The Impact of Social Comparisons on Ultimatum Bargaining*

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Abstract

We use a laboratory experiment to investigate the impact of social comparisons on ultimatum bargaining. Three individuals compete in a real-effort task for the role of the proposer in an ultimatum game. The role of the responder is randomly allocated to one of the other two subjects. The third individual - the non-responder - receives a commonly known fixed payment and makes no decision. The proposer makes a single take-it-or-leave-it offer to the responder which, if rejected, leads to zero earnings for both parties. We find that the existence of a non-responder has a dramatic effect on bargaining outcomes and in some cases increases rejection rates by more than 45 percentage points. Behavior suggests that individuals exhibit self-serving bias in the way they define their reference groups.

JEL Classification: C78, C91, D63

Keywords: social comparisons, ultimatum bargaining, laboratory experiments, self-serving bias, real-effort.

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1 Introduction

Empirical evidence suggests that individuals often compare themselves with others and that these social comparisons can influence one's well being (Clark et al., 2008), performance (Torgler and Schmidt, 2007; Torgler et al. 2008) and bargaining decisions (Babcock et al. 1996; Babcock and Loewenstein, 1997). The evidence in these, as well as most, studies on social comparisons come from the field. By way of contrast, in this paper, we present results from a laboratory experiment investigating the significance of social comparisons in a novel three-player ultimatum game.

The game is divided into two stages. In the first stage, three subjects compete in a real-effort task. The winner is assigned the role of the proposer in the second stage. The other two subjects are randomly allocated the roles of the responder and the non-responder. In the second stage, the proposer is given an endowment of 100 Experimental Currency Units (ECU). His task is to propose a split of the endowment between himself and the responder. Simultaneously, the responder must decide her minimum acceptable offer (MAO). If the proposer's offer is equal to or above the MAO, the proposed split is implemented; if not then both parties earn zero. The non-responder has no active role to play in the second stage and receives a fixed payment irrespective of the outcome of the bargaining process. The exogenous payment to the non-responder is our treatment variable.

If individuals are self-regarding then our game has a unique trembling-hand perfect equilibrium which is independent of the non-responder's payoff. In this equilibrium the proposer offers the smallest possible amount to the responder who accepts the offer. However, if the responder cares about her relative earnings and the non-responder is considered to be part of the responder's reference group, then the responder's minimum acceptable offer will be affected by the payment to the non-responder. In this way, we can test for the existence of a social comparison effect.

Laboratory experiments are uniquely suited for the study of social comparisons as they can offer insights that are difficult to obtain otherwise (Clark et al., 2008; Manski, 2000). The reason is that the detection of social comparisons requires (1) knowledge about the individuals' relevant reference groups, (2) precise information about the variables of interest (e.g. salary), (3) knowledge about the information individuals have, and (4) knowledge about when this information reaches individuals. These requirements can be easily satisfied in laboratory experiments which, in addition, permit the creation of endogenous reference groups.

Field data, by contrast, lacks the level of detail found in laboratory data. As a result, virtually all studies to this point using field data have relied on hypothetical reference groups, imperfect information about the timing of the information arrival and on the assumption that group formation is exogenous (Clark et al., 2008). This fact raises questions about whether social comparisons exist and are of economic significance. This is reflected in the conclusion of the extensive survey of Clark et al. (2008; p.112): "In the absence of accurate information about reference groups, we should be cautious in claiming to have evaluated the importance of social comparisons over income from happiness data". Even if one was to obtain a more detailed dataset, another problem with field data is that one cannot preclude the possibility that social comparisons are driven by strategic considerations. For example, if delays in negotiation are more costly for an employer than for her employees, it might pay for the latter to claim that the employer's offer is unfair and that other "equally qualified" employees have received higher offers as the delay in negotiations might force the employer to improve her offer. Laboratory experiments allow researchers to create one-shot interactions and therefore ensure that strategic motives are not the driving factor of social comparisons.

Of course, laboratory data may also have limitations. Typically, groups are formed by the experimenter (or randomly) and participants interact in an artificial, context-free environment. These factors might limit the external validity of laboratory experiments. For this reason, we agree with Clark et al. (2008) and Manski (2000) that both laboratory and field data are required to understand social comparisons: Field data can lend external validity to laboratory data, and laboratory data can support the internal validity of field data. In light of this, it seems surprising that most studies on social comparisons rely on field data and few laboratory experiments have being conducted to investigate the topic (see next section).

