both Employees get a wage of 16 and Employee 1 chooses 1 unit of effort

- Both Employees get a wage of 16 and Employee 1 chooses 2 units of effort;
- ….and so on…

Since there are 4 possible levels of effort and 4 possible wage combinations, 16 situations in all may arise.

If you are **Employee 2** you must indicate an effort choice for each of the sixteen possible situations. Remember, one of these sixteen decisions will be the one that is actually relevant, so make your choice carefully.

Which decision is actually relevant will be determined at the end of the experiment, once everyone in the firm has taken his or her decisions: the actual wage combination chosen by the Employer will determine which of the four possible situations is relevant for Employee 1. Employee 1's choice in this relevant situation will determine which of the sixteen possible situations is relevant for Employee 2. Please, raise your hand if you have any questions.

CHAPTER 5

THE IMPACT OF PAY COMPARISONS ON EFFORT BEHAVIOUR

5.1. Introduction

Naturally occurring interactions between individuals take place in complex social environments which typically contain considerable amounts of social comparison information about referent others (i.e. information about how similar others behave or are treated in similar circumstances). The availability of information about others' treatment and behaviour can affect the way individuals evaluate their own treatment and behaviour, and this may ultimately influence their actions.[51] These effects may be particularly prominent in workplaces, where employees are often aware of co-workers' pay and may use this information to evaluate the fairness of their own pay.

[51] For example, Fliessbach et al. (2007) show that reward-related brain processes are significantly influenced by information about the treatment of comparison others in a neuroimaging experiment.

Employees may feel treated unfairly if they discover to be paid less than co-workers who are in comparable positions within the firm. This may in turn affect their work morale and performance negatively. This type of argument is often invoked to justify firms' preference for wage secrecy norms. Moreover, it constitutes a central component in a number of theoretical approaches to labour market relations (e.g. Akerlof and Yellen, 1990), and to social relations more generally (e.g., Adams, 1965). Surprisingly, empirical studies have produced weak evidence that pay comparisons have a systematic influence on work behaviour.[52]

This chapter uses laboratory experiments to study pay comparison effects in a multilateral version of the gift-exchange game (Fehr et al., 1993). In the standard bilateral gift-exchange game a first-mover (the 'employer') decides on the size of the gift ('wage') she sends to a second-mover (the 'employee'), who can in turn reciprocate by choosing costly actions ('effort') that reward the first-mover. A typical result of bilateral gift-exchange game experiments is that employees are often willing to incur costs in order to reward employers who have treated them favourably (see Fehr et al., 2009 for a recent review of the experimental literature). In the multilateral version of the game used in this study the employer interacts with two employees at the same time, and has to pay a wage to each of them. Employees receive their wage and then independently choose an effort level. We observe effort choices in three different conditions. In a first condition employees only learn their own wage while co-workers' wages remain secret. We use this benchmark condition to

[52] Survey and case-based studies have instead found some support for the importance of pay comparison effects at workplaces (see, e.g., Campbell III and Kamlani, 1997; Bewley, 1999).

assess how employees respond to given levels of their own wage in the absence of pay comparison information. In two other treatments employees have full information about co-workers' wages at the time they choose effort. We use these treatments to study how information about co-workers' wages affects employees' willingness to expend effort in response to own wage offers. The two 'public wages' treatments differ in how co-workers' wages are determined. In one treatment the employer can choose which wage to pay to the co-worker, while in the other treatment co-workers' wages are mandated exogenously. We use the former treatment to study pay comparison effects in settings where the employer has full discretion on the firm's wage structure, while the latter treatment allows us to study employees' reactions to pay comparison information in settings where an employer's wage policy is partly constrained by exogenous labour market regulations. Such exogenous constraints to firms' wage policies are commonplace in natural workplaces (e.g. minimum wage laws; centralized pay regulations; etc.), and previous experimental research has shown that they can affect workers' pay fairness considerations and effort behaviour in important ways.[53] Differently from previous studies, which either focused on bilateral labour relations or studied multilateral relations where workers received no information about the treatment of co-workers, in our experiment we can observe how employees' effort is affected by labour market

[53] For example, Falk et al. (2006) show that introducing a nonbinding wage guideline in a previously unregulated experimental labour market shifts employees' perceptions of what constitutes a fair wage and increases their reservation wages considerably. The effect is even stronger if the same wage level chosen for the wage guideline is set as a (binding) 'minimum wage'. On the impact of minimum wages on effort in gift-exchange games see also Brandts and Charness (2004) and Owens and Kagel (*forthcoming)*.

regulations that affect the wage of their co-workers, and we can thus study how these effects extend to horizontal pay fairness considerations.

The three main results from our study are as follows. 1) As in many other related gift-exchange game experiments, in all treatments we observe a strong positive own wage-effort relation: employees in our experiment reciprocate high wages with higher effort. 2) Pay comparisons have an overall detrimental effect on the own wage-effort relation: employers trigger higher effort from employees when they cannot observe what the co-worker earns than when co-workers' wages are public. 3) The negative effects of pay comparisons are amplified in the treatment where co-workers' wages are fixed exogenously. Here employees respond negatively to pay comparison information regardless of whether they learn that the co-worker is paid an exogenously low or high wage, while in the treatment where employers can choose the level of the co-worker's wage marked responses are only detected when the employer chooses a high wage for the co-worker.

This study is related to a small but growing literature that uses experiments to study how effort behaviour is affected by information about others' pay relative to situations where co-workers' pay is secret. Güth et al. (2001) study a setting where a principal has to design separate contracts for two agents who differ in productivity. They compare a treatment where agents only learn their own contract with a treatment where agents also learn the contract offered to the other agent. While principals in their experiment anticipate the existence of pay comparison effects and offer less asymmetric contracts in the treatment where contracts are public information, there is only weak evidence that pay comparisons actually affect agents' behaviour.

Charness and Kuhn (2007) use a multilateral version of the gift-exchange game where one employer interacts with two differently productive employees. They compare effort choices across treatments that vary in whether co-workers' wages are public or secret and find that pay comparisons have negligible effects on effort behaviour.[54] Differently from these studies, employees in our experiment do not differ in their productivity: thus, in our setting unequal pay decidedly implies unfair treatment on the part of the employer.

A few other experimental studies investigate pay comparison effects by examining how employees' effort decisions change across situations that differ in the wage the employer pays to the co-worker. These studies do not include explicit comparisons of treatments differing in whether co-workers' wages are secret or public. Results from these experiments are mixed: in some cases information about co-workers' wages systematically affects employees' effort decisions (Gächter and Thöni, 2009; Abeler et al., *forthcoming*), while in other cases such effects are weak or absent (Gächter et al., 2009).

The remainder of the chapter is organized as follows. In the next Section we discuss our experimental design and procedures. Section 5.3 presents our results. We offer concluding comments in Section 5.4.

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[54] Pay comparison effects are also absent in the real effort experiment by Hennig-Schmidt et al. (*forthcoming*). However, employees in their experiment are also unwilling to provide high effort in return for high own wages. Subsequent laboratory experiments (where no pay comparisons were possible) revealed that this was due to the lack of surplus information, which limited the scope for employees to develop adequate fairness attributions. Clark et al. (*forthcoming*) use standard bilateral gift-exchange games that vary in whether employees receive information about the wages offered in four other firms present in the market (i.e. they study inter-firm pay comparison effects as compared to the intra-firm comparison effects studied here). They find that effort choices are significantly affected by how the own wage is ranked relative to others' wages.

5.2. Experimental Design & Procedures

5.2.1. The Constituent Game

Our experiment is based on the following three-person game. At the outset of the game an Employer is endowed with £22 from which she pays a wage to two Employees, labelled RED and BLUE. The wages w_{RED} and w_{BLUE} can take three values: £1, £4 or £7. Employees are paid their wage and then select simultaneously an effort level. Each Employee $i \in \{RED, BLUE\}$ can choose an effort e_i among three possible levels: low (-1), medium (0) or high (+1). The effort technology is adapted from Charness and Levine (2007). Low effort costs an employee £1 and reduces the Employer's earnings by £4. Medium effort is costless and does not affect the Employer's earnings. High effort costs an employee £1 and increases the Employer's earnings by £4. Note that, differently from the usual setup where employees can only decide whether to reward or not to reward the Employer, the effort technology used in our study allows both rewarding (when employees choose high effort) and punishments (when employees choose low effort) of fair/unfair wage offers. Also note that employees are *ex ante* symmetric as they do not differ in their productivity at the time the Employer sets wages. After employees have chosen their efforts the game ends and earnings (in British Pounds) are computed as:

$$\pi_{ER} = 22 + 4 \cdot (e_{RED} + e_{BLUE}) - w_{RED} - w_{BLUE}$$

for the Employer, and

$$\pi_i = w_i - (e_i)^2$$

for Employee $i \in \{RED, BLUE\}$.

In our experiment subjects played a one-shot version of the game, which was described to them using the same labour market frame that we use throughout the text. The implementation of the game used the strategy method (Selten, 1967), i.e. subjects had to specify complete strategies in the game-theoretic sense.

5.2.2. The Experimental Treatments

Our constituent game was implemented in three different treatments which vary along two dimensions. The first dimension is whether wages are public (both employees learn both wages before making an effort choice) or private (each employee learns only her own wage). The second dimension is whether the Employer can choose *both* the wages she pays to the employees, or whether she can instead choose only *one* wage, while the other wage is fixed exogenously. Table 5.1 provides an overview of the treatments used in the experiment.

Table 5.1. Overview of Treatments

Wages	both determined by the Employer	one determined exogenously
public	*PUBLIC*	*PUBLIC / EXO* £1 *PUBLIC / EXO* £7
private	*SECRET*	-

In our SECRET treatment the Employer chooses a wage for the RED Employee and a wage for the BLUE Employee. Each employee then learns her own wage but is not informed of the wage that the Employer chose for the co-worker: thus, co-workers' wages are *secret*.

In our PUBLIC treatment wages are also determined by the Employer, but they are *public* information as employees are informed of both wages before they choose effort.

In our PUBLIC/EXO treatment wages are also *public* information, but the Employer chooses only one of the two wages she pays to the employees, namely the wage for the BLUE Employee. The wage for the RED Employee is instead determined *exogenously* by the experimenter. We conducted two versions of the PUBLIC/EXO treatment where the RED Employee's wage was either fixed equal to £1 (PUBLIC/EXO £1) or equal to £7 (PUBLIC/EXO £7). The level of the RED wage was mandated using a neutral language: in the instructions (reproduced in Appendix A) subjects were simply told that "*the Employer* ***must*** *pay a £1* [£7 in PUBLIC/EXO £7] *wage to the Red Employee, while he/she can decide on what wage (£1, £4 or £7) to pay to the Blue Employee.*" (emphasis in original).

5.2.3. Discussion of the Design

The aim of our experiment is to assess the impact of pay comparison information on effort behaviour. We study a setup where employees are *ex ante* symmetric such that any pay differential between employees has a straightforward interpretation in terms of pay fairness. We compare effort choices made in a treatment where co-workers' wages are not observable (the SECRET treatment) with choices made in two different 'public wages' treatments where co-workers' wages are observable. In the PUBLIC treatment co-workers' wages are freely chosen by the employer. This is the type of environment that has also been studied in previous experiments on pay comparison effects. In the PUBLIC/EXO treatment co-workers' wages are mandated exogenously, and we can thus study pay comparison effects in environments where the employer is constrained by external regulations to pay a given wage to a portion of the workforce.

It is not clear *a priori* whether the presence of exogenous constraints on a firm's wage structure may strengthen or weaken pay comparison effects. On the one hand, the fact that co-workers' wages are not determined by the employer in PUBLIC/EXO may actually *reduce* the scope for effective pay comparisons, as only wages actively chosen by employers may be considered as relevant for pay fairness attributions. Indeed, Gächter and Thöni (2009) find reduced pay comparison effects in their '*Non-intentional*' treatment where a random device chooses employees' wages on behalf of the employer. Nevertheless, we note that, differently from Gächter and Thöni (2009), employers in our PUBLIC/EXO treatment do have some discretion over wages as they can set the level of the BLUE wage: hence in our game the Employer *is* responsible for any wage inequality within the firm, as she can always treat employees symmetrically if she wishes to do so. Moreover, the RED wages in PUBLIC/EXO are *not* determined by a random mechanism: they are fixed exogenously by the rules of the experiment, and workers may view the realization of a random process differently from an exogenous wage mandated by the experimenter. In fact, similarly to how the introduction of an exogenous minimum wage shifted workers' perceptions of what is a fair wage in previous experimental studies (e.g., Falk et al., 2006), our exogenous intervention on RED wages in PUBLIC/EXO may end up increasing the prominence of co-workers' wages as a reference point for what constitute a fair wage in an experimental firm, and thus *promote* the use of horizontal pay comparisons in pay fairness evaluations.[55]

[55] This may be particularly true for settings where the exogenous constraints contain value-laden elements that may speak to workers' feelings of entitlement. On this respect, it should be noted that we opted for a conservative approach to mandate the levels of the RED wage in PUBLIC/EXO rather than for more value-laden interventions, e.g. one where the experimenter somehow justifies the choice of the RED wage level.

Pay comparison effects in PUBLIC/EXO are studied focusing on two 'extreme' levels of the co-worker's wage, £1 and £7. These seem the most attractive cases to isolate pay comparison effects: it appears in fact unlikely that we would observe any effect of moderate amounts of wage inequality had we failed to observe reactions to larger amounts.

We collected employees' effort decisions using the strategy method. We believe that the use of the strategy method in our experiment is necessary because it allows us to observe effort behaviour across all possible paths of play without either using deception or resorting to repeated play, which does not guarantee the collection of a sufficient number of observations for all information sets in the game and can introduce strategic confounds in the design. With the strategy method we can observe how each employee in our experiment chooses his or her effort for *all* possible combinations of the own wage and the co-worker's wage, and we can thus measure the impact of pay comparisons on effort at the individual level.[56]

Note that the number of effort decisions that employees have to submit with the strategy method depends on the treatment they are playing, as they control a different number of information sets in different treatments. In the PUBLIC treatment employees control nine information sets, one for each wage combination that could possibly be chosen by the Employer: thus, we collect nine effort choices from RED and BLUE Employees in the PUBLIC treatment.

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[56] The use of the strategy method in economic experiments has undergone a methodological debate about whether behavioural responses differ when they are elicited in a "hot" version of the game (i.e., when subjects directly respond to decisions made by other players) rather than in a "cold" version (i.e., when the game is played with the strategy method). The issue has been addressed in a number of experimental studies suggesting that the use of the strategy method is unlikely to distort behaviour in experiments (see Brandts and Charness, 2009 for a review).

In the PUBLIC/EXO treatment the RED Employee's wage is exogenously fixed at either £1 or £7 depending on which version of the treatment is implemented. Thus, only three wage combinations are actually feasible, and vary in the wage the Employer chooses for the BLUE Employee. Thus, we collect three effort choices from BLUE and RED Employees in our two versions of the PUBLIC/EXO treatment. Lastly, because in the SECRET treatment employees only learn their own wage and not the co-worker's wage, they control three information sets corresponding to the three wage levels that could possibly be paid to them by the Employer. Thus, we collect three effort choices from employees in the SECRET treatment.

5.2.4. Experimental Procedures

The experiment was conducted at the University of Nottingham using subjects recruited from a university-wide pool of students who had previously indicated their willingness to be paid volunteers in decision-making experiments.[57] Twelve sessions with a total of 180 participants were conducted: we had 30 subjects participate in two sessions of the PUBLIC treatment, 30 subjects participate in two sessions of the SECRET treatment, and 120 subjects participate in eight sessions of the PUBLIC/EXO treatment, equally divided between its two versions PUBLIC/EXO £1 and PUBLIC/EXO £7. No subject took part in more than one session. The average age of participants was 20.7 years, and 52% of them were male.

All sessions used an identical protocol. Upon arrival, subjects were welcomed and randomly seated at visually separated computer terminals.

[57] The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007). Subjects were recruited through the online recruitment system ORSEE (Greiner, 2004).

Subjects were then given a written set of instructions that the experimenter read aloud. Subjects were also given a set of Earnings Distributions tables (reproduced in Appendix B for the PUBLIC treatment), showing Employer's and Employees' earnings for all combinations of efforts and wages. Subjects were then randomly assigned to a group and a role (Employer, RED Employee or BLUE Employee), and were asked to solve a set of control questions to corroborate their understanding of the experimental game. Subjects had to answer all questions correctly before the experiment could continue. The decision-making phase of the session consisted of a one-shot play of the relevant experimental game. All decisions were made anonymously: neither during nor after the experiment were subjects informed about the identity of the other people in the room they were matched with.

At the end of the experiment subjects completed a short post-experimental questionnaire. Subjects were then privately paid a £3 show-up fee plus their earnings from the experimental game. Subject earnings, inclusive of the show-up fee, ranged from £3 to £27, with an average of £10.12 and a standard deviation of £6.95. Sessions lasted about 50 minutes on average.

5.3. Results

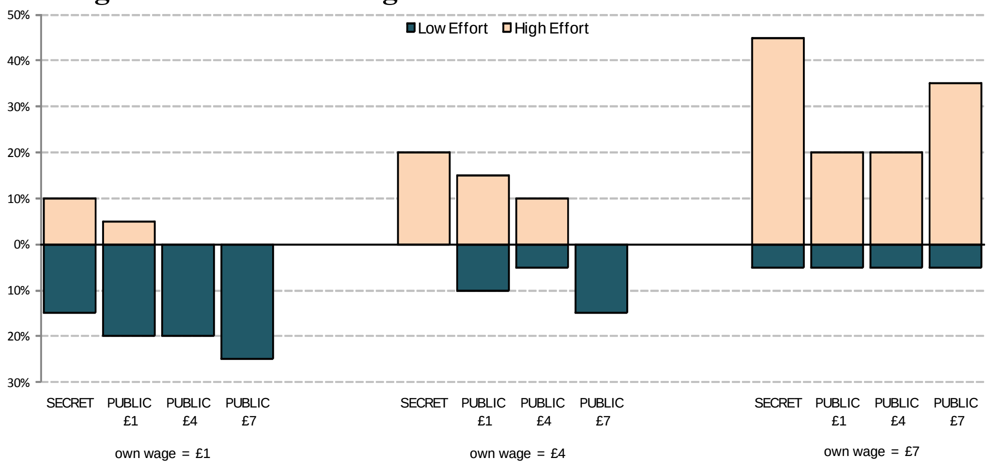
Our data analysis will be focused on employees' effort behaviour across the three treatments used in the experiment. We start by comparing effort behaviour across our SECRET and PUBLIC treatments. We then turn to a comparison of effort choices made in SECRET and PUBLIC/EXO. Because the focus of our study is on the impact of pay comparisons on own wage-effort reciprocal relations, only the effort decisions made by BLUE Employees are relevant to the analysis of effort choices made in PUBLIC/EXO: since RED

Employees' wage was exogenously mandated by the experimenter their effort responses cannot be interpreted as a form of reciprocation towards the employer. Though not the focus of the experiment, we will briefly discuss the effort chosen by RED Employees in PUBLIC/EXO at the end of this section. Employers' wage decisions will also be briefly presented there.

5.3.1. SECRET vs. PUBLIC

Figure 5.1 reports the proportions of low effort choices (punishments) and high effort choices (rewards) made by employees in SECRET and PUBLIC for different levels of the own wage. In PUBLIC, for any given level of the own wage, we also differentiate the effort choices made when the co-worker's wage was low (PUBLIC £1), medium (PUBLIC £4) or high (PUBLIC £7). Effort rates for SECRET are not disaggregated according to the co-worker's wage as this was not known to employees at the time they made an effort choice.

Figure 5.1. Low and High Effort Rates: SECRET vs. PUBLIC*



* Medium effort is the omitted category, thus % low effort + % high effort + % medium effort = 100%. Bars are based on choices made by 20 employees in each treatment.

A first evident feature of Figure 5.1 is that, in both treatments, employees expend higher effort when they are paid a high wage. Employees rarely reward the employer with high effort when the own wage is £1, but the proportion of high effort choices when the own wage is £7 varies from 20% to 45% depending on the treatment and the relative pay conditions. Conversely, there are virtually no low effort choices when the own wage is £7, while a £1 wage triggers punishment between 15% and 25% of the times. Thus, in these two treatments, as in many other related gift-exchange game experiments, a positive own wage-effort relation emerges whereby employees are willing to expend more effort to reciprocate generous wage offers made by the employer.

A second noticeable feature of Figure 5.1 is that, irrespective of the own wage level, employees respond more favourably to the employer's wage offers in SECRET than in PUBLIC. For any given level of the own wage, and irrespective of the level of the co-worker's wage, reward rates are highest in SECRET. Punishment rates are generally lower in SECRET than in PUBLIC. This detrimental impact of pay comparison information in PUBLIC appears to be sensitive to relative pay conditions: when the own wage is low or medium employees generally respond less favourably to the employer's wage offers the higher is the wage paid to the co-worker. Thus, pay comparisons are mostly detrimental when a worker is paired with a co-worker who receives a £7 wage. The pattern is somewhat reversed when the own wage is high: here employees tend to act less favourably towards the employer when the latter discriminate against the co-worker.

We corroborate these observations by performing a regression analysis of effort behaviour in the two treatments. In a first model (Model I) we regress effort on a variable measuring the different levels of the own wage ('*Own wage*') and on dummy variables measuring the different levels of the co-worker's wage: *PUBLIC £1*, *PUBLIC £4* and *PUBLIC £7*. These dummies assume value 1 for effort choices made in wage combinations where the co-worker earns respectively £1, £4 or £7, and 0 otherwise. Notice that the baseline category is represented by effort choices made in the SECRET treatment. Dummy variables controlling for individual characteristics (gender and field of study) are added as regressors in Model II, while Model III expands Model II by including interaction terms between the '*Own wage*' variable and the co-worker's wage dummies. Ordered probit estimations of the three regression models are reported in Table 5.2.

Table 5.2. Effort Behaviour in SECRET and PUBLIC

	I	II	III
Own wage	0.186*** *(0.050)*	0.195*** *(0.052)*	0.205** *(0.088)*
PUBLIC £1	-0.436 *(0.290)*	-0.301 *(0.305)*	-0.064 *(.510)*
Own wage * PUBLIC £1	-	-	-0.059 *(0.102)*
PUBLIC £4	-0.486** *(0.245)*	-0.352 *(0.261)*	-0.219 *(0.463)*
Own wage * PUBLIC £4	-	-	-0.033 *(0.103)*
PUBLIC £7	-0.584** *(0.247)*	-0.460* *(0.262)*	-0.683 *(0.434)*
Own wage * PUBLIC £7	-	-	0.055 *(0.096)*
1 if Male	-	-0.710*** *(0.274)*	-0.712*** *(0.277)*
1 if studies Social Sciences (incl. Economics)	-	0.109 *(0.271)*	0.110 *(0.272)*
N.	*240*	*240*	*240*
Pseudo R^2:	*0.100*	*0.147*	*0.152*

Ordered probit regressions. Dependent variable is effort. Robust standard errors in parentheses adjusted for intragroup correlation (individuals are used as independent clustering units). * $.05 < p \leq .10$; ** $.01 < p \leq .05$; *** $p \leq .01$.

Starting with Model I, the coefficient on the '*Own wage*' variable is positive and highly significant, confirming the existence of a positive own wage-effort relation in our experiment: own wages are a powerful determinant of effort. Consistent with our second observation, the coefficients on the three dummies for the PUBLIC treatment are all negative: employees expend more effort when co-workers' wages are secret. The effect is significant at the 5%

level for medium and high levels of the co-worker's wage (p = 0.048 for *PUBLIC £4*; p = 0.018 for *PUBLIC £7*).

In Model II we add controls for gender and field of study. The coefficient on the '*Own wage*' variable remains positive and highly significant. The coefficients on the three dummies for PUBLIC remain negative, but only the coefficient on the dummy controlling for the highest co-worker's wage (*PUBLIC £7)* is now significantly different from zero (p = 0.079). The coefficients on the two other co-worker's wage dummies fall instead short of conventional significance levels (p = 0.323 for *PUBLIC £1*; p = 0.178 for *PUBLIC £4*).

The gender dummy enters significantly in the regression: men expend lower effort than women and the difference is significant at the 1% level. This result compares with findings on second-mover's behaviour in related trust and sequential prisoner's dilemma games, where men are sometimes found to act more selfishly than women (e.g., Croson and Buchan, 1999), although the effect is not always significant (e.g. Clark and Sefton, 2001).[58]

Model III extends Model II by introducing interactions between the '*Own wage*' variable and the co-worker's wage dummies. None of the coefficients on the interaction terms differ significantly from zero. Thus, pay comparisons do not alter the shape of the own wage-effort relationships, but do induce lower effort levels irrespective of own wage.

Overall these findings show that the availability of pay comparison information does generally depress employees' willingness to provide costly

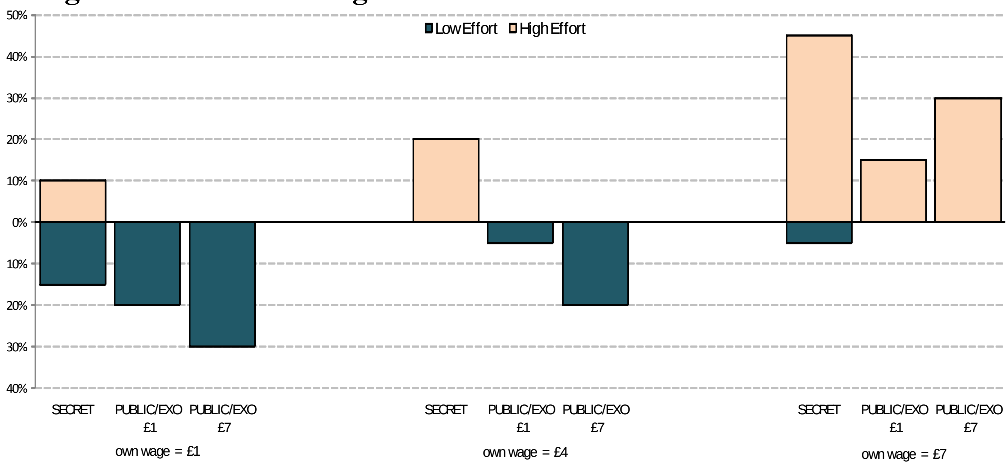
[58] For a review of gender effects in experiments see Croson and Gneezy (2009).

effort relative to the case where no pay comparison information is available, and that the effect is particularly marked for high levels of the co-worker's wage.

5.3.2. SECRET vs. PUBLIC/EXO

We now turn to effort choices made in PUBLIC/EXO. Figure 5.2 reports the proportions of low and high effort choices made by BLUE employees in PUBLIC/EXO for different levels of the own wage. We distinguish between effort choices made in sessions where the co-worker's wage was low (PUBLIC/EXO £1) and sessions where the co-worker's wage was high (PUBLIC/EXO £7). For comparison, effort choices made in SECRET are also included. As in Figure 5.1 we do not disaggregate effort choices in SECRET according to the co-worker's wage.

Figure 5.2. Low and High Effort Rates: SECRET vs. PUBLIC/EXO*



* Medium effort is the omitted category, thus % low effort + % high effort + % medium effort = 100%. Bars are based on choices made by 20 employees in SECRET and by 20 BLUE Employees in each version of the PUBLIC/EXO treatment.

As in PUBLIC and SECRET, also in PUBLIC/EXO a positive own wage-effort relation emerges from the data: irrespectively of the wage of the co-worker, higher own wage levels decrease the frequency of low effort and increase the frequency of high effort (in fact, high effort is only chosen when the own wage is £7). A second important feature of Figure 5.2 is that also in

PUBLIC/EXO the availability of pay comparison information appears to have a marked negative impact on effort choices. While in SECRET employees choose high effort in response to low or medium own wages between 10% and 20% of the times, the same level of the own wage only triggers medium or low effort in PUBLIC/EXO. High own wage offers also trigger less generous responses from employees in PUBLIC/EXO than in SECRET.

Table 5.3 reports a regression analysis of effort behaviour in PUBLIC/EXO and in SECRET. Similarly to the analysis performed in the previous sub-section, in a first model (Model I) we regress effort on a variable for the own wage ('*Own wage*') and on dummies for different levels of the co-worker's wage, *PUBLIC/EXO £1* and *PUBLIC/EXO £7*, assuming value 1 for effort choices made in sessions where the co-worker earns £1 and £7 respectively, and 0 otherwise. In Model II we add controls for individual characteristics, while in Model III we add interaction terms between the '*Own wage*' variable and the co-worker's wage dummies.

Table 5.3. Effort Behaviour in SECRET and PUBLIC/EXO

	I	II	III
Own wage	0.246*** *(0.052)*	0.248*** *(0.052)*	0.222** *(0.096)*
PUBLIC/EXO £1	-0.654*** *(0.227)*	-0.611*** *(0.214)*	-0.559 *(0.518)*
Own wage * PUBLIC/EXO £1	-	-	-0.016 *(0.115)*
PUBLIC/EXO £7	-0.761*** *(0.254)*	-0.745*** *(0.262)*	-1.169** *(0.535)*
Own wage * PUBLIC/EXO £7	-	-	0.103 *(0.111)*
1 if Male	-	0.137 *(0.208)*	0.140 *(0.209)*
1 if studies Social Sciences (incl. Economics)	-	0.186 *(0.207)*	0.196 *(0.206)*
N.	*180*	*180*	*180*
Pseudo R^2:	*0.164*	*0.169*	*0.176*

Ordered probit regressions. Dependent variable is effort. Robust standard errors in parentheses adjusted for intragroup correlation (individuals are used as independent clustering units). * $.05 < p \leq .10$; ** $.01 < p \leq .05$; *** $p \leq .01$.

The coefficient on the '*Own wage*' variable in Model I to III is positive and significant and the interaction terms added in Model III are all insignificant: this confirms that the level of the own wage is an important determinant of effort choices also in PUBLIC/EXO. The dummies controlling for pay comparison effects in PUBLIC/EXO are negative and highly significant both in Model I and in Model II: the availability of information about the co-worker's wage is detrimental for effort behaviour, regardless of the level of the co-worker's wage. Contrary to the results reported in Table 5.2, the coefficient on the gender dummy is positive and insignificant in both models presented in Table 5.3, showing that overall we do not observe a clear gender effect in our experiment. The dummy for field of study also falls short of statistical significance in both models where it is included.

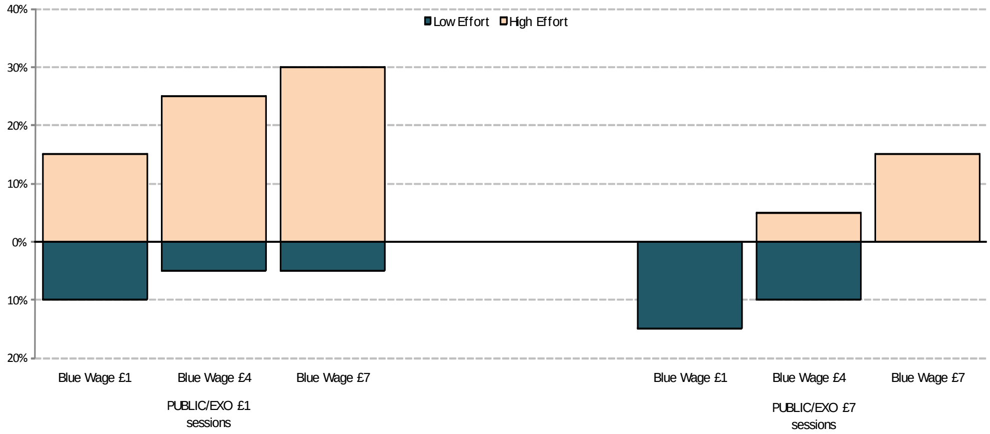
Overall, these results confirm that pay comparisons have a detrimental impact on effort. In fact, the negative effects of pay comparisons appear even more marked in PUBLIC/EXO: here employees are less willing to act favourably towards the employer regardless of relative pay conditions, while in PUBLIC the detrimental impact of pay comparisons was only detected for high levels of the co-worker's wage. Hence, the presence of exogenous constraints to a firm's wage structure seems to amplify the importance of relative wages for pay fairness considerations.

5.3.3. RED Employees' Effort in PUBLIC/EXO and Employers' Wage Choices

Though not the focus of the experiment, our design also delivers data on wage choices by Employers across the three treatments, and on effort responses to fixed wages by RED Employees in PUBLIC/EXO. Here we briefly present these data for completeness.

In PUBLIC/EXO, RED Employees knew that their wage had been fixed exogenously by the experimenter at either £1 (PUBLIC/EXO £1 sessions) or £7 (PUBLIC/EXO £7 sessions), and were asked to make an effort choice for each possible level of the BLUE wage chosen by the Employer. Figure 5.3 shows the proportions of low and high effort choices made by RED employees in the PUBLIC/EXO sessions for different levels of the BLUE wage.

Figure 5.3. Low and High Effort Rates: RED Employees in PUBLIC/EXO*



Medium effort is the omitted category, thus % low effort + % high effort + % medium effort = 100%. Bars are based on choices made by 20 RED Employees in each version of PUBLIC/EXO.

It is interesting to note how RED Employees' willingness to punish the Employer decreases and their willingness to reward the Employer increases as the Employer pays higher wages to the BLUE Employee. Thus, while as outlined in the previous sub-sections co-workers' wages have an overall negative impact on the effort decisions of those employees whose wage was *actively* chosen by the Employer, the same wage inequalities seem to have a positive impact on the effort of RED employees, whose wage was *not* chosen by the Employer. A second interesting pattern emerging from Figure 5.3 is that RED Employees seem to expend higher effort when paid a £1 wage than a £7 wage. Thus, a *negative* relation between own wage and effort seems to exist when own wages are not chosen by the Employer. [59]

Turning to Employers' wage choices, Table 5.4 shows the combinations of wages paid by Employers across the three treatments of our experiment. The most noticeable feature of the Table is the high frequency of unequal wages choices made by Employers across the three treatments (55% of the cases). Unequal wages were chosen more often in PUBLIC/EXO (about 72% of the cases) than in the two other treatments (20% of the cases).

Table 5.4. Frequencies of Employers' Wage Choices across Treatments (SECRET / PUBLIC / PUBLIC/EXO)

	BLUE wage = £1	BLUE wage = £4	BLUE wage = £7
RED wage = £1	6 / 5 / 8	0 / 1 / 11	0 / 0 / 1
RED wage = £4	2 / 1 / -	2 / 2 / -	0 / 0 / -
RED wage = £7	0 / 0 / 11	0 / 0 / 6	0 / 1 / 3

5.4. Discussion & Conclusions

This study reports an experiment designed to examine the impact of pay comparison information on effort behaviour in a multilateral version of the gift-exchange game. We compare effort choices made by employees in a treatment where they receive no information about the wage paid to their co-worker, with effort choices made in two treatments where co-workers' wages are public information. The two 'public wages' treatments vary in whether co-

[59] To explore these patterns we ran an ordered probit regression (clustering on individuals) of RED Employees' effort on a dummy variable assuming value 1 for the sessions where the RED wage was £7, and on two co-worker's wage dummies assuming value 1 when the BLUE wage was either £4 or £7 respectively. The co-worker's wage dummies were both significant (the £4 dummy at the 10% level, the £7 dummy at the 1% level), revealing that RED Employees expended more effort when the Employer paid a medium or high wage to the co-worker relative to the case where the co-worker was paid a £1 wage. The own wage dummy was negative and significant at the 10% level.

workers' wages are chosen by the employer, or fixed exogenously by the experimenter.

In all treatments of our experiment employees reciprocate high wages with high effort: a strong positive own wage-effort relationship exists in our setting, as it has been observed in many other gift-exchange game experiments (e.g., Fehr et al., 1993; Fehr et al., 1997; Gächter and Falk, 2002; Charness, 2004). Pay comparison information is found to be detrimental for the own wage-effort relation. In the treatment where co-workers' wages are chosen by the employer we find that learning that the co-worker is paid the highest wage reduces an employee's willingness to expend high effort relative to the treatment where co-workers' wages are secret. Pay comparison effects are instead weaker for medium or low co-workers' wage levels. These findings compare with those by Gächter and Thöni (2009), who also observe stronger pay comparisons effects when employees are paired with a highly-paid co-worker. Pay comparison effects are amplified in the treatment where the employer is forced to pay an exogenously fixed wage to a portion of the workforce. Here the detrimental effects of pay comparisons are found regardless of the level of the co-worker's wage. These findings suggest that the presence of exogenous constraints to a firm's wage policy may increase the prominence of relative pay comparisons as a useful source of information about what constitutes a fair wage, thus strengthening the importance of horizontal fairness concerns for pay fairness evaluations.

Overall, our results show that the presence of information about others' well-being and the resulting ability to discover interpersonal inequalities can do substantial harm to reciprocal relations and to pro-social behaviours more in general: for any given level of the own wage, employees in our experiment are less willing to reward the employer and more willing to incur costs to reduce her earnings when co-workers' wages are public than when they remain undisclosed. That the ability to contemplate income inequalities can be detrimental for social behaviours has also been found in a variety of experimental settings. For example, it has been found that having subjects start the experiment with unequal distributions of endowments can reduce cooperativeness in public goods experiments (e.g., Anderson et al., 2008) and harm trust and trustworthiness in investment and trust games (e.g., Greiner et al., 2007; Hargreaves-Heap et al., 2009), although the effects are not always marked (see e.g., Anderson et al., 2006). Overall, these findings lend support to the argument (sometimes invoked to explain firms' preferences for wage secrecy) that the confidentiality of earnings within groups of individuals can effectively minimize the losses that arise due to negative reactions to observed inequalities and, more generally, to the dampening of their pro-social inclinations.

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Appendix A: Experimental Instructions

In this Appendix we report the instructions used in the PUBLIC/EXO £7 sessions. Instructions used in the PUBLIC/EXO £1 sessions are identical with the exception that RED Employees' wage was fixed at £1. Any difference between the instructions used in the PUBLIC/EXO sessions and those used in the SECRET and PUBLIC sessions is italicized and reported in square brackets.

Instructions

Welcome!

You are about to take part in an experiment in the economics of decision making. This experiment is run by the "Centre for Decision Research and Experimental Economics" and has been financed by various foundations for research promotion.

There are other people in this room, who are also participating in this experiment. All participants are reading the same instructions as you are and have been recruited in the same fashion. Likewise, all participants are participating in this experiment for the first time, as you are.

It is important that you do not talk, or in any way try to communicate with the other participants during the experiment. If you have a question, raise your hand and a monitor will come over to where you are sitting and answer your question in private.

The experiment will take no more than 60 minutes, and at the end you will be paid in private and in cash. You will be paid a £3 show-up fee, plus an additional amount that will depend on the decisions that you and the other participants make. It is therefore very important to read these instructions with care.

1. Introduction

In this experiment you will be randomly paired with two other participants to form a group of three people. We will refer to each group as a "**firm"**, and to the three group members as "**the Employer**", "**the Blue Employee**" and "**the Red Employee**".

The computer will randomly determine whether you are the Employer, the Blue Employee or the Red Employee just at the beginning of the experiment.

You will *not* be informed about who of the other participants are in your firm, either during or after the experiment. Therefore, all decisions are made anonymously.

In this experiment you will be asked to perform the following decision task and you will do it only once.

At the end of the experiment you will be paid a £3 show-up fee plus your earnings from this task.

2. The decision task

The structure of the decision-making within each firm is as follows.

- **The Employer** is initially endowed with £22 from which he/she pays a wage to the two employees with whom he/she is paired.

  Wages can take three values: £1, £4 or £7.

  The Employer **must** pay a £7 wage to the Red Employee, while he/she can decide on what wage (£1, £4 or £7) to pay to the Blue Employee. *[SECRET and PUBLIC: The Employer can decide on what wage (£1, £4 or £7) to pay to the Red Employee and on what wage (£1, £4 or £7) to pay to the Blue Employee.]*

  The wage the Employer chooses for the Blue Employee and the wage he/she must pay to the Red Employee will be subtracted from his/her £22 endowment. *[SECRET and PUBLIC: The wages the Employer chooses for the Blue Employee and the Red Employee will be subtracted from his/her £22 endowment.]*

- Each **Employee** is then informed of the wages paid by the Employer, i.e. employees learn their own wage and the wage that the Employer pays to the other employee. *[SECRET: Each* ***Employee*** *is then informed of the wage the Employer pays to him/her, i.e. employees learn their own wage but not the wage that the Employer pays to the other employee.]*

  Each employee chooses then independently and in private an effort level: low, medium or high.

  Low effort costs the employee £1 and reduces the Employer's earnings by £4.
  Medium effort costs the employee nothing and leaves the Employer's earnings unchanged.
  High effort costs the employee £1 and increases the Employer's earnings by £4.

On the next pages you will find a couple of hypothetical examples which will illustrate how to calculate the earnings of each member in the firm.

HYPOTHETICAL EXAMPLE FOR DEMONSTRATION PURPOSES, #1

The Employer must pay the £7 wage to the Red Employee. Suppose the Employer chooses to pay a £4 wage to the Blue Employee. *[SECRET and PUBLIC: Suppose the Employer chooses to pay a £1 wage to the Red Employee and a £4 wage to the Blue Employee.]*

Suppose that the Red Employee chooses LOW effort and the Blue Employee chooses HIGH effort.

This situation results in the following earnings:

==> **EMPLOYER'S EARNINGS:**

The Employer pays a total of £11 *[SECRET and PUBLIC: £5]* to the employees: £7 *[SECRET and PUBLIC: £1]* to the Red Employee and £4 to the Blue Employee.

The Employer receives a £4 revenue from the HIGH effort of the Blue Employee, but the Red Employee's LOW effort choice decreases his/her earnings by £4.

Therefore the Employer's earnings are: £22 – £11 *[SECRET and PUBLIC: £5]* + £4 - £4 = **£11** *[SECRET and PUBLIC: £17]*.

==> **RED EMPLOYEE'S EARNINGS**:

The Red Employee is paid a £7 *[SECRET and PUBLIC: £1]* wage. LOW effort costs £1 to the employee.

Therefore the Red Employee's earnings are: £7 *[SECRET and PUBLIC: £1]* - £1 = **£6** *[SECRET and PUBLIC: £0]*.

==> **BLUE EMPLOYEE'S EARNINGS**:

The Blue Employee receives a £4 wage. HIGH effort costs £1 to the employee.

Therefore the Blue Employee's earnings are: £4 - £1 = **£3**.

HYPOTHETICAL EXAMPLE FOR DEMONSTRATION PURPOSES, #2

The Employer must pay the £7 wage to the Red Employee. Suppose the Employer chooses to pay a £1 wage to the Blue Employee. *[SECRET and PUBLIC: Suppose the Employer chooses to pay a £7 wage to the Red Employee and a £4 wage to the Blue Employee.]*

Suppose that the Red Employee chooses MEDIUM effort and the Blue Employee chooses MEDIUM effort.

This situation results in the following earnings:

==> **EMPLOYER'S EARNINGS:**

The Employer pays a total of £8 to the employees: £7 to the Red Employee and £1 to the Blue Employee.

Employees' effort choices do not produce any revenue for the Employer, as MEDIUM effort leaves the Employer's earnings unchanged.

Therefore the Employer's earnings are: £22 – £8 + £0 + £0 = **£14**.

==> **RED EMPLOYEE'S EARNINGS**:

The Red Employee is paid a £7 wage. MEDIUM effort costs nothing to the employee.

Therefore the Red Employee's earnings are: £7 - £0 = **£7**.

==> **BLUE EMPLOYEE'S EARNINGS**:

The Blue Employee receives a £1 wage. MEDIUM effort costs nothing to the employee.

Therefore the Blue Employee's earnings are: £1 - £0 = **£1**.

Although the structure of the decision-making within each firm is the one we have just described, <u>in this experiment employees make their decisions **before** learning the wage that the Employer has actually chosen for the Blue Employee</u>.

Employees know however that **the Employer must pay a £7 wage to the Red Employee.**

If you are an employee, you will then be asked to indicate what you would do in each of the following **THREE SITUATIONS**:

I. The Employer chooses to pay a **£1 wage** to the Blue Employee.
II. The Employer chooses to pay a **£4 wage** to the Blue Employee.
III. The Employer chooses to pay a **£7 wage** to the Blue Employee.

[SECRET: Although the structure of the decision-making within each firm is the one we have just described, <u>in this experiment employees make their decisions before learning the wages that the Employer has actually chosen for them.</u>

*If you are an employee, you will then be asked to indicate what you would do in each of the following **THREE SITUATIONS**:*

I. *The Employer chooses to pay a* ***£1 wage*** *to you.*
II. *The Employer chooses to pay a* ***£4 wage*** *to you*
III. *The Employer chooses to pay a* ***£7 wage*** *to you.]*

[PUBLIC: Although the structure of the decision-making within each firm is the one we have just described, <u>in this experiment employees make their decisions before learning the wages that the Employer has actually chosen.</u>

If you are an employee, you will then be asked to indicate what you would do in each of the following ***NINE SITUATIONS****:*

I. *The Employer chooses to pay a* ***£1 wage*** *to you and a* ***£1 wage*** *to the other Employee.*
II. *The Employer chooses to pay a* ***£1 wage*** *to you and a* ***£4 wage*** *to the other Employee.*
III. *The Employer chooses to pay a* ***£1 wage*** *to you and a* ***£7 wage*** *to the other Employee.*
IV. *The Employer chooses to pay a* ***£4 wage*** *to you and a* ***£1 wage*** *to the other Employee.*
V. *The Employer chooses to pay a* ***£4 wage*** *to you and a* ***£4 wage*** *to the other Employee.*
VI. *The Employer chooses to pay a* ***£4 wage*** *to you and a* ***£7 wage*** *to the other Employee.*
VII. *The Employer chooses to pay a* ***£7 wage*** *to you and a* ***£1 wage*** *to the other Employee.*
VIII. *The Employer chooses to pay a* ***£7 wage*** *to you and a* ***£4 wage*** *to the other Employee.*
IX. *The Employer chooses to pay a* ***£7 wage*** *to you and a* ***£7 wage*** *to the other Employee.]*

Please note that <u>one of these situations will actually count for determining your and the other firm members' earnings</u>, so make your choices carefully.

You will be informed of which situation is actually relevant at the end of the experiment, once everyone in the firm has taken his/her decision. The wage the Employer has actually chosen for the Blue Employee will determine which of the three situations above (I, II or III) counts for the computation of earnings. [*SECRET: The wages the Employer has actually chosen will determine, for each employee, which of the three situations above (I, II or III) counts for the computation of earnings.] [PUBLIC: The wages the Employer has actually chosen will determine, for each employee, which of the nine situations above (I to IX) counts for the computation of earnings.]* Employees' choices in *that* situation will determine the final outcome for each firm member.

A complete list of all the possible earnings distributions resulting from the employees' effort choices in *each* of these three situations is provided in a separate sheet. *[SECRET and PUBLIC: A complete list of all the possible earnings distributions resulting from the employees' effort choices in each of these three [nine in PUBLIC] situations is provided separately. Two sets of Tables are provided. The two sets contain <u>exactly the same information</u>, just organised differently for your convenience. If you are a Red Employee you should refer to the set labelled "USE IF YOU ARE A RED EMPLOYEE". If you are a Blue Employee you should refer to the set labelled "USE IF YOU*

ARE A BLUE EMPLOYEE". If you are an Employer you can refer to either one.]

3. What happens next?

- When the experiment starts, the computer will randomly assign you to a firm and randomly determine whether you are the Employer, the Blue Employee or the Red Employee.
- You will then access a couple of screens where you will be asked to answer a few questions. You will also have to calculate the earnings of all members of your firm for six hypothetical scenarios, with the help of the attached Tables. Press the "Check answers" button on the screen once you have answered all the questions. The computer will let you know whether your answers are correct.
- Once everyone has answered all the questions correctly, you will access the "Decision task" screen. Depending on whether you are an employer or an employee you will have to choose wage or effort levels, as described above in Section 2.

At the end of the experiment, you will be paid a £3 show-up fee plus your earnings from the decision task.
Please, raise your hand if you have any questions.

Appendix B: Earnings Distributions Tables

In this Appendix we report the Earnings Distributions tables used by BLUE Employees in the PUBLIC sessions. In order to treat the two employee types symmetrically, columns and rows were inverted in the tables used by Red Employees and the order in which wage combinations were presented was modified accordingly. The tables used in the SECRET treatment were identical, but had different captions to account for the fact that only three 'situations' could occur in SECRET, depending on the level of the own wage. Tables with the same own wage and different levels of the co-worker's wage were referred to as different 'cases' of the same 'situation'. In the PUBLIC/EXO treatment only the tables where the co-worker's wage was equal to £1 (in PUBLIC/EXO £1) or £7 (in PUBLIC/EXO £7) were used.

EARNINGS DISTRIBUTIONS

TABLE I: Earnings distributions resulting from the employees' effort choices in **SITUATION I**, i.e. when the Employer chooses a **£1 wage** for the Blue Employee and a **£1 wage** for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£12	£0	£0	£16	£0	£1	£20	£0	£0
Blue Employee chooses **MEDIUM** effort	£16	£1	£0	£20	£1	£1	£24	£1	£0
Blue Employee chooses **HIGH** effort	£20	£0	£0	£24	£0	£1	£28	£0	£0

TABLE II : Earnings distributions resulting from the employees' effort choices in **SITUATION II**, i.e. when the Employer chooses a **£1 wage** for the Blue Employee and a **£4 wage** for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£9	£0	£3	£13	£0	£4	£17	£0	£3
Blue Employee chooses **MEDIUM** effort	£13	£1	£3	£17	£1	£4	£21	£1	£3
Blue Employee chooses **HIGH** effort	£17	£0	£3	£21	£0	£4	£25	£0	£3

TABLE III : Earnings distributions resulting from the employees' effort choices in SITUATION III, i.e. when the Employer chooses a £1 wage for the Blue Employee and a £7 wage for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£6	£0	£6	£10	£0	£7	£14	£0	£6
Blue Employee chooses **MEDIUM** effort	£10	£1	£6	£14	£1	£7	£18	£1	£6
Blue Employee chooses **HIGH** effort	£14	£0	£6	£18	£0	£7	£22	£0	£6

TABLE IV : Earnings distributions resulting from the employees' effort choices in **SITUATION IV**, i.e. when the Employer chooses a **£4 wage** for the Blue Employee and a **£1 wage** for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£9	£3	£0	£13	£3	£1	£17	£3	£0
Blue Employee chooses **MEDIUM** effort	£13	£4	£0	£17	£4	£1	£21	£4	£0
Blue Employee chooses **HIGH** effort	£17	£3	£0	£21	£3	£1	£25	£3	£0

TABLE V : Earnings distributions resulting from the employees' effort choices in SITUATION V, i.e. when the Employer chooses a £4 wage for the Blue Employee and a £4 wage for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£6	£3	£3	£10	£3	£4	£14	£3	£3
Blue Employee chooses **MEDIUM** effort	£10	£4	£3	£14	£4	£4	£18	£4	£3
Blue Employee chooses **HIGH** effort	£14	£3	£3	£18	£3	£4	£22	£3	£3

TABLE VI: Earnings distributions resulting from the employees' effort choices in SITUATION VI, i.e. when the Employer chooses a £4 wage for the Blue Employee and a £7 wage for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£3	£3	£6	£7	£3	£7	£11	£3	£6
Blue Employee chooses **MEDIUM** effort	£7	£4	£6	£11	£4	£7	£15	£4	£6
Blue Employee chooses **HIGH** effort	£11	£3	£6	£15	£3	£7	£19	£3	£6

TABLE VII : Earnings distributions resulting from the employees' effort choices in **SITUATION VII**, i.e. when the Employer chooses a **£7 wage** for the Blue Employee and a **£1 wage** for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£6	£6	£0	£10	£6	£1	£14	£6	£0
Blue Employee chooses **MEDIUM** effort	£10	£7	£0	£14	£7	£1	£18	£7	£0
Blue Employee chooses **HIGH** effort	£14	£6	£0	£18	£6	£1	£22	£6	£0

TABLE VIII : Earnings distributions resulting from the employees' effort choices in **SITUATION VIII**, i.e. when the Employer chooses a **£7 wage** for the Blue Employee and a **£4 wage** for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£3	£6	£3	£7	£6	£4	£11	£6	£3
Blue Employee chooses **MEDIUM** effort	£7	£7	£3	£11	£7	£4	£15	£7	£3
Blue Employee chooses **HIGH** effort	£11	£6	£3	£15	£6	£4	£19	£6	£3

TABLE IX : Earnings distributions resulting from the employees' effort choices in **SITUATION IX**, i.e. when the Employer chooses a **£7 wage** for the Blue Employee and a **£7 wage** for the Red Employee.

	Red Employee chooses **LOW** effort			Red Employee chooses **MEDIUM** effort			Red Employee chooses **HIGH** effort		
	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee	Employer	Blue Employee	Red Employee
Blue Employee chooses **LOW** effort	£0	£6	£6	£4	£6	£7	£8	£6	£6
Blue Employee chooses **MEDIUM** effort	£4	£7	£6	£8	£7	£7	£12	£7	£6
Blue Employee chooses **HIGH** effort	£8	£6	£6	£12	£6	£7	£16	£6	£6

CHAPTER 6

CONCLUSIONS

In this thesis we have reported four studies that used novel laboratory experiments to explore different research topics in the field of behavioural and experimental economics. In two studies (reported in chapter 2 and chapter 3) we have explored issues related to cooperation in settings where commitment opportunities are available. The two other studies included in this thesis (reported in chapter 4 and chapter 5) have explored whether and how the presence of social comparison information affects individuals' social motivation.

In chapter 2 we studied cooperation in a setting where collective interest and individual interest are in conflict. In our setup some individuals can credibly commit to cooperate by announcing an early contribution to a public good. Previous experiments with this sort of setup have shown that this typically encourages others to cooperate as well, which is beneficial for the group. We examined the question of which characteristics should early contributors possess to be most effective in promoting the interests of the group. Our main finding is that collective interest is best served when early contributors are intrinsically inclined to cooperate: these individuals make the highest contributions to the public good when they move first, and thus succeed in guaranteeing highest efficiency levels for their group. The fact that cooperatively inclined individuals are more willing to commit to high contributions relative to other types of contributors is partly due to their more optimistic expectations that others will cooperate when they commit to cooperate, and partly due to their higher intrinsic motivation to act cooperatively. Our findings suggest that selective group composition, whereby the most cooperative types are chosen to lead the group by committing to an initial cooperation level, may be beneficial for the collective interest in settings where individuals interact anonymously and non-repeatedly. An interesting question, which we leave for further research, is whether our findings extend to settings that involve repeated interactions, or to settings where sanctioning or rewarding devices are available to group members: in such settings it might be more beneficial for the group not to have the most cooperative individuals commit to an initial cooperation level, but rather to have them observe the contributions of other, less cooperative group members and discipline them through the use of costly sanctions or rewards.

Chapter 3 also studies voluntary contributions to a public good in a setting where an agent can credibly commit to an early contribution decision. Differently from the usual setup used in experimental economics (and which we also used in chapter 2), in chapter 3 we focused on a quasi-linear public goods setting where standard theory makes a stark prediction about the effectiveness of sequential contribution mechanisms: the availability of commitment opportunities can damage public good provision as first-movers enjoy a first-mover advantage by committing to free-ride on the contributions of others. The results from our laboratory experiments, together with the results from the closely related study by Andreoni et al. (2002), suggest that commitment opportunities may be less damaging than predicted by standard theory. The reason is that the 'pull of equilibrium' in this framework is countervailed by the 'pull of fairness' which originates from the presence of individuals who are willing to sacrifice part of their material well-being to punish free-riders and/or reward contributors. While in Andreoni et al. (2002) the 'pull of fairness' prevails over the 'pull of equilibrium' and sequential mechanisms do not lead to lower overall public good provision relative to simultaneous contribution mechanisms, in our setting we do observe lower total contributions to the public good when commitment opportunities are present. However, early contributors who commit to free-ride trigger systematic punishment on the part of late contributors, and this eliminates any predicted first-mover advantage. Because there is no advantage in committing to be a free-rider, an interesting question is whether individuals will actually *choose* to commit if they are given the opportunity to do so.

In Nosenzo and Sefton (2009) we started investigating this question. We used a laboratory experiment where we embedded Varian's (1994) two-player contributions game used in chapter 3 into a setup where subjects endogenously determine the timing of their contributions. Thus, depending on subjects' timing decisions sequential or simultaneous games may arise in the course of the experiment. We seldom observe sequential games in our experiment: most of the times subjects avoid exploiting the possibility of making an early commitment, and they end up playing a simultaneous move game. Overall, these results, together with the findings reported in chapter 3, suggest that the existence of commitment opportunities may not necessarily result in an exacerbation of the free-rider problem.

In chapter 4 and chapter 5 we investigated whether the availability of social comparison information affects second-movers' behaviour in trilateral versions of the gift-exchange game. In both settings studied in these chapters, a first-mover is matched with two second-movers who can observe the actions of the first-mover and choose whether to respond favourably or unfavourably to these actions. In chapter 4 we studied how second-movers' responses are affected by information about how the first-mover treats the other second-mover in the group, and by information about how the other second-mover has responded to the first-mover. We found that information about others' treatment does not affect second-movers' behaviour, whereas information about how others behave can have an important impact on second-movers' willingness to act favourably towards the first-mover. In particular, a positive relation between second-movers' actions emerge in our setting, whereby a second-mover's willingness to act favourably towards the first-mover increases when she observes that the other second-mover is also acting favourably. In chapter 5 we used a different strategy to identify the impact of information about others' treatment on second-movers' responses. Here we compared a treatment where no social comparison information was available with treatments where second-movers were fully informed about first-movers' actions. We find evidence that second-movers' responses are negatively affected by information about how the first-mover treats the other second-mover.

A limitation of these two studies is that, while they reveal the existence of important social comparison effects in this type of environment, they are not informative about the nature of such effects. For example, the observed positive relation between second-movers' actions documented in chapter 4 could be rationalized within current behavioural models of interdependent preferences (e.g. through a model accounting for inequity averse preferences, as in Fehr and Schmidt, 1999), as well as within a framework that explains behaviour as the result of a pressure to comply with two distinct social norms: a norm to act reciprocally, and a norm to conform to peers' actions. To address these issues, we have recently started working on a follow-up study where we modify the trilateral gift-exchange game used in chapter 4 to disentangle these two possible explanations for the observed social comparison effects: genuine inequity aversion vs. pressures to comply with norms of reciprocity and conformity. Preliminary findings reveal that the observed social comparison effects can by and large be rationalized within a framework that models behaviour as responding to aversion to payoff inequalities.

Another interesting development for future research on social comparison processes that we have started considering is the use of laboratory experiments to explore whether individuals actively manipulate the selection of the relevant reference group in comparison processes. With a few exceptions (e.g., Falk and Knell, 2004), the common assumption in economics is that individuals take as given the relevant reference group in social comparison processes. However, research in other disciplines has revealed that individuals do take active part in the selection of the referent others in comparison processes. For example, studies in social psychology (e.g., Wood et al., 1985; Gibbons et al., 1994) have shown that individuals strategically manipulate comparison targets and dimensions to achieve favourable outcomes when threatened with unfavourable social comparisons. It would be particularly interesting to explore experimentally whether such desire to escape threatening comparisons can actually lead individuals to make sub-optimal comparison choices, e.g. shying away from (potentially beneficial) performance comparisons with reference targets that would put them in a disadvantaged position while favouring (unbeneficial, or possibly damaging) comparisons with inferior reference targets.

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