ESSAYS ON REPUTATION AND INFORMATION

by

DANIEL WILLIAM DERBYSHIRE

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Department of Economics
Birmingham Business School
College of Social Sciences
University of Birmingham
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ABSTRACT

The following thesis presents the results and analysis of three distinct experiments. The first features an experimental auction market designed to resemble eBay and other such peer-to-peer markets, including the presence of a reputation mechanism. The results presented suggest that the presence of a reputation mechanism will improve consumer welfare. Further, having a greater number of possible feedback ratings available leads to even further improvements in consumer welfare. The second features a repeated auction that also involves Bayesian uncertainty, about the `type' of the seller. In addition, we present the predictions of a theoretical model that extends the existing sequential equilibrium literature into multi-player, market-based games. We find that reputational concerns remain an important consideration in such settings. The final experiment examines the role of within group heterogeneity (in the endowment and marginal return) in public goods games. The novel experimental designed allows for a full schema of relationships between the endowment and marginal return. The results presented suggest that there are significant behavioural differences depending on the relationship between the endowment and marginal return. When they are inverse, subject's *absolute* contributions are not different. When the two are proportionally related, *relative* contributions are not different.

This thesis is dedicated to my parents.

Cassio *Reputation! Reputation! Reputation! Oh, I have lost my reputation! I have lost the immortal part of myself, and what remains is bestial. My reputation, Iago, my reputation!*

Iago *As I am an honest man, I thought you had received some bodily wound. There is more sense in that than in reputation. Reputation is a false and most idol imposition, oft got without merit and lost without deserving. You have lost no reputation unless you repute yourself such a loser.*

- *Othello*, Act 2, Scene 3, William Shakespeare

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Nonetheless, any errors or omissions remain my own.

Table of Contents

CHAPTER 1

Introduction

Experimental economics has grown immensely since the early experiments of, for example, Chamberlin (1948) and Allais (1953). A survey of this early literature can be found in Roth (1995). Since then, experimental methods have been used to study a wide variety of economic contexts. Each of the proceeding chapters present the results of an experiment. The primary advantage of the experimental method is the ability to isolate specific effects in situations where that may not be possible with naturally occurring, pre-existing data.

For example, Chapter 2 features an experiment that isolates the effect of having a reputation mechanism in an eBay `style' auction. In reality, such marketplaces don't have multiple reputation mechanisms at the same time and comparing between marketplaces is difficult because there may be many other covariates. By using an experiment, we can examine the behaviour in two otherwise identical markets, the only difference being the type of reputation mechanism on offer. Chapter 3 presents a theoretical and experimental investigation into the effects of Bayesian uncertainty in a repeated auction setting. The advantage of using the experimental method in this case is that we can control the nature of the Bayesian uncertainty (homemade priors such as those identified by Camerer and Weigelt (1988) notwithstanding). Chapter 4 presents a study on the effect of within group heterogeneity in public goods games. The public goods game is one of the most commonly used constructs in experimental economics as it allows investigation into social dilemma situations; those where individual and group (social) interests are in conflict.

Since the various treatments vary according to only a single variable or parameter (i.e. the treatment variable); any systematic differences in subjects' behaviour is therefore attributed to the treatment variable. To flesh out an earlier example, in Chapter 2, the difference in behaviour between two of the present treatments (a baseline control and a binary reputation mechanism) is attributed solely to the presence of a reputation mechanism since everything else is held constant. The random assignment of subjects to treatments ensures that any other affective variable is uniformly distributed across the treatment groups. As such, the experimental method allows us to analyse a particular *ceteris parabus* effect.

The primary focus of Chapter 2 *("Reputation Mechanisms and the Number of Possible Feedback Ratings: A Comparative Experiment")* is an investigation into the effect on reputation mechanisms (of the kind featured on websites such as eBay and Amazon) of the range of possible ratings that can be given. In particular, the results of an experiment featuring an auction market with multiple buyers and sellers are presented. The auction is designed to mirror eBay and Amazon style transactions in the sense that the seller can decide, *ex-post*, to renege on the transaction having already been paid. The auction presented in Chapter 2 thus features moral hazard. Two reputation mechanisms are compared to a baseline control treatment that featured no reputation mechanism. One treatment featured a reputation mechanism with only two possible ratings; 0 and 1. The other featured a total of seven possible ratings from 1 to 7. Since there are multiple sellers in each market and the bidders are able to see the feedback history, it is hypothesised that bidders are better off when there is a reputation mechanism available (since they can avoid bad sellers or at least lower their bids accordingly).

Since reputation mechanisms are an increasingly important, and salient, feature of online market places, it is important to understand the determinants of the efficacy of such mechanisms. Indeed, the number of possible feedback ratings is one of the simplest design parameters and as our results show it can affect both the behaviour of both buyers and sellers in such markets. As such, it is important that care is taken when designing reputation mechanisms.

The results presented do indeed suggest that in both treatments featuring a reputation mechanism, bidders are significantly better off than the baseline control treatment that features no reputation mechanism. It is also apparent that sellers respond to the final auction price in deciding whether to send the good (and incur the associated cost of doing so, despite having no formal obligation). Further, the sellers are more likely to honour the sale when there is a reputation mechanism available. The results further suggest that buyers are better off when there are greater number of possible feedback ratings available in the sense that they earn higher profits.

In Chapter 3 *("Sequential Equilibrium and Moral Hazard Auctions")*, the notion of different 'types' of sellers is formalised within the sequential equilibrium framework. In the sequential equilibrium framework, reputation emerges as a rational response to the existence of some behavioural type. This type typically has some desirable characteristic, such as being able to credibly pre-commit to certain actions. This gives rise to incentives to mimic the behavioural type, to gain a reputation for *being* the behavioural type that can be exploited in the final rounds. The particular investigation of Chapter 3 involves an auction similar to that featured in Chapter 2. To simplify the design the markets featured a single seller. A theoretical model of a moral hazard auction with some probability that a given seller is a 'good' seller (in the sense that they always send the good) is developed and a unique equilibrium of the one-shot auction is presented. In addition, a sequential equilibrium including a full multi-period bidding strategy, Bayesian beliefs and a full seller strategy is derived for the 2 and 3-period version of the auction. The chapter contributes to the literature through the extension of the sequential equilibrium framework into a multi-player, market-based game such as an auction. This allows a more formal analysis of the role of reputation formation in markets as opposed to repeated 'one on one' interactions.

An experiment featuring a repeated, multi-period game, i.e. a 3-period game was played 4 times in succession, was conducted in which each group of bidders was assigned to a different seller for each new 3-period game. Three treatments were conducted that vary according to the nature of the sellers. In one baseline, all the sellers are other experimental subjects and thus are making real decisions depending on the outcome of the auction and their own cost parameter (i.e. the exact structure used in Chapter 2). In the other baseline control, all the sellers' actions were implemented automatically by the computer, always sending the good. This is a canonical private value auction. The novel treatment of the experiment featured in Chapter 3 involves half of the sellers being real subjects and half being implemented automatically by the computer. In this case, as per the sequential equilibrium logic presented in the chapter, the real subjects

have incentives to mimic the computer sellers in the first 2 periods of the 3-period game, by sending the goods and receiving lower immediate profits, in order to not send the good in the final period. On the other hand, if a seller does not send the good then the bidders must know that they are not a computer seller and will bid accordingly in the future.

The results presented suggest that bidders behaviour significantly differently across the three treatments. In the novel, sequential equilibrium treatment there is strong evidence that bidders reduce their bids after the seller reneges on the contract – something bidders in the baseline featuring only real sellers fail to do. Similarly, there is evidence that sellers in the sequential equilibrium treatment are willing to renege on the sale but only in the final period of the 3-period blocks. Again, this is behaviour that is not exhibited by the sellers in the baseline control treatment. Thus, despite some substantial quantitative deviations, there are clear patterns of behaviour that are qualitatively consistent with the expected behaviour and equilibrium predictions of the reputation building hypothesis.

In Chapter 4 *("Public Goods Games with Structural Heterogeneities in Endowment and Marginal Return")*, the results of two Public Goods game are presented. The Public Goods game framework models a social dilemma in which private and social (group) interest are in conflict. The purpose of Chapter 4 is to investigate the effect of *ex-ante* heterogeneities in the initial endowment and the marginal return. The chapter was inspired by the empirical observation that in the United Kingdom, those with the highest incomes also have the highest rate of acquiring private healthcare and educational services (see ONS, 2013, p.10 and IFS, 2010, p.39,46) – suggesting that they have lower than average benefits from the provision of these public goods. Thus, in one treatment, the initial endowment and marginal return are inversely related such that those with the highest endowment have the lowest marginal return. As such there is a conflict between the *ability* (i.e. the endowment) and the *incentives* (i.e. the marginal return) to contribute to the provision of the public good. In another treatment, the initial endowment and marginal return are proportionally related such that those with the highest endowment also have the highest marginal return. Further treatments were included for control purposes in which there is heterogeneity in only the initial endowment or marginal return. A final control treatment features groups that are homogeneous in both initial endowment and marginal return. In this first experiment the game is a one-shot encounter. Chapter 4 features an additional experiment featuring only the treatments in which both the initial endowment and marginal return are heterogeneous being repeated to investigate the dynamics of any potential contribution norms that may arise, in addition to the robustness of the one-shot results.

Interestingly, there are no overall differences between the various treatments conducted. There are, however, important and significant within treatment differences, as well as between treatment differences at the individual level. The results in the initial, one-shot experiment suggest that subjects are responsive to heterogeneities in only the endowment and marginal return (or both). When the initial endowment and marginal return were inversely related, the results suggested a conflict of both beliefs and behaviour between those with high endowment/low marginal return and those with low endowment/high marginal return. The repeated experiment sheds further light on this conflict, highlighting the importance of different contribution rules. When the endowment and marginal return are inverse, each type contributes on average the same in *absolute* terms. On the other hand, when they are proportional, each type contributes on average the same in *relative* terms (i.e. contributions were proportional to endowment/marginal return). This highlights the crucial importance of contribution rules in determining behaviour in such settings. Additionally, there is an analysis of the welfare implications of the public goods mechanism by considering the effect on the Gini coefficient for each group. In the treatment which has initial endowment and marginal return inversely (or negatively) related, the public goods mechanism has a significant, positive effect on inequality as measured by the Gini coefficient (i.e. the level of inequality decreases). On the other hand, when initial endowment and marginal return are proportionally (or positively) related, the mechanism has no effect on the pre-existing inequality.

We now present the three independent chapters outlined above that are thematically tied together by the concept of behavioural heterogeneity and a final chapter, Chapter 5, draws overall conclusion.

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Chapter 2

Reputation Mechanisms and the Number of Possible Ratings: A Comparative Experiment

Abstract

We present the results of an experimental auction market featuring seller side moral hazard. We compare behaviour in the absence of a reputation mechanism to two alternative reputation mechanisms which vary the number of possible feedback ratings that can be given as either 2 (binary) or 7 (non-binary). In the absence of a reputation mechanism, the market persists to the detriment of bidders. The presence of a reputation mechanism makes bids fall rapidly over time. This suggests the reputation mechanism facilitates learning by bidders. Prices fall significantly quicker when more possible feedback ratings are available (the non-binary case), in direct contrast to the theoretical prediction that a greater number of possible feedback ratings introduce noise and thus inefficiency.

JEL Codes: C73, C92, D44, D82

Keywords: Reputation, Auctions, Private Information, Markets, Experiments

2.1 Introduction

Reputation mechanisms are an increasingly important feature of many websites, and especially of those that allow users to conduct peer-to-peer financial transactions, i.e. to buy and sell things directly from and to one another. The need for a reputation mechanism arises from the moral hazard implicit in such an undertaking. Buyers are often required to pay upfront without a meaningful guarantee that the items purchased will be shipped (or the services rendered). Given the variation between sellers, such marketplaces also feature adverse selection (in seller quality). The reputation mechanism is crucial in overcoming the moral hazard and adverse selection and allowing for the functioning of a successful market. The most notable example of such a market is eBay but there are many others. The reputation mechanism enables a functioning market by allowing buyers to share their experiences of particular sellers with one another. Interestingly, even the clandestine internet marketplace Silk Road (which was shut down by the FBI in 2013) features a (non-binary) reputation mechanism. This may seem strange, though it is perhaps particularly important on such sites given the other anonymisation procedures that are often in place and the corresponding lack of consumer protection. The effect of reputation is two-fold; it constrains seller behaviour (overcoming moral hazard) by allowing the possibility of credible reductions in future profits (lower/fewer bids)[1] and enables bidder learning (overcoming adverse selection) about seller quality (i.e. did the good arrive (on time)? Was it as described? etc. Also see Cabral (2005)). Reputation mechanisms can be broadly thought of in two categories;

Binary: A binary reputation mechanism allows agents to rate an interaction as either positive or negative, often represented as + and -[2] (e.g. eBay).

Non-Binary: A non-binary reputation mechanism allows agents to rate an interaction along a multi-point Likert scale, common examples are a score out of 5, 7 or 10 (e.g. Amazon).

The current paper presents the results of an experiment in which we implement a repeated auction market featuring seller side moral hazard (the seller does not have to honour the auction contract after being paid) and vary 1) whether a reputation mechanism is available or not and 2) the number of possible feedback ratings when a reputation mechanism is available. It is important to understand the

[1] In a situation in which there is no feedback, only the winning bidder can (attempt to) impose future payoff reductions.

[2] Many sites also allow neutral feedback, though evidence suggests this is rarely used; see Resnick and Zeckhauser (2002), Dellarocas and Wood (2008) or Cabral and Hortaçsu (2010).

effect the total number of feedback ratings has on the use and efficacy of such a mechanism since they are an increasingly salient aspect of many online marketplaces.

Dellarocas (2005) theoretically shows that in the case of non-binary feedback metrics, cooperation rates (the rate of honoured contracts) are *at least as high* when agents treat the metric as though it were binary (that is, when agents create a binary partition of the non-binary metric), compared to a more nuanced conditional strategy. As such, the number of possible feedback ratings is irrelevant. Crucial to this result is that the seller faces a binary decision; to send the good or not. As such, only two possible feedback ratings are required to communicate the entire possible action space of the seller. The extent of any behavioural differences between the use of a binary or non-binary mechanism is little understood and remains an open question in the literature. As such, the current paper seeks to provide some insight by presenting experimental evidence of repeated auction markets featuring no reputation mechanism, a binary mechanism or a non-binary mechanism.

Bolton, Katok and Ockenfels (2004) conduct an experiment using a repeated trust game with three treatments; perfect-strangers, perfect-partners and perfect-strangers with a feedback mechanism. On all three measures used by the authors, efficiency, trust and trustworthiness, the treatment with a feedback mechanism outperforms the strangers treatment, though it does not do quite as well as the partners treatment. It is worth noting that all the feedback used is generated exogenously and completely accurately (perfect monitoring) by a computer. It is thus different from the experiment presented here in which the ratings given are subjective assessments by the subjects themselves. Chen, Hogg and Wozny (2004) implement an experimental auction market with non-binding contracts. The experiment consists of three treatments: `low information' - only history of transactions a player was involved in are available, `high information' - a full transactions history is available to all players and `self-reporting' - following a transaction, players can assign the other party positive or negative feedback, and a feedback score is publicly visible. Unsurprisingly, contract fulfilment rates were lowest in the `low information' context (always below 70%). On the other hand, fulfilment rates in the `high information' and `self-reporting' contexts were similar (above 80% for most periods). This suggests that a reputation mechanism can create similar levels of trustworthiness as a high informational environment. Bolton, Greiner and Ockenfels (2013) present the results of an experimental repeated auction market with seller moral hazard and three different reputation mechanisms designed to investigate the nature of reciprocal feedback[3] and the distortions in reputation information this can

[3] This means that sellers can also leave feedback about buyers, something not possible in the experiment presented in this paper.

create. The authors show that a blind system, in which feedback received cannot be viewed until the person has also left feedback, and a Detailed Seller Rating system[4] can improve upon a baseline case in which feedback is immediately visible.[5] The seller's decision is implemented as a quality scalar. It is thus different from the experiment we present in which the seller faces a binary decision, which allows us a closer comparison with the theory of Dellarocas (2005).

It should be noted that a vast empirical literature on reputation in auctions exists examining the effect of many different aspects such as; an empirical literature examining the existence of a price premium (see Resnick et al (2006), Jin and Kato (2008) or Cabral and Hortaçsu (2010)), quality certification and highlighting `top' sellers (Elfenbein, Fisman and McManus (2014), Nosko and Tadelis (2015)), procurement auctions (Brosig-Koch and Heinrich (2014), Spagnolo (2012)), interventions to increase giving of feedback when it is not compulsory (Cabral and Li (2015)) and that we discuss in detail only those studies most closely related to the current paper.

We present evidence that, in line with the literature presented above, the presence of a reputation mechanism leads to improvements in buyer welfare. In addition, we find that having a greater number of possible feedback ratings (i.e. the non-binary case) leads to further improvements in buyer welfare. This is important because reputation mechanisms are an increasingly important feature of online market places and the number of possible feedback ratings is a fairly basic aspect of the design process yet as our results show it can have a significant impact on behaviour.

The remainder of this paper is structured as follows; section 2 outlines the experimental design and procedures followed, section 3 provides formal hypotheses, section 4 presents the results and section 5 provides some discussion and concluding remarks.

2.2 Experimental Design and Procedures

We first outline the framework environment for the experiment presented in this paper before describing in detail the various treatments under consideration and finally the procedures involved.

[4] Indeed, eBay now employs a Detailed Seller Rating system on its site that allows the user to, in addition to the normal positive/negative feedback, rate the seller along various aspects of the sales process along a five-point scale.

[5] The problem being that sellers would immediately leave buyers negative feedback after receiving negative feedback for seemingly no reason, discouraging people from leaving negative feedback in the first place and thus distorting the reputation information available to future buyers.

2.2.1 Framework: The Internet Auction

All relevant parameters are as used in the experiment presented below. The framework is as follows;

1. Each player was assigned as either a bidder or a seller and groups were randomly formed of 7 bidders $i = \{1, 2, 3, 4, 5, 6, 7\}$ and 3 sellers $j = \{A, B, C\}$. This role and group remained fixed throughout.
2. Bidders received a valuation $v_{i,j} \sim U[5, 10]$ for $j = \{A, B, C\}$ corresponding to each of the three sellers. Valuations are independent between bidders and across sellers. Sellers receive a cost $c_j \sim U[0, 5]$ associated with sending their good. Costs are independent between sellers. All of this is common knowledge.
3. Bidders submitted simultaneous, sealed bids $b_{i,j} \epsilon [0, 10] \forall j = \{A, B, C\}$ for each of the goods. The winner and price P_j were then determined via the second price format.[6] The winning bidder then paid the realised price.[7]
4. Each seller was informed of the price P_j for their respective good. Each seller then decided whether to `send' the good ($S_j = 1$) and incur the associated cost $c_j$ or to not `send' the good ($S_j = 0$) which is costless.
5. Each bidder was informed of the price P_j and whether they were the winning bidder for each good. Only the winning bidder for a particular good is informed of whether the seller `sent' the good or not.[8] Each bidder is then informed of their total profit for the round, given as $u_i = D_{i,A}(S_A v_{i,A} - P_A) + D_{i,B}(S_B v_{i,B} - P_B) + D_{i,C}(S_C v_{i,C} - P_C)$ where $D_{i,j}$ is an indicator variable indicating whether the bidder won the good ($D_{i,j} = 1$) or not ($D_{i,j} = 0$). Each seller is informed of their profit, given as $u_j = P_j - S_j c_j$.
6. Stages 2-5 are repeated a finite number of times with $T = 30$.

Each subject began the experiment with an initial endowment of 300 ECU (Experimental Currency Units) and the valuation and cost parameters described above refer to numbers of ECU.

[6] The winner is the bidder who places the highest bid. In the case of a tie each joint highest bidder wins with equal probability. The price paid is equal to the highest bid amongst the remaining non-winning bidders.
[7] The second price auction format was used for a number of reasons. Firstly, it is the price format used on existing auction websites such as eBay. Secondly, the optimal strategy is not a function of the number of bidders (which was either 5, 6 or 7, depending on the number of subjects arriving for the session). Finally, this optimal strategy is in general easier to calculate than the first price format which involves bid shading.
[8] This information does not form part of the history available to non-winning bidders.

2.2.2 Treatments

A total of three treatments were conducted. The control treatment (C) is precisely the internet auction as described above. The remaining two treatments introduce a binary and a non-binary reputation mechanism. The binary treatment (B) has a reputation mechanism in which the winning bidder can leave feedback of either 0 or 1, where 0 was interpreted as the worst possible feedback and where 1 was interpreted as the best possible feedback (this was explicitly told to subjects in the instructions, an indicative set of instructions for the NB treatment can be seen in an appendix). This is analogous to a situation in which a rating is either negative or positive. The non-binary treatment (NB) has a reputation mechanism in which there are 7 possible feedback ratings (the numbers 1 to 7), where higher ratings are interpreted as meaning better feedback (again, this was explicitly told to subjects), with 1 the worst possible feedback and 7 the best possible feedback. The choice of 7 was motivated by two considerations; creating sufficient difference between the binary and non-binary mechanisms and also ensuring that the non-binary mechanism had a well-defined midpoint (which rules out, for example, the use of a 10-point scale).

2.2.3 Procedures

All sessions were conducted at the University of Birmingham. Each treatment was programmed and conducted using the zTree software (Fischbacher (2007)) and subjects were recruited using ORSEE (Greiner (2004)). A total of 114 subjects were recruited; 40 in the control, 38 in the binary and 35 in the non-binary treatment. Each subject took part in only one treatment (session) and thus the design was between-subject. Each session consisted of two independent groups for a total of 20 subjects (14 bidders and 6 sellers). On occasions when 20 subjects were not available, an alternative was implemented in which the number of bidders was reduced as appropriate with each group always having three sellers. Therefore, in some sessions one or both of the groups consisted of 5 or 6 bidders and 3 sellers. A total of two sessions were run per treatment to give four independent matching groups per treatment. Subjects were asked to complete a series of control questions to ensure understanding. The average session length was 90 minutes and the average payment per subject was £12.10.

2.3 Hypotheses

We now present hypotheses, beginning by setting up the null hypothesis as the purely self-interested theoretical prediction. We expect, based on the results of the literature discussed above[9], to reject the null hypothesis.

Hypothesis 0 (H0): In all treatments and all periods the winning price is zero (that is, $P_{j,t} = 0 \; \forall j = \{A, B, C\}, t = \{1, .., T\}$) since $b_{i,j,t} = 0 \; \forall i, j, t$. Moreover, conditional on out of equilibrium play ($P_{j,t} > 0$), the seller will never send the good ($S_{j,t} = 0 \; \forall j, t$).

The unique weak perfect Bayes equilibrium[10] of the game presented above, obtained through backwards induction and introducing a time subscript, is $S_{j,t} = 0 \;\; \forall \;\; j = \{A, B, C\}, t = \{1, .., T\}$ and $b_{i,j,t} = 0 \; \forall \; i = \{1, .., 7\}, j = \{A, B, C\}, t = \{1, .., T\}$. That is, none of the sellers choose to send the good in any period and, anticipating this, bidders never choose to place positive bids. This remains the case whether a reputation mechanism is available or not (i.e. it is not treatment specific).

Hypothesis 1B (H1B): In the binary and non-binary treatments bidders are better off in the sense that they earn higher profits, compared to the baseline control treatment.

Hypothesis 1S (H1S): In the binary and non-binary treatments sellers are worse off in the sense that they earn lower profits, compared to the baseline control treatment.[11]

Together, they represent a replication of the previous results that the opportunity to use a reputation mechanism can improve market outcomes for first-movers. As such, we expect not to reject these hypotheses.

Hypothesis 2B (H2B): Bidders are no better off in the non-binary treatment compared to the binary treatment, in the sense that profits are not higher in the non-binary treatment than the binary treatment.

[9] For example, Bolton and Ockenfels (2004) or Chen, Hogg and Wozny (2004).

[10] Uncertainty over other bidders' valuations, as in all auctions, introduces Bayesian uncertainty though it is trivial due to the backwards induction logic.

[11] Note that for given valuation and cost, the total surplus is fixed and an increase in bidder profits will be matched with decreased seller profits.

Hypothesis 2S (H2S): Sellers are no worse off in the non-binary treatment compared to the binary treatment, in the sense that profits are not lower in the non-binary treatment than the binary treatment.

Failure to reject these hypotheses would provide support for the results of Dellarocas (2005) presented above. The intuition is as follows; if the feedback metric is integer values from 1 to 7 inclusive, then efficiency is at least as high when agents identify a `tipping point' (formally; a binary partition) above which feedback is homogenously good and below which it is homogenously bad, compared to a more complex conditional strategy that differentiates (for example) between a feedback score of 6 and 7 (clearly, this assumes that this is not the location of the binary partition). Given that the seller has a binary choice a reputation mechanism with more than a single partition can add no value in terms of information transmission since two messages are sufficient to communicate the entire possible action space of the seller.

2.4 Results

2.4.1 Descriptive Statistics and Results Overview -

We begin by presenting descriptive statistics on bidding behaviour and the seller decisions for each treatment. Figure 2.1 shows the average bid as a proportion of valuation per period for each of the three treatments pooled across all three of the sellers' goods, along with the 95% confidence interval. As can be seen there is a notable fall in bids in both the binary and non-binary treatment across the 30 periods under consideration. It is also worth noting that there is no such decline in the control treatment - bids remain high throughout. The average bid across all periods is 6.28 ECU in the control treatment, 5.00 ECU in the binary treatment and 4.02 ECU in the non-binary treatment.[12] Across all three treatments, the average bid is positive in all periods. The proportion of all bids equal to zero, the prediction of the self-interested null hypothesis (H0), is 11.03% in the control treatment, 20.21% in the binary treatment and 24.97% in the non-binary treatment.[13]

[12] The average valuation is essentially constant across treatments (7.48, 7.48 and 7.53 respectively across the control, binary and non-binary treatments.

[13] 278/2520, 473/2340 and 517/2070 in the control, binary and non-binary treatments respectively.

Figure 2.1: Average bid as a proportion of value across all thirty periods by treatment. 95% confidence intervals are also shown.

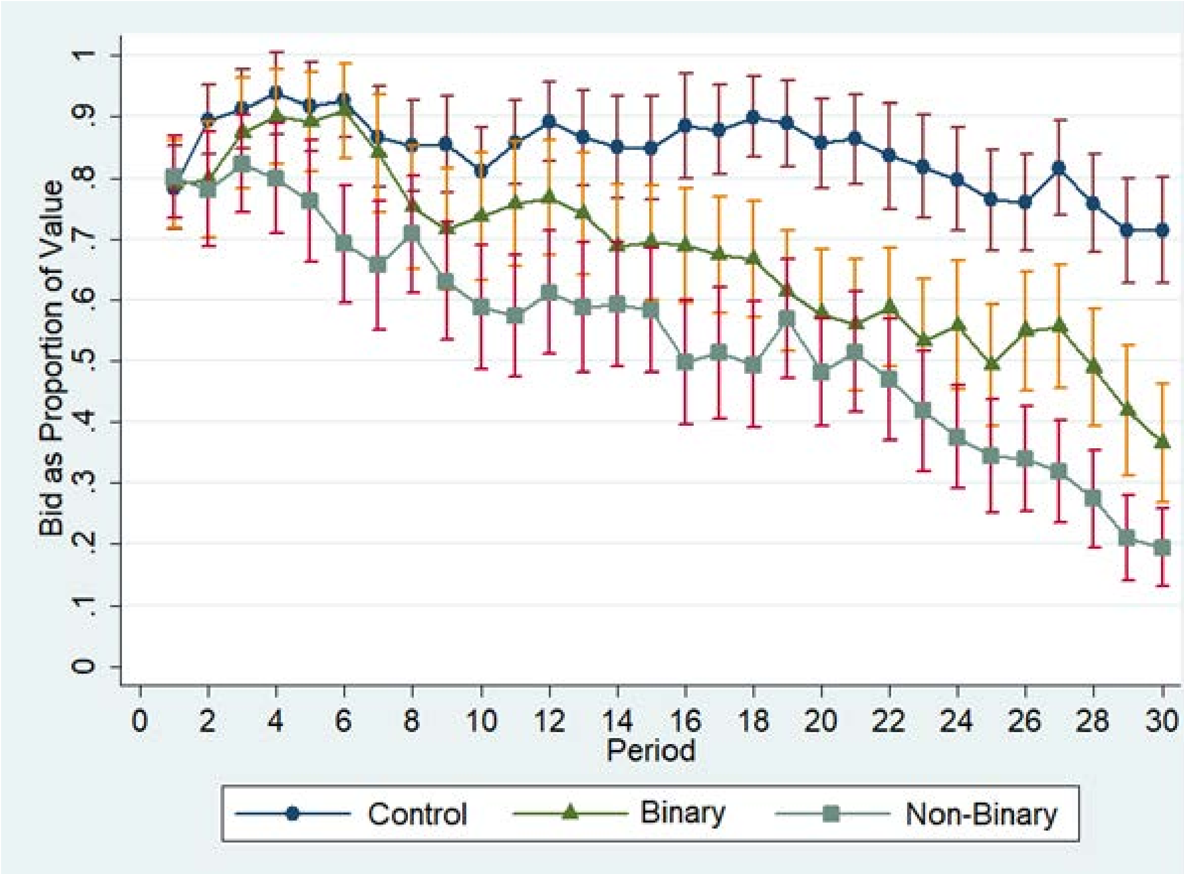
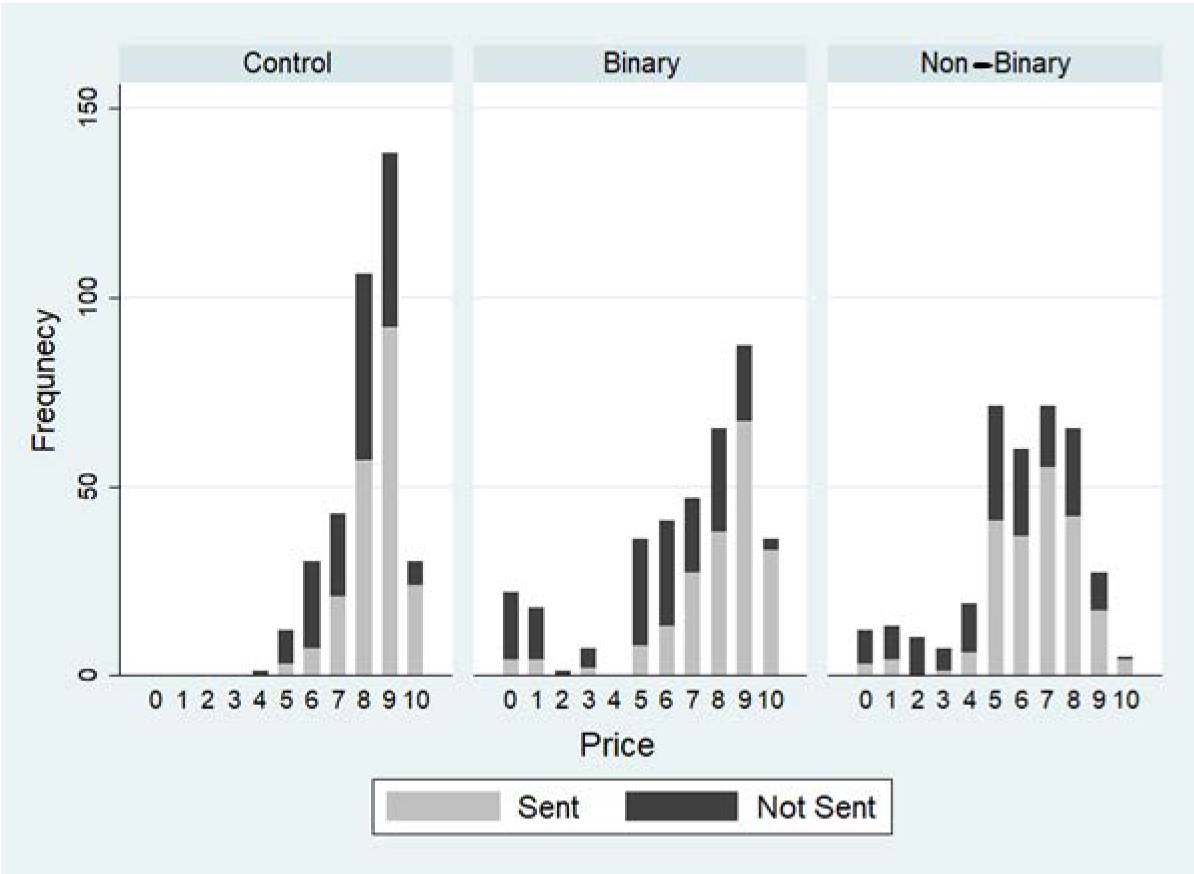


Figure 2.2 shows firstly the distribution of (integer rounded) prices across all three treatments and the frequency with which the good was sent, conditional on each price interval. The most noticeable aspect is the differing distribution of prices, which is commensurate with the analysis on bidding behaviour presented above. As can be seen, there is a clear pattern that the good is sent more frequently as the price increases, across all three treatments. The average propensity for the good to be sent across all periods and goods is 0.57 in the control treatment, 0.54 in the binary treatment and 0.58 in the non-binary treatment. In all three treatments, the average propensity for the good to be sent was positive in all periods. On this basis (Figures 2.1 and 2.2) the null hypothesis (H0) that bidders would not bid having anticipated that sellers would not send the good is overwhelmingly rejected for all treatments.

We now analyse our alternative hypotheses. Average bidder profit per period is *negative* in all three treatments (-0.45 ECU per period in the control; -0.35 and -0.20 ECU in the binary and non-binary treatments, respectively[14]), such that on average bidders end the experiment with less than their initial endowment. This is indicative of the moral hazard present in the market. Nonetheless, winning bidders earn significantly higher profits (i.e. lower losses) in the non-binary treatment compared to the control (Mann-Whitney Rank Sum: p=0.0833).[15] There is no difference in bidder profit between the binary and control treatment, however (Mann-Whitney Rank Sum: p=0.1489). The non-binary treatment also leads to significantly higher bidder profit compared to the binary treatment (Mann-Whitney Rank Sum: p=0.0833).

Figure 2.2: Frequency of transactions in which the seller sent the good according to the distribution of prices across each treatment.



Note: the price has been rounded to the nearest integer value.

Average seller profit was, of course, positive in all treatments (6.97 ECU per period in the control; 5.79 and 4.86 ECU in the binary and non-binary treatments, respectively) since sellers do not face any moral

[14] This corresponds to -3.15, -2.27 and -1.17 ECU on average per *winning* bidder across the three treatments.
[15] In all cases, N=8 (i.e. there are four independent observations (markets) per treatment). Unless otherwise stated, all tests are two-tailed.

hazard.[16] The seller profit is significantly higher in the control treatment than the binary (Mann-Whitney Rank Sum: p=0.0833) and the non-binary treatment (Mann-Whitney Rank Sum: p=0.0209). There is no difference in seller profit, however, between the binary and non-binary treatment (Mann-Whitney Rank Sum: p=0.1489). We can now state our results.

Result 1: Bidder profit is not significantly different between the control treatment and the binary treatment. Bidder profit is significantly higher in the non-binary treatment than both the control treatment and the binary treatment.

Result 2: Seller profit is significantly lower in both the binary and non-binary treatment than in the control treatment. Seller profit is not significantly different between the binary and the non-binary treatment.

2.4.2 Bidder Behaviour

Table 2.1: Distribution of Ratings Across the Binary and Non-Binary Treatments.

Binary		Non-Binary	
Rating	**Frequency**	**Rating**	**Frequency**
0	210 (57)	**1**	199 (62)
		2	11 (9)
		3	5 (5)
		4	21 (17)
		5	17 (17)
		6	26 (26)
1	150 (139)	**7**	81 (78)

Note: The distribution of ratings given by winning bidders for both the non-binary and binary treatment. The numbers in parenthesis are the distribution of ratings conditional on the seller sending the good.

We now turn to analyse actual decisions to find the underlying mechanism for these results. We begin by investigating differences in bidder behaviour across treatments. The average bid as a proportion of

[16] As discussed below, sellers can incur losses when the price is below the cost of sending the good and the seller chooses to send the good.

a subject's valuation is 0.84 (0.83 excluding the first five periods[17]) in the control, 0.67 (0.64) in the binary treatment and 0.54 (0.48) in the non-binary treatment. Only the control and the non-binary treatment are significantly different (z=1.732, p=0.0833). The proportion of zero bids is significantly higher in the non-binary than the binary treatment ($p<0.01$[18]), and significantly higher in the binary than the control treatment. Conversely, the amount of overbidding is significantly higher in the control (30.87%) than in the binary (22.56%) treatment ($p<0.01$), and in the binary than the non-binary (11.74%[19]) treatment ($p<0.01$).

We now consider the use of the reputation mechanism available in the binary and non-binary treatments. The average rating in the binary treatment was 0.416 and in the non-binary treatment was 3.13. Conditional on the good being received the average rating is 0.709 and 4.50 and conditional on the good not being received the average rating is 0.07 and 1.21, in the binary and non-binary treatments, respectively. Table 2.1 shows the distribution of ratings given in each treatment across all periods and also provides a breakdown conditional on whether the good was received or not. Figure 2.3 displays this information visually. As can be seen, the modal choice in the binary treatment was 0 and in the non-binary treatment was 1. Thus, in both cases the lowest possible feedback rating was the modal choice. The ratings given are also conditioned on whether the good was sent or not, with the majority of lowest possible ratings being given after the good not sent and the (vast) majority of highest possible ratings coming after the good was sent. There is therefore evidence that the bidders use the reputation mechanism in a manner that is broadly to be expected - higher ratings are given more frequently when the good is received and vice versa. Nonetheless, there are some instances in which, for example, a rating of 7 is given in the non-binary treatment following the good *not being received*. It is difficult to explain such behaviour. There are also instances in which, for example, a rating of 1 was given in the non-binary treatment following the good *being received*. This behaviour is particularly interesting. It may, of course, simply be a misunderstanding of the reputation mechanism as in the previous example. From a post-experimental questionnaire, however, another explanation arises. Two bidders (one each in the binary and non-binary) explicitly stated they gave low ratings to dissuade other bidders from bidding in the future.[20] Thus, while most use of the reputation mechanism is as would be expected, there are some notable instances in which that is not the case.

[17] Excluding the first five periods allows for a rating history to be built up consisting of five previous transactions and increases familiarity with the environment.
[18] Test for equality of proportions, see an appendix for the exact methodology.
[19] 778/2250, 528/2340 and 243/2070 in the control, binary and non-binary treatments, respectively.
[20] For example, a subject in the *non-binary* treatment stated "i tended to give feedback of 1 to put other buyers off" (sic).

To more directly compare rating behaviour across the binary and non-binary treatments the ratings given in the non-binary treatment were transformed on the [0,1] line.[21] The difference between the correspondingly adjusted ratings is not significant (*p*=0.1450). Furthermore, the rating in neither the binary nor the non-binary treatment exhibits a significant time trend (*p*=0.1780 and *p*=0.2196, respectively). Thus, we fail to reject the hypothesis that the bidders in the binary and non-binary treatments use the reputation mechanisms available in a similar fashion. This is consistent with the fact that the sizeable majority of ratings in the non-binary treatment are at the extreme; either 1 or 7. As mentioned above, empirical observations of existing reputation mechanisms observe a notable `J'-shaped curve (i.e. the highest and lowest possible ratings are most common. See Hu, Pavlou and Zhang (2009)). Similar to these observations, we find the vast majority of feedback in the non-binary treatment to be at the extremes of either 1 or 7 (as per Figure 2.3). Different to the existing literature, however, we witness a backwards `J'-shaped curve, with the most frequent feedback being given as 1 followed by 7 in the non-binary treatment. This is, however, consistent with the observation that trust does not pay off in this market and is therefore not totally unexpected.

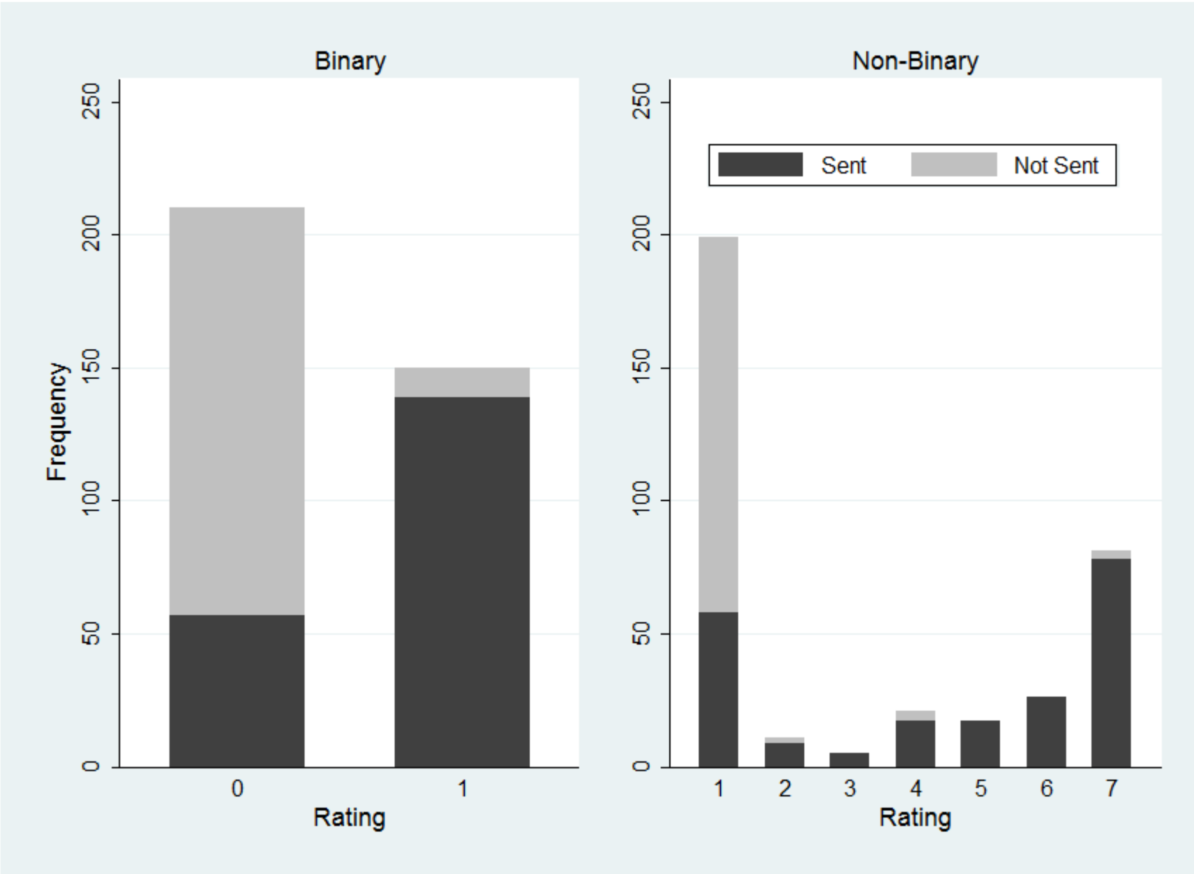
Table 2.2 presents the results of a Tobit regression[22] with the subject's bid as the dependent variable. Standard errors are clustered at the independent matching group level and presented in parenthesis. There is a significant treatment effect on bidding behaviour, as can be seen from the treatment dummy variables included in the regression. In both the binary and non-binary case the effect is negative and the effect is significantly greater in the non-binary case (F-test, p=0.065). We also include an interaction between these treatment dummies and the rating given to the particular seller in the previous period. The coefficient on the previous rating is positive and significant for both the binary and non-binary treatments, suggesting higher previous ratings are associated with higher bids, as would be expected and indeed is confirmed by the above descriptive statistics. We also include a number of additional controls for robustness. Valuation has a positive and significant effect on bidding behaviour which is not surprising. There is also positive serial correlation as can be seen by the significant coefficient on the previous bid (one period lagged dependent variable). We also include a linear time trend which is significant and negative, though of small magnitude. To control for the effect of being the winner of the particular seller's good in the previous period, we include a dummy indicating whether the bidder was the winning bidder in the previous period. This has a large, negative effect on bidding behaviour

[21] By taking the rating from 1 to 7, subtracting 1 and dividing by 6.

[22] Random Effects GLS Regression are included in an appendix as a robustness check.

that is highly significant. This is consistent with the above observation regarding negative bidder profits. Finally, to analyse the effect of the seller sending the good, we include an interaction between whether the bidder was the winning bidder and a dummy indicating whether the seller sent the good or not. The effect is significant and positive though the overall effect of being the winning bidder and receiving the good remains negative (F-test, p=0.0004). It is also worth noting that the results presented here are robust to instead running the regression `good-wise' (i.e. for each seller's good independently). There is therefore evidence of a (negative) bidder treatment effect, and furthermore there is evidence that this effect is stronger in the non-binary than the binary treatment.

Figure 2.3: The distribution of ratings given across the binary and non-binary treatment.



Note: Each bar also contains a breakdown for a particular rating depending on whether the good was sent or not.

Table 2.2: Bidding Behaviour Regression Results.

Estimation Technique	**Tobit**
Dependent Variable	**Bid**
Treatment = Binary	-0.858** (0.368)
Treatment = Non-Binary	-1.601*** (0.461)
Treatment = Binary * Previous Rating	0.893*** (0.116)
Treatment = Non-Binary * Previous Rating	0.221*** (0.051)
Value	0.681*** (0.065)
Previous Bid	0.802*** (0.052)
Previous Winning Bidder	-2.172*** (0.143)
Previous Winning Bidder * Previous Sent	1.276*** (0.232)
Period	-0.040*** (0.007)
Constant	-3.170*** (0.670)
Observations	6468
Log-Likelihood	-13, 537.9

Note: Tobit regression with the bid as the dependent variable. Regression is censored on the available bidding space. Standard errors are clustered at the level of the independent matching group (N=12) and are presented in parenthesis.

2.4.3 Seller Behaviour

In this section, we analyse in detail sellers' decisions regarding whether to send the good or not. We begin by presenting summary statistics. As can be seen in Table 2.4, there is little difference across the three treatments and indeed none of the differences are significant when using non-parametric statistics. In all treatments (and indeed, in all matching groups) there is a drop in the frequency with which the good was sent between the periods 1-15 and periods 16-30. One interesting aspect is the seller behaviour when a sale is potentially unprofitable i.e. the winning price is below the seller's cost of sending. Given the persistence of high prices in the control treatment, there are no potentially unprofitable sales. In the treatments with a reputation mechanism (and falling prices), though, it is a relatively frequent occurrence, happening in 38/360 and 37/360 transactions in the binary and non-binary treatments, respectively (i.e. approximately 10% of all auctions in the binary and non-binary treatment had a winning price below the seller's cost to send the good). That the seller still sends the good in 4/38 and 5/37 transaction, respectively, we take as evidence of a form of `pure' reputation building. When the price is above 5, the seller is effectively choosing between a high and a low (but still positive) level of profit, however when the price is below 5 the seller may be choosing between a positive and a negative level of profit. Indeed, some sellers explicitly refer to attempts to gain reputation by sending the good at a loss in a post-experimental questionnaire (available in supplementary material on request).

Table 2.3: Seller Behaviour Regression Result.

Estimation Technique	**Probit**
Dependent Variable	**Send**
Treatment = Binary	1.122*** (0.428)
Treatment = Non-Binary	2.059*** (0.337)
Treatment = Binary * Previous Rating	0.024 (0.277)
Treatment = Non-Binary * Previous Rating	-0.062 (0.056)
Price	0.341*** (0.044)
Treatment = Binary * Price	-0.116* (0.062)
Treatment = Non-Binary * Price	-0.179*** (0.065)
Cost	-0.538*** (0.065)
Previous Send	0.586* (0.326)
Period	-0.022*** (0.007)
Constant	-1.191*** (0.311)
Observations (Clusters)	1012 (12)
Log Pseudolikelihood	-476.39

Note: Probit Regression on Seller Behaviour. Standard errors are clustered at the level of the independent matching group (N=12) and presented in parenthesis.

Table 2.4: Send Frequency by Periods for each Treatment.

Treatment	**Periods 1 – 15**	**Periods 16 – 30**
Control	117/180 (65%)	87/180 (48.33%)
Binary	128/180 (71.11%)	68/180 (37.78%)
Non-Binary	122/180 (67.78%)	88/180 (48.89%)

Note: Overview of sellers' decisions to send the good or not for each treatment. The table also provides a breakdown for periods 1-15 and 16-30.

Table 2.3 presents the results of a Probit regression with standard errors clustered at the level of the independent matching group. Both treatments have a positive and significant effect impact on the likelihood of the seller sending the good. In addition, the non-binary treatment haws a significantly greater effect than the binary treatment (F-test, p=0.015). We also include the rating that the seller received in the previous period (interacted with the treatment dummies). In neither the binary nor the non-binary treatment is this effect significant - this suggests that the despite the existence of an overall treatment effect, the actual rating received does not have a significant impact on seller behaviour. To see the varying effect of the winning price across the three treatments the specification includes not only the winning price, but also interactions between the winning price and the treatment dummies. It can therefore be seen that increases in the winning price have a positive and significant effect on the probability that the seller will send the good. It should however be noted that this effect is stronger in the control treatment than both the binary and non-binary treatment, though the binary and non-binary treatments do not have different effects. This suggests that the winning price is more important in determining seller behaviour in the control than the treatments involving a reputation mechanism. Finally, we also include a number of additional controls; the cost of sending the good has a negative and significant effect, there is an overall negative time trend and the seller is more likely to send the good if they sent the good in the previous period. Our results are thus robust to the inclusion of several control variables, all of which have the expected sign. The evidence presented here therefore suggests that a (positive) seller treatment effect exists in both the binary and non-binary treatment, and further that this effect is stronger in the non-binary than the binary treatment. Interestingly, the actual rating the seller received in the previous period is not significant.

2.5 Discussion and Conclusion

We present evidence that replicates the existing results that the presence of a reputation mechanism can lead to improvements in buyer welfare when there is seller moral hazard. In both treatments with a reputation mechanism, bids are lower and the frequency with which the good was sent are higher (once price has been controlled for) than in the control treatment where no reputation feedback is provided. In all cases the market fails to function properly due to the existence of moral hazard, as on average bidders receive negative profits.

The results presented in this paper suggest that having more possible feedback ratings leads to improvements in buyer (`consumer') welfare, even when the seller's decision is binary (send or not send). It is important to note that the intuition of Dellarocas (2005) relies heavily on the fact that the seller has only two possible actions - if the seller only has two possible actions then two messages (i.e. a binary mechanism) are sufficient to create a one-to-one mapping from the action space to the message space. In other words, it is not clear from a theoretical point of view why allowing more feedback ratings is beneficial and this remains an open theoretical question posed by the results presented here. This is compounded by the fact that the actual rating received in the previous period is not significant in determining seller behaviour. Nonetheless, we find evidence that suggests more possible feedback ratings will lead to improvements in consumer welfare in a market featuring moral hazard on the seller side. Regression analysis suggests that this effect operates on both the bidders (who decrease their bids) and sellers (who increase the probability that they will send the good). One plausible explanation for this is that it is easier for bidders to see a deterioration in feedback ratings that range from 1-7 (i.e. initially high ratings (5/6/7) being replaced by low ratings (1/2/3)) than in the case where feedback is binary (i.e. initially high proportion of 1's being replaced by a high proportion of 0's) that means it takes bidders in the binary treatment longer to figure out the auctions are, on average, unprofitable. This is potentially surprising given that in the environment under consideration any reputation effect should operate through constraining seller behaviour and not through bidder learning (which would indicate adverse selection not formally present in the experiment presented in this paper).

In reality, the seller's decision has multiple dimensions (stated vs delivered quality, `customer service', Postage and Packaging quality/speed). This is the inspiration behind the `Detailed Seller Rating' system employed on Ebay (given the increasing prevalence of such systems there are numerous other examples), in which the seller is rated independently on multiple aspects of the sales process. Nonetheless, the results presented here suggest the number of possible ratings that can be given can have significant effects on both buyer and seller behaviour in a rich marketplace consisting of multiple buyers and sellers. As such, when implementing such reputation mechanism systems, careful consideration should be given to one of the seemingly most simple design decisions - the number of possible feedback ratings that users can choose from.

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Appendix 2.A: Experimental Instructions

Economic Decision Making Experiment - Instructions

Welcome to the Birmingham Experimental Economics Laboratory (BEEL). This is an experiment in decision making. The University of Birmingham has provided the funding for this research. It is very important that you read the instructions carefully.

It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your following of these rules.

We will firstly go over the instructions together. You will then have a chance to ask clarifying questions. Each of you will then answer a few questions to make sure everybody understands.

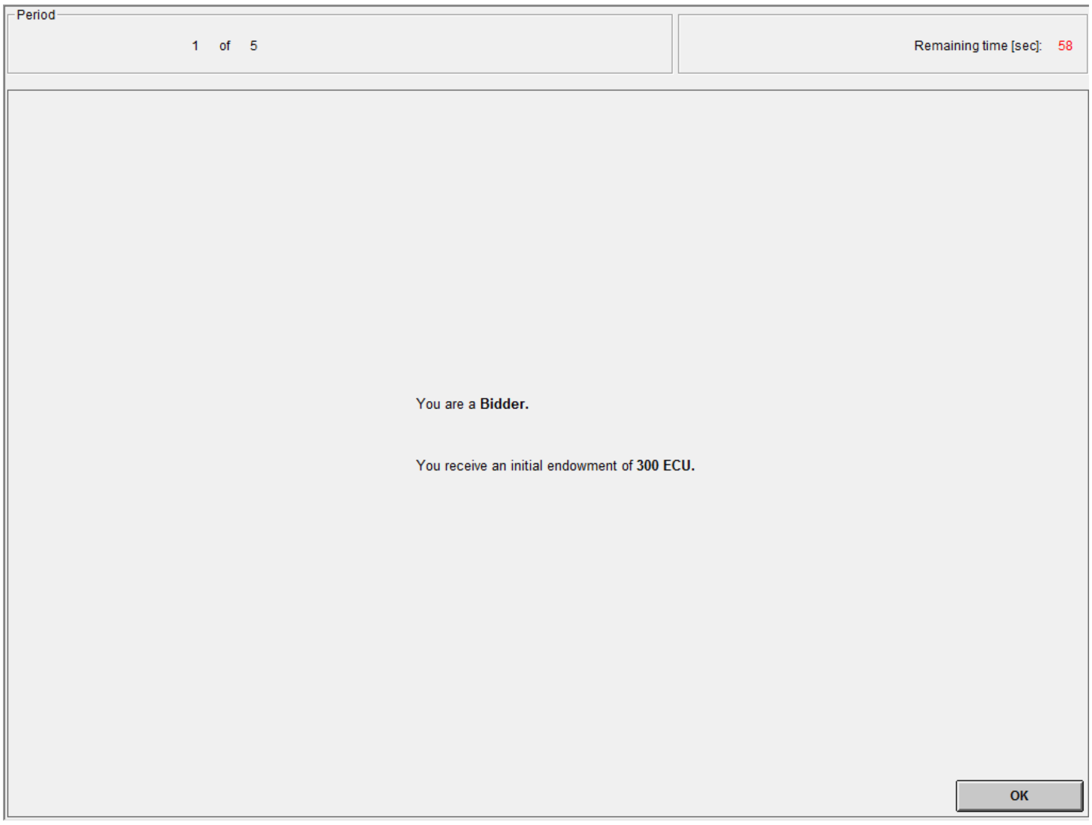
You will be given an initial endowment of 300 ECU. You will be paid according to the number of ECUs you have at the end of the experiment. You will be paid according to following exchange rate;

$$\mathbf{1\ ECU = 3p}$$

In addition to this initial endowment, you have already earned a **£2.50** show up fee. We would like to stress that any decision you make is completely anonymous. You will be paid privately in cash at the end of experiment.

Introduction

During this experiment, you will be a member of a group. A group consists of 10 subjects. In addition to being a member of a group, you will also be assigned a role. Your role will either be *bidder* or *seller.* Each group will contain exactly 7 ***bidders*** and 3 ***sellers.*** You will remain in the same role *and* group throughout the experiment. Once the experiment begins, you will be informed of your role. A screenshot showing this can be seen below.



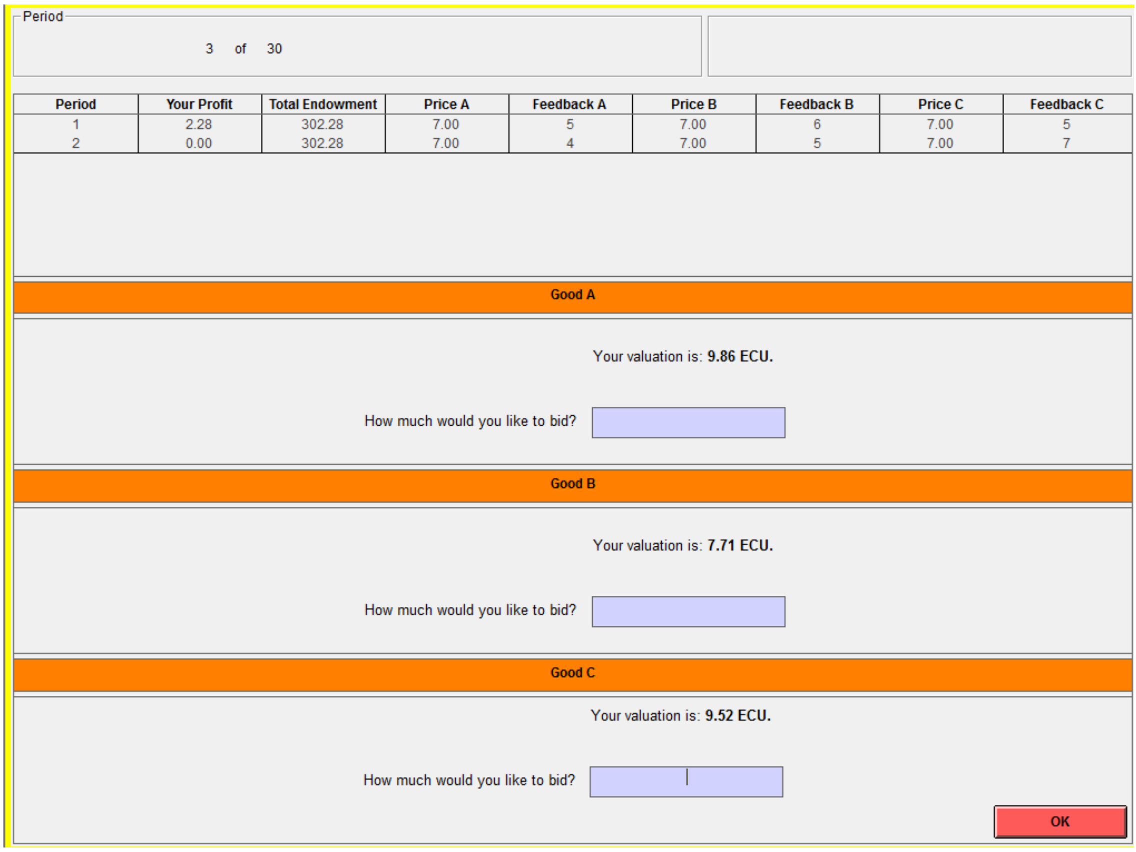
Once everyone has been informed of their role, there will be 30 periods. A round consists of three stages; the bidding stage, the seller stage and the feedback stage. We shall now go through each stage in detail.

The Bidding Stage

During the bidding stage, each *bidder* will have the opportunity to place a bid on a fictional good belonging to each of the three *sellers*. The *sellers* are randomly denoted as A, B and C. Each *bidder* will have an independent valuation for each of the three goods, which is between 5 and 10. *Bidders* must then decide how which they wish to bid for each of the three items. *Bidders* input their choices on the following screen;

The winning bidder is the *bidder* who places the highest bid. If there is a tie (two equal highest bids), then the winner is determined randomly. The price the winning *bidder* must pay is equal to the *second highest bid.* The winning bidder then pays the winning price.

In all rounds *after* the first round, you will be to see a history of what has happened in previous rounds, before making a decision to bid. You will be shown this information as follows;



The history box contains the following information;

Period - The current period

Your profit - The profit you received *in that period*

Total profit - Total profit from all past periods *including the 300 ECU endowment*

Price A - The winning *price* for good A

Feedback A - The feedback *given to seller A* by the winning bidder

Price B - The winning *price* for good B

Feedback B - The feedback *given to seller B* by the winning bidder

Price C - The winning *price* for good C

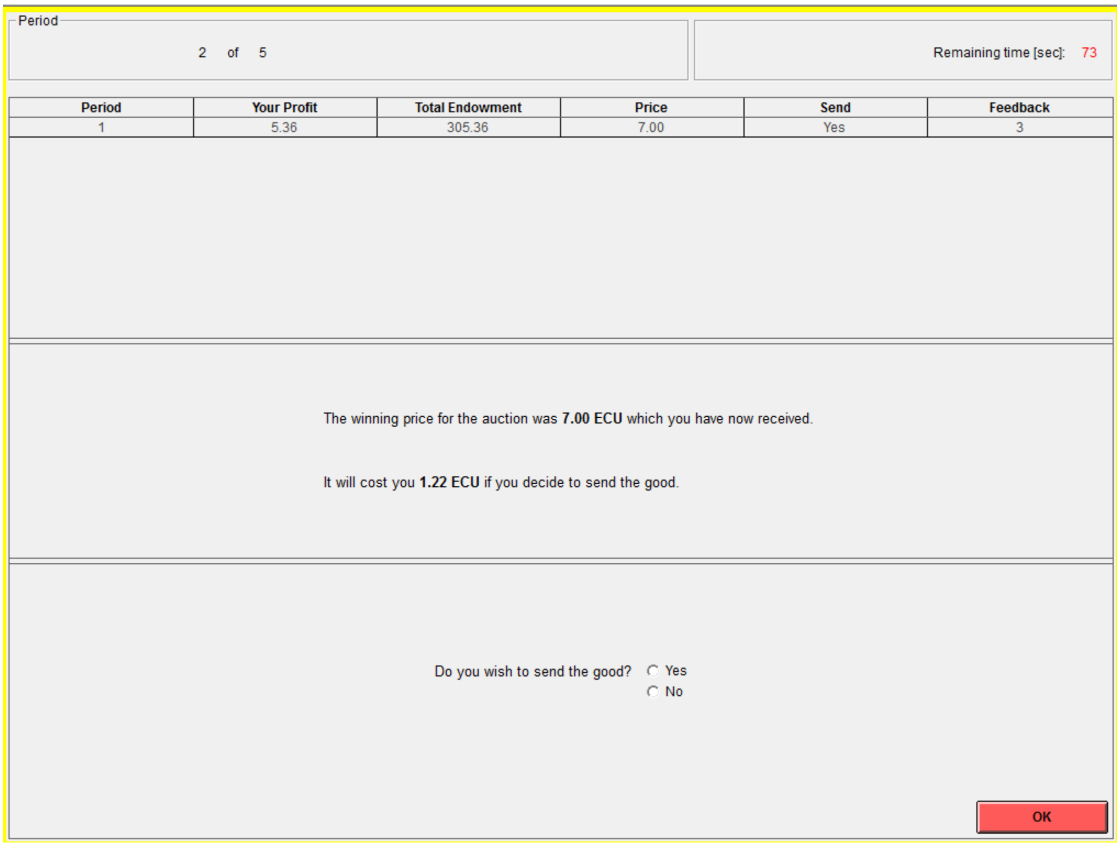
Feedback C - The feedback *given to seller C* by the winning bidder

The feedback will be thoroughly explained in the feedback stage. This concludes the bidding stage.

The Seller Stage

Once the bidding stage is over, the seller stage begins. Each of the *sellers* will be given an independent cost, between 0 and 5. The *seller* will also be informed of the winning price for their respective good. Therefore, *seller A* is told the winning price for good A, and so on. Each seller must then decide whether they wish to 'send' the good, or whether they wish to 'not send' the good. If the *seller* chooses to 'send' the good, they will be charged their cost for doing so. Not sending the good costs nothing.

After the first period, *sellers* will also have a history box available to them. Each *seller* will make their decision on a screen that looks like this;



The history box for the *seller* contains the following information;

Period - The current period

Your profit - The profit you received *in that period*

Total profit - Total profit from all past periods *including the 300 ECU endowment*

Price - The winning *price* for the *seller's* good

Send - Displays whether the *seller* sent the good in previous periods

Feedback - Shows the feedback given to the *seller* in previous periods

Profit

In between the seller stage and the feedback stage profits for the round are calculated. The profit is calculated in the following way.

Bidder Profit

A *bidder* can only make profit if they were the highest bidder for a specific auction. If a *bidder* is not the highest bidder in any auction, then they receive profit of zero for this round. If the *bidder* was the highest bidder for any of the three items, the profit per item is as follows;

$$Value - Price \quad \text{if the seller sent the good}$$

$$-Price \quad \text{if the seller did not send the good}$$

Note that the profit for the highest *bidder* ***negative*** if the seller does not send the good. The winning bid is still paid but the good is not received. Total profit is then given as the summation of any profit earned in each of the three auctions.

Bankruptcy

Since it is possible for a *bidder* to receive negative profit, a *bidder* may end up with no ECU left. In this case, the *bidder* is declared bankrupt and will take no further part in experiment and will only receive the show up fee of X.

Seller Profit

A *seller* can only make a profit if there are any positive bids. If no *bidder* decides to place a bid, then the *seller* receives zero profit for the round. If there are positive bids placed, then the profit for the *seller* is;

$$Price - Cost \quad \text{if the seller chose to send the good}$$

$$Price \quad \text{if the seller chose not to send the good}$$

The Feedback Stage

The feedback stage is divided into two sub-stages; *bidder* feedback and *seller* feedback.

Bidder Feedback

Once all of the *sellers* have made their decision, *bidders* are shown the outcome of the auction.

For each of the three goods, *bidders* are informed of whether they were the highest bidder. All *bidders* are then shown the winning price in the particular auction. *Bidders* are reminded of their

Feedback is given along a 7-point scale. That is, a *bidder* can leave feedback of 1, 2, 3, 4, 5, 6 or 7.

1 is the worst possible feedback rating, indicating the worst transaction possible.

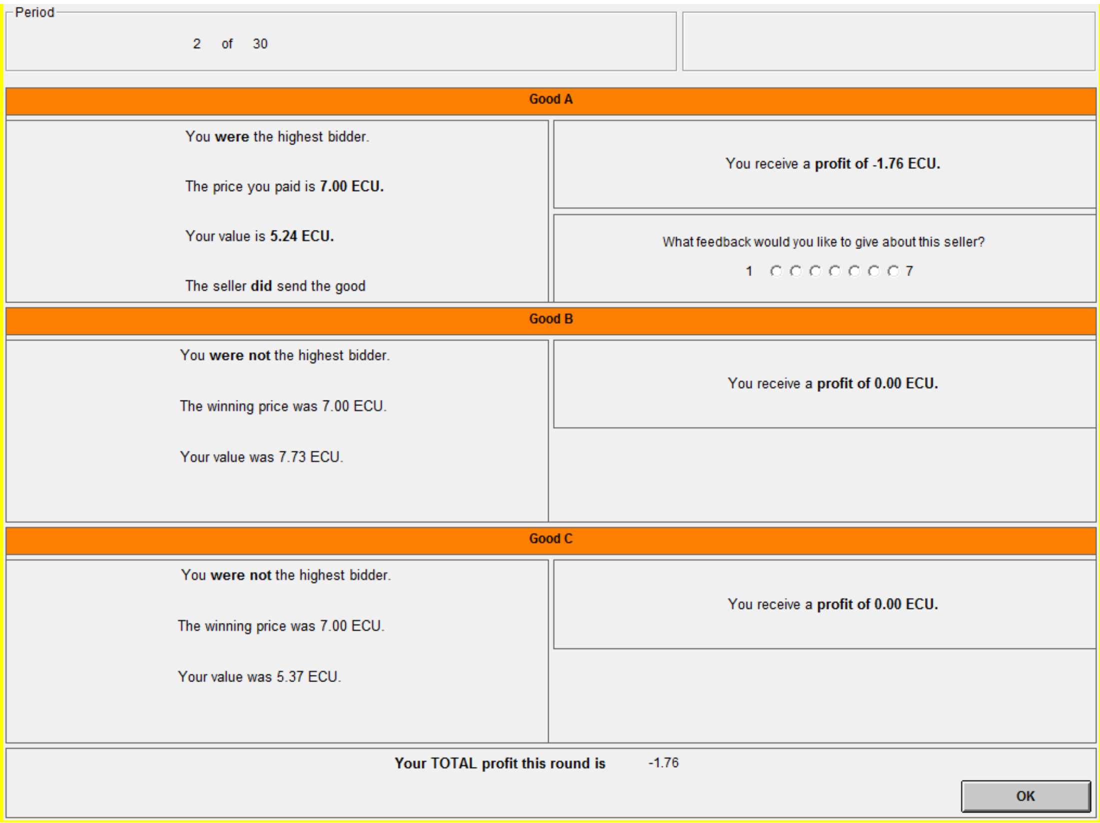
7 is the best possible feedback rating, indicating the best transaction possible.

In future periods, **all the feedback** left to a particular *seller* by all the previous winning *bidders* will be visible to *bidders* before bids are placed.

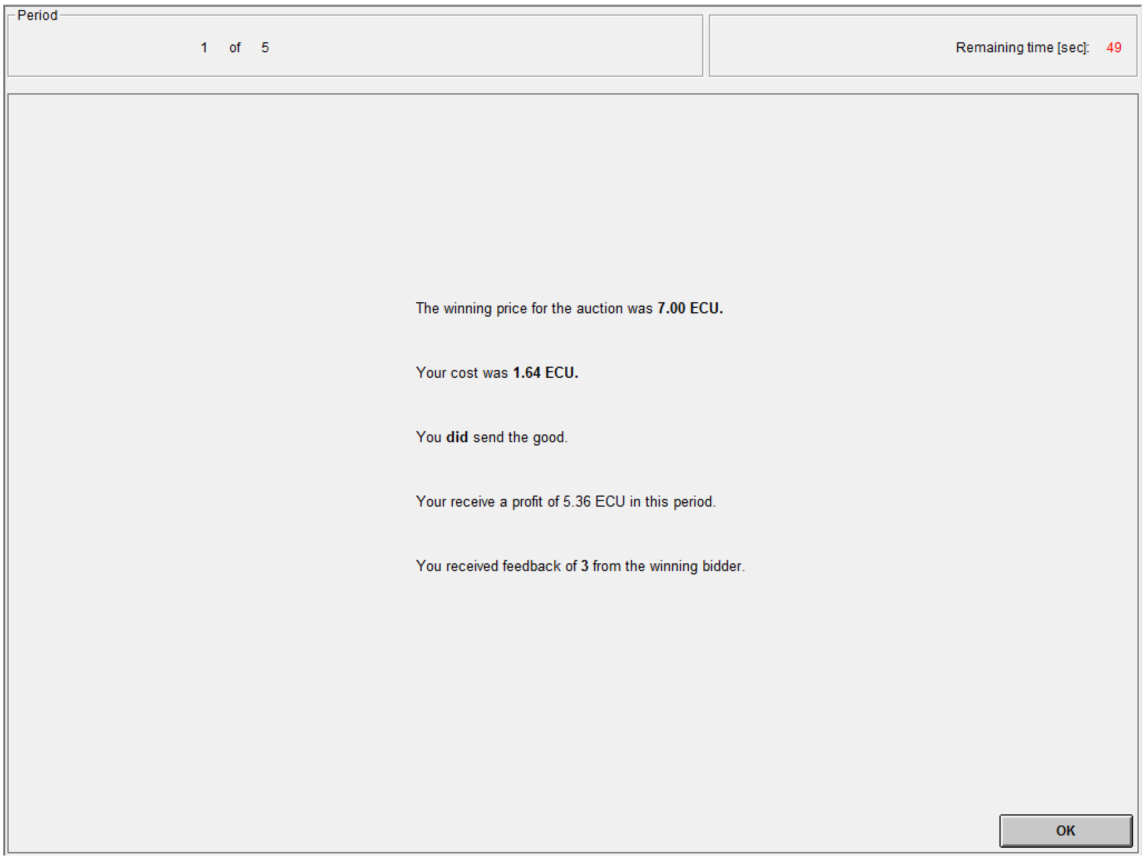
Total profit for the period, across all three auctions, is displayed at the bottom.

Seller Feedback

Once all decisions regarding feedback have been made, *sellers* are shown the outcome for this period.



A *seller* is given a breakdown of their profit for the period. This includes the winning price for the good, the cost of sending and whether the *seller* sent the good or not. The *seller* profit is then calculated, as described above. The *seller* is then informed of the feedback rating given to them in that period. The screen will look as follows;



This concludes a period of the experiment. The experiment will end once the final period is completed. Does anybody have any questions? Please raise your hand and an experimenter will come and answer your question. Please do not ask questions out loud.

If nobody has any further questions, you will be asked to fill out a series of questions to ensure everybody understands the instructions. You will then be asked to make your decisions for the experiment. You will then be asked to fill out a short questionnaire; you do not have to answer any of these questions if you do not wish.

<u>Control Questions</u>

1. Suppose you have been assigned the role of *bidder.* Will this change throughout the experiment? ....

2. Will you change groups throughout the experiment? ....

Imagine the following scenario. *Bidder 1* is the highest bidder. *Bidder 2* did not win the auction. The price is 7. *Bidder 1* has a value of 5.96 and *bidder 2* has a value of 6.54. The *Seller* did not send the good. The *Seller* cost was 3.14.

3. What is the profit for *Bidder 1?* ....

4. What is the profit for *Bidder 2*?...............................

5. What is the profit for the *Seller?*.............................

Now imagine the following scenario. *Bidder 1* is the highest bidder. *Bidder 2* did not win the auction. The price is 7. *Bidder 1* has a value of 5.96 and *bidder 2* has a value of 6.54. The *Seller* did send the good. The *Seller* cost was 3.14.

6. What is the profit for *Bidder 1?* ....

7. What is the profit for *Bidder 2*?...............................

8. What is the profit for the *Seller?*.............................

2.B: Appendix: Tests for the Equality of Proportions

We test the equality of proportions using the following test;

$$Z = \frac{p_1 - p_2}{S_{pc}}$$

where p_i is the proportion of zero choices in subsample i, $S_{pc} = \{(p_c(1-p_c))\left[\frac{1}{N_1} + \frac{1}{N_2}\right]\}^{0.5}$ and $p_c = \frac{p_1 N_1 + p_2 N_2}{N_1 + N_2}$, where N_i is the size of subsample i. p_c is an estimate of the population proportion under the assumed null hypothesis of the equality of proportions. S_{pc} is then an estimate of the standard error of $p_1 - p_2$.

Proportion of Zero Bids

Control vs Non-Binary

Denote the control as $i = 1$ and the non-binary as $i = 2$. Then $p_1 = 0.1103$ and $p_2 = 0.2497$. Additionally, $p_c = \frac{278+517}{4590} = 0.1732$ and therefore;

$$S_{pc} = \left\{(0.1732 * (0.8268)) * \left[\frac{1}{2520} + \frac{1}{2070}\right]\right\}^{0.5} = 0.0112$$

$$Z = \frac{0.1103 - 0.2497}{0.0112} = -12.45$$

Control vs Binary

Denote the control as $i = 1$ and the binary as $i = 2$. Then $p_1 = 0.1103$ and $p_2 = 0.2021$. Additionally, $p_c = \frac{278+473}{4860} = 0.1545$ and therefore;

$$S_{pc} = \left\{(0.1545 * (0.8455)) * \left[\frac{1}{2520} + \frac{1}{2340}\right]\right\}^{0.5} = 0.0103$$

$$Z = \frac{0.1103 - 0.2021}{0.0103} = -8.91$$

Non-Binary vs Binary

Denote the non-binary as $i = 1$ and the binary as $i = 2$. Then $p_1 = 0.2497$ and $p_2 = 0.2021$. Additionally, $p_c = \frac{517+473}{4410} = 0.2245$ and therefore;

$$S_{pc} = \left\{\left(0.2245 * (0.7755)\right) * \left[\frac{1}{2070} + \frac{1}{2340}\right]\right\}^{0.5} = 0.0126$$

$$Z = \frac{0.2497 - 0.2021}{0.0126} = 3.78$$

Proportion of Overbidding

Define overbidding (Bid > Value) as a positive outcome.

Control vs Non-Binary

Denote the control as $i = 1$ and the non-binary as $i = 2$. Then $p_1 = 0.3087$ and $p_2 = 0.1174$. Additionally, $p_c = \frac{778+243}{4590} = 0.2224$ and therefore;

$$S_{pc} = \left\{\left(0.2224 * (0.7776)\right) * \left[\frac{1}{2520} + \frac{1}{2070}\right]\right\}^{0.5} = 0.0123$$

$$Z = \frac{0.3087 - 0.1174}{0.0123} = 15.55$$

Control vs Binary

Denote the control as $i = 1$ and the binary as $i = 2$. Then $p_1 = 0.3087$ and $p_2 = 0.2256$. Additionally, $p_c = \frac{778+528}{4860} = 0.2687$ and therefore;

$$S_{pc} = \left\{\left(0.2687 * (0.7313)\right) * \left[\frac{1}{2520} + \frac{1}{2340}\right]\right\}^{0.5} = 0.0127$$

$$Z = \frac{0.3087 - 0.2256}{0.0127} = 6.54$$

Non-Binary vs Binary

Denote the control as $i = 1$ and the binary as $i = 2$. Then $p_1 = 0.1174$ and $p_2 = 0.2256$. Additionally, $p_c = \frac{243+528}{4410} = 0.1748$ and therefore;

$$S_{pc} = \left\{\left(0.1748 * (0.8252)\right) * \left[\frac{1}{2070} + \frac{1}{2340}\right]\right\}^{0.5} = 0.0115$$

$$Z = \frac{0.1174 - 0.2256}{0.0115} = -9.41$$

2.C: Appendix: Additional Regression Analysis

Table C.1: Bidding Behaviour Regression Results.

Estimation Technique	**Random Effects GLS**
Dependent Variable	**Bid**
Treatment = Binary	-1.001*** (0.073)
Treatment = Non-Binary	-0.538*** (0.063)
Treatment = Binary * Previous Rating	0.162*** (0.037)
Treatment = Non-Binary * Previous Rating	0.397* (0.241)
Value	0.602*** (0.028)
Previous Bid	0.647*** (0.017)
Previous Winning Bidder	-1.847*** (0.183)
Previous Sent	0.626*** (0.083)
Previous Winning Bidder * Previous Sent	0.697*** (0.239)
Constant	-2.451*** (0.184)
Observations	6699

Note: GLS regression clustering on the period (29 clusters). Robust standard errors are presented in parentheses.

Table C.2: Bidding Behaviour Regression Results.

Estimation Technique	**Random Effects GLS**
Dependent Variable	**Bid**
Treatment = Binary	-0.978*** (0.265)
Treatment = Non-Binary	-0.525* (0.279)
Treatment = Binary * Previous Rating	0.165*** (0.057)
Treatment = Non-Binary * Previous Rating	0.411* (0.211)
Value	0.605*** (0.062)
Previous Bid	0.658*** (0.026)
Previous Winning Bidder	-1.891*** (0.116)
Previous Sent	0.679*** (0.102)
Previous Winning Bidder * Previous Sent	0.695*** (0.182)
Constant	-2.560*** (0.452)
Observations	6699

Note: GLS regression clustering on the MG (12 clusters). Robust standard errors are presented in parentheses.

Chapter 3

Sequential Equilibrium and Moral Hazard Auctions

Abstract

We present the results of an experiment that implements the sequential equilibrium framework, where the stage game is a moral hazard auction in which the seller can choose not to send the good. We introduce a commitment type seller who always sends the good (overcoming the moral hazard) with some prior probability. We find that as the prior probability on this commitment type increases, bidders on average place higher bids, as predicted by a theoretical model we develop. We also find limited evidence that sellers respond to the presence of the commitment type by increasing the probability with which they send the good in early periods, consistent with the canonical sequential equilibrium literature in which agents attempt to mimic existing commitment types. As with much of the literature of sequential equilibrium, there is significant quantitative deviation from equilibrium. Nonetheless, we find patterns of behaviour that are broadly consistent with the qualitative features of such a model of reputation formation and conclude that sequential equilibrium (adverse selection) reputation effects remain an important consideration in multi-player market-based games such as auctions.

JEL CODES: C73, C92, D02, D44, D82

Keywords: Reputation, Auction, Markets, Repeated Games, Sequential Equilibrium, Adverse Selection

3.1 Introduction and Objectives

Reputation is a pervasive phenomenon of interest to researchers in many varied fields including, but not limited to, computer science, psychology, marketing and economics. A wide range of agents seek to gain reputation which is supposed to have long term benefits. From skilled tradesmen who wish to build a reputation for not being `cowboys', to expert consultants who wish to form a reputation for giving good advice, many individuals seek to acquire reputation for being or doing certain things. Reputation is also important when using peer-to-peer internet markets, such as eBay. In the latter case, reputation enables the proper functioning of a market that would otherwise break down in its absence, due to the presence of moral hazard.

The present paper seeks to extend the existing literature on the sequential equilibrium (hereafter SE; the model is outlined in detail in Section 3.2) model of reputation formation by introducing group dynamics in the form of an auction market with seller side moral hazard. The stage game that is repeated in the sequential equilibrium model is typically a two-player two-stage game. The current paper seeks to extend the literature by incorporating the Bayesian uncertainty of the SE model to multi-player market-based games. In particular, we implement a commitment type seller that will always send the good in a moral hazard auction (i.e. if matched against the commitment type seller, there is no moral hazard present). It is important to understand how the dynamics of reputation formation witnessed in canonical SE games extend to market-based institutions such as auctions.

The results of an experiment are presented that implements a moral hazard auction and varies the existence of a commitment type seller that always sends the good. Bidders are uncertain as to the type of the seller (whether they are committed or free-to-choose) they are currently matched with. We present results that suggest both bidders and sellers respond to the reputational considerations present, even despite the competition amongst bidders. Bidders, on average, bid higher amounts when the prior probability of the commitment type increases. Sellers choose to send the good with higher probabilities on average when the commitment type can theoretically exist. The model presented here can thus be seen as either extending the canonical SE framework (in a trust game setting) to incorporate group competition amongst the trustors (first-movers) or as extending an auction featuring seller side moral hazard to the SE setting. The results presented here extend the existing literature by suggesting that reputational considerations do continue to play a role in market-based games that feature Bayesian uncertainty about the distribution of types of agents.

The rest of the paper is structured as follows. This section concludes with a literature review providing motivation for the experiment presented. A theory section outlines the theoretical (and experimental) construct used throughout the paper, highlighting equilibrium predictions and hypotheses. We next outline in detail the experimental design and procedure before proceeding to present the results. Finally, a concluding section features an overview and discussion of the results presented throughout the paper.

3.2 Literature and Motivation

Within economics a primary paradigm for understanding reputation is the sequential equilibrium model of strategic reputation formation due to Kreps and Wilson (hereafter KW, 1982) and Milgrom and Roberts (MR, 1982). The idea is that a phenomenon of strategic reputation formation may emerge in a theoretical environment of repeated interaction[23], where otherwise it would not, if agents entertain the possibility of different behavioural types and are uncertain as to the type of the agent they are matched with. Strategic reputation formation may then see a rational agent mimicking a particular behavioural type to gain a reputation, *for being of that type*, that can be exploited for future benefit sufficient to offset the initial (opportunity) costs of acquiring reputation. The SE reputation model therefore explicitly posits the strategic reputation formation process as a response to the prevailing behavioural heterogeneities of a given environment. Many of these behavioural types can be rationalised by and correspond to either pro- or anti-social preferences. The SE model can thus be seen as an attempt to formalise the prevailing behavioural heterogeneities of a given environment.

The sequential equilibrium (SE) model of reputation formation stems from the seminal papers of KW and MR, in response to the Chain-Store paradox presented by Selten (1978). The Chain-Store paradox involves the impossibility of intuitive reputation building equilibria in finite multi-period, two-player stage games due to the backwards induction logic. SE solves this paradox by doing two things; introducing a behavioural (or commitment) type and relaxing the assumption of common knowledge. The behavioural type is committed to the action that allows the agent to acquire reputation - in practice this is, therefore, often the Stackelberg action[24]. The realisation of an agent's type is private information. In this case, each agent engages in a Bayesian updating process as to the type of other agents whenever new information is received. Thus, rational agents may have an incentive to mimic the induced behavioural type to raise others' beliefs that they are of the behavioural type. These

[23] Not necessarily between the same agents, if a history of past play is available.
[24] The action to which the agent would themselves commit, were they able to do so credibly.

beliefs can then be exploited as the final periods approach and the strategic value of acquiring and/or maintaining reputation falls. SE provides a quantifiably testable theory of the formation of reputation by a long-run agent (the monopolist) in repeated strategic interaction under uncertainty. It is also a solution concept that can be extended beyond the Chain-Store paradox to situations involving repeated interaction, past reporting and uncertainty over behavioural types. Experimental evidence seeks to test directly whether behaviour is consistent with a Bayesian updating process given the initial parameterisation of the model and the history of play up to the current round.

The model has been extended to many varied theoretical settings such as a repeated prisoner's dilemma (Kreps, Milgrom, Roberts and Wilson 1982), a bargaining model (Abreu and Gul (2000)), cheap talk (Brandts and Figueras (2003)) and the trust game (Anderhub, Engelmann and Güth (hereafter AEG) 2002). In addition, many experiments have been conducted seeking to test the validity of the quantifiable predictions made by the SE model. The first such experiments were by Camerer and Weigelt (1988), Neral and Ochs (1992), Andreoni and Miller (1993) and Jung, Kagel and Levine (1994). Whilst patterns broadly consistent with reputation formation are often found, there is still significant deviation from the prediction of SE including but certainly not limited to a lack of Bayesian updating. For example, the experimental findings for Camerer and Weigelt (1988) are qualitatively consistent with the SE prediction, though there is significant under-reneging. Pooled data on lending decisions are also broadly consistent with the predictions of SE. Nonetheless, the authors still report significant under reneging and conclude that an appeal to homemade priors[25] can only partially account for deviation from the predictions of SE.

More recently, Brandts and Figueras (2003) show that SE has greater predictive power when the equilibrium is in pure as opposed to mixed strategies. This is fairly intuitive since in previous experiments, the SE often involved multiple periods of monotonically increasing (or decreasing, game dependent) mixed strategies which can be cognitively difficult to employ. Embrey, Frechette and Lehrer (2011) implement the Abreu and Gul (2000) SE extension of the Rubinstein bargaining model. They find significant evidence for the mimicking of obstinate (or stubborn) bargainers. Interestingly, the authors find the emergence of a `complementary' type who immediately accommodates the obstinate bargainer's demand[26] - something SE is simply unable to account for. As with the other

[25] This refers to pre-existing beliefs the subjects may hold about the proportion of subjects who will behave in a particular way, independent of the implementation of commitment types.

[26] In other words, when splitting a pie of size 30, a sizeable proportion of people make an initial demand of only 10 when a commitment type who will not accept less than 20 exists with some probability. Agents should not concede to give the other person 20 until they are sufficiently convinced they are facing an obstinate bargainer and not just a person copying one.

experiments described, there are nonetheless significant deviations from the SE prediction. Grosskopf and Sarin (2010) incorporate `bad' reputation models such as Ely and Valimaki (2003) by introducing uncertainty about a state of nature, known only to the agent seeking reputation, that determines the optimal course of action. They also vary whether reputation can be acquired or not. They find that reputation need not be as beneficial nor as harmful as previously thought. AEG extend the literature further by explicitly allowing the use of mixed strategy play which SE frequently predicts. Nonetheless, they still find significant deviation from the SE prediction in a repeated trust game setting.

The two most closely related papers are AEG and Bolton, Greiner and Ockenfels (2013, hereafter BGO). AEG implement a repeated trust game with incomplete information (regarding the type of the second mover). The authors vary the sequential equilibrium prediction by varying the length of each repeated game. The key differences between AEG and the experiment presented here are that the auction introduces competition amongst bidders (first-movers; analogously `trustees') and that bidders have a continuous and unrestricted action space[27] whereas AEG implement a binary trust game such that the second mover has only two options; to be `trustworthy' or not. Conversely, BGO implement a repeated auction market with seller moral hazard (a similar construct can be seen in Hogg, Chen and Wozny (2004) and Bolton, Katok and Ockenfels (2004)). They vary the type of available feedback and analyse the efficacy of the various feedback mechanisms in overcoming the moral hazard. The key differences between the environment in BGO and the one presented here is that in BGO the seller chooses a quality scalar whereas we have a binary choice for the seller and that BGO does not include any commitment types and thus (formalised) behavioural uncertainty.

3.3 Theory and Hypotheses

3.3.1 The Model: A Commitment Moral Hazard Auction

1. Nature chooses a type for the seller denoted s. With probability μ_0 the seller is a commitment type and with the complementary probability $1 - \mu_0$ the seller is a rational (free to choose) type. This is private information belonging to the seller and is not known by the bidders denoted $i = \{1, \dots, N\}$.
2. Bidders receive valuations $v_{i,t} \sim U[\underline{V}, \overline{V}]$ and the seller receives a cost $c_{s,t} \sim U[0, \underline{V}]$.
3. Bidders place simultaneous sealed bids $b_{i,t} \in [0, \infty]$.

[27] Bidders are prevented from bidding above the upper support on the value (10 ECU).

4. The winner is the bidder such that $b_{i,t} > b_{j,t} \, \forall \, i \, \neq$. If some bidders tie for the highest bid, then each is selected with equal probability to the winner. Denote the winning bidder i as $W_{i,t} = 1$ and all other bidders $i \neq j$ as $W_{i,t} = 0$.
5. The price is determined as $P_t = \max_{W_{i,t}=0} b_{i,t}$ (i.e., the second highest bid) which the winning bidder pays.
6. The seller then decides whether to send the good (denoted $D_{s,t} = 1$) or not ($D_{s,t} = 0$). γ denotes a mixed strategy and refers to the probability of sending the good. Sending the good incurs cost $c_{s,t}$ and not sending the good incurs cost 0.
7. Bidder profit is defined as $u_{i,t} = W_{i,t}(D_{s,t}v_{i,t} - P_t)$ and seller profit is defined as $u_{s,t} = P_t - D_{s,t}c_{s,t}$.
8. Stages 2-7 are repeated for $T = \{1, \dots, T\}$ periods.

It is interesting at this point to analyse the limiting cases of the prior probability. When the prior probability μ_0 is zero, the environment is a pure moral hazard auction similar to BGO. When the prior probability is 1, the environment described above collapses to a repeated canonical second price auction. For intermediate priors, this environment represents an auction analogue of the environment typically described in the sequential equilibrium reputation literature (most closely, the trust game used in AEG). As such, the environment described here advances on the existing literature by introducing competitive group dynamics (i.e. an auction) in an environment of multi-period Bayesian uncertainty. The key focus of the current paper is thus to extend the insights of the literature regarding reputation formation in two player games of incomplete information to multi-player market-based games also featuring incomplete information.

3.3.2 Solving for Reputation Equilibria

The sequential equilibrium reputation approach would see a single bidder choose a bidding path which makes the seller just indifferent between `not sending' in the penultimate period (and then also in the last period) and `sending' then `not sending' in the final two periods. The seller then chooses the strategy that `induces their own indifference' i.e. the strategy for which the bidders' best response is the strategy that makes the seller indifferent. The bidder is then playing a best response and the seller, being indifferent, is best responding too. This logic is not possible in the construct presented here for a simple reason; the competition amongst bidders. Bidders are unable to coordinate on the bidding vector that generates seller indifference due to the competitive forces of the auction. Bidders instead bid their expected values and the seller then unilaterally maximises profits (this logic can be especially seen in the two-period game, below). Despite the breakdown of the canonical reputation logic, we are nonetheless able to find reputation equilibria in which the free to choose seller chooses to send the good (or, in the language of sequential equilibrium, mimic the commitment type) in all but the final period (see the two- and three- period games, below).

The One-Shot Commitment Moral Hazard Auction

Suppose $T = 1$. It is clear that the seller choosing $\gamma_{s,1} = 0$ is the unique sequentially rational strategy. Thus, in any Perfect Bayesian equilibrium the seller chooses $D_{s,1} = 0$ with probability 1. Consider ex-ante symmetric bidders who bid according to $E(u_{i,t})$[28]. This implies that all bidders $i = \{1, \dots, N\}$ will play the same ex-ante bidding strategy $b_{i,t} = b_{j,t}\ \forall\ i\ \neq j$ and win with equal probability $\frac{1}{N}$ giving $P_1 = b_{i,1}\ \forall\ i$. Consider then a bidder who faces a choice between a bid of $b_{i,1}$ and not bidding which earns nothing. The winning bidder will earn ex-ante expected profit given by[29];

$$u_{i,1}^{Bidder} = \mu_0(v_{i,1} - b_{i.1}) + (1 - \mu_0)[\gamma_{s,1}(v_{i,1} - b_{i.1}) - (1 - \gamma_{s,1})b_{i,1}] \qquad \mathbf{(3.1)}$$

where $\gamma_{s,1}$ is the probability (strictly the bidder belief about the probability – initial beliefs are assumed to be correct and are updated using Bayes rule thereafter) that the rational type seller sends the good, which as above is 0 when $T = 1$. Simplification gives;

$$u_{i,1}^{Bidder} = \mu_0 v_{i,1} - b_{i,1} \qquad \mathbf{(3.2)}$$

Thus, a positive bid requires;

$$\mu_0 v_{i,1} \geq b_{i,1} \qquad \mathbf{(3.3)}$$

[28] This corresponds to the optimal bidding function in a canonical second price auction. The same logic, that there is no 'penalty' to bidding your value (c.f. a first price auction) still holds. It is thus assumed that bidders are risk-neutral.
[29] Suppressing the equal probability of winning a given auction in equilibrium.

Therefore, Equation 3.3 tells us that the optimal bid is capped from above by $\mu_0 v_{i,1}$. The auction logic pushes Equation 3.3 to equality, at which point each bidder is indifferent between bidding $\mu_0 v_{i,1}$ and not bidding. The unique Perfect Bayesian equilibrium of the commitment internet auction described above is characterised by $b_{i,1} = \mu_0 v_{i,1} \forall\ i$ and $\gamma_{s,1} = 0$. We also require that bidder beliefs are correct, i.e. $B_{i,1} = \gamma_{s,1} = 0$ where B is the bidder belief but since no Bayesian updating is required in the one-shot game this can trivially be satisfied.

For $T = 1$, the unique ex-ante symmetric bidding strategy, for any value of γ_1, is given by $b_{i,1} = (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$. The proof is in two parts; first we prove that no bidder will deviate from this equilibrium strategy and second we prove that no other bid represents an equilibrium bidding strategy.

Consider a bidder who wishes to deviate when all other bidders bid the stated equilibrium bidding strategy. $b_{i,1} > (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$ guarantees that bidder $i \neq j$ is the winning bidder since $b_{j,1} = (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{j,1} \forall\ j$, however it also ensures zero ex-ante[30] expected profit and thus there is nonetheless no strong incentive to deviate. Consider $b_{i,1} < (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$, then the bidder i does not win the auction with probability 1 and receives zero profit, and thus has no strong incentive to deviate from the stated equilibrium strategy. Therefore, no bidder has a strong incentive to deviate from the stated equilibrium strategy.

Suppose now an alternate symmetric equilibrium strategy $b_{i,1}'$. Suppose $b_{i,1}' > (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$, then all bidders receive negative ex-ante expected profit. Suppose $b_{i,1}' < (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$, then all bidders receive positive ex-ante expected profit, however every bidder has an incentive for an small increase in their bid $b_{i,t}$, which would mean that they win for sure and receive positive ex-ante expected profit since $P_1 = \max_{W_{j,t}=0} b_{j,t}' < (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$. Iterative logic then shows that $b_{i,1} = (\mu_0 + \gamma_1 - \mu_0\gamma_1)v_{i,1}$ is the unique ex-ante symmetric bidding strategy. As above, for $T = 1$ and since $\gamma_{s,1} = 0$, the unique symmetric bidding strategy is given by $b_{i,1} = \mu_0 v_{i,1}$.

Consider the limit as $\mu_0 \to 0$. The environment collapses to a situation in which no commitment type sellers exist. All sellers will not send the good, and knowing this no bidder places a positive bid, as can be seen in Equation 3. This is of course the unique equilibrium described in the case where no commitment type exists[31]. Consider the limit as $\mu_0 \to 1$. The environment collapses to a canonical

[30] Note that, ex-ante, all subjects have the same valuation.
[31] In this case, positive bids require trust in the seller.

second price auction and according to both Equation 3 and canonical theory bidders should bid their valuations $b_{i,1} = v_{i,1}$. Thus, both limiting cases of the prior probability are consistent with existing theory.

The Twice-Repeated Commitment Moral Hazard Auction

Consider now the case in which $T = 2$. In the twice repeated game, the second period outcome is the equilibrium of the one-shot game; $\gamma_{s,2} = 0$ and $b_{i,2} = (\mu_1 + \gamma_{s,2} - \mu_1\gamma_{s,2})v_{i,2}$ with $\gamma_{s,2} = 0$.

In this case, beliefs are updating using Bayes' rule whenever new information becomes available (i.e. after seeing whether the seller sends the good or not) where;

$$\mu_1 = \begin{cases} \dfrac{\mu_0}{(1-\mu_0)\gamma_1 + \mu_0} & if\ the\ seller\ sent\ the\ good \\ 0 & if\ the\ seller\ did\ not\ sent\ the\ good \end{cases}$$

Consider the first period bidding behaviour and acknowledge that $\gamma_{s,2} = 0$. The ex-ante expected profit for bidder i is given by;

$$u_{i,1} = \mu_0\left[(v - b_{i,1}) + (v - b_{i,2})\right] + (1-\mu_0)\left[\gamma_1\left[(v - b_{i,1}) - b_{i,2}\right] - (1-\gamma_1)b_{i,1}\right] \qquad \mathbf{(3.4)}$$

Solving in the same manner as the one period game (above);

$$\left[2\mu_0 + \gamma_1 - \mu_0\gamma_1 + \frac{(\mu_0\gamma_1 - \mu_0 - \gamma_1)\mu_0}{(1-\mu_0)\gamma_1 + \mu_0}\right] v \geq b_{i,1} \qquad \mathbf{(3.5)}$$

In a similar fashion to the one period game, Equation 3.5 sets an upper limit on the first period bid as a function of μ_0 and γ_1. A similar logic to that stated above pushes Equation 3.5 to equality.

Thus for $T = 2$, the optimal bidding strategy is given by $\left[2\mu_0 + \gamma_1 - \mu_0\gamma_1 + \frac{(\mu_0\gamma_1 - \mu_0 - \gamma_1)\mu_0}{(1-\mu_0)\gamma_1 + \mu_0}\right] v = b_{i,1}$ and $\mu_1 v = b_{i,2}$. Note that this states the optimal bidding strategy *for any* γ_1. This has an important implication: the seller can unilaterally choose γ_1 to maximise their profit, considering the effect of γ_1 on the optimal bidding strategy. Thus, all that remains to characterise Perfect Bayesian equilibria of the two-period game is to determine γ_1. In particular, we wish to consider $1 \geq \gamma_1 > 0$.

The seller profit function is given by;

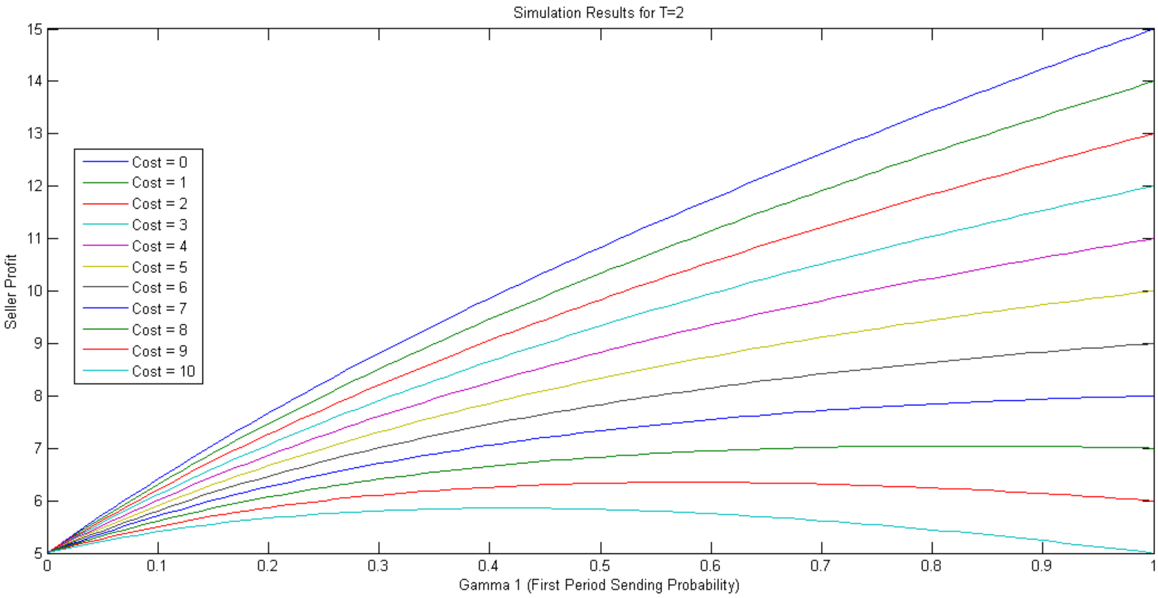
$$u_s = (P_1 - \gamma_1 c_{s,1}) + \gamma_1 (P_2 - \gamma_2 c_{s,2})$$

where $\gamma_2 = 0,\ P_1 = \max_{W_{j,t}=0} b_{j,t} = \left[2\mu_0 + \gamma_1 - \mu_0\gamma_1 + \frac{(\mu_0\gamma_1 - \mu_0 - \gamma_1)\mu_0}{(1-\mu_0)\gamma_1 + \mu_0}\right] v$ and $P_2 = \mu_1 v$.

Table 3.1: Unique Symmetric Equilibrium for the Twice Repeated Game for $v = 10, c = 5$ and $\mu_0 = 0.5$

Period	Prior/Posterior	Seller Strategy	Bidder Strategy
1	μ_0	$\gamma_{s,1} = 1$	$b_{i,1} = v$
2	μ_0	$\gamma_{s,2} = 0$	$b_{i,2} = \mu_0 v$

Figure 3.1: Simulation Results for the Optimal Seller Strategy in the Twice Repeated Game



Note: The figure presents, for each integer value of the cost (i.e. for multiple cost-to-value ratios) with v = 10 and $\mu_0 = 0.5$.

A numerical simulation was run at this point to obtain the optimal (i.e. profit maximising) strategy for the seller i.e. the optimal probability of sending the good in the first period. The problem was solved by calculating first the optimal bidder strategy, for all possible values of the seller's first period decision (allowed to be a mixed strategy with up to 2 significant figures) and consistent with a history in which the good is received in the first period, as described above. Finally, seller profit is calculated as described above, for all possible values of the seller's own decision. From here, finding the optimal value is a simple search problem since, as above, the seller is able to unilaterally maximise their own profit. The parameterisation was such that $\overline{v}$ = 10 and μ_0 = 0.5. The results of the simulation, for various values of the cost parameter are presented in Figure 3.1. As can be seen, for the majority of `low' costs, the profit maximising choice for the seller is to send the good for certain during the first period. Unsurprisingly, as the cost approaches the value (and the ex-ante gains from trade disappear altogether), the optimal choice becomes strictly interior. Even in the case where the price is 10 and the cost is 9, the seller sends the good with positive probability. As in the canonical sequential equilibrium logic, the seller mimics the computerised seller with positive probability.

The Three Period Commitment Moral Hazard Auction

For $T = 3$, the optimal bidding strategy conditional on receiving the goods in periods 1 and 2 is; $b_{i,1}$ given by Equation 3.6, $b_{i,2}$ given by Equation 3.7 and $b_{i,3}$ given by Equation 3.8, obtained using the same logic and intuition as above, iterated (backwards) another period. μ_i is updated using Bayes' rule where possible and in the case of not receiving the good, the posterior becomes zero and bidders do not bid positively again.

$$[3\mu_0 + \gamma_1 - \mu_0\gamma_1 + \gamma_1\gamma_2 - \mu_0\gamma_1\gamma_2]v + [\mu_0\gamma_1 - \gamma_1 - \mu_0]b_{i,2} + [\mu_0\gamma_1\gamma_2 - \gamma_1\gamma_2 - \mu_0]b_{i,3} = b_{i,1} \tag{3.6}$$

$$[2\mu_0 + \gamma_2 - \mu_0\gamma_2]v + [\mu_0\gamma_2 - \gamma_2 - \mu_0]b_{i,3} = b_{i,2} \tag{3.7}$$

$$\mu_2 v = b_{i,3} \tag{3.8}$$

As an example, suppose $\mu_0 = 0.5$, $v = 10$ and $\gamma_1 = \gamma_2 = 1$. Also, $\gamma_3 = 0$ by the logic of the one-shot game. Notice first that $\mu_0 = \mu_1 = \mu_2 = 0.5$ by Bayes' rule and hence $b_{i,3} = 5$. Substitution then gives $b_{i,2} = 10$ and $b_{i,1} = 10$. Therefore, in this example, the optimal bidding vector[32] is given by ($b_{i,1}$, $b_{i,2}$, $b_{i,3}$) = (10, 10, 5). As another example, suppose $\mu_0 = 0.5$, $v = 10$ and $\gamma_1 = 1$ as before but

[32] Consistent with only realisations of the good being sent - not receiving the good unambiguously reveals the normal sellers type and results in off-the-equilibrium path bids of zero everywhere.

now that $\gamma_2 = 0.5$ with $\gamma_3 = 0$ as before. In this case $\mu_0 = \mu_1 = 0.5$ and $\mu_2 = 0.67$ the optimal bidding vector defined by Equation 3.6, Equation 3.7 and Equation 3.8 is given by $(b_{i,1},\ b_{i,2},\ b_{i,3}) = (10, 7.5,\ 6.7)$. It is important to note that these bidding vectors are conditional upon the history in which in the good is always received.

The sellers expected profit function is given by Equation 3.9 where $P_t = b_{i,t}$ by the ex-ante symmetry of the bidders. Suppose also now that $c = \underline{v} = 5$.

$$u_s = P_1 + \gamma_1(P_2 - c_1) + \gamma_1\gamma_2(P_3 - c_2) \qquad \mathbf{(3.9)}$$

Since the competitive logic of the auction determines the optimal bidding strategy completely as a function of $\boldsymbol{\gamma} = (\gamma_{s,1}, \gamma_{s,2}, \gamma_{s,3})$ (as opposed to generating seller indifference (see above)), the seller is able to unilaterally maximise their profit, taking account of the optimal bidding strategy. For the parameters $\mu_0 = 0.5$, $v = 10$ and $c = 5$, this is achieved by choosing $\gamma_1 = \gamma_2 = 1$, again obtained via simulation, the results of which can be seen in Figure 2 which presents the results of a simulation of all possible seller strategies (combination of first and second period actions).

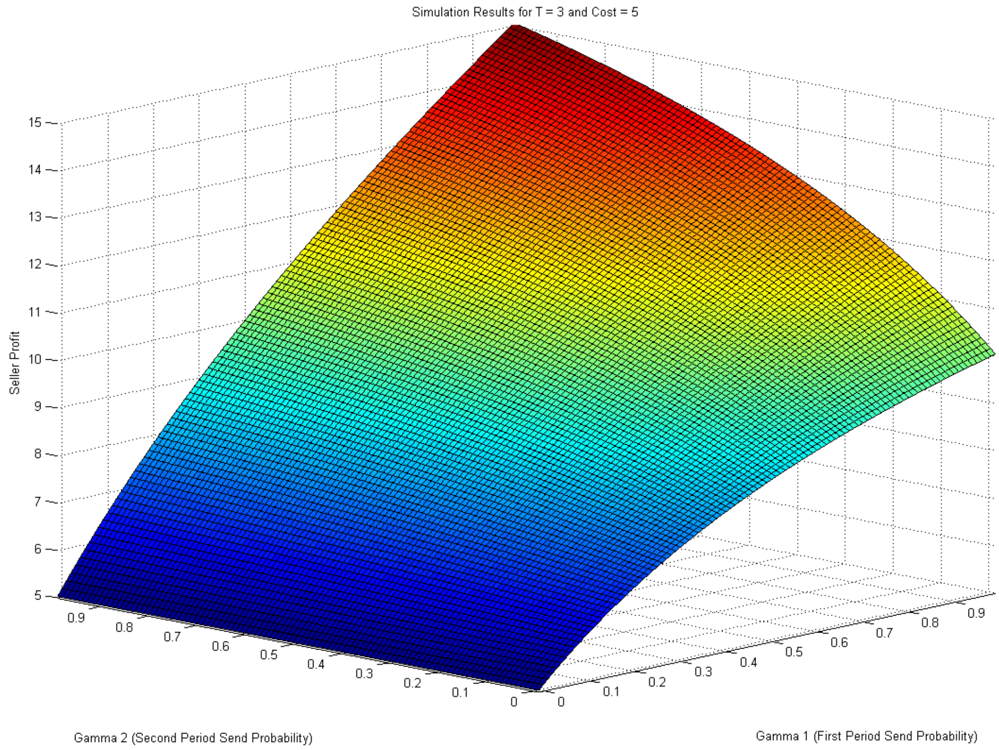
Thus, for $\mu_0 = 0.5$, $\overline{v} = 10$ and $c = \underline{v} = 5$, an ex-ante reputation equilibrium of the three-period game exists in which;

- Free-to-choose sellers send the good with probability 1 in periods 1 and 2 ($\gamma_1 = \gamma_2 = 1$) and with probability 0 in period 3 ($\gamma_3 = 0$). Commitment sellers, of course, send the good with probability 1 in all three periods.
- Bidders bid their expected valuation in periods 1 and 2 and reduce their expected valuation by the prior probability in period 3[33].
    - Conditional upon off-the-equilibrium path play (the good not being received in periods 1 or 2), the bidders cease to bid and set $b_{i,t+1} = 0$ thereafter.

- Bidder beliefs are updating using Bayes' rule whenever possible.

[33] In this case, the prior probability is equal to the posterior probability since the free-to-choose seller sends the good with probability one in the first two periods, preventing any Bayesian updating.

Figure 3.2: Simulation Results for the Optimal Seller Strategy in the Thrice Repeated Game



Note: Since the seller strategy now varies in two dimensions ($\gamma_1$and γ_2) the figure displays the resulting level of seller profit for $c = \underline{v} = 5$ only. As above, $\overline{v} = 10$ and $\mu_0 = 0.5$.

Table 3.2: Unique Symmetric Equilibrium for the Thrice Repeated Game for $v = 10, c = 5$ and $\mu_0 = 0.5$

Period	Prior/Posterior	Seller Strategy	Bidder Strategy
1	μ_0	$\gamma_{s,1} = 1$	$b_{i,1} = v$
2	μ_0	$\gamma_{s,2} = 1$	$b_{i,2} = v$
3	μ_0	$\gamma_{s,3} = 0$	$b_{i,3} = \mu_0 v$

We now present hypotheses derived directly from this theoretical construct that correspond to the parameters used in the experimental design presented below.

3.3.3 Predictions and Hypotheses

We now present hypotheses relating to the various possible values of the prior probability.

Hypothesis 1: (Prior Probability = 0) When the prior probability is equal to zero, the prediction is that sellers will never send the good with any positive probability and, anticipating this, bidders never place positive bids (obtained through the backwards induction logic).

We now state a hypothesis relating to our experimental parameters.

Hypothesis 2: (1 > Prior Probability > 0) For our experimental parameters $\mu_0 = 0.5, \overline{v} = 10$ and $c = \underline{v} = 5$, the prediction is as stated above; (real) sellers send the good with probability one in all but the final period. Bidders bid their value in the first and second period, and multiple their value by the prior probability in the final period, anticipating that real sellers do not send the good. Bidders bid their value multiplied by the prior probability because the seller sends the good with probability one in the first and second period (as can be seen in Figures 3.1 and 3.2), preventing any Bayesian updating.

Hypothesis **3:** (Prior Probability = 1) When the prior probability is one, canonical auction theory (Krishna 2001) predicts that bidders should bid their values.

3.4 Methodology and Experimental Design

3.4.1 Experimental Design

A total of three treatments were conducted. Commitment types are implemented as a computerised seller. The balance between computerised and free-to-choose (hereafter real) sellers was varied between treatments. The computerised seller always sends the good with probability 1. The first treatment contains only real sellers and corresponds to a prior probability of 0 (referred to as P0). The second treatment has two computerised and two real sellers corresponding to a prior probability of 0.5 (P0.5). The final treatment contained only computerised sellers and corresponds to a prior probability of 1 (P1). Therefore, P0 is equivalent to a pure moral hazard auction as in BGO and P1 corresponds to a canonical second price auction (see above regarding the limiting cases of the prior).

The experimental design closely mirrors the model outlined above. In particular, $v = 7.5$ (uniformly distributed between 5 and 10) and $c = 2.5$ (uniformly distributed between 0 and 5) for all treatments. $\mu_0 = 0$ in P0, $\mu_0 = 0.5$ in P0.5 and $\mu_0 = 1$ in P1. Each value and cost is independent and identically distributed between subjects and periods. The value and cost parameters were chosen to ensure a pure strategy equilibrium prediction and the prior probability for P0.5 was chosen to provide maximum variation to both the other treatments that represent limiting cases. Each session consists of 12 bidders and 4 sellers (albeit possibly computerised). The bidders are matched into four groups of three that remain fixed throughout the experiment. Each group of bidders is then matched to one of the four sellers. Bidders receive valuations between 5 and 10 ECU (Experimental Currency Units) and sellers receive a cost between 0 and 5 ECU. Bidders place sealed bids and the winner is determined by the second price format. The seller receives the payment from the winning bidder and then decides whether to send the good or not. Sellers are explicitly able to play mixed strategies using a random device into which they enter the probability with which they would like to send the good. All participants are then informed of the winning price for the auction, whether the seller sent the good or not and their profit for the period. After 3 periods have been played, each group of bidders is then matched to a new seller and plays another 3 periods of the auction described. Each three-period repeated game is referred to as a block. This process continues until each seller has been matched with each group of bidders, for a total of 12 rounds (3 periods x 4 blocks). In treatment P0.5, participants are explicitly informed that they will meet two real and two computerised sellers across the four blocks[34]. In all treatments, whilst sellers were making their decisions, bidders were asked the probability with which they thought the seller would send the good. This was done in an unincentivised manner. This was done to avoid income effects that may arise due to subjects gaining income from the belief elicitation that could affect bidding behaviour[35]. All of the above is common knowledge. The independent observation is technically the session, of which there are three per treatment. The nature of the rematching means that each of the four groups of (three) bidders in a given session faces a unique order of either real or computerised sellers.

3.4.2 Procedure

All sessions were run at the Birmingham Experimental Economics Laboratory (BEEL) in May 2014. A total of three sessions were conducted for each treatment. Table 3.3 gives a breakdown of the numbers of subjects for each session and treatment. The average session lasted 75 minutes and subjects received on average £12. All subjects were recruited using ORSEE (Greiner 2004) and came from a variety of disciplines from across the University of Birmingham. The experiment was programmed using zTree (Fischbacher 2007). Instructions were read aloud to all subjects. Subjects were then required to answer control questions to ensure understanding. Finally, at the end of the

[34] This induces a super-game in which if sellers unambiguously reveal their type, bidders may be certain that they are matched with a computerised seller. Unfortunately, small samples prevent any meaningful analysis of this phenomena in the current paper.

[35] We did not want bidders to attempt to gain income solely or predominantly through an incentivised belief component of the experiment.

session, subjects were asked to conduct a post experimental questionnaire to elicit demographic information that can be used for control purposes.

Table 3.3: Treatment Table showing the number of participants per treatment

	Per Session					
Treatment	**Bidders**	**Real Sellers**	**Computer Sellers**	**Participants**	**Sessions**	**Total Participants**
P0	12	4	0	16	3	48
P0.5	12	2	2	14	3	42
P1	12	0	4	12	3	36

3.5 Results and Analysis

3.5.1 Bidders

Table 3.4: Bidder summary statistics for each treatment.

	Bid as Proportion of Valuation (BPV)		
Treatment	**P0**	**P0.5**	**P1**
Period 1	0.75	0.67	0.95
Period 2	0.78	0.72	0.95
Period 3	0.75	0.76	0.95
Total	0.76	0.72	0.95

Note: The data is pooled across all 3-period blocks.

Table 3.4 shows the mean BPV across all 3-period blocks for each treatment. We refer to the proportion of the valuation bid as the BPV (Bid as Proportion of Value). The BPV is not significantly different between treatments P0 and P0.5 (Mann-Whitney Rank-Sum; p=0.468). The BPV in both P0 and P0.5 is however significantly different from the BPV in P1 (p<0.002 and p<0.005 respectively)[36]. Across all three treatments the BPV and the belief are positively correlated (Pearson correlation coefficient r=0.45, p<0.000). In addition, across all three treatments the average BPV is larger than the average belief. This difference is larger in the P0 treatment (0.25) than the P0.5 treatment (0.12). This

[36] Beliefs are only weakly different between treatments P0 and P0.5 (p=0.074) and both P0 and P0.5 are significantly different from P1 (p<0.000 and p<0.002 respectively).

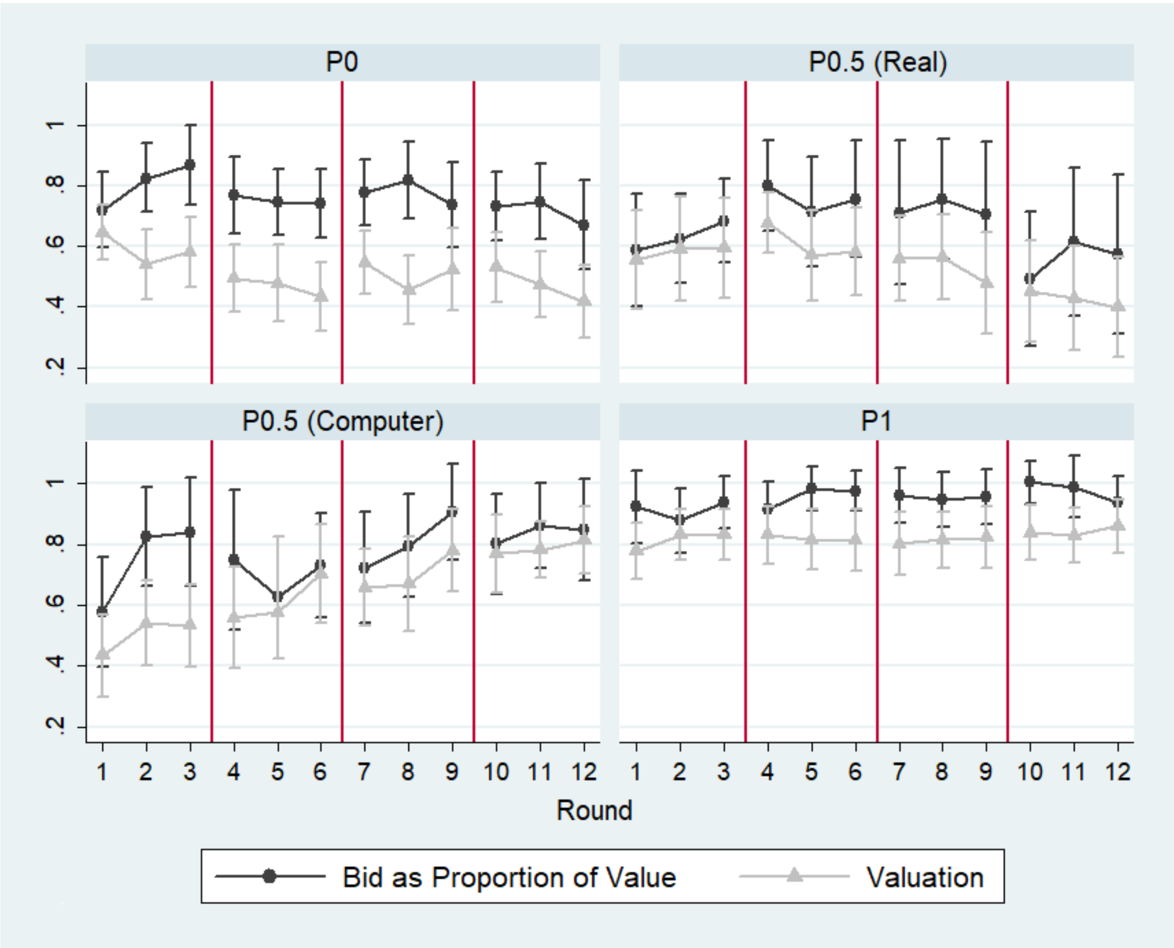
suggests on average bidders bid above their expected value of receiving the good. We take this as being consistent with evidence on overbidding in auctions.

Table 3.5: Conditional Summary Statistics for Treatments P0 and P0.5.

Variable	BPV – P0		BPV – P0.5		
Condition	**Real, Sent**	**Real, Not Sent**	**Computer**	**Real, Sent**	**Real, Not Sent**
Period 1	0.84	0.71	0.71	0.67	0.66
Period 2	0.85	0.72	0.77	0.79	0.45
Period 3	0.79	0.72	0.83	0.75	0.46
Total	0.83	0.72	0.77	0.74	0.52

Note: The table presents the mean BPV for treatment P0 and P0.5, categorised according to whether matched with a computer or a real seller, and if real whether the good was received in the previous period.

Figure 3.3: Average BPV for each Round and for every Treatment.



Note: P0.5 is subcategorised according to whether the group of bidders were currently matched with a computer or real seller.

Table 3.5 shows a conditional breakdown of P0 and P0.5. It gives the mean BPV when matched against a computer seller (in P0.5 only) and when matched against a real seller having previously received the good or not. There is no significant difference between being matched against a computer compared to a real seller having previously received the good (Wilcoxon matched pairs signed rank; $p<0.480$) in treatment P0.5. Of course, the bidders do not know who they are matched against and can only condition on whether the good was sent. Both being matched with a computer and a real seller conditional on having received the good in the previous period are significantly different from a real seller having not previously received the good ($p=0.033$ and $p=0.041$ respectively). Also note that in the first period after being rematched there is no difference between whether the good was sent or not which we would expect since in the previous period the bidders were with a different seller. There is thus evidence that suggests bidders do indeed condition on whether they received the good in the previous period or not in the P0.5 treatment - when that information is relevant (i.e. in the second or third period). This difference between whether the good was previously sent is considerably smaller for the P0 treatment. Figure 3.3 shows the mean BPV for each period of each block by treatment. It also confirms the results of the non-parametric tests presented above; only in P1 is the BPV different from the other treatments.

Table 3.6 shows two Tobit regressions for the bidding behaviour with the bid as the dependent variable. Specification I has treatment dummies, the value, belief, previous bid and whether the good was previously sent. All coefficients are significant at the 5% level. The treatment dummies for P0.5 and P1 are both positive and significant and consistent with the non-parametric statistics and Figure 3.3. The coefficient on the value and belief are both positive and statistically significant as would be expected. Previous period bid, the lagged dependent variable, is positive and significant suggesting the presence of autocorrelation in bidding behaviour which is not surprising. We also include interactions between the previous period bid and the treatment dummies. In both cases the coefficient is negative and significant. We run an additional specification II in which we include also whether the good was sent in the previous period. All coefficients maintain their sign, magnitude and significance. As expected, the coefficient on whether the good was received in the previous period is positive.

Table 3.6: Tobit regression with the bid as the dependent variable.

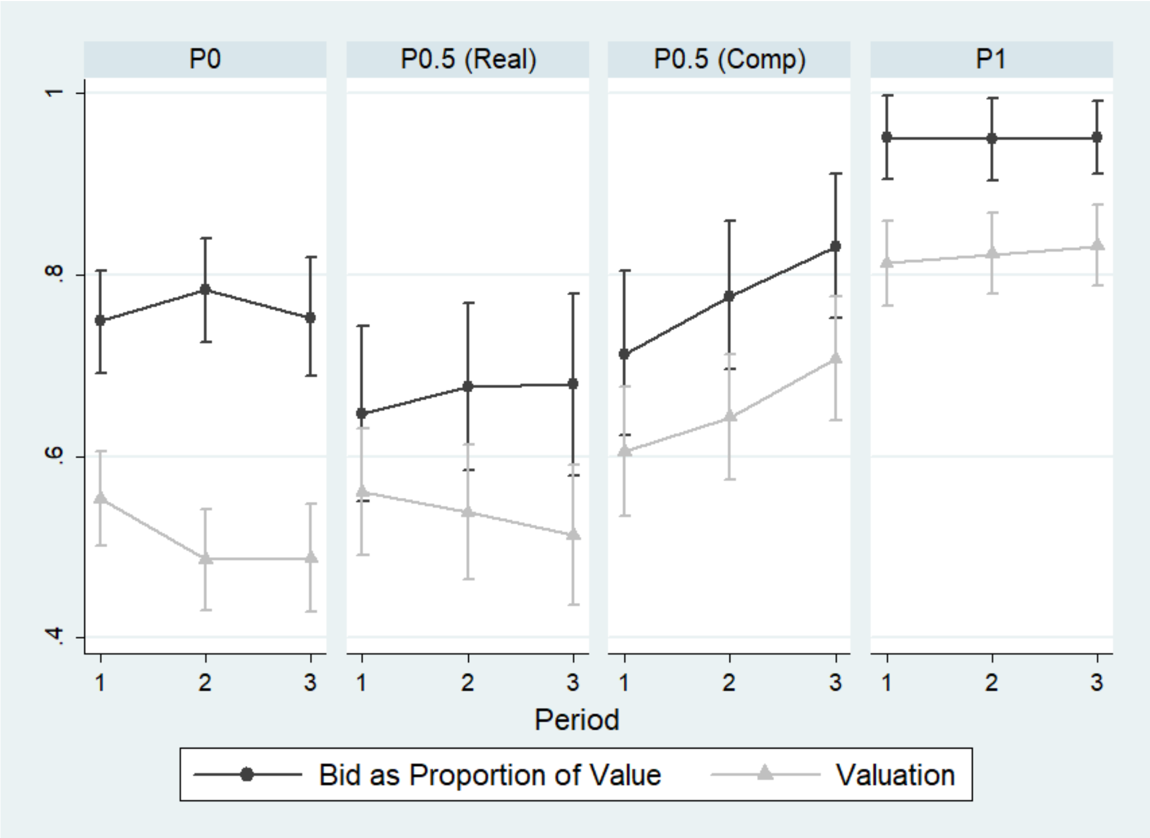
Estimation Technique	Tobit	
Dependent Variable	Bid	
Specification	I	II
P0.5	1.381*** (0.513)	1.229*** (0.513)
P1	2.315*** (0.810)	1.983*** (0.818)
Value	0.794*** (0.047)	0.790*** (0.047)
Belief	0.032*** (0.004)	0.031*** (0.004)
Previous Bid	0.655*** (0.057)	0.659*** (0.055)
Previous Bid * P0.5	-0.338*** (0.082)	-0.352*** (0.081)
Previous Bid * P1	-0.394*** (0.111)	-0.396*** (0.110)
Previous Sent	-	0.698*** (0.215)
Constant	-5.542*** (0.463)	-5.848*** (0.455)
Pseudo R^2	0.133	0.136
N	1188	

Note: Robust standard errors are clustered on each individual 3 period block and are presented in parenthesis. The regression is censored on a lower limit of 0 and an upper limit of 10.

- Result **1B:** Bidders respond to the different distribution of seller types across the P0 and P0.5 treatments in a manner broadly supportive of the reputation building hypothesis presented above. Nonetheless, there is significant quantitative deviation from equilibrium.

Support: While the non-parametric statistics presented suggest there is no significant difference in bidding behaviour between P0 and P0.5, this masks some important differences between the two treatments. The regression results presented in Table 3.6 show a significant and positive treatment effect for the P0.5 treatment compared to the P0 baseline. Furthermore, there is evidence that in the P0.5 treatment bidders condition on whether they received the good in the previous period or not whereas there is little evidence that this occurs in the P0 treatment. We also see from the regression presented that the previous period bid has a smaller effect in the P0.5 treatment than the P0 treatment, indicating a greater level of autocorrelation and thus path dependence in bidding behaviour.

Figure 3.4: Average BPV and Belief per Period for all Treatments



Note: P0.5 is subcategorised according to whether the group of bidders were currently matched with a computer or real seller. The data is pooled across all 3-period blocks.

- Result **2B:** Bidders bid higher, on average, in treatment P1 than either P0 or P0.5.

Support: Non-parametric tests show a significant difference in the BPV between P1 and both P0.5 and P0. Table 3.6 shows a significant treatment effect for both P0.5 and P1 compared to P0. We also see that in P1 the value has a greater effect than for P0 and P0.5. Furthermore, the equilibrium prediction that bidders bid their valuation in the P1 treatment is on average not rejected, as can be seen from Table 3.3 where on average in P1 bidders bid 95% of their value.

3.5.2 Sellers

Table 3.7 presents summary statistics for the average probability that the seller chooses to send the good with for each period and for each treatment. This information can also be seen in Figure 3.5. The sending probability of the seller is weakly different between the P0 and P0.5 treatments (p=0.054).

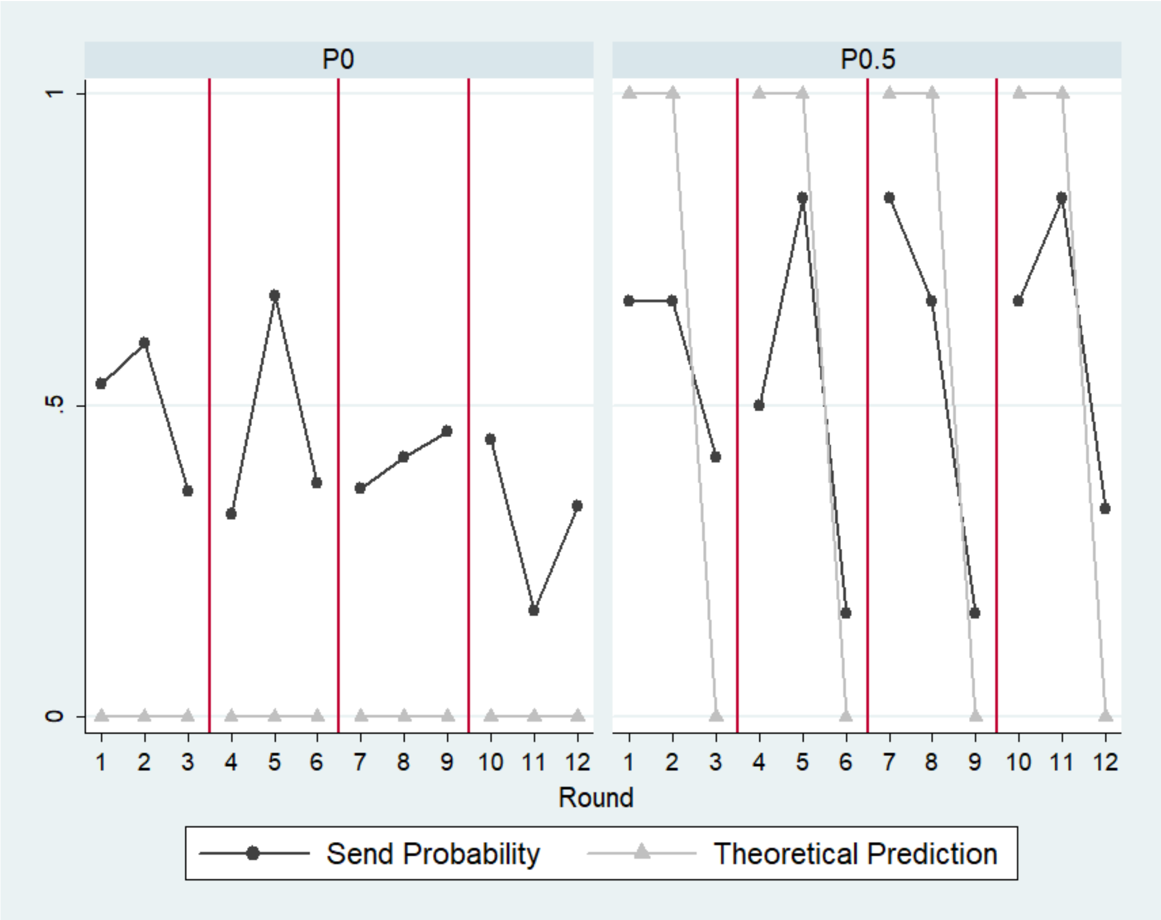
Table 3.7: Seller summary statistics for treatments P0 and P0.5.

Variable	Probability of Sending the Good	
Treatment	**P0**	**P0.5**
Period 1	41.77	66.67
Period 2	46.58	75
Period 3	38.33	27.08
Total	42.23	56.25

Note: The send probability is the choice variable of the seller and can take values between 0 and 100 inclusive.

Table 3.8 shows results of Tobit regressions on the send probability chosen by the seller. We begin with a simple specification I which includes the winning price, the seller's cost, a dummy for the final period of a matching block (i.e. before being rematched or the experiment ending), a linear time trend and a treatment dummy. Specification I shows a significant and positive (as expected) treatment effect. All other coefficients have the expected sign; the cost has a negative and significant effect, as does being in the final period of a given block. Interestingly, the price is not significant, though the coefficient is positive. Specification II includes two interaction terms; one between the treatment dummy and the dummy for the final period within a block and another between the treatment dummy and the period. Again, the results are robust across estimation techniques and overall period have a significant, negative effect. However, in specification II there is no significant overall treatment effect. Instead, any treatment effect is entirely captured by the two interaction terms. In the absence of the commitment types (P0), there is no significant effect of being in the final period of a block but there is a significant, negative effect in the presence of commitment types. Indeed, this effect can be seen visually in Figure 3.5 and in Table 3.7 where the send probability in period 3 is significantly lower than in periods 1 or 2, for treatment P0.5 only. This is taken as supportive of the reputation building hypothesis outlined above. The effect of the overall period is also interesting. In specification I, the period has a small significant negative effect. In specification II, the coefficient on the interaction term is significant and positive (indicating the probability a seller sends the good in P0.5 increases throughout the four blocks), whereas the coefficient on the overall period (capturing such an effect solely in treatment P0) is significant and negative.

Figure 3.5: Average Send Probability and Theoretical Prediction for P0 and P0.5



Note: The vertical lines represent the start of a new block of 3 rounds.

- Result **1S:** Sellers behave differently in treatment P0 compared to P0.5 in a manner broadly supportive of the reputation building hypothesis.

Support: First, consider the extent of equilibrium play. For P0, the equilibrium prediction was zero send probability in every period. This was the strategy used in 10 of the 48 blocks conducted. In treatment P0.5, the equilibrium strategy was send with probability one in periods 1 and 2 and probability zero in period 3. This was the strategy used by the seller in 11 out of 24 blocks. Thus, in treatment P0.5, roughly half of seller behaviour is consistent with the equilibrium prediction highlighted above. In neither P0 nor P0.5 is the equilibrium prediction in mixed strategies. Nonetheless, sellers are able to play mixed strategies. In P0, of 144 decisions a mixed strategy was employed on 19 occasions. Of the 72 decisions in P0.5, a mixed strategy was used only once. Despite finding only weakly significant difference via non-parametric tests, regression analysis shows some significant differences between the two treatments. As Table 3.8 shows, the direction of the ceteris parabus time trend is different between the two treatments. In addition, the effect of being in the final period of a block is only significant for the P0.5 treatment and suggests that the sellers respond to the reputational considerations created in P0.5.

Table 3.8: Tobit Regression with the send probability as dependent variable

Estimation Technique	**Tobit**	
Dependent Variable	**Send Probability**	
Specification	**I**	**II**
P0.5	**14.86*** (0.015)**	-0.784 (0.950)
Price	2.20 (0.133)	2.55* (0.073)
Cost	**-13.88*** (0.000)**	**-13.91*** (0.000)**
Final Period of Block	**-18.42*** (0.006)**	-7.08 (0.362)
Final Period of Block * P0.5	-	**-34.35*** (0.013)**
Period	**-1.82*** (0.033)**	**-3.320*** (0.003)**
Period * P0.5	-	**4.21*** (0.008)**
Constant	**83.70*** (0.000)**	**87.02*** (0.000)**
Pseudo R^2	0.241	0.276
N	216	

Note: Regression is censored on the available strategy space 0-100. Robust standard errors are clustered on each individual 3 period block and are presented in parenthesis.

Table 3.9 shows average profits across each treatment for bidders and sellers. Recall that the initial endowment was 100. Thus, in the P0 treatment bidders lost on average 4.11% of their initial endowment. In addition, sellers made an average profit of 29.65%. In the P0.5 treatment, on the other hand, bidders made positive profits on average (albeit only equal to 2.03% of their initial endowment). Sellers still earn positive profits, in this case 26.38% of their initial endowment. It is interesting to note that the profit earned by bidders barely increases between the P0.5 and P1 treatments, suggesting that the P0.5 treatment does not fare badly at overcoming the moral hazard present in P0.5 that is absent in P1.

Table 3.9: Average Final Endowment per Treatment

Treatment	Final Endowment (All)	Final Endowment (Bidder)	Final Endowment (Seller)
P0	104.34	95.89	129.65
P0.5	105.5	102.03	126.38
P1	102.65	102.65	N/A

Note: Average profit is the final endowment minus the initial endowment of 100.

3.6 Discussion and Conclusion

We present here a nascent theoretical construct of a repeated moral hazard auction that features Bayesian uncertainty about the type of seller that bidders are matched with. We additionally present the results of the first experimental test of such a theoretical environment. We conduct three treatments in which the distribution of seller types varies between treatments. The current paper thus seeks to extend the canonical SE literature by incorporating competitive group dynamics through the form of a market-based institution, in this case an auction.

We find that when there is no behavioural Bayesian uncertainty of the SE kind, the results closely mirror both the theoretical prediction and the vast literature on experimental auction markets, which of course coincide. Bidders place higher bids in as the probability of encountering a seller that overcomes the moral hazard of the auction increases. There is also evidence that bidders employ conditional strategies on the basis of the information available to them - whether the good was sent in the previous period or not - but only when there is Bayesian behavioural uncertainty present. In a pure moral hazard auction, many bidders systematically make positive and substantial bids, something the theory presented is unable to account for, though such findings are not surprising.

We also find evidence that sellers respond to the different reputational considerations present the Bayesian uncertainty introduces. Both regression analysis and non-parametric statistics show a significant difference in the probability with which the seller sends the good, with sellers on average choosing a higher probability when bidders could entertain Bayesian uncertainty as to the seller type. In other words, sellers are more likely to send the good in the early periods, *when they can mimic the computerised seller*, as per the canonical sequential equilibrium model. In addition, the sellers in the treatment featuring Bayesian uncertainty substantially lower the probability with which they choose to send the good immediately before being rematched (or the experiment ending). This suggests sellers engage in strategic reputation formation - this is backed up by the fact that the equilibrium prediction in this case is able to explain approximately half of all seller behaviour. In addition, some sellers make explicit reference to "copying the computer" in early rounds to be exploited for benefit in later periods. The results presented thus indicate that the intuition behind the sequential equilibrium framework can be extended to multi-player games.

It is important to understand how the existing insights on reputation from the SE literature can be extended to market-based institutions. The results here extend the canonical literature on SE by suggesting that reputational considerations are also an important phenomenon in multi-player, market-based games. Of course, as with much SE literature there are significant deviations from equilibrium that are not reconcilable with any form of SE. Nonetheless, many of the qualitative and intuitive findings of the SE reputation literature are still present even when the stage game takes the form of a market-based institution such as an auction.

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Appendix 3.A: Experimental Instructions

Economic Decision Making Experiment - Instructions

Welcome to the Birmingham Experimental Economics Laboratory (BEEL). This is an experiment in decision making. The University of Birmingham has provided the funding for this research. It is very important that you read the instructions carefully.

It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your following of these rules.

We will firstly go over the instructions together. You will then have a chance to ask clarifying questions. Each of you will then answer a few questions to make sure everybody understands. You will be given an initial endowment of **100** ECU. You will be paid according to the number of ECUs you have at the end of the experiment. You will be paid according to the following exchange rate;

$$1\ ECU = 10p$$

In addition to this initial endowment, you have already earned a **£2.50** show up fee. We would like to stress that any decision you make is completely anonymous. You will be paid privately in cash at the end of experiment. All further references to amounts refer to ECU.

Instructions

Role

Throughout this experiment, you will be randomly assigned to a role by the computer. This role will remain fixed throughout the duration of the experiment. Each subject will be assigned as either a **bidder** or a **seller**. In total there are 14 participants in this session, **12** participants will be assigned the role of **bidder** and the remaining **2** participants will be assigned the role of **seller.**

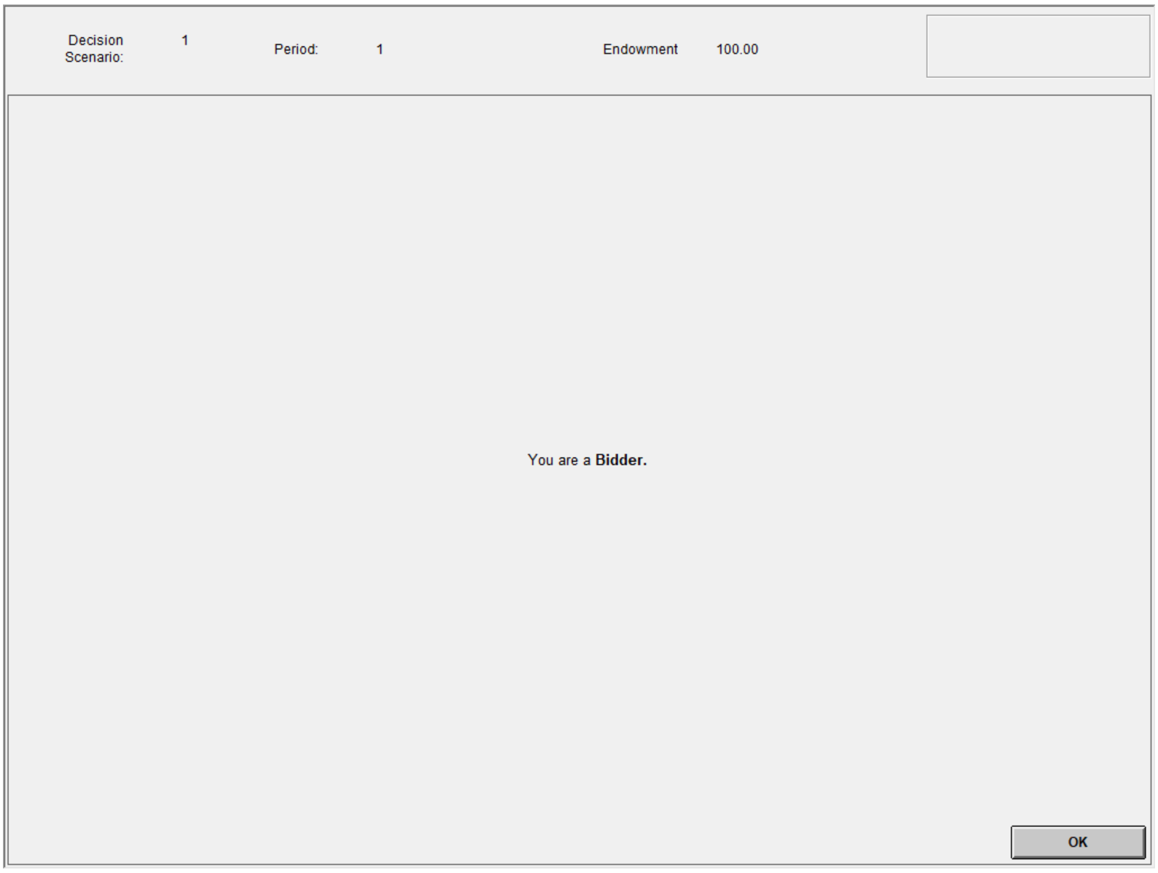
Groups

From the **12 bidders**, **4 groups** of **3 bidders** each will be formed. This **group of bidders** will remain fixed throughout the experiment. Each **group of bidders** will then be matched to **1 of 4 sellers**; either **1 of the 2** participants assigned the role of **seller** or **1 of 2 computer**

programmed sellers. The behaviour of the computer programmed seller will be thoroughly explained below.

Once all **4 groups** have been formed, **3 periods** of a **decision scenario** described below will be played. Once **3 periods** have been played, the **group of bidders** will then be matched to a **different seller**. This process then continues until **each group of bidders** have been matched with **each seller**. You will **never be matched** with the **same group of bidders or seller** in **more than one** decision scenario. This implies each subject will take part in **4 different decision scenarios** each lasting **3 periods** and each with a **different group of bidders or seller**. The order of the matching is decided at random.

Each subject will be informed of their role on a screen that looks like this;

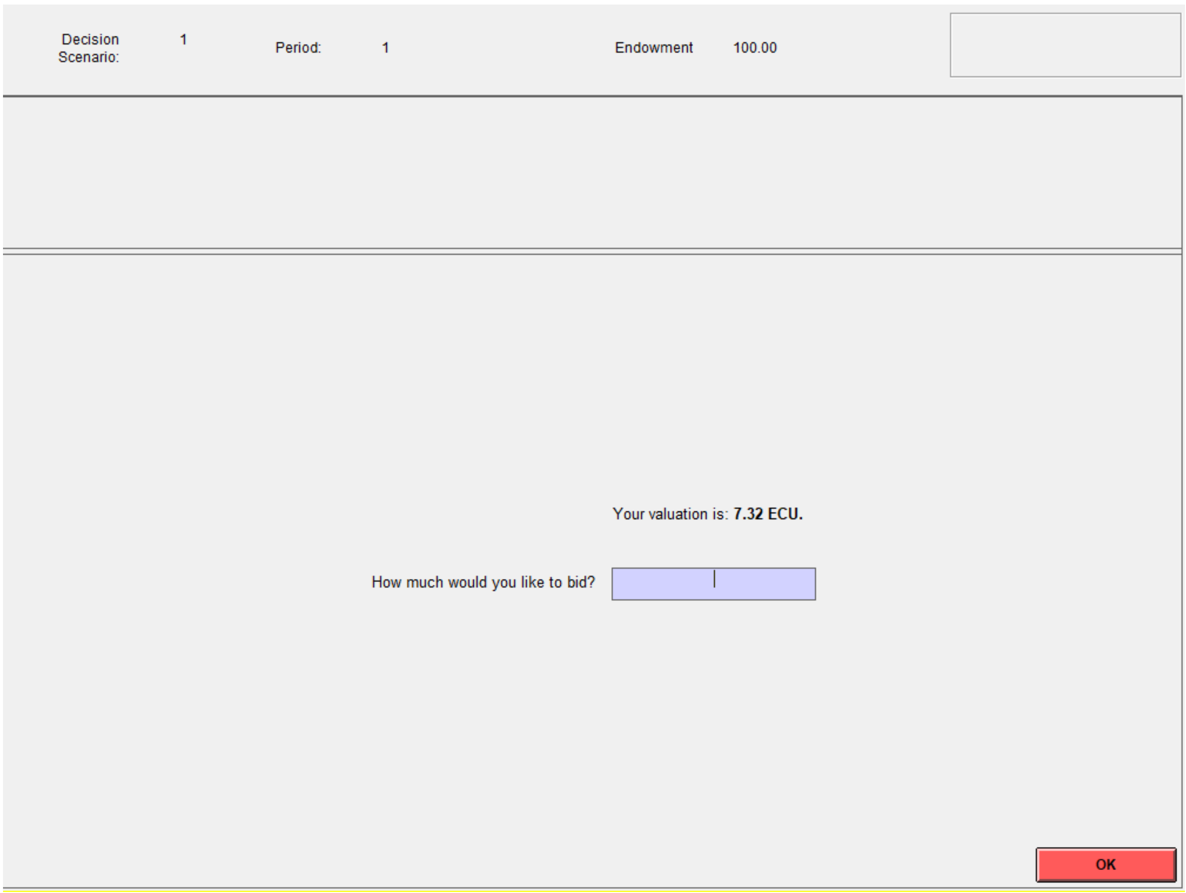


Decision Scenario

In **each period, each bidde**r will have the opportunity **to bid** on a fictional product belonging to the seller. The winning bidder then exchanges the fictional good for a certain number of ECUs. That certain number is referred to as the **bidder's valuation. Sellers then decide** whether they wish **to send the good or not**. If the seller does **not send** the good, the winning **bidder cannot exchange** it for ECUs. The **seller has a cost**, in ECU, that must be **paid in order to send the good**.

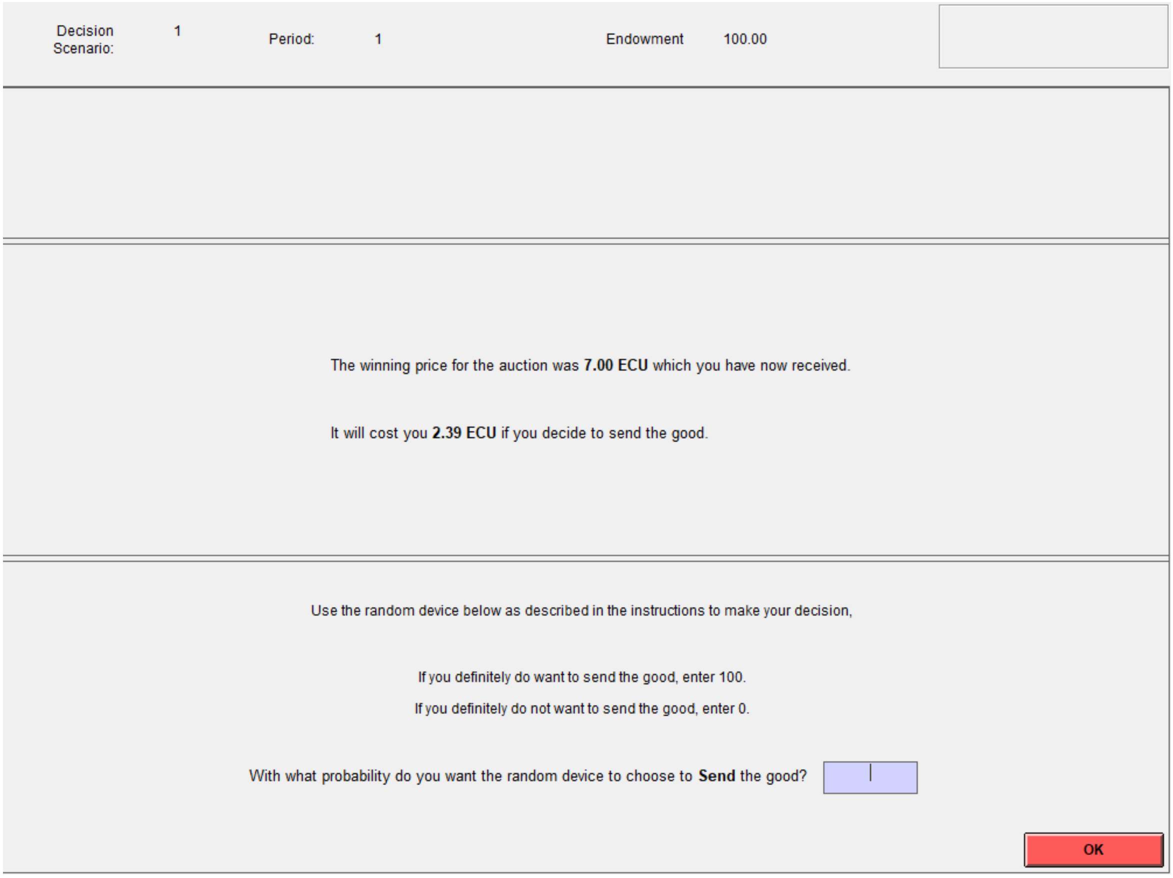
A single period of the **decision scenario** is as follows;

- Each **bidder** will receive a **valuation -** this is the number of ECU **a bidder receives if** they were the **winning bidder** and the **seller sent** the good. For each bidder, this value will be a random number between 5 and 10. The **seller** will receive a **cost associated with sending** the good. This cost will be a random number between 0 and 5.
- Each **bidder will then place a bid** for the fictional good, which can be **any number** (up to 2 decimal places) between 0 and 10. A screen showing this can be seen below;



- The **winning bidder** is the bidder who places the **highest bid**. In the case of a tie, each bidder with the equal highest bid wins with equal probability. The **price** is equal to the **second highest bid**. The price is removed from the winning bidder's endowment of ECUs.

- The **seller** then decides **whether to send the good or not**. The **seller** is **charged their cost** if they choose **to send** the good and **receives the price** from the winning bidder **regardless**. If the **seller** does **not send the good**, then the winning **bidder does not receive the good** and cannot exchange it for their value - even though they have **already paid** for it. If the **seller** does s**end the good**, the **bidder** will **receive the good and can exchange** it for their **valuation** in ECUs. The **seller** makes their decision on a screen that can be seen below;



- Each **bidder** and the **seller** is then informed of **their profit** for the period. A detailed description of profit calculation will be outlined below.

This concludes **a single period of the decision scenario**. As described above, **each decision scenario** will consist of **3 periods**. After the third period, **each group of bidders** will be matched with a **new seller** and a new decision scenario will begin. The **experiment will end** after the **final decision scenario** is concluded.

Profit

We will now go through how profit is calculated in detail.

Bidder Profit

A bidder can only make profit if they were the highest bidder. If a bidder is not the highest bidder, then they receive profit of zero for this round. If the bidder was the highest bidder, the profit is as follows;

Value - Price **if the seller sent the good**

-Price **if the seller did not send the good**

Note that the profit for the highest bidder can be negative if the seller does not send the good. The price is still paid but the good is not received.

Bankruptcy

Since it is possible for a bidder to receive negative profit, a bidder may end up with no ECU left. In this case, the bidder is declared bankrupt and will take no further part in experiment and will only receive the show up fee of £2.50.

Seller Profit

A **seller** can only make a profit if there are any positive bids. If no **bidder** decides to place a bid, then the **seller** receives zero profit for the period. If there are positive bids placed, then the profit for the **seller** is;

Price – Cost if the seller chose to send the good

Price if the seller chose not to send the good

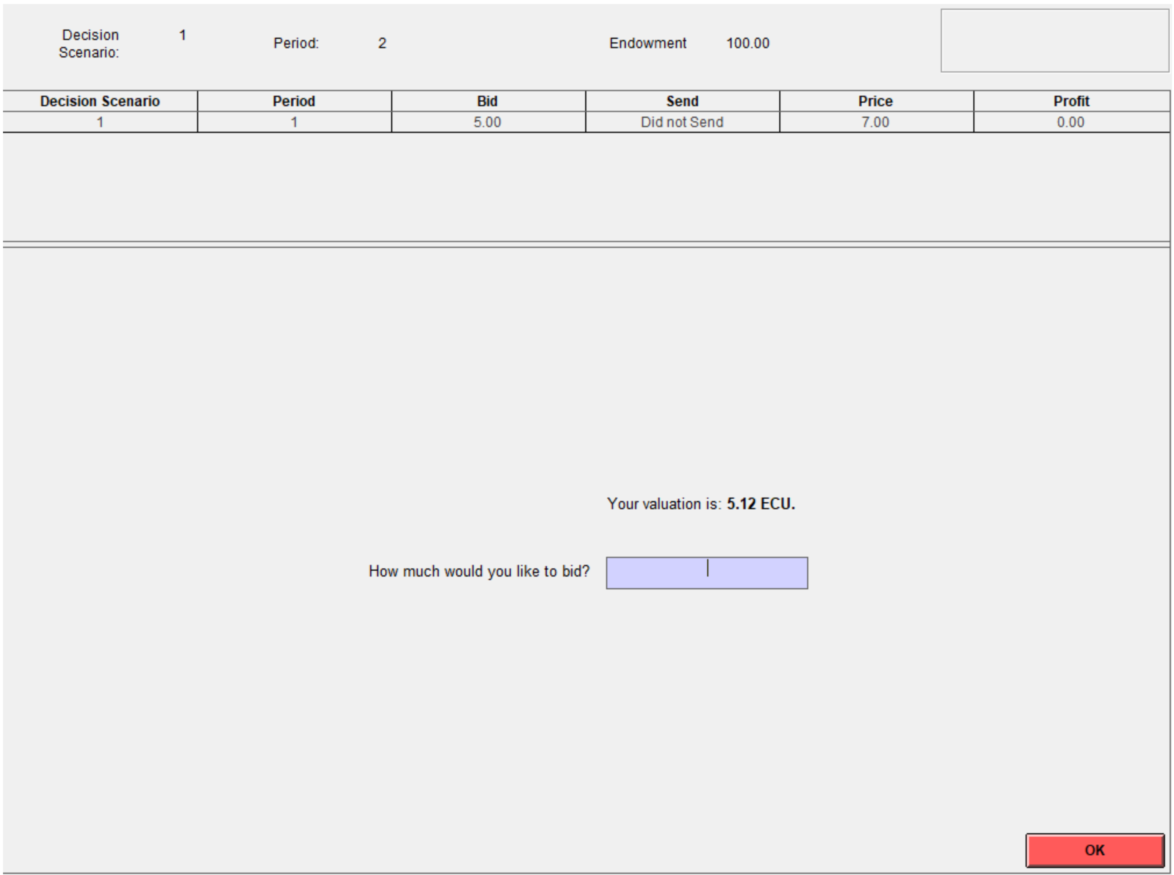
Random Device

Each **seller** can, if they wish, make their decision using a **random device**. For example, **instead of definitely choosing to send the good**, each seller is able to **enter the**

probability with which they would like **to send the good**. Clearly, 100 corresponds to sending the good for sure and 0 corresponds to not sending the good for sure. However, if the seller chooses some probability strictly between 0 and 100 then **the computer will randomly choose whether to send the good or not, according to the chosen probability**.

<u>History</u>

Within a **single decision scenario**, you will be able to see a history of **what has happened in the previous periods** of the particular decision scenario. You **will only** be able to see what happened in **previous decision scenarios that you were part of**. An example screenshot can be seen below.



The history available to the bidder includes the following information;

- **Decision scenario** - The **decision scenario**, out of 4.

- **Period** - The **period**, out of 3.
- **Price** - The **price** in this period.
- **Send** - Indicates **whether the seller sent the good**, or not.
- **Profit** - **Your profit** at the end of the period.
- **Endowment** - **Your total endowment** at the end of the period.

<u>The computer programmed seller</u>

As mentioned above, only **2 of the 4 sellers will be actual subjects**, with the remaining **2 sellers** being operated by a **computer programmed to always send the good**. The computer is programmed to **always send** the good, regardless of the winning price. As mentioned above, each **group of bidders** will take part in **2 decision scenarios involving a 'real' seller** (i.e. another participant) and **2 decision scenarios involving a computer programmed seller**.

Please raise your hand and an experimenter will come and answer your question. Please do not ask questions out loud.

If nobody has any further questions, you will be asked to fill out a series of questions to ensure everybody understands the instructions. You will then be asked to make your decisions for the experiment. You will then be asked to fill out a short questionnaire; you do not have to answer any of these questions if you do not wish.

Control Questions

- You will either be a bidder or a seller. Will this change throughout the experiment? ....
- How many decision scenarios will you take part in?.....................................
- How many periods are in each decision scenario?....................................

Imagine the following scenario. A bidder has a valuation of 9. They place a bid of 8 and are the highest bidder. The price is 7. The seller had a cost of 2 and did send the good.

- How much profit does the bidder receive?...............................
- How much profit does the seller receive?.................................

Imagine the following scenario. A bidder has a valuation of 9. They place a bid of 8 and are the highest bidder. The price is 7. The seller had a cost of 2 and did not send the good.

- How much profit does the bidder receive?...............................
- How much profit does the seller receive?.................................

Appendix 3.B: Additional Regression Analysis

Table 3.1: Tobit regression with the bid as the dependent variable.

Estimation Technique	Random Effects GLS
Dependent Variable	Bid
P0.5	1.107*** (0.313)
P1	2.048*** (0.485)
Value	0.717*** (0.034)
Belief	0.027*** (0.002)
Previous Bid	0.575*** (0.026)
Previous Bid * P0.5	-0.311*** (0.067)
Previous Bid * P1	-0.376*** (0.053)
Previous Sent	0.581*** (0.183)
Constant	-4.554*** (0.264)
N	1188

Note: Random Effects GLS Regression clustered on the period (11 clusters). Robust standard errors are presented in parentheses.

Table 3.2: Tobit regression with the bid as the dependent variable.

Estimation Technique	Random Effects GLS
Dependent Variable	Bid
P0.5	1.107*** (0.563)
P1	2.048*** (0.557)
Value	0.717*** (0.051)
Belief	0.027*** (0.005)
Previous Bid	0.575*** (0.047)
Previous Bid * P0.5	-0.311*** (0.082)
Previous Bid * P1	-0.376*** (0.075)
Previous Sent	0.581*** (0.166)
Constant	-4.554*** (0.472)
N	1188

Note: Random Effects GLS Regression clustered on the group of bidders (36 clusters). Robust standard errors are presented in parentheses.

Chapter 4

Public Goods Games with Structural Heterogeneities in Endowment and Marginal Return

Abstract

We present the results of two public goods experiments (a one-shot and repeated variant) that seek to investigate the interaction between the endowment and marginal return in heterogeneous groups. We implement two novel treatments that vary the relationship between the endowment and marginal return in either an inverse or a proportional way. In the inverse treatment, two normatively appealing contribution rules are in conflict; contributions being proportional to endowment and contributions being proportional to marginal return. In a one-shot setting, we find that those with high income but low public good benefits contribute significantly less than expected by other subjects in their group. In particular, there is also a conflict in expectations between those with low income but high public goods benefit. In a repeated setting, we find that when the relationship is inverse there is equality of absolute contributions and when the relationship is proportional there is equality of contributions as a proportion of the initial endowment (equivalently, the marginal return). Additionally, we consider the welfare and distributional impacts of the public goods mechanism and find that whilst the inverse relation between endowment and marginal return reduces inequality, the proportional relationship does not.

JEL Codes: C92, H41, D63

Keywords: Public goods experiments, Heterogeneous endowment, Heterogeneous return

4.1 Introduction

Many societies can be characterised by an uneven income distribution. For example, according to the Office for National Statistics (ONS), the income distribution of the United Kingdom is such that the top quintile of households have pre-tax income of sixteen times that of the bottom quintile of households (ONS, 2013, p.10). The benefits acquired from the provision of certain public goods are also in many cases not homogeneous. For example, according to the Institute for Fiscal Studies (IFS), approximately 25% of those earning in excess of £100, 000 per annum utilise private education and/or healthcare services (IFS, 2010, p.39/46).[37] This corresponds to average healthcare expenditures of £400 and education expenditures of £4, 000 per annum. By contrast, of those earning £20, 000, only approximately 5% utilise private healthcare and education services. Income and/or the benefits accrued from public goods provision are therefore not homogeneous. Further, it appears that benefits from public goods provision and income may be *inversely* related to one another since higher income is associated with higher procurement of *privately provided* public goods. The purpose of the current study is to investigate how the relationship between income and the benefits from public goods provision impact on pro-social behaviour and ultimately welfare in social dilemmas.

The public goods game (or voluntary contribution mechanism (VCM)) involves a mechanism whereby provision of the public good is beneficial to all in the group, but it is not beneficial to any given individual to provide contributions towards the public good. The benefit each group member receives per unit of public goods provision is known as the marginal per capita return (MPCR) and if the cost of providing one unit of the public good exceeds the MPCR, then it is not individually rational to provide contributions. As such, it is an important construct for understanding the conflict between private and social (or group) benefit that is implicit in many real life social dilemmas. Two of the most fundamental parameters of the public goods game are the initial endowment and MPCR. [38] There have been many studies investigating what behavioural consequences are when either the initial endowment (for example, Kocher et al (2008)) or the MPCR vary (for example, Isaac and Walker (1988), Isaac et al (1994) or Nosenzo et al (2015)) in isolation. A typical finding in the existing literature is that the higher the endowment or the marginal per capita return from the public good, the higher the absolute levels of contributions are. Indeed, in a meta-analysis using 27 studies, Zelmer (2003) finds that the MPCR is

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[37] Naturally, one can imagine specific examples of public goods for which benefits are *necessarily* homogeneous, such as defence spending or street lighting, such examples are not the focus of the current study.

[38] The only other parameter which could feasibly be considered fundamental to the game is the number of players. Whilst the group size has been clearly shown to effect behaviour in public goods games (especially with respect to the effect of the MPCR, see Isaac, Walker and Williams (1994)), the effect of the group size is not the focus of the current study.

crucial in affecting pro-social behaviour in public good game environments. In all the studies reported, the impact of the endowment or the marginal per capita return are examined separately by changing one of these two parameters. However, we know little about whether and if so how the interaction of these two effects shapes behaviour in public good games and subsequently, how welfare changes (as measured by net earnings) are determined in such environments. Given the extant heterogeneities in both income and public goods benefit observed in naturally occurring data, understanding how these heterogeneities interact and affect voluntary contributions is an important and open question in the literature.

The most closely related literature is that which features groups that are heterogeneous in either the endowment or the marginal return. Chan et al (1996) present an experimental test that is broadly supportive of the Bergstrom et al (1986) predictions[39], at least in terms of directions of differences. This result contrasts, however, with Reuben and Riedl (2013) who find no difference in contribution between a 'high' and 'low' endowment type in a baseline control treatment of an investigation into the efficacy of punishment mechanisms in such settings. Reuben and Riedl (2013) also find that in groups with heterogeneous marginal return, those with higher marginal return contribute more, albeit weakly. Further still, van Dijk et al (2002) find heterogeneous endowments may lead to lower contributions by those with a higher endowment. Gächter et al (2017) implement a repeated public goods game with endogenous differences in initial endowments (previous period income carries over throughout the periods). They find that the availability of a punishment mechanism has little effect on contributions. Additionally, Gächter et al (2017) analyse the ex-post income distribution in terms of the Gini coefficient to assess the welfare impacts of the public goods mechanism under various treatment conditions.

In this paper, we present the results of two public goods experiments (the first one-shot and the second repeated) that seek to investigate the interaction between the endowment and the marginal return. The experiment features a comprehensive set of possible relationships between the endowment and marginal return to better understand the interaction between these two fundamental parameters. In the first experiment, we implement a one-shot public goods game in which the relationship between the endowment and marginal return (within a group) is either inverse or proportional, depending on the treatment. In the inverse treatment, those with the highest endowment have the lowest marginal return and vice versa (i.e. negative correlation) whilst in the

[39] Bergstrom et al (1986) derive a theoretical prediction that heterogeneous endowments (and thus income inequality) lead to greater overall contributions to the public good projects that have a strictly interior solution. The experimental evidence on this result is mixed.

proportional treatment those with the highest endowment also have the highest marginal return (i.e. positive correlation). For control purposes, we also include treatments whereby we only vary either the endowment, the marginal return or neither. This allows us to understand the causal pure effects that the endowment and/or the marginal return generate in terms of pro-social behaviour, when reputation and learning effects have been ruled out. In the second experiment, only the inverse and proportional treatments are repeated for 15 periods, to better understand the dynamics and stability of behaviour in our novel settings. It is important to note that once we introduce heterogeneity, then the marginal return is, by definition, not *per capita* anymore.

In the one-shot game, we present results that are broadly in line with the existing literature in terms of the effect of the endowment and marginal return individually. We find that those with high income but low public good benefits contribute significantly less than expected by other subjects in their group. The main findings from these experiments thus indicate a potential conflict between two normatively appealing contribution rules; contributions *proportional to endowment* and contributions *proportional to marginal return*.

To gain a better understanding of the behavioural effects, we conduct a separate series of experiments, whereby we examine how behaviour in the inverse and the proportional treatments is shaped and evolves over time. We find that in a repeated setting, when the marginal return and endowment are inversely related, contributions are such that the *absolute* contributions are not different. When the marginal return and endowment are proportionally related, we find instead that there are no significant differences in contributions *as a proportion of endowment* (equivalently *as a proportion of marginal return*). Further, we find additional evidence of the conflict between equality of contributions *as a proportion of endowment* and *as a proportion of marginal return* in the inverse treatment. In response to this conflict, subjects seem to default to another contribution rule; equality of *absolute* contributions. By contrast, these two normatively appealing contribution rules coincide exactly in the proportional treatment and thus there is no conflict. As such, subjects have contributions that are approximately proportional to both endowment and marginal return throughout the experiment. Notably, whilst the inverse relation leads a significantly improved Gini coefficient in every period, a proportional relationship has no effect on reducing the extant inequality.

We contribute to the experimental literature on public goods games by providing the first experimental study in which both the endowment and marginal return vary simultaneously within a group. This is important given the empirical observations highlighted above. As such, we provide valuable insight into public goods games with heterogeneous groups generally (including the effect of income heterogeneity which as discussed above is not clear across the literature) but also into the precise nature of the interaction between the endowment and the marginal return when both can potentially vary (in particular, the significant differences between an inversely and proportionally related endowment and marginal return). This is potentially important when considering the possibility of comparing different public goods games that do not use identical parameters. Since our results highlight the importance of the interaction between the endowment and marginal return, two experiments that feature different marginal returns or endowments may not be comparable. Further, we provide evidence on the welfare implications of the public goods game under various groups that are heterogeneous in the endowment and/or the marginal return.

In the next section, we provide precise details of the experimental design for the two experiments, in Section 3 we analyse the results of both experiments and in Section 4 we provide discussion and concluding remarks.

4.2 Experimental Design

4.2.1 Experiment 1

In our initial experiment, we conduct a one-shot, six-player public goods game to investigate the relationship (or interaction) between two fundamental parameters of the public goods game; the endowment and the marginal return (commonly called the marginal-per-capita-return or MPCR, but we refrain from using this terminology due to the introduction of heterogeneities that renders the phrase *per-capita* obsolete).

We implement a total of five treatments in which we introduce heterogeneity in; either the **endowment** or the **marginal return** or both or **neither**. Heterogeneity in both endowment and marginal return is implemented in two opposed ways; either the endowment and marginal return are **proportional** or **inversely** proportional to one another.

Subjects earn income according to the standard public goods game profit function (of course, we apply an individual subscript to the endowment and return from project as they are not necessarily homogeneous);

$$U_i = (E_i - X_i) + r_i * \sum_{i=1}^{6} X_i$$

where U_i is payment in ECUs (experimental currency units), E_i is endowment, X_i is contribution and r_i is marginal return.

The treatments are therefore as follows;

Treatment 1 (Control): In the control treatment, all subjects have the same endowment and same marginal return.

Treatment 2 (Heterogeneous Return): Subjects types vary only by Marginal Return. All subjects have the same endowment.

Treatment 3 (Heterogeneous Endowment): Subjects types vary only by Endowment. All subjects have the same Marginal Return.

Treatment 4 (Inverse): Endowments and Marginal Returns are inversely proportional to one another.

Treatment 5 (Proportional): Endowments and Marginal Returns are proportional to one another.

The possible endowments were either 10, 20 or 30 ECUs and the marginal return was either 0.25, 0.5 or 0.75. It is worth noting that the five treatments afford us a complete schema of all possible combinations of the endowment and return from the project (as can be seen in Table 4.1).

Table 4.1: Combinations of Endowment and Marginal Return per Treatment

		Marginal Return		
		0.25	**0.5**	**0.75**
Endowment	**10**	Proportional (Treatment 5)	Heterogeneous Endowment (Treatment 3)	Inverse (Treatment 4)
	20	Heterogeneous Return (Treatment 2)	All Treatments (Treatments 1-5)	Heterogeneous Return (Treatment 2)
	30	Inverse (Treatment 4)	Heterogeneous Endowment (Treatment 3)	Proportional (Treatment 5)

In particular, each subject can be thought of as having a 'type' that consists of an (Endowment, Marginal Return) pair, for example (20, 0.5) referring to Endowment = 20 and Marginal Return = 0.5. In treatments 2-5, a total of three types therefore exist and there are exactly two of each type in each group (for a total of six subjects per group). Treatment 1 features only one type and thus all six subjects have the same type. Six subjects were used per group such that we could elicit a given subject's belief about others with the same type as themselves. A full overview of the types available in each of the treatments can be seen in Table 4.2. In this sense, a total of nine types exist across the five treatments with the baseline (20, 0.5) type present in all treatments. Across the five treatments both the sum of endowments and the sum of marginal returns are identical – eliminating the possibility of any observed differences being due to wealth or social efficiency effects. The sum of endowments represents the total wealth in the 'economy' and by keeping it constant (at 120 ECU), we are only changing the *distribution* of wealth within the economy. The sum of marginal returns represents the number of tokens generated per token contributed (in that sense, a measure of the social efficacy of contributions) and is similarly kept constant (at a 3:1 ratio) – this is especially important in the presence of pro-social preferences. It is worth noting that the fundamental structure of the public goods game remains intact across all types in all treatments – free riding is still a dominant strategy for all types in all treatments.

Table 4.2: An Overview of Group Composition per Treatment

Treatment	**Control**		**Heterogeneous Return**		**Heterogeneous Endowment**		**Inverse**		**Proportional**	
Subject	**E**	**M.R**	**E**	**M.R**	**E**	**M.R**	**E**	**M.R**	**E**	**M.R**
1	20	0.5	20	0.25	10	0.5	10	0.75	10	0.25
2	20	0.5	20	0.25	10	0.5	10	0.75	10	0.25
3	20	0.5	20	0.5	20	0.5	20	0.5	20	0.5
4	20	0.5	20	0.5	20	0.5	20	0.5	20	0.5
5	20	0.5	20	0.75	30	0.5	30	0.25	30	0.75
6	20	0.5	20	0.75	30	0.5	30	0.25	30	0.75
Total	120	3	120	3	120	3	120	3	120	3

At the beginning of each session, instructions (a copy of which can be found in an appendix) were read aloud to all subjects. Subjects were then required to answer some control questions to ensure that everybody understood the instructions. Subjects were randomly assigned types and each subject was informed of their own type. Subjects then made their contribution decisions. Beliefs were then elicited in an incentivised manner. Subjects were asked to estimate the average contribution of the other subjects in their group *for all possible types* i.e. in treatments 2-5 subjects were asked for three separate estimates – one for each of the three possible types. The beliefs were incentivised using a step-loss function[40] (closer estimates earned more money and sufficiently incorrect estimates earned nothing). Subjects were then informed of their earnings for the experiment and asked to fill out a short demographic questionnaire that concluded the experiment.

A total of 312 subjects took part across all five treatments.[41] Each subject participated in only one treatment. All subjects were recruited using ORSEE (Greiner, 2004) and the experimental software was programmed using zTree (Fischbacher, 2007). All sessions were run at BEEL at the University of Birmingham (UK) and all subjects were students at the university from across a wide range of disciplines. Sessions lasted on average 50 minutes and subjects received an average payment of £9.38 (including a £2.50 show-up fee).

4.2.2 Experiment 2

In an attempt to investigate the validity and dynamic stability of the results of Experiment 1, we ran a set of additional experiments. In these experiments, Treatments 4 (Inverse) and 5 (Proportional) from Experiment 1 (the novel treatments of this paper) were repeated for 15 periods within fixed groups that remained the same throughout the entire experiment. We refer to these as Treatments 4R and 5R respectively. All other parameterisation was identical to Experiment 1. Subjects were paid according to one randomly selected period to avoid income effects accruing throughout the experiment. A total of 120 subjects[42] took part across the two treatments and all other procedures were identical to those of Experiment 1. Sessions lasted on average 80 minutes and subjects received an average payment of £9.82 (including a £2.50 show-up fee).

[40] For a given belief X tokens and true value X^* tokens, the subject was paid £1 if $|X^* - X| \leq 0.5$ tokens, £0.60 if $0.5 < |X^* - X| \leq 1.5$ tokens and £0.30 if $1.5 < |X^* - X| \leq 3.5$ tokens. Beliefs further than 3.5 tokens from the true value received nothing.

[41] The Control (1) treatment had 6 groups (36 participants). The Heterogeneous Return (2) and Proportional (5) treatments had 11 groups each (66 participants each). The Heterogeneous Endowment (3) and Inverse (4) treatments had 12 groups each (72 participants each).

[42] The Inverse Repeated had 11 groups (66 participants). The Proportional Repeated had 9 groups (54 participants).

4.3 Results

We begin with a comprehensive analysis of the results of Experiment 1 before presenting a complementary analysis for Experiment 2.

4.3.1 Experiment 1

4.3.1.1 Summary Statistics

An overview of the aggregate results for Experiment 1 can be seen in Table 4.3 which presents the average contribution and belief across all five treatments. We conduct tests for significant differences in contribution levels between treatments (a full table of test statistics is available as an Appendix, we take as the unit of observation the individual subject since the game is one-shot). Contributions in the Heterogeneous Return treatment are marginally higher than the Control (Mann-Whitney Rank Sum: p = 0.052) and Inverse (Mann-Whitney Rank Sum: p = 0.066) treatments. No other differences in overall contributions are significant (Mann-Whitney Rank Sum: p ≥ 0.238). Similarly, the overall beliefs[43] are not significantly different between any treatments (Mann-Whitney Rank Sum: p ≥ 0.426). This is perhaps unsurprising given that the game is one-shot and that the *average* type is the same in all treatments (i.e. (20, 0.5)). We thus state our first formal result.

Table 4.3: Summary Statistics for Experiment 1

Treatment	Observations	Contribution	Belief
Control	36	4.22	6.67
Heterogeneous Return	66	6.27	6.48
Heterogeneous Endowment	72	6.09	6.77
Inverse	72	4.58	6.37
Proportional	66	5.92	6.99
All	312	5.53	6.65

<u>Result 1:</u> There are no substantial overall treatment differences in contributions or beliefs (Experiment 1).

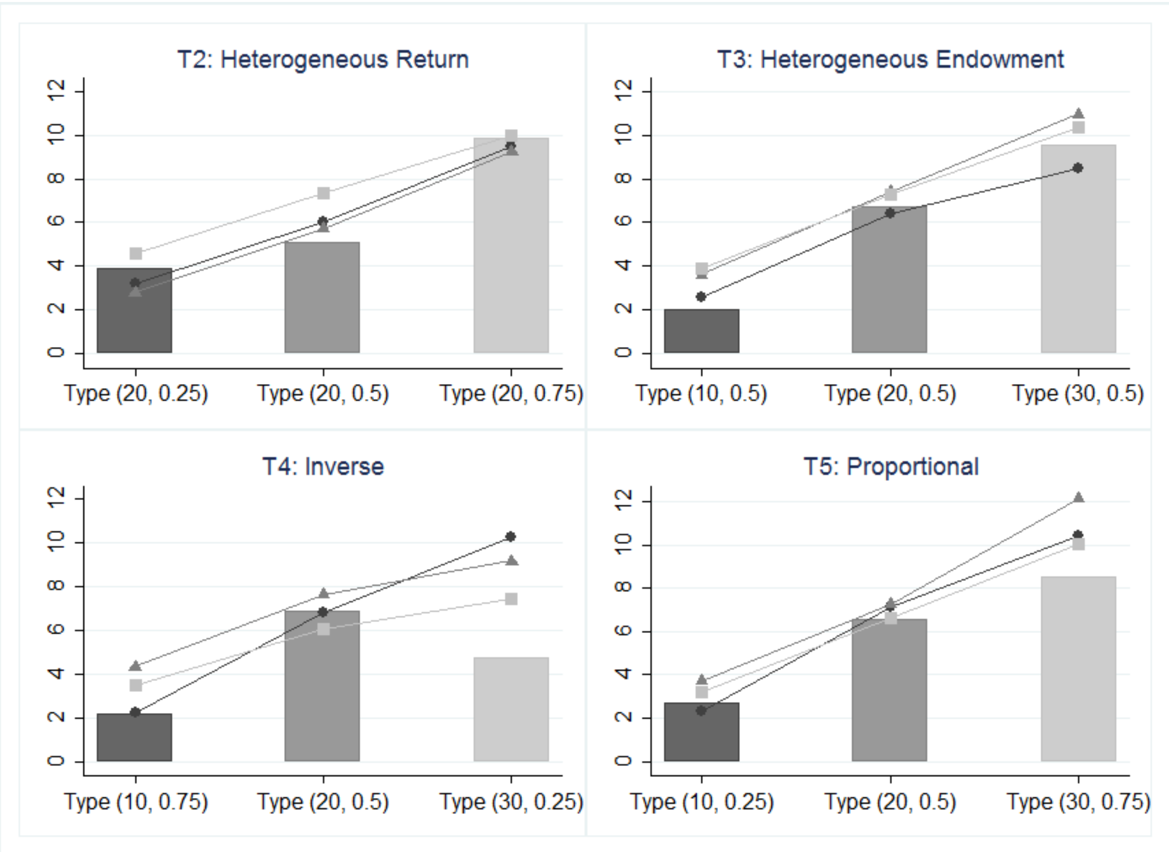
[43] The overall belief is an equally weighted average of the three belief estimates where appropriate (i.e. in all but the control treatment).

Therefore, there is little difference in aggregate behaviour across the five treatments. We now move on to a more detailed breakdown of the results within each treatment, parsing the results by type.

4.3.1.2 Within-Treatment Difference

Table 4.4 shows the contribution within each treatment according to the subjects' type. In addition, it shows the contribution as a proportion of the endowment. Further, Table 4.5 presents test statistics for within-treatment differences in contributions between the various types present. In both the Heterogeneous Return and Heterogeneous Endowment treatments, contributions are (weakly) monotonically increasing in the marginal return and endowment, respectively. The effect of the marginal return in heterogeneous groups is similar to that in Reuben and Riedl (2013), though we also find a significant effect of the endowment in heterogeneous groups that Reuben and Riedl (2013) do not.

Figure 4.1: Average Contribution For, and Belief about, each Type by Treatment (Experiment 1).



Note: Each bar represents the average contribution of the labelled type. Each line represents the beliefs *by* the type with the same coloured bar. [I.e. the darker line in the T2 panel is the beliefs *by* the (20, 0.25) type about the three types present in the T2 treatment.]

Similarly, in the Proportional treatment, as both the marginal return and endowment increase together, contributions (weakly) monotonically increase. This is perhaps not unexpected given that it is a compounding of two positive effects on contribution. Behaviour in the Inverse treatment is however more interesting, with only the (10, 0.75) and (20, 0.5) types being significantly different. It is worth noting that the contribution *as a proportion of endowment* (see Table 4.4) is higher for the (20, 0.5) type at 0.34 than the (30, 0.25) type at 0.16. This difference is statistically significant (Mann-Whitney Rank Sum: p = 0.027).[44]

Table 4.4: Summary Statistics for each Treatment by Type (Experiment 1).

Treatment	Type	Contribution…	…as Proportion of Endowment
Control	**(20, 0.5)**	4.22	0.21
Heterogeneous Return	**(20, 0.25)**	3.91	0.20
	(20, 0.5)	5.09	0.25
	(20, 0.75)	9.82	0.49
Heterogeneous Endowment	**(10, 0.5)**	2.04	0.20
	(20, 0.5)	6.71	0.34
	(30, 0.5)	9.54	0.32
Inverse	**(10, 0.75)**	2.17	0.22
	(20, 0.5)	6.83	0.34
	(30, 0.25)	4.75	0.16
Proportional	**(10, 0.25)**	2.68	0.27
	(20, 0.5)	6.55	0.33
	(30, 0.75)	8.55	0.29

<u>Result 2:</u> **Within each treatment, contributions increase when the endowment, marginal return or both increase in all treatments except the Inverse treatment (Experiment 1).**

Subjects were asked their beliefs about the average contribution for each type present.[45] A summary of subjects' responses can be seen in Table 4.6. This information, in addition to the average

[44]The contribution as a proportion of the endowment is not different between the (10, 0.75) and (20, 0.5) types (p=0.146). In all other treatments, the results remain the same as Table 4.3 in terms of significance.
[45] As above, the belief elicitation was incentivised.

contribution decisions, can also be seen in Figure 4.1. As can be seen, the beliefs are largely consistent with behaviour despite slight over-optimism. It is worth noting that for all types in all treatments, beliefs are monotonically increasing in endowment (or marginal return when endowment is homogeneous, as in the Heterogeneous Return treatment). Beliefs increase in endowment regardless of whether the marginal return co-varies positively or negatively. This difference is significant in all treatments[46] (Wilcoxon Signed-Rank: $p \leq 0.006$ in all cases, we take the group as the unit of observation since we are implicitly comparing beliefs by the same subjects). This suggests that the endowment 'dominates' the marginal return in determining subjects' beliefs (though notably these beliefs are incorrect in the Inverse treatment for the (30, 0,25) type).

Table 4.5: Mann-Whitney Rank Sum Test z-Statistics for Within-Treatment Differences in Contributions (Experiment 1).

Heterogeneous Return			Heterogeneous Endowment		
Type	**(20, 0.5)**	**(20, 0.75)**	**Type**	**(20, 0.5)**	**(30, 0.5)**
(20, 0.25)	-0.558	-3.325***	**(10, 0.5)**	-2.679***	-3.730***
(20, 0.5)	-	-2.767***	**(20, 0.5)**	-	-1.101
Inverse			**Proportional**		
Type	**(20, 0.5)**	**(30, 0.25)**	**Type**	**(20, 0.5)**	**(30, 0.75)**
(10, 0.75)	-3.049***	-1.316	**(10, 0.25)**	-2.155**	-2.173**
(20, 0.5)	-	1.248	**(20, 0.5)**	-	-0.272

Note: The test is the two-tailed Wilcoxon rank-sum (Mann-Whitney-U) test. The figures presented are the *p*-values.

<u>Result 3:</u> Overall beliefs *about* each type's contribution increase as that types' endowment increases, or as the marginal return increases if the endowment is homogeneous (Experiment 1).

Interestingly, there are also some within-treatment differences in beliefs. These can be seen in Table 4.7. None of the types present in the Heterogeneous Return treatment show significantly different beliefs. Similarly, in the Proportional treatment none of the types have significantly different beliefs about any types. There are some significant differences in the Heterogeneous Endowment case; in

[46] Beliefs are actually monotonically increasing in endowment in *every group*.

both cases where the difference is significant[47], the (10, 0.5) type is being significantly more pessimistic in the sense that they expect lower contributions.

Table 4.6: Beliefs *by* each Type, *about* each Type, by Treatment (Experiment 1)

Heterogeneous Return	Type	Belief About			
		(20, 0.25)	(20, 0.5)	(20, 0.75)	Total
Belief By	(20, 0.25)	3.18	6	9.5	6.22
	(20, 0.5)	2.81	5.72	9.23	5.92
	(20, 0.75)	4.59	7.36	9.95	7.30
	Total	3.53	6.36	9.56	
Heterogeneous Endowment	**Type**	**Belief About**			
		(10, 0.5)	(20, 0.5)	(30, 0.5)	Total
Belief By	(10, 0.5)	2.58	6.38	8.5	5.81
	(20, 0.5)	3.63	7.42	10.96	7.33
	(30, 0.5)	3.88	7.29	10.33	7.16
	Total	3.36	7.03	9.93	
Inverse	**Type**	**Belief About**			
		(10, 0.75)	(20, 0.5)	(30, 0.25)	Total
Belief By	(10, 0.75)	2.21	6.79	10.25	6.42
	(20, 0.5)	4.33	7.63	9.17	7.04
	(30, 0.25)	3.46	6.04	7.42	5.63
	Total	3.33	6.81	8.94	
Proportional	**Type**	**Belief About**			
		(10, 0.25)	(20, 0.5)	(30, 0.75)	Total
Belief By	(10, 0.25)	2.32	7.14	10.45	6.63
	(20, 0.5)	3.73	7.27	12.14	7.71
	(30, 0.75)	3.18	6.64	10.05	6.62
	Total	3.08	7.02	10.88	

[47] The (10, 0.5) and (30, 0.5) types have significantly different beliefs about the (10, 0.5) type. The (10, 0.5) and (20, 0.5) types have significantly different beliefs about the (30, 0.5) type. See Table 4.7.

The most interesting case, however, is the Inverse treatment. In this case, we witness a significant `crossing' effect in beliefs between the (10, 0.75) and (30, 0.25) types, such that each of these two types has higher beliefs about how much the other will contribute then how much the other thinks they should contribute themselves. This can also be seen visually in Figure 4.1. For example, the (10, 0.75) have beliefs of 2.21 about *their own* type, whereas the (30, 0.25) type have beliefs of 3.46 about the (10, 0.25). Thus the (30, 0.25) type believes the (10, 0.75) type will contribute more than they

Table 4.7: Test-statistics for differences in beliefs *about* each type *between* each type (Experiment 1).

Hetero. Return	**Beliefs About (20, 0.25)**		**Beliefs About (20, 0.5)**		**Beliefs About (20, 0.75)**	
Type	**(20, 0.5)**	**(20, 0.75)**	**(20, 0.5)**	**(20, 0.75)**	**(20, 0.5)**	**(20, 0.75)**
(20, 0.25)	0.231	-1.354	0.790	-1.254	0.595	-1.022
(20, 0.5)	-	-1.624	-	-1.808*	-	-0.595
Hetero. Endowment	**Beliefs About (10, 0.5)**		**Beliefs About (20, 0.5)**		**Beliefs About (30, 0.5)**	
Type	**(20, 0.5)**	**(30, 0.25)**	**(20, 0.5)**	**(30, 0.5)**	**(20, 0.5)**	**(30, 0.5)**
(10, 0.5)	-1.454	-1.945	-0.666	-1.072	-1.650*	-0.984
(20, 0.5)	-	-0.786*	-	-0.116	-	0.434
Inverse	**Beliefs About (10, 0.75)**		**Beliefs About (20, 0.5)**		**Beliefs About (30, 0.25)**	
Type	**(20, 0.5)**	**(30, 0.25)**	**(20, 0.5)**	**(30, 0.25)**	**(20, 0.5)**	**(30, 0.25)**
(10, 0.75)	-2.753***	-1.890	-0.841	0.783	0.839	2.026**
(20, 0.5)	-	1.108*	-	1.275	-	0.753
Proportional	**Beliefs About (10, 0.25)**		**Beliefs About (20, 0.5)**		**Beliefs About (30, 0.75)**	
Type	**(20, 0.5)**	**(30, 0.75)**	**(20, 0.5)**	**(30, 0.75)**	**(20, 0.5)**	**(30, 0.75)**
(10, 0.25)	-1.812*	-1.022	-0.033	0.430	-0.824	0.033
(20, 0.5)	-	0.895	-	0.464	-	0.888

themselves believe. In contrast, the (30, 0.25) type have beliefs of 7.42 about *their* own type, whereas the (10, 0.75) type have beliefs of 10.25 about the (30, 0.25) type. Thus the (10, 0.75) type believes the (30, 0.25) type will contribute more than they themselves believe. This difference is significant in both directions (see Table 4.7; Mann-Whitney Rank Sum: p = 0.059 and p = 0.043 respectively). No other treatment has this `crossing' effect in terms of beliefs between any of the types. Thus, beliefs in the Inverse treatment appear to be self-serving. As noted in Reuben and Riedl (2013), both contributions being *proportional to endowment* and contributions being *proportional to marginal return* represent significant and focal contribution rules. It is worth noting that the Inverse treatment is the only treatment in which these two are directly opposed, leading to tensions that are characteristic of the tensions present in the empirical evidence on the link between income and the benefits from (certain) public goods.

Result 4: Beliefs *about* each type *by* each type are largely not different except in the Inverse treatment. In the Inverse treatment, we witness significantly different beliefs between the (10, 0.75) and (30, 0.25) types about each other in a 'self-serving' manner.

To summarise the results of Experiment 1, whilst we find little difference in aggregate behaviour between the various treatments, we do find important and significant differences both within treatments and between treatments at the individual level (i.e. accounting for the subjects' types). To investigate both the stability and the robustness of these results, a second experiment was designed in which the two novel (Inverse and Proportional) treatments were repeated for 15 periods, as outlined in detail in Section 2.

4.3.2 Experiment 2

In this section, we provide an analysis of Experiment 2 - the repeated version of Experiment 1 for the Inverse and Proportional treatments - in a manner analogous to the analysis of Experiment 1.

4.3.2.1 Summary Statistics

A summary of the average contribution and belief across all types and all periods can be seen in Table 4.8. The difference in average contributions across the two treatments is not significant (Mann-Whitney Rank-Sum: p = 0.239*)*. Similarly, the difference in average beliefs across the two treatments is also not significant (Mann-Whitney Rank-Sum: p = 0.342). We turn now to more detailed analysis of within-treatment differences across types.

Result 5: There are no overall differences in contributions or beliefs between the two treatments (Experiment 2).

Table 4.8: Summary Statistics for Experiment 2

Treatment	Observations	Contribution	Belief
Inverse	9	4.17	4.30
Proportional	11	5.34	5.33
All	20	4.70	4.77

Note: Each observation refers to a group of 6 subjects.

4.3.2.2 Within-Treatment Differences

Table 4.9 shows the contribution and the contribution as a proportion of the endowment within each treatment, according to the subjects' type. There is no significant difference in contributions between any of the types present in the Inverse Repeated treatment (Wilcoxon Signed-Rank: $p \geq 0.722$ in all cases). In the Proportional Repeated treatment, the (10, 0.25) type contributes significantly less than both the (20, 0.5) and (30, 0.75) types (Wilcoxon Signed-Rank: $p = 0.066$ and $p = 0.015$ respectively). The (20, 0.5) and (30, 0.75) types are not significantly different (Wilcoxon Signed-Rank: $p = 0.110$). This mirrors exactly the pattern found in the one-shot Proportional treatment.

Table 4.9: Summary Statistics for each Treatment by Type (Experiment 2).

Treatment	Type	Contribution…	…as Proportion of Endowment
Inverse	**(10, 0.75)**	4.21	0.42
	(20, 0.5)	3.99	0.20
	(30, 0.25)	4.34	0.14
Proportional	**(10, 0.25)**	2.91	0.29
	(20, 0.5)	5.14	0.26
	(30, 0.75)	7.97	0.27

It is worth noting that whilst the actual contribution is not significantly different between any of the types in the Inverse Repeated treatment, the (10, 0.75) type contributes significantly more *as a proportion of the endowment*, compared to both the (20, 0,5) and (30, 0.25) types (Wilcoxon Signed-Rank: $p = 0.004$ and $p = 0.013$ respectively). On the other hand, the contribution *as a proportion of the endowment* is not significantly different between any of the types in the Proportional treatment (Wilcoxon Signed-Rank: $p \geq 0.679$), despite significant differences in absolute contributions. This may be due to the establishment of differing fairness norms between the two treatments (for example, equity of contributions in the Inverse treatment and equity of contributions *as a proportion of endowment* in the Proportional treatment).

<u>Result 6:</u> There are no significant differences in contributions between the types in the Inverse treatment. In the Proportional treatment, the type with low endowment and marginal return contributes significantly less than the other types.

We now look at overall beliefs between types. Only the difference between the (10, 0.75) and (20, 0.5) types in the Inverse Repeated treatment is significant (Wilcoxon Signed-Rank: $p=0.075$). No other differences in overall beliefs are significant ($p \geq 0.168$ in all cases, see the Appendix). This is mostly consistent with the one-shot experiment and the fact that the *average* type is (20, 0.5) in both treatments.

Once again, subjects were asked their beliefs about the average contribution for each type present[48]. A summary of subjects' responses can be seen in Table 4.10. As can be seen, there is very little difference in beliefs about each of the types in the Inverse treatment (Wilcoxon Signed-Rank: $p > 0.328$ for 7 of the 9 tests). The only significant differences are between the beliefs about the (10, 0.75) and (20, 0.5) types. On the other hand, all but one of the differences are significant in the Proportional treatment, showing that beliefs increase as both the endowment and marginal return increases (Wilcoxon Signed-Rank: $p \leq 0.038$ for 8 of the 9 tests).

<u>Result 7:</u> Overall beliefs *about* each type are largely not different in the Inverse Repeated treatment. Overall beliefs *about* each type increase in that types' endowment (and marginal return) in the Proportional Repeated treatment.

There are few significant differences between any of the types in terms of their beliefs about the other types in either treatment. Interestingly, in the Inverse Repeated treatment, both the (10, 0.75) and (30, 0.25) have significantly higher beliefs about the (20, 0.5) than the (20, 0.5) have about their own type (Wilcoxon Signed-Rank: $p=0.026$ and $p=0.041$, respectively). Notably, the `crossing' effect that was witnessed in the one-shot Inverse treatment (and was interpreted as 'self-serving beliefs') is no

[48] As before, the belief elicitation remained incentivised, though a random period was chosen for payment.

longer present (Wilcoxon Signed-Rank: p≥0.248 in all other cases in the Inverse Repeated treatment). In the Proportional Repeated treatment, only one difference is (weakly) significant; the beliefs of the (20, 0.5) and (30, 0.75) about the (10, 0.25) type (Wilcoxon Signed-Rank: p=0.066). No other differences are significant (Wilcoxon Signed-Rank: p≥0.110 in all cases).

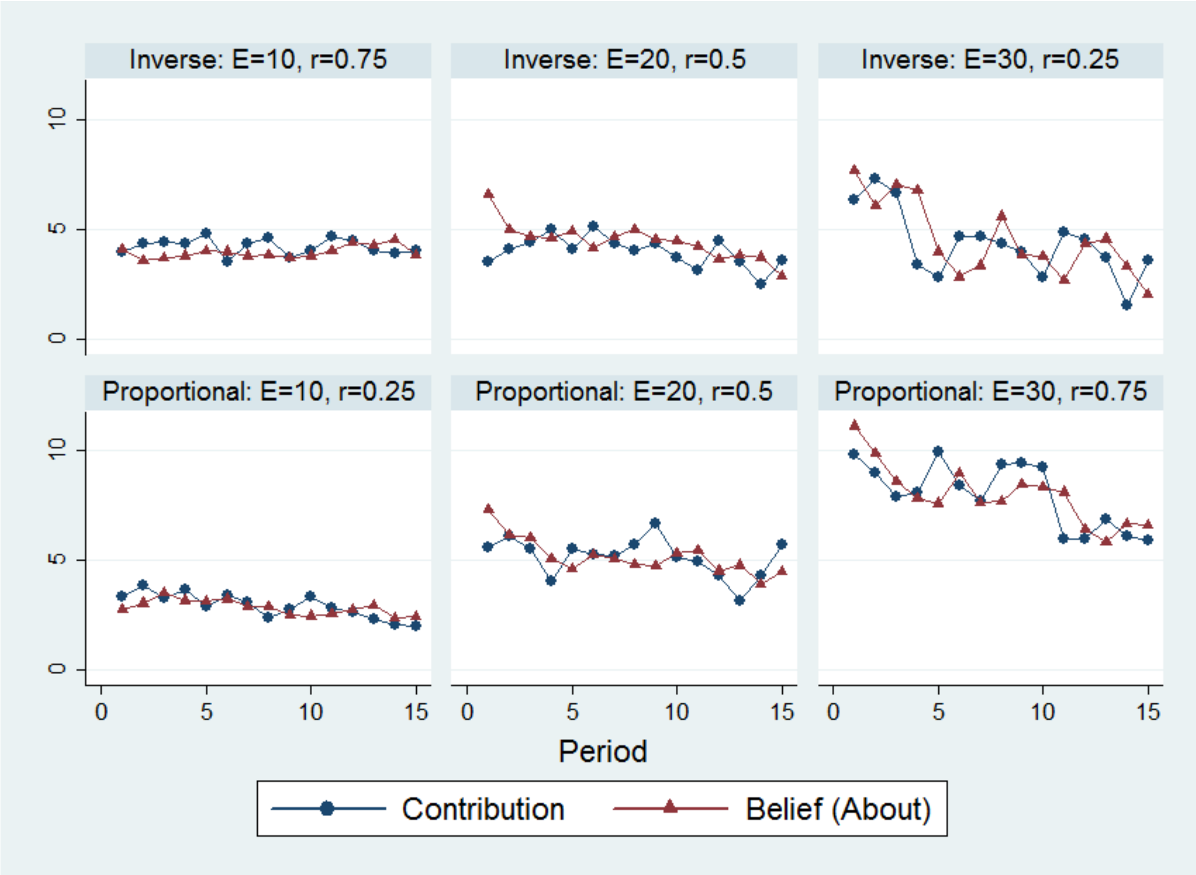
Table 4.10: Beliefs *by* each Type, *about* each Type, by Treatment (Experiment 2).

Inverse	**Type**	**Belief About**			
		(10, 0.75)	**(20, 0.5)**	**(30, 0.25)**	**Total**
Belief By	**(10, 0.75)**	3.95	4.80	4.79	4.51
	(20, 0.5)	3.98	3.85	4.55	4.12
	(30, 0.25)	3.91	4.70	4.21	4.27
	Total	3.94	4.45	4.52	
Proportional	**Type**	**Belief About**			
		(10, 0.25)	**(20, 0.5)**	**(30, 0.75)**	**Total**
Belief By	**(10, 0.25)**	2.69	5.33	8.19	5.40
	(20, 0.5)	2.81	5.15	8.54	5.50
	(30, 0.75)	3.03	5.01	7.21	5.08
	Total	2.84	5.16	7.98	

In the Proportional Repeated treatment, only 12.35% (100/810) of subjects believe the (30, 0.75) type will contribute less than the (10, 0.25) endowment type. By contrast, in the Inverse Repeated treatment 41.11% (407/990) of subjects believe the (30, 0.25) type will contribute less than the (10, 0.75) type. Similarly, in Inverse Repeated, 9.49% (94/990) believe contributions will be equal whereas in Proportional Repeated this is only 3.46 % (28/810). This lends support to the suggestion that the Inverse and Proportional Repeated treatments feature different focal points – the equality of *absolute* versus *relative* contributions. As was noted before, the Inverse treatment features a conflict between contributions that are *proportional to endowment* and contributions that are *proportional to marginal return*. In this case it may be that subjects use an *equality of contributions* rule previously contribution rules identified in Reuben and Riedl (2013).

To summarise Experiment 2, the Inverse and Proportional treatments from Experiment 1 were repeated for 15 periods. Whilst in the Inverse Repeated treatment *absolute* contributions are the same for all types, in the Proportional Repeated treatment it is *relative* contributions that are not different between the various types. Subjects' beliefs across the two treatments are also consistent with this observation. We now move on to assess the welfare implications of the voluntary contribution mechanisms under our various treatment conditions.

Figure 4.2: Average Contribution by and Belief about each type across the 15 Periods for each Treatment (Experiment 2).



4.3.3 Inequality, Welfare and Income (Re-)Distribution

In this subsection, we wish to consider the distributional impacts of the voluntary contribution mechanism under our treatment conditions and the differing initial income distributions. To do this, we calculate the *ex-ante* Gini coefficient for each treatment and compare it with the Gini coefficient of the *ex-post* income distribution.[49] In this way, we can see whether the voluntary contribution mechanism has a positive or negative effect on inequality in terms of income (re-)distribution, at least as measured by the Gini coefficient.

We first consider the average *ex-post* income for each type within each treatment. This information can be seen in Table 4.11, which shows the *ex-ante* endowment[50], the *ex-post* income and the percentage difference. In all cases the average *ex-post* income is higher than the *ex-ante* endowment such that all types in all treatments benefit from the mechanism on average. In all but the Heterogeneous Return and Proportional treatments, the types with the lowest endowment experience the greatest percentage increase between their initial endowment and final income.

In the Heterogeneous Return treatment, the percentage increase in *ex-post* income compared to *ex-ante* endowment increases with the marginal return. This is due the nature of the voluntary contribution mechanism (recall that the (20, 0.75) have significantly higher contributions but still experience the largest percentage increase in income). By contrast, in the Heterogeneous Endowment treatment, the increase in income is decreasing in the endowment. Since all subjects have the same marginal return, this is again consistent with the observed contributions.

With regards to the Inverse and Proportional treatments, the first thing to note is the similarity between the one-shot and repeated treatments. In both the Inverse and Inverse Repeated treatments, the percentage increase in income is decreasing in endowment (and thus increasing in marginal return). The Proportional and Proportional Repeated treatments, however, have similar increases in the percentage increase in income for all the types. This is once again consistent with the observed behaviour. In the Proportional treatments, contributions as a proportion of endowment are not significantly different and thus the percentage increase in income is not different. In the Inverse treatments, since contributions are not significantly different the types with the lowest endowments receive the biggest percentage increase as a *proportion* of their original income. This is further compounded by the lowest endowment types having the highest marginal return.

[49] For each group and for each period where appropriate.
[50] Which is, of course, directly determined by the subjects' type.

Table 4.11: Ex-Ante Endowment, Ex-Post Income and % Difference for each Type (Experiment 1 and 2)

Treatment	Type	Ex-Ante	Ex-Post	% Increase
Control	**(20, 0.5)**	20	28.44	42.2
Heterogeneous Return	**(20, 0.25)**	20	25.50	27.5
	(20, 0.5)	20	33.73	68.65
	(20, 0.75)	20	38.40	92.00
Heterogeneous Endowment	**(10, 0.5)**	10	26.25	162.50
	(20, 0.5)	20	31.58	57.90
	(30, 0.5)	30	38.75	29.17
Inverse (One-Shot)	**(10, 0.75)**	10	28.46	184.60
	(20, 0.5)	20	26.91	34.55
	(30, 0.25)	30	32.13	7.10
Proportional (One-Shot)	**(10, 0.25)**	10	16.20	62.00
	(20, 0.5)	20	31.23	56.15
	(30, 0.75)	30	48.11	60.37
Inverse (Repeated)	**(10, 0.75)**	10	24.59	145.90
	(20, 0.5)	20	28.54	42.70
	(30, 0.25)	30	31.92	6.40
Proportional (Repeated)	**(10, 0.25)**	10	15.09	50.90
	(20, 0.5)	20	30.89	54.45
	(30, 0.75)	30	46.06	53.53

Table 4.12 shows for each treatment both the *ex-ante* and *ex-post* Gini coefficient calculated using the *ex-ante* endowment and *ex-post* income distribution, as appropriate. Both the Control and Heterogeneous Return treatments have an initial Gini coefficient of 0 since all subjects have the same endowment. In both cases the voluntary contribution mechanism is therefore inequality increasing. In all other treatments, the initial Gini coefficient is 0.22. Of these, all treatments except the Proportional treatments have a positive effect on inequality as interpreted by a reduced Gini coefficient (i.e. it reduces the level of inequality). The Proportional treatments leave the Gini coefficient at largely the

same level, which is entirely consistent with all types having similar % increase in income due to the nature of the Gini coefficient calculation[51].

Table 4.12: Inequality and Efficiency Measures (Experiments 1 and 2).

Treatment	Ex-Ante Gini Coefficient	Ex-Post Gini Coefficient	Efficiency
Control	**0**	**0.074**	**21.11%**
Heterogeneous Return	**0**	**0.107**	**31.36%**
Heterogeneous Endowment	**0.222**	**0.132**	**30.48%**
Inverse (One-Shot)	**0.222**	**0.111**	**22.92%**
Proportional (One-Shot)	**0.222**	**0.245**	**29.61%**
Inverse (Repeated)	**0.222**	**0.141**	**20.89%**
Proportional (Repeated)	**0.222**	**0.238**	**26.71%**

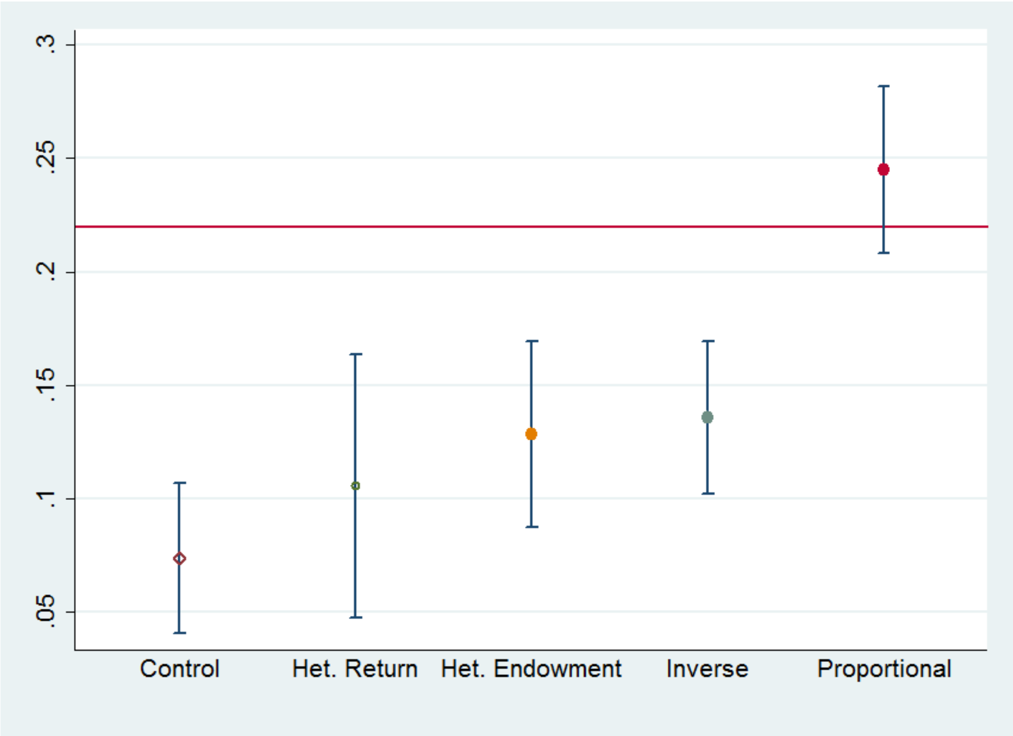
Note: The *ex-ante* Gini coefficient is calculated using the initial endowments. The *ex-post* Gini coefficient is calculated using the final income distribution (in ECUs). Efficiency is calculated as the percentage of maximum possible ECUs (120) contributed to the project, which is equivalent to the maximum potential gains realised.

Figure 4.3 displays the 95% confidence intervals for the Gini coefficient for each of the one-shot treatments (i.e. Experiment 1). As above, the Control and Heterogeneous Return treatments feature an increase in Gini since the initial Gini was 0. As can also be seen, the Gini coefficient in the Proportional treatment is not significantly different from the initial value of 0.22. In both the Heterogeneous Endowment and Inverse treatment the Gini is significantly reduced from the initial value of 0.22. Similarly, Figure 4.4 displays the 95% confidence intervals for the repeated treatments (i.e. Experiment 2) on a per period basis. The 95% confidence intervals do not overlap for any of the 15 periods. As in Experiment 1, the Proportional Repeated treatment results in no change in the Gini

[51] Which effectively compares the cumulative proportion of all income earned by each cumulative proportion of the population. For example, if all incomes double, the Gini coefficient will remain the same since the cumulative proportions of total income (at any given cumulative proportion of the population) will be unaffected.

coefficient from the initial value in any of the 15 periods. By contrast, in the Inverse Repeated treatment the Gini is significantly reduced in every single period. Thus, whilst the Inverse treatment features a significant reduction from the initial inequality, the Proportional treatment leaves this initial inequality the same.

Figure 4.3: Average Ex-Post Gini Coefficient for each Treatment (Experiment 1).

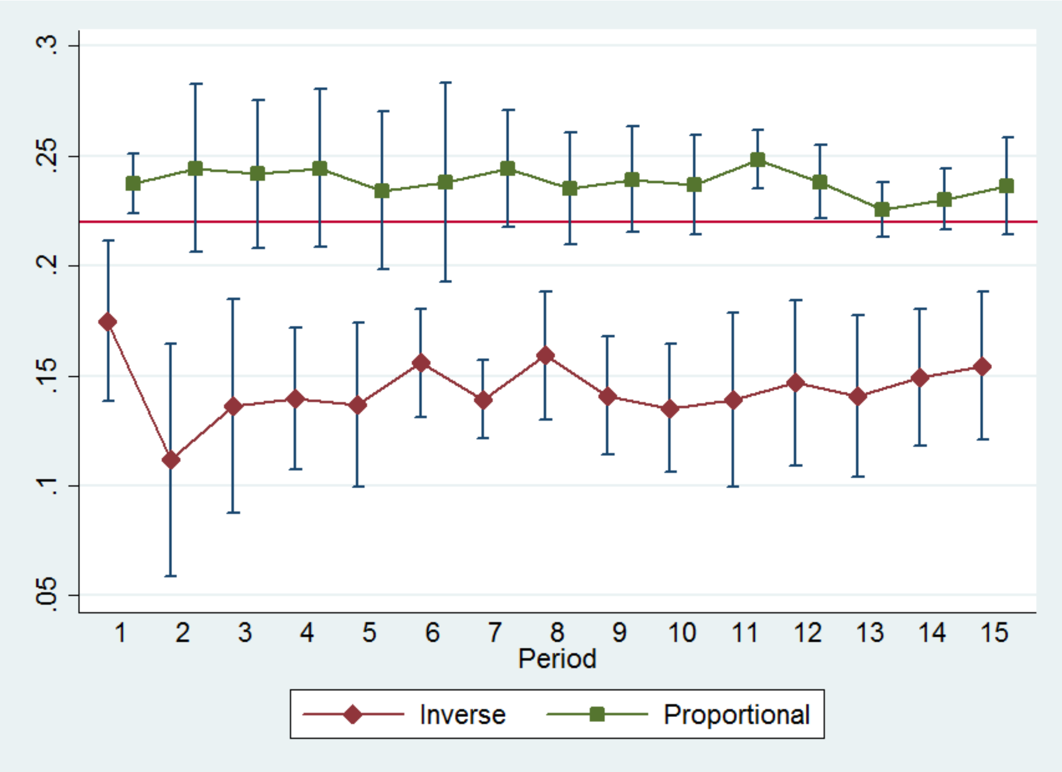


Note: The bars represent 95% confidence intervals. The horizontal line at 0.22 represents the ex-ante Gini coefficient in the Heterogeneous Endowment, Inverse and Proportional treatments. In the Control and Heterogeneous Return treatments, the ex-ante Gini coefficient is 0.

<u>Result 8:</u> The Inverse and Inverse Repeated treatments lead to a significant reduction in the *ex-post* Gini coefficient. The Proportional and Proportional Repeated treatments have no effect on the *ex-post* Gini coefficient.

To summarise, the Control and Heterogeneous Return treatments lead to an increase in the Gini since they initially have perfect equality. The Heterogeneous Endowment, Inverse and Inverse Repeated treatments all lead to a significant reduction in inequality as measured by the Gini coefficient. The Proportional and Proportional Repeated treatments, however, have neither a positive nor negative effect on the level of inequality.

Figure 4.4: Average Ex-Post Gini Coefficient Across the 15 Periods for each Treatment (Experiment 2).



Note: The bars represent 95% confidence intervals. The horizontal line at 0.22 represents the ex-ante Gini coefficient in the Heterogeneous Endowment, Inverse and Proportional treatments. In the Control and Heterogeneous Return treatments, the ex-ante Gini coefficient is 0.

4.4 Conclusion

We present the results of two experiments both featuring a voluntary contribution mechanism with heterogeneous groups. Experiment 1 was one-shot and featured four heterogeneous treatment conditions; Heterogeneous Return, Heterogeneous Endowment, Inverse and Proportional. In the Inverse treatment, the marginal return and endowment are both heterogeneous and inversely related. In the Proportional treatment, they are proportionally related. To our knowledge, these are the first experimental results featuring an Inverse or Proportional relationship between the marginal return and the endowment.

Whilst we find no significant differences at the aggregate level, there are significant differences within treatments. We add to the varied literature concerning the effect of income heterogeneity with the Heterogeneous Endowment treatment, which shows no overall increase in contributions, though contributions do increase with endowment. In the Heterogeneous Return, we find a (weak) overall increase in contributions and also that contributions increase in the marginal return. In the Proportional treatment, we find that contributions increase as the marginal return and endowment both increase.

In Experiment 2, the Inverse and Proportional treatments were repeated for 15 periods. We find that in the Inverse Repeated treatment, there are no significant differences in *absolute* contributions, though contributions *as a proportion of the endowment* decrease as the endowment increases. By contrast, in the Proportional Repeated treatment, the *absolute* contribution increases as the endowment and marginal return increase and the contributions *as a proportion of the endowment* are not significantly different.

We also analyse the impact the voluntary contribution mechanism has on the level of inequality by considering the Gini coefficient. Treatments with a homogeneous endowment (i.e. the Control and Heterogeneous Return) necessarily lead to an increase in inequality since they start with perfect equality. The Heterogeneous Endowment and both Inverse treatments lead to reduction in inequality whereas the both Proportional treatments have no effect on inequality at all.

In the Inverse and Inverse Repeated treatments, two normatively appealing contribution rules are directly opposed – *proportional to endowment* and *proportional to marginal return* – since the endowment and marginal return are inversely related. It may be in this case that subjects default to another normatively appealing contribution rule – *equality of absolute contributions.* On the other hand, these two contribution rules are exactly the same in the Proportional and Proportional Repeated treatments since they are proportionally related and therefore there is no conflict between them. Our research suggests that the effect of heterogeneities in both the marginal return and endowment may have significant effects on behaviour in the simplest form of the voluntary contribution mechanism. These results are robust across both one-shot and repeated settings.

The voluntary contribution mechanism has consistently proved to be a rich vein of research for an extended period of time. The current paper contributes to this literature by presenting the first experimental results of heterogeneous groups that vary by both the endowment and marginal return. This opens new avenues for future research into the potentially differential effect of, for examples, pre-play communication or punishment opportunities under inverse or proportional structuring between the endowment and marginal return.

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Appendix 4.A: Experimental Instructions – EXPERIMENT 1

Decision Making Experiment - Instructions

Welcome! You are about to take part in a decision-making experiment. This experiment is run by the "Birmingham Experimental Economics Laboratory" and has been financed by various research foundations. Just for showing up you have already earned £2.50. You can earn additional money depending on the decisions made by you and other participants. It is therefore very important that you read these instructions with care.

It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your following of these rules.

We will first jointly go over the instructions. After we have read the instructions, you will have time to ask clarifying questions. We would like to stress that any choices you make in this experiment are entirely anonymous. Please do not touch the computer or its mouse until you are instructed to do so. Thank you.

In the instructions, unless otherwise stated, we will not speak in terms of Pounds, but in terms of Experimental Currency Units (ECUs). Your entire earnings will, thus, be calculated in ECUs. At the end of the session the total amount of ECUs you have earned will be converted to Pounds at the following rate: **1 ECU = 0.20 Pounds**. The converted amount will privately be paid to you in cash.

Detailed Information about the Experiment

The experiment consists of two parts. The total amount you will earn from the experiment will be the sum of the earnings you make in the two parts of the experiment as specified in the instructions below.

Part 1

At the beginning of Part 1, participants are divided into groups of six. You will therefore be in a group with 5 other participants. At no point during the experiment, nor afterwards will you be informed about the identity of the other participants in your group and the other participants will never be informed about your identity.

In Part 1, each of you receives a number of tokens. We call this your endowment. In addition to receiving an endowment, each of you receive a number between 0 and 1. We call this your return from project. How the return from project affects your earnings will be made clear below.

There are three possible combinations of endowment and return from project as follows;

Endowment = 10 tokens, Return from project = 0.75

Endowment = 20 tokens, Return from project = 0.5

Endowment = 30 tokens, Return from project = 0.25

Within each group, exactly 2 participants will be given each of the three possible combinations of the endowment and return from project. This means that in each group, 2 participants will have an endowment of 10 tokens and a return from project of 0.75, 2 participants will have an endowment of 20 tokens and a return from project of 0.5 and 2 participants will have an endowment of 30 tokens and a return from project of 0.25. The allocation of these combinations of endowment and return from project within the group is random.

Your task is to decide how to use your endowment. You have to decide how many of the tokens you want to contribute to a project and how many of them to keep for yourself. The five other members of your group have to make the same decision.

Every token that you do not contribute to the project automatically belongs to you and earns you one ECU.

Every token that you do contribute to the project will earn each group member (including yourself) their respective return from project in ECU.

Your income therefore consists of:

(1) The tokens which you have kept for yourself ("Income from retained tokens") whereby 1 token = 1 ECU.

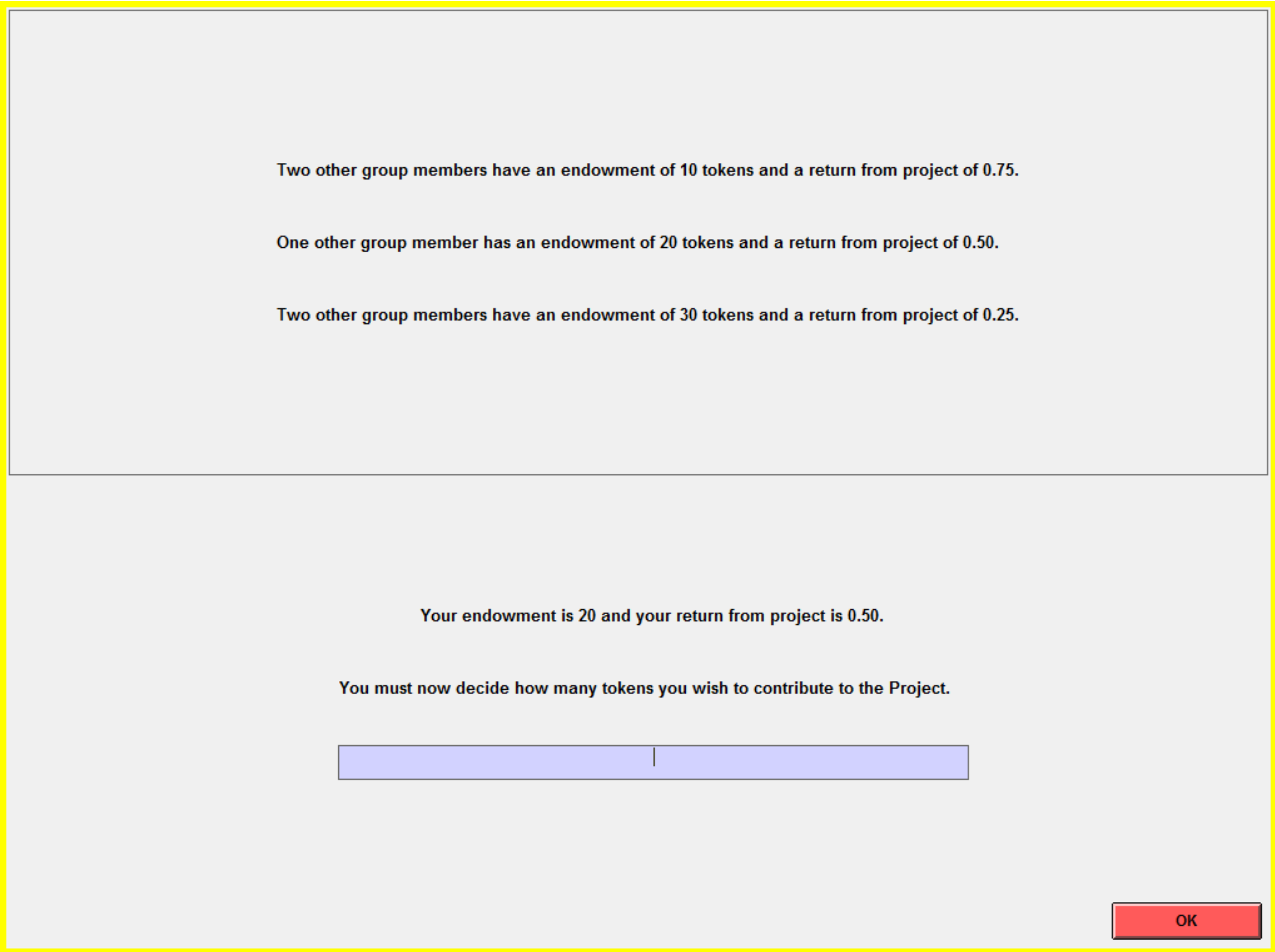
(2) The "Income from the project". This income is calculated as follows:

Your income from the project = your return from project *times* the total contributions to the project.

Your Part 1 income in ECUs is:

(*Your Endowment* – tokens contributed to the project by you) + *Your Return from project(sum of all tokens contributed to the project by all members of your group)**

When making your decision, the following input-screen will appear:



At the top of the screen, you will be informed of the endowment and return from project of the other members of your group. (The example used above is only for illustrative purposes). You have to decide how many tokens you contribute to the project by typing a number between 0 and *your endowment* in the input field. This field can be reached by clicking it with the mouse. By deciding how many tokens to contribute to the project, you automatically decide how many tokens you keep for yourself. After entering the amount of tokens you contribute you must press the O.K. button using the mouse. Once you have done this, your decision can no longer be revised.

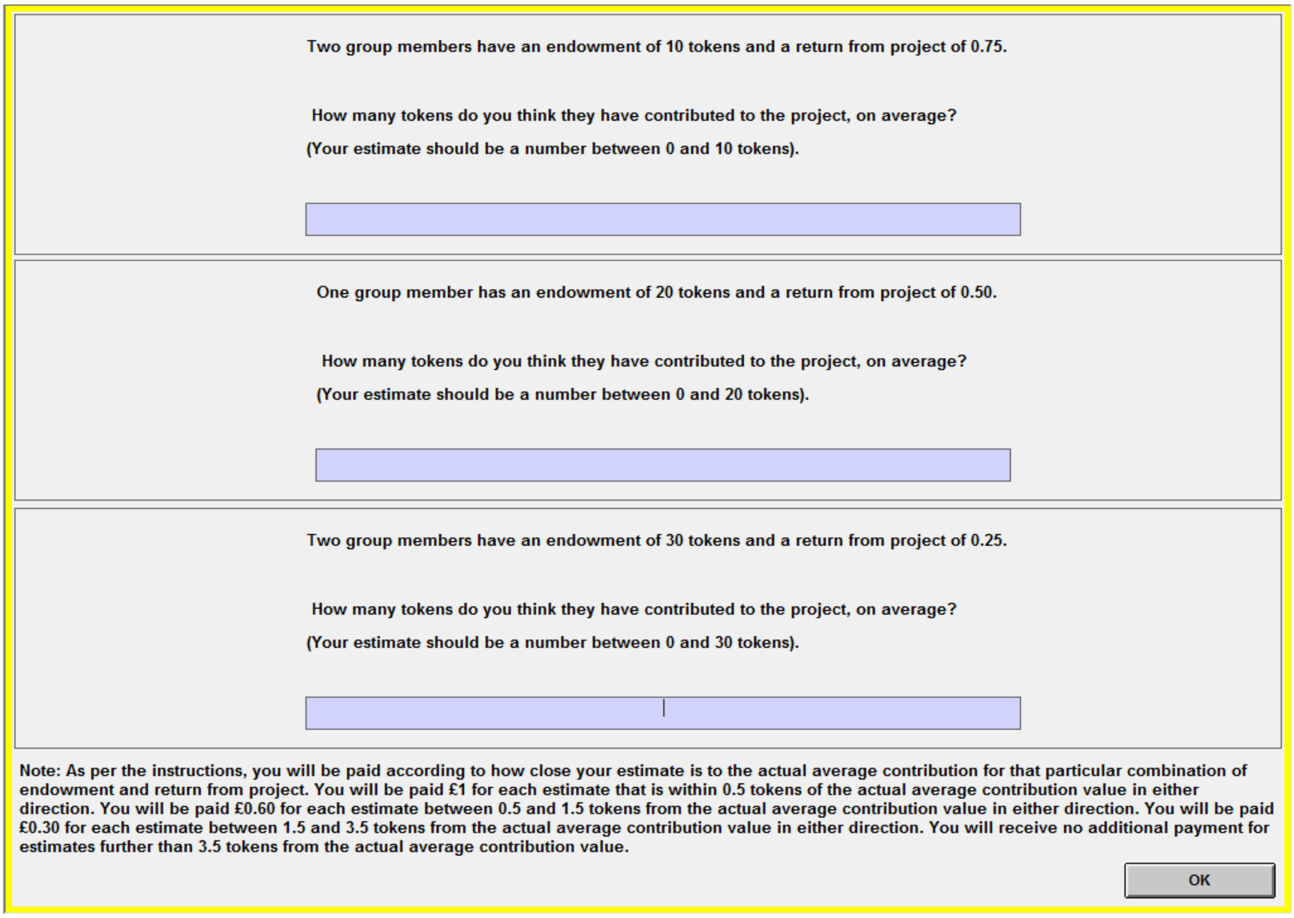
Part 2

In Part 2 of the experiment, you will be asked to estimate for each of the three possible combinations of endowment and return from project the average contribution to the project within your group. You will therefore be required to make three decisions. Note that when estimating the average contribution for your combination of the endowment and return from project, you should estimate the contribution of the other participant with the same combination as you.

You will be paid according to how close your estimate is to the actual average contribution for that particular combination of endowment and return from project. You will be paid £1 for each estimate that is within 0.5 tokens of the actual average contribution value in either direction. You will be paid £0.60 for each estimate between 0.5 and 1.5 tokens from the actual average contribution value in either direction. You will be paid £0.30 for each estimate between 1.5 and 3.5 tokens from the actual average contribution value in either direction. You will receive no additional payment for estimates further than 3.5 tokens from the actual average contribution value.

Your Part 2 earnings are therefore the sum of your earnings for each of three estimates.

When making your decision, the following input-screen will appear:


Two group members have an endowment of 10 tokens and a return from project of 0.75.

How many tokens do you think they have contributed to the project, on average?

(Your estimate should be a number between 0 and 10 tokens).

One group member has an endowment of 20 tokens and a return from project of 0.50.

How many tokens do you think they have contributed to the project, on average?

(Your estimate should be a number between 0 and 20 tokens).

Two group members have an endowment of 30 tokens and a return from project of 0.25.

How many tokens do you think they have contributed to the project, on average?

(Your estimate should be a number between 0 and 30 tokens).

Note: As per the instructions, you will be paid according to how close your estimate is to the actual average contribution for that particular combination of endowment and return from project. You will be paid £1 for each estimate that is within 0.5 tokens of the actual average contribution value in either direction. You will be paid £0.60 for each estimate between 0.5 and 1.5 tokens from the actual average contribution value in either direction. You will be paid £0.30 for each estimate between 1.5 and 3.5 tokens from the actual average contribution value in either direction. You will receive no additional payment for estimates further than 3.5 tokens from the actual average contribution value.

OK

At the top of the screen, you will be reminded of your endowment and return from project. In the remaining three boxes you will see each of the three possible combinations of endowment and return from project. (The example used above is only for illustrative purposes). In all three boxes, you must estimate the average contribution of the other members of your group with that particular combination of endowment and return from project. To input your estimate you must type a number between 0 and the endowment for that particular estimate. For example, when estimating the average contribution for those with endowment of 10 tokens and return from project of 0.75, you must enter a number between 0 and 10. As another example, when estimating the average contribution for those with endowment of 30 and return from project of 0.25, you must enter a number between 0 and 30.

Your total earnings for the experiment are therefore equal to your Part 1 earnings converted into £ at the above rate plus your Part 2 earnings, in addition to the show up fee.

Do you have any questions? Please raise your hand and an experimenter will come to your desk. Please do not ask any question out loud.

To ensure everybody understands, each of you will need to answer a few control questions, which you can find in the next page.

Control Questionnaire

Please complete the questions below. In a couple of minutes someone will come to your desk to check your answers. Once everybody answers the following questions correctly, the experiment will start. (The decisions and earnings used for the questions below are simply for illustrative purposes. In the experiment decisions and earnings will depend on the actual choices of the participants.).

1. Suppose you have an endowment of 20 and a return from project of 0.5 Suppose that nobody (including yourself) contributes any token to the project. What is:

Your income ?...........

The income of the other group members with endowment of 10 tokens?...........

The income of the other group member with endowment of 20 tokens?...........

The income of the other group members with endowment of 30 tokens?...........

2. Please complete the following table:

Endowment	Return from Project	Contribution to Project	Income from Retained Tokens	Income from the Project	Total Income
10	0.75	0			
10	0.75	1			
20	0.5	4			
20	0.5	8			
30	0.25	12			
30	0.25	15			
Total Contributed to Project:					

3. Suppose the average contribution for a particular combination of endowment and return from project was 15.5. Suppose you estimate the average will be 13, another group member estimates 10, another group member estimates 12 and another group member estimates 16. What is the payment for this particular estimate for:

You?...........

The group member with estimate 10?...........

The group member with estimate 12?...........

The group member with estimate 16?...........

Appendix 4.B: Experimental Instructions – EXPERIMENT 2

Instructions

Welcome! You are about to take part in a decision-making experiment. This experiment is run by the "Birmingham Experimental Economics Laboratory" and has been financed by various research foundations. Just for showing up you have already earned £2.50. You can earn additional money depending on the decisions made by you and other participants. It is therefore very important that you read these instructions with care.

It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your following of these rules.

We will first jointly go over the instructions. After we have read the instructions, you will have time to ask clarifying questions. We would like to stress that any choices you make in this experiment are entirely anonymous. Please do not touch the computer or its mouse until you are instructed to do so. Thank you.

The experiment will consist of 15 periods. Each period will consist of 2 parts, detailed below. At the end of the experiment, the computer will randomly select 1 period that will be used to determine your payment from the experiment.

In the instructions, unless otherwise stated, we will not speak in terms of Pounds, but in terms of Experimental Currency Units (ECUs). Your entire earnings will, thus, be calculated in ECUs. At the end of the session the total amount of ECUs you have earned in the randomly selected period will be converted to Pounds at the following rate: **1 ECU = 0.20 Pounds**. The converted amount will privately be paid to you in cash.

Detailed Information about the Experiment

The experiment consists of two parts. The total amount you will earn from the experiment will be the sum of the earnings you make in the two parts of the experiment as specified in the instructions below.

Part 1

At the beginning of Part 1, participants are divided into groups of six. This group will remain the same throughout the entire experiment. You will therefore be in a group with 5 other participants. At no point during the experiment, nor afterwards will you be informed about the identity of the other participants in your group and the other participants will never be informed about your identity.

In Part 1, each of you receives a number of tokens. We call this your endowment. In addition to receiving an endowment, each of you receives a number between 0 and 1. We call this your return from project. How the return from project affects your earnings will be made clear below.

There are three possible combinations of endowment and return from project as follows;

2. Endowment = 10 tokens, Return from project = 0.75
3. Endowment = 20 tokens, Return from project = 0.5
4. Endowment = 30 tokens, Return from project = 0.25

Within each group, exactly 2 participants will be given each of the three possible combinations of the endowment and return from project. This means that in each group, 2 participants will have an endowment of 10 tokens and a return from project of 0.25, 2 participants will have an endowment of 20 tokens and a return from project of 0.5 and 2 participants will have an endowment of 30 tokens and a return from project of 0.75. The allocation of these combinations of endowment and return from project within the group is random. Your combination of endowment and return from project will remain the same throughout the entire experiment.

Your task is to decide how to use your endowment. You have to decide how many of the tokens you want to contribute to a project and how many of them to keep for yourself. The five other members of your group have to make the same decision.

Every token that you do not contribute to the project automatically belongs to you and earns you one ECU.

Every token that you do contribute to the project will earn each group member (including yourself) their respective return from project in ECU.

Your income therefore consists of:

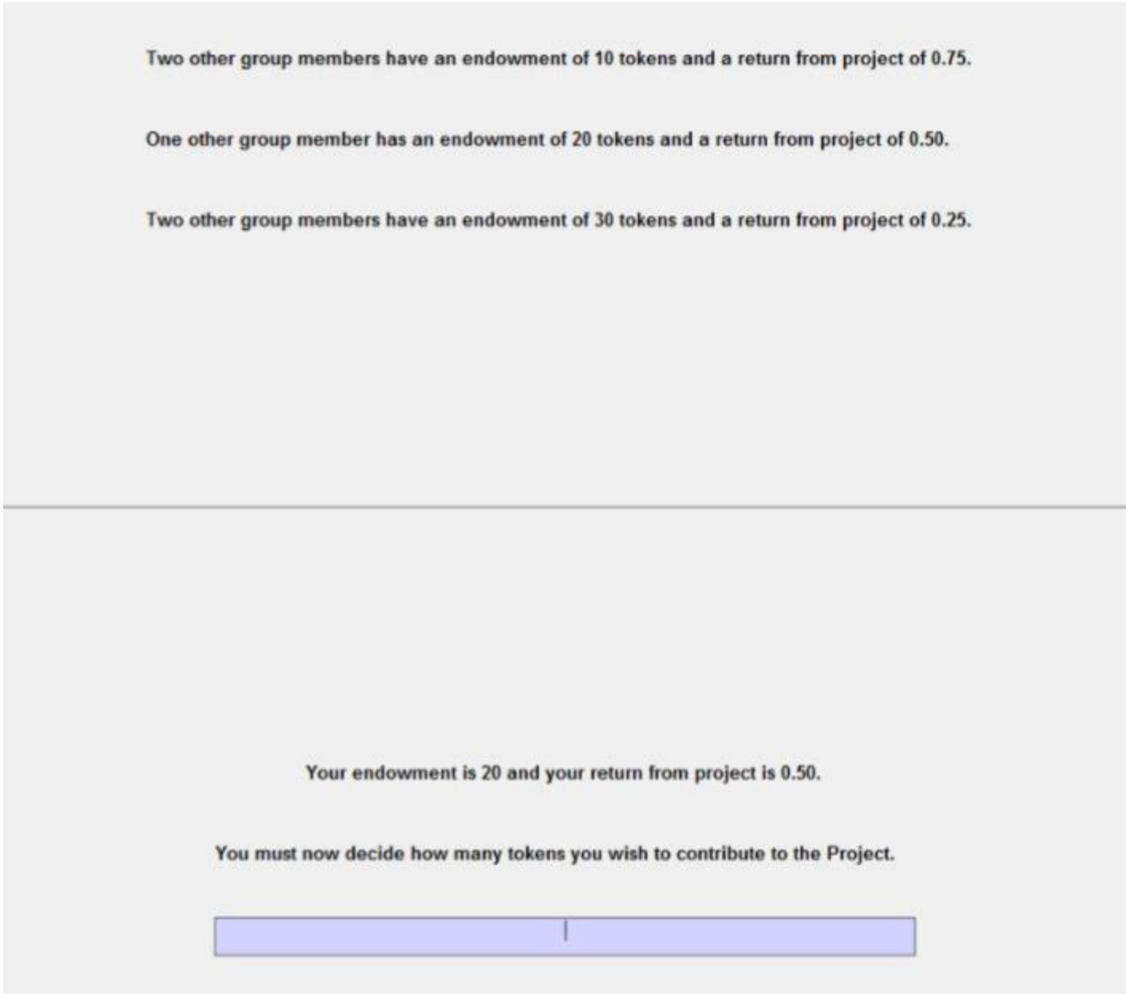
(1) The tokens which you have kept for yourself (“Income from retained tokens”) whereby 1 token = 1 ECU.

(2) The “Income from the project”. This income is calculated as follows:

Your income from the project = your return from project *times* the total contributions to the project.

Your Part 1 income in ECUs is:
(*Your Endowment* – tokens contributed to the project by you) + *Your Return from project(sum of all tokens contributed to the project by all members of your group)**

When making your decision, the following input-screen will appear:



At the top of the screen, you will be informed of the endowment and return from project of the other members of your group. (The example used above is only for illustrative purposes). You have to decide how many tokens you contribute to the project by typing a number between 0 and *your endowment* in the input field. This field can be reached by clicking it with the mouse. By deciding how many tokens to contribute to the project, you automatically decide how many tokens you keep for yourself. After entering the amount of tokens you contribute you must press the O.K. button using the mouse. Once you have done this, your decision can no longer be revised.

Part 2

In Part 2 of the experiment, you will be asked to estimate for each of the three possible combinations of endowment and return from project the average contribution to the project within your group. You will therefore be required to make three decisions. Note that when estimating the average contribution for your combination of the endowment and return from project, you should estimate the contribution of the other participant with the same combination as you.

You will be paid according to how close your estimate is to the actual average contribution for that particular combination of endowment and return from project. You will be paid £1 for each estimate that is within 0.5 tokens of the actual average contribution value in either direction. You will be paid £0.60 for each estimate between 0.5 and 1.5 tokens from the actual average contribution value in either direction. You will be paid £0.30 for each estimate between 1.5 and 3.5 tokens from the actual average contribution value in either direction. You will receive no additional payment for estimates further than 3.5 tokens from the actual average contribution value.

Your Part 2 earnings are therefore the sum of your earnings for each of three estimates.

When making your decision, the following input-screen will appear:

At the top of the screen, you will be reminded of your endowment and return from project. In the remaining three boxes you will see each of the three possible combinations of endowment and return from project. (The example used above is only for illustrative purposes). In all three boxes, you must estimate the average contribution of the other members of your group with that particular combination of endowment and return from project. To input your estimate you must type a number between 0 and the endowment for that particular estimate. For example, when estimating the average contribution for those with endowment of 10 tokens and return from project of 0.25, you must enter a number between 0 and 10. As another example, when estimating the average contribution for those with endowment of 30 and return from project of 0.75, you must enter a number between 0 and 30.

Your total earnings for the experiment are therefore equal to your Part 1 earnings converted into £ at the above rate plus your Part 2 earnings, in addition to the show up fee.

Do you have any questions? Please raise your hand and an experimenter will come to your desk. Please do not ask any question out loud.

To ensure everybody understands, each of you will need to answer a few control questions, which you can find in the next page.

Control Questionnaire

Please complete the questions below. In a couple of minutes someone will come to your desk to check your answers. Once everybody answers the following questions correctly, the experiment will start. (The decisions and earnings used for the questions below are simply for illustrative purposes. In the experiment decisions and earnings will depend on the actual choices of the participants.).

1. Suppose you have an endowment of 20 and a return from project of 0.5 Suppose that nobody (including yourself) contributes any token to the project. What is:

Your income ?...........

The income of the other group members with endowment of 10 tokens?...........

The income of the other group member with endowment of 20 tokens?...........

The income of the other group members with endowment of 30 tokens?...........

2. Please complete the following table:

Endowment	Return from Project	Contribution to Project	Income from Retained Tokens	Income from the Project	Total Income
10	0.75	0			
10	0.75	4			
20	0.5	1			
20	0.5	12			
30	0.25	8			
30	0.25	15			
Total Contributed to Project:					

3. Suppose the average contribution for a particular combination of endowment and return from project was 15.5. Suppose you estimate the average will be 13, another group member estimates 10, another group member estimates 12 and another group member estimates 16. What is the payment for this particular estimate for:

You?...........

The group member with estimate 10?...........

The group member with estimate 12?...........

The group member with estimate 16?...........

4. Will your group change or remain the same during the experiment? …………….

5. How many periods will be relevant in determining your payment from the experiment? ………

Appendix 4.C: Additional Statistical Tests

Table A: Test Statistics for Between Treatment Differences in Contribution (Experiment 1)

	Heterogeneous Return	Heterogeneous Endowment	Inverse	Proportional
Control	-1.946*	-1.179	-0.417	-1.125
Heterogeneous Return	-	0.844	1.838*	0.919
Heterogeneous Endowment	-	-	0.877	0.084
Inverse	-	-	-	-0.867

Note: The test is the Wilcoxon rank-sum (Mann-Whitney-U) test. The figures presented are the z-statistic.

Table B: Test Statistics for Between Treatment Differences in (Overall) Beliefs (Experiment 1).

	Heterogeneous Return	Heterogeneous Endowment	Inverse	Proportional
Control	0.351	0.173	0.509	-0.179
Heterogeneous Return	-	0.073	0.134	-0.563
Heterogeneous Endowment	-	-	0.230	-0.542
Inverse	-	-	-	-0.795

Note: The test is the Wilcoxon rank-sum (Mann-Whitney-U) test. The figures presented are the z-statistic.

Table C: Wilcoxon Signed-Rank Test z-Statistics for differences in beliefs *about* each type (Experiment 1).

Treatment	Beliefs About		
Hetero. Return	**(20, 0.25)|(20, 0.5)**	**(20, 0.5)|(20, 0.75)**	**(20, 0.25)|(20, 0.75)**
	0.003***	0.003***	0.002***
Hetero. Endowment	**(10, 0.5)|(20, 0.5)**	**(20, 0.5)|(30, 0.5)**	**(10, 0.5)|(30, 0.5)**
	0.002***	0.003***	0.002***
Inverse	**(10, 0.75)|(20, 0.5)**	**(20, 0.5)|(30, 0.25)**	**(10, 0.75)|(30, 0.25)**
	0.002***	0.006***	0.002***
Proportional	**(10, 0.25)|(20, 0.5)**	**(20, 0.5)|(30, 0.75)**	**(10, 0.25)|(30, 0.75)**
	0.003***	0.003***	0.003***

Table D: Wilcoxon Signed-Rank Test z-Statistics for Within-Treatment Differences in Contributions (Experiment 2).

Inverse				Proportional		
Type	**(20, 0.5)**	**(30, 0.25)**		**Type**	**(20, 0.5)**	**(30, 0.75)**
(10, 0.75)	0.178	-0.356		**(10, 0.25)**	-1.836*	-2.429**
(20, 0.5)	-	-0.356		**(20, 0.5)**	-	-1.599

Table E: Wilcoxon Signed-Rank Test z-Statistics for Within-Treatment Differences in (Overall) Beliefs (Experiment 2).

Inverse		Proportional

Type	(20, 0.5)	(30, 0.25)
(10, 0.75)	1.778*	1.379
(20, 0.5)	-	-1.156

Type	(20, 0.5)	(30, 0.75)
(10, 0.25)	-0.533	0.889
(20, 0.5)	-	0.770

Table F: Wilcoxon Signed-Rank Test z-Statistics for differences in beliefs *about* each type *by* each type (Experiment 2).

Inverse	Type	Beliefs About		
		(10, 0.75)|(20, 0.5)	(20, 0.5)|(30, 0.25)	(10, 0.75)|(30, 0.25)
Belief By	**(10, 0.75)**	-1.867*	-0.267	-0.978
	(20, 0.5)	0.356	-0.622	-0.622
	(30, 0.25)	-2.134**	0.356	0.000

Proportional	Type	Beliefs About		
		(10, 0.25)|(20, 0.5)	(20, 0.5)|(30, 0.75)	(10, 0.25)|(30, 0.75)
Belief By	**(10, 0.25)**	-2.310**	-2.192**	-2.547**
	(20, 0.5)	-2.073**	-2.429**	-2.547**
	(30, 0.75)	-2.312**	-1.481	-2.134**

Table G: Wilcoxon Signed-Rank Test z-statistics for differences in beliefs *about* each type *between* each type (Experiment 2).

Inverse	Beliefs About (10, 0.75)		Beliefs About (20, 0.5)		Beliefs About (30, 0.25)	
Type	**(20, 0.5)**	**(30, 0.25)**	**(20, 0.5)**	**(30, 0.25)**	**(20, 0.5)**	**(30, 0.25)**
(10, 0.75)	-0.622	-0.133	2.224**	0.533	0.889	1.156
(20, 0.5)	-	0.356	-	-2.045**	-	0.889

Proportional	Beliefs About (10, 0.25)		Beliefs About (20, 0.5)		Beliefs About (30, 0.75)	
Type	**(20, 0.5)**	**(30, 0.75)**	**(20, 0.5)**	**(30, 0.75)**	**(20, 0.5)**	**(30, 0.75)**
(10, 0.25)	-0.652	-1.599	0.533	0.712	-0.889	0.889
(20, 0.5)	-	-1.836*	-	-0.296	-	1.599

CHAPTER 5

CONCLUDING REMARKS

In this final chapter, the main findings and contributions to the literature from each of the preceding three chapters are discussed, including potential avenues for future research that the results may point towards.

Chapter 2 *("Reputation Mechanisms and the Number of Possible Feedback Ratings: A Comparative Experiment")* presented an experiment featuring a multi-period auction with multiple buyers and sellers. There is moral hazard in the sense that the seller does not have to honour the sale having been paid; the seller incurs a cost if they decide to send the good to the winning bidder. The auction was designed this way to mirror peer-to-peer internet auction/commerce websites as much as possible. Two treatments featured reputation mechanisms with different numbers of possible ratings that could be given (2 and 7). A third treatment was a control and featured no reputation mechanism. As such, Chapter 2 presented an investigation firstly into the effect of having access to a reputation mechanism. Specifically, the effect of having a greater number of possible ratings that can be assigned and whether this increases the benefit to consumers.

The results presented suggest that the presence of a reputation mechanism leads to improvements for the bidders, which is in line with the existing literature. In the experimental market, trust in the sellers (implicit in the bidding process) did not, on average, give positive profits. Thus, the presence of a reputation mechanism enabled the bidders to learn this and lower their bids over time due to the nature of the reputation mechanism as an information transmission device. The bidders used the mechanism broadly as expected, the higher ratings were given more frequently if the seller sent the good. The reputation mechanism benefit bidders in another way; they act as a constraint on sellers' (bad) behaviour. The analysis presented suggests that sellers are indeed more likely to honour the sale when bidders have a reputation mechanism available to them. Nonetheless, the winning auction price remained a significant determinant of seller behaviour across all treatments.

The real contribution of Chapter 2 is in the comparison between two different reputation mechanisms that vary only in the number of possible ratings that can be assigned. All the above effects of the reputation mechanism are even stronger when the mechanism has 7 possible ratings as opposed to only 2. It is worth noting that the seller only has two possible actions (a binary send or not decision) and thus 2 possible feedback values should be sufficient to transmit the entire action space of the seller. This suggests that having a greater number of possible feedback ratings will indeed be beneficial in terms of the core aims of a reputation mechanism system; enabling learning and limiting bad behaviour. Since a parameter as simple as the number of possible ratings can have a significant effect of the behaviour of users, it is important to be careful when designing reputation mechanisms in order to maximise their effectiveness for consumers. This makes these findings particularly important for internet based peer to peer marketplaces. Possible avenues for future research include investigating the limits of these findings i.e. the optimal number of possible ratings that can be given for a particular situation and its determinants.

Chapter 3 *("Sequential Equilibrium and Moral Hazard Auctions")* developed theoretical predictions for a reputation based sequential equilibrium in a multi-period moral hazard auction market featuring multiple bidders and a single seller. In addition, the results of an experiment designed to test the derived predictions are presented. For control purposes, we also include a canonical private value auction and a moral hazard auction (i.e. a simplified version of the construct featured in Chapter 2).

The theoretical model featured a multi-period auction with moral hazard and some known probability that the seller was an automated seller that would always overcome the moral hazard by sending the good to the winning bidder. A reputation sequential equilibrium is developed featuring; a fully contingent conditional bidding strategy, a Bayesian belief system and a profit-maximising complete seller strategy. It matches a canonical sequential equilibrium in the sense that the (rational) seller mimics a 'good' seller by sending the good in the early periods before not sending it in the final period. A primary contribution of Chapter 3 is thus the extension of the sequential equilibrium reputation framework to a multi-period, multi-buyer auction market.

The results of the two control treatments are broadly in line with existing literature. Behaviour in the novel treatment shows broad support for the qualitative features of the reputation building equilibrium prediction, despite substantial qualitative deviation from the precise equilibrium predictions. Bidders make significantly reduced bids if the winning bidder does not receive the good (the equilibrium prediction is that they should stop bidding completely). Sellers send the good with high probability in the first 2 periods and then substantially reduce the probability in the final period of each 3-period block (the equilibrium prediction is to send the good with certainty in the first 2 periods and not send with certainty in the final period). The key is that the very presence of the computerised, 'good' seller is enough to encourage better behaviour in some of the periods by sellers than otherwise in a baseline treatment that does not feature the 'good' sellers at all. Thus, Chapter 3 shows that the behavioural patterns characteristic of the sequential equilibrium reputation framework extends into a repeated, multi-bidder moral hazard auction setting. From here, future research agendas may wish to consider other multiplayer (more than two) scenarios where the sequential equilibrium reputation building hypothesis may also be expected to apply. It may also prove to be a rich vein of inquiry for theorists as there is much work to do be done in terms of generalising the results of Chapter 3 to other multi-player market-based games within the sequential equilibrium reputation framework.

Chapter 4 *("Public Goods Games with Structural Heterogeneities in Endowment and Marginal Return")* featured two public goods experiments; the first one-shot and the second repeated. A total of 5 treatments were conducted. In the two novel treatments, both the initial endowment and marginal return co-vary either positively (i.e. proportionally) or negatively (i.e. inversely). The treatment with the inverse relationship was inspired by the empirical observation that in the UK higher earners are more likely to obtain private provision of public provided goods such as health care and education. In two more control treatments, only the initial endowment *or* the marginal return are heterogeneous. A final control has fully homogeneous groups.

In the treatment where the initial endowment and marginal return are inversely related, there is a conflict between two normatively appealing contribution rules; equality of contributions as a proportion of endowment and equality of contributions as a proportion of marginal return. Indeed, the behaviour in the one-shot experiment is indicative of this conflict. In particular, the elicited beliefs showed a clear conflict between those with high endowment and those with high marginal return. This conflict is not present in any of the other treatments, neither in theory nor behaviourally.

A second experiment was conducted in which the two novel treatments were repeated to investigate the robustness of the one-shot results. When the endowment and marginal return were proportionally related, contributions were proportional to endowment/marginal return i.e. contributions *as a proportion of endowment/marginal return* were not different. By contrast, when the endowment and marginal return were inversely related, there was no difference in absolute contributions i.e. those with higher endowments were contributing less *as a proportion of endowment* but more *as a proportion of marginal return.* Thus, it appears that when the two normatively appealing contributions rules are opposed, subjects revert to another appealing contribution rule; equality of absolute contributions. This highlights the importance of normative appeal in deciding focal points or social norms in experimental public goods games. Since these are the first experimental tests of groups that are heterogeneous in both the initial endowment and marginal return, these findings represent one of the major contributions of the chapter. It gives a valuable insight into the importance of contribution norms in heterogeneous public goods games. There are also many other potential routes for future projects, to consider the potential effect of, for example, reward/punishment mechanisms or pre-play communication on the behaviour of heterogeneous groups of the kind presented in Chapter 4.

Further, the equity implications of the public goods mechanism under the various structures on the initial endowment and marginal return are explored. The inverse relationship leads to a significant reduction in inequality as measured by the Gini coefficient in both the one-shot and every period of the repeated experiment. In contrast, a proportional relationship between endowment and marginal return has no effect on the Gini coefficient whatsoever, leaving the pre-existing inequality at the same level. Thus, under the inverse structure, the public goods mechanism is equity improving whilst under the proportional structure it is not.