The results from the experiment show that social comparisons play an important role in ultimatum bargaining. The payment to the non-responder has a non-monotonic influence on the minimum offers responders are willing to accept. The strongest effect, however, is on the rejection rates. To the best of our knowledge, we find the highest rejection rates ever observed in ultimatum bargaining (see Camerer, 2003). The introduction of a non-responder increases rejection rates from 10.7% in a standard two-person ultimatum game, to 27.6% when the non-responder's payoff is close to the equal split of the surplus between proposer and responder. Reducing the non-responder's payoff, increases rejection rates monotonically. When the non-responder's payoff is close to zero rejection rates are 58.1%; as the game is played only once, the majority of subjects leave the experiment with zero earnings. Analysis of the data suggests that the high rejection rates may be attributed to a self-serving bias in the way subjects form their reference groups.

The next section compares our study to previous experiments. In section 3 we present the experimental design in detail and in section 4 we derive predictions for our experimental treatments. We proceed to present the impact of social comparisons on MAOs, offers, and rejection rates in section 5. In section 6 we conclude.

2 Related Experimental Studies

In a laboratory experiment, social comparisons can be defined as *payoff* comparisons between individuals belonging in a reference group. This general definition implies that behavior in most laboratory experiments could be affected by social comparisons. However, at the same time, isolating the effect of social comparisons from other effects is impossible in most cases. For example, rejections of low offers in two-person ultimatum games can, in principle, be attributed to social comparisons between responders and proposers, but could also be attributed to reciprocal preferences and a willingness to avenge unkind actions. To identify a social comparison effect, therefore, it is essential that the payoff of the reference group is independent of an agent's payoff and actions.

We are aware of two studies in ultimatum bargaining that focus on the impact of social comparisons. The first study is by Knez and Camerer (1995) who use a variant of the ultimatum game in which a proposer makes two offers simultaneously to two different responders. Each responder can accept or reject the offer. The study has an interesting twist: each party receives a different (positive) payment in the event of a rejection. This creates an incentive for proposers to offer higher amounts to responders with higher outside options. Knez and Camerer find that half of the responders demand more when they know that their counterparts are offered more. Proposers, however, fail to anticipate this behavior.

One drawback of the design of Knez and Camerer (1995) is that subjects could see each other during the experiment. The authors state that this was done in an attempt to give social comparisons their "best chance." However, the visual contact amongst subjects could transform the game to a repeated game. Showing a concern for social comparisons might, therefore, be beneficial in such a set up. Another issue is that subjects participated in two versions of the same game (first in one in which responders had to decide whether to accept the offer in isolation, and then in one in which they could condition their decision on the other responder's offer). The within-subject design could reveal the aim of the study and trigger an experimenter effect. The latter could be also triggered by the fact that responders could condition their decision (in particular, the minimum acceptable offer) on the offer made to the other subject. This was unavoidable as responders made their decisions simultaneously, but could also encourage subjects to take into account the offer made to the other responder. As we will see later, our results turn out to be similar to those reported in Knez and Camerer (1995).

The second study is Bohnet and Zeckhauser (2004). Participants in their experiment play a standard ultimatum game for 5 rounds. Proposers and responders are randomly rematched in every round. Prior to deciding whether to accept or reject the proposer's offer, responder's are informed about the *average* offer in the session in the previous round. This information leads to offers that are closer to the 50-50 split relative to a treatment in which there is no information about the average offer. Therefore, the study of Bohnet and Zeckhauser provides evidence that social comparisons can be important in ultimatum bargaining.

There are many differences between the study of Bohnet and Zeckhauser and our study. The most important are the following three. First, rejections in Bohnet and Zeckhauser could be used strategically to increase future offers. As all proposers observed the average offer, a rejection by responder X of proposer A's offer could lead A to increase his offer in the next round. This would increase the average offer and indirectly the offer of proposer B who will be matched with responder X in the future. In contrast, our game is played only once, which we believe provides a harder test for social comparisons as subjects cannot learn from the actions of other participants. Second, responders in Bohnet and Zeckhauser (2004) evaluate the offer of the proposer relative to the whole set of proposers. This creates a "social norm" (Bohnet and Zeckhauser, 2004; p.496) and, therefore, could trigger the need to conform to it (e.g. Cason and Mui, 1998; Frey and Meier, 2004). Such need is absent in our experiment. Third, in their study the reference level is endogenously determined (50-50 split). In contrast, in our experiment, we can influence the reference level by exogenously changing the non-responder's payoff. This permits us to see what happens as the non-responder's payoff changes and when it moves far away from the equal split which is a natural focal point.

Our experimental design is also related to the studies of Gueth and van Damme

(1998), Kagel and Wolfe (2001), and Shupp et al. (2006). These studies employ three-player ultimatum games to investigate the influence of a non-active party on ultimatum bargaining. In the experiments of Gueth and van Damme (1998) and Kagel and Wolfe (2001) proposers suggest *a three-way split* between participants. One subject is responsible for accepting or rejecting the offer. If the offer is accepted the proposed split is implemented. A rejection results to zero earnings for the proposer and the responder. The rejection implies zero earnings for the non-active party in Gueth and van Damme (1998) and an (exogenously determined) consolation prize in Kagel and Wolfe (2001). In Shupp et al. (2006) the non-active party receives an exogenously determined fixed payment if the responder accepts the proposer's offer and zero otherwise.

Unlike the experiments of Knez and Camerer (1995) and Bohnet and Zeckhauser (2004), all three studies find little evidence that responders compare themselves to non-responders; the latter are largely ignored.[1] Given the many differences with the aforementioned studies it is difficult to explain the "invisibility" of the non-active parties. One explanation could be the fact that the payoff to the non-active party (who presumably is part of the responder's reference group) is *not* independent of the responder's actions; for this reason the non-responder is often called a "hostage".

An important difference between our experimental design and that in previous studies is that in all previous experiments the participants' roles were randomly allocated to subjects. In contrast, in our experiment, subjects compete in a real-effort task which determines who will be assigned the role of the proposer. Festinger (1954), who coined the term "social comparisons", suggests that a person's reference group consists of people who are "close to [their] own ability" (p. 121). The task should strengthen the link between responders and non-responders given that responders and non-responders are the "losers" in the real-effort task and that the offer is made by the "winner". Therefore, the payment to the non-responder could be taken by both proposers and responders as a valid reference point for the amount responders deserve to receive. Another reason for including the real-effort task is that previous experiments have shown that subjects who earn their role (or endowment) in the experiment behave differently than when roles (endowments) are randomly allocated (e.g. Hoffman et al. 1994). Given that people usually earn their positions/salaries outside the laboratory, we believe that the real-effort task improves the external validity of our results.

[1] Kagel and Wolfe (2001, p.216) make this point graphically be asking "what accounts for the near *invisibility* of the third player in terms of the responders' reactions to the [proposers'] offers?" (emphasis added)

3 The Experiment

The experiment was conducted in the Experimental Economics Laboratory at the University of Melbourne between November 2007 and August 2008 using z-Tree (Fischbacher, 2007). The 329 participants were students from the University of Melbourne recruited randomly using ORSEE (Greiner, 2004) from a pool of more than 1400 volunteers. Subjects were from different academic backgrounds including (first-year) economics. Each subject took part in only one of the four treatments, and none of the subjects had previous experience with a similar experiment.

Upon arrival at the laboratory, participants are seated in partitioned computer terminals and are randomly divided into groups of three players. Individuals then must read a set of instructions which explains the experiment. Subjects are informed of the payoff (y) to be received by the third player - the non-responder (nr). That is, $\pi^{nr} = y$. This payoff is not affected by the decisions of the responder and the proposer. Also, y does not affect the amount given to the proposer which is always 100 ECU. Before the experiment can begin each participant must answer a number of control questions which aim to help participants understand the game.

The experiment is divided into two stages. In the first stage, individuals perform a real effort task, which is the Encryption Task (Erkal et al., 2009). In the Encryption task, participants are given a table assigning a number to each letter of the alphabet. They are then presented with different words in a sequence (the same for all players) which they must encrypt using a computer screen as in Figure 1.[2] If a subject encrypts a word correctly he is presented with a new word. A subject cannot proceed until he encrypts the word correctly. The player with the highest number of encrypted words is assigned the role of the proposer in the second stage.

The main goal of the first stage (which lasts for seven minutes) is to strenghten the relation between the responder and the non-responder in an attempt to address the "invisibility" of the non-responder in previous experiments. We wanted to do this in a way that is consistent with Festinger's (1954) idea about how reference groups are formed. For this reason, we chose to assign the role of the responder randomly to one of the two remaining subjects after a one-minute delay. The actual number of words encrypted by each individual is not revealed to the subjects.

In the second stage, individuals play a three-player ultimatum game. The game is as follows. The proposer (p) is given 100 Experimental Currency Units (ECU) which he

[2] Neutral language was used in the instructions were subjects were asked to "encode" words.

must divide with the responder.by making an offer (x). Simultaneously the responder decides the minimum acceptable offer (MAO), m, $m\epsilon[0, 100]$. These two decisions are made privately. If the proposer's offer is not smaller than the MAO, i.e. if $x \geq m$, then the proposed offer is implemented. The proposer's and responder's earnings in that case are respectively $\pi^p = 100 - x$, $\pi^r = x$. If the offer is rejected, then both have zero earnings.[3] To ensure that the rules of the game are common knowledge the experimenter read aloud a detailed summary of the instructions as well as the answers to the control questions (once participants answers had been privately checked).

The non-responder's payoff is the treatment variable. We chose to study three treatments with a non-responder in which $y\epsilon\{5, 20, 40\}$. The rationale for choosing the particular values for y is explained in the following section where we offer hypotheses for the impact of y on ultimatum bargaining. In order to evaluate the impact of non-responders on bargaining outcomes we also conducted a two-player ultimatum game with roles being determined by the Encryption Task; apart from the absence of a non-responder, the treatments were otherwise identical. This treatment (Baseline) allows us to compare our results with those in previous studies. Table 1 summarizes the experimental design.

Both the encryption task and the ultimatum game were played only once. Therefore, there was a considerable chance that some individuals would walk away with zero earnings from the experiment. Despite this possibility, we were reluctant to pay participants a show-up fee as this could affect the reference point of the agents (see Knez and Camerer, 1995). Instead we chose to compensate them for their opportunity cost indirectly by giving them a daily tram ticket. The card at the time of the experiment was worth A$3.40 (although this was not explicitly written in the instructions). The exchange rate used in the experiment implied that the 100 ECU given to the proposer equaled A$40. The experimental sessions lasted 40 minutes on average.

4 Hypotheses

In this section, we offer hypotheses that serve as benchmarks against which we will evaluate our results. A well-known fact is that low offers in ultimatum games are often

[3] To avoid confusion, the instructions repeated three times the fact that responders should state the *minimum* amount they are willing to accept and not the amount that they would like the proposer to offer. Instructions can be downloaded at *www.economics.unimelb.edu.au/nnikiforakis/research.htm* together with the software code.

rejected (Camerer, 2003). In fact, the modal offer is half the size of the proposer's endowment. Hence, the game-theoretic prediction based on the assumption that individuals are self-regarding money maximizers - which prescribes that proposers offer the smallest possible amount and that responders accept it - is not a useful benchmark.

To derive hypotheses, we utilize the model proposed by Rabin (1993) to incorporate fairness into game theory.[4] In doing this, our intention is not to test a particular model of fairness, but to identify plausible equilibrium mechanisms through which social comparisons may affect ultimatum bargaining.

Rabin's model is based on the assumption that individuals are reciprocal. That is, agents are concerned about their monetary payoff, but also about the kindness with which they think others are treating them. If someone is judged to be unkind to a player, then he can increase his utility by also being unkind to him. The assumption that many individuals are reciprocal has received considerable support in a range of environments (Camerer, 2003). In his theory Rabin assumes that individuals use a reference point (which is exogenous to the game) to judge kindness. Dickinson (2000) applies Rabin's model to the ultimatum game.

Our adaptation of Rabin's model to the experiment described above is described in the appendix. In this adaptation we assume an arbitrary fair offer, X^R, which is common to all participants. Proposition (1) in the appendix shows that, on the assumptions underlying Rabin's model, the responder's minimum acceptable offer and the proposer's offer are positively correlated with the fair offer. The null hypothesis is based on the assumption that responders do not care about the non-responder or their payment when deciding which offers are fair. That is, $X^R = X^R(P)$, where P is the amount the proposer must divide. The alternative hypothesis is based on the assumption that the non-responder is part of the responder's reference group and hence the payment to the non-responder affects the definition of a fair offer. That is, $X^R = X^R(P, y)$, where y is the fixed payment to the non-responder.

From Proposition (1) we therefore obtain the following hypotheses:

Minimum Acceptable Offer (m)

$\mathbf{H}_m$: *The responder's MAO is invariant to the non-responder's payment.*

$\mathbf{H}'_m$: *The responder' MAO increases monotonically with the payment to the non-responder.*

[4] Kagel and Wolfe (2001) show that outcome-based models of other-regarding preferences do not accurately predict behavior in three-player ultimatum games. The authors argue that the intentions of the participants matter and that one should use intention-based models like Rabin's (1993).

Offer (x)
$\mathbf{H}_o$: *The proposer's offer is invariant to the non-responder's payment and equal to the the proposer's MAO.*
$\mathbf{H}_o'$: *The proposer's offer increases monotonically with the payment to the non-responder.*

5 Experimental Results

We start by examining responders' decisions. We then turn our attention to the offers made by the proposers before discussing rejection rates. All tests are two-tailed when comparing distributions, and one-tailed when comparing medians and averages.

Result 1: *The non-responder's payoff has a non-monotonic effect on minimum acceptable offers.*

SUPPORT: Table 1 presents the median, modal and average minimum acceptable offer (MAO) across treatments. Let us first compare the three treatments with a non-responder. All three measures of central tendency reveal that there is a non-monotonic (V-shaped) relation between the non-responder's payoff and the MAO: reducing the non-responder 's payoff (y) from 40 ECU to 20 ECU lowers MAOs**.** However, when y is further reduced to 5 ECU, MAOs increase. A Fisher's exact test rejects the hypothesis that the three medians are the same across treatments (two-tailed, p-value<.1). Pairwise comparisons reveal that the median in T20 is significantly lower than it is in T40 (p-value<.05) and in T5 (p-value=.06). The median in T5, although lower, is not significantly different from the median in T40 (p-value=.3).

While the lower MAO in T20 relative to T40 is consistent with both the prediction that that as y decreases, responders will be willing to accept lower offers, $\mathbf{H}_m'$, the higher MAO in T5 relative to T20 is clearly at odds with this prediction. To better understand whether the non-monotonic relation extends across the entire distribution of MAOs, Figure 2 presents the cumulative distribution of MAOs across treatments. First, notice that the MAOs in T20 are lower than they are in T40. This means that the drop of y from 40 to 20 ECU has the predicted effect. A Kolmogorov-Smirnov test rejects the hypothesis that the distribution of MAOs is the same in T20 and T40 (p-value=.05). Second, while more responders are willing to accept offers between 20 and 40 ECU in T20 than in T5, a Kolmogorov-Smirnov test cannot reject the hypothesis that the distribution of MAOs is the same in T5 and T20 (p-value=.32). The same applies for the distributions in T5 and T40 (p-value=.81).[5]

To summarize, the impact of the non-responder's payoff on MAOs is consistent with our predictions for T40, T20, and Baseline (see below), but not with T5. The question that arises, therefore, is: What could account for the higher MAOs in T5? We offer a conjecture:

Conjecture: *The non-monotonic relationship is due to some responders ignoring the non-responder when the latter receives a very low payment.*

SUPPORT: Table 1 shows that the modal MAO (50 ECU) is the highest in T5. This evidence suggests that a large percentage of responders ignore the non-responder completely. Indeed, the median MAO in T5 (33 ECU), is very similar to the median MAO in the Baseline treatment(32.5 ECU) where there is no non-responder (p-value>.5). The non-responders' payoffs are clearly taken into consideration in T20 where y has the effect of lowering the median MAO significantly relative to T40 (p-value<.05) and relative to the Baseline (p-value<.1). Presumably, the responders in T5 that ignore the non-responders, do this because of the low earnings associated with low values of y.

Not all responders in T5, however, appear to ignore the non-responder. A non-trivial proportion of responders in T5 (29%) is willing to accept offers of 10 ECU or less. This is higher than the proportion of responders willing to do the same in T20 (25.8%), T40 (20.7%) or in Baseline (17.9%) as can be seen in Figure 2. This implies that the non-responder's payoff is still taken into consideration by some responders when making their decisions. The polarization in T5 is not found in any of the other treatments. This can be seen clearly in Figure 2 and in Figure A1 in the appendix which presents the distribution of MAOs across treatments.

Next, we turn our attention to the offers made by the proposers.

Result 2: *The non-responder's payoff has modest impact on the offers made by the proposers.*

SUPPORT: Table 1 presents the median, modal and average offer across treatments. Each of the three measures of central tendency suggests a slightly different relationship between the non-responder's payoff and proposer's offers. However, all measures

[5] The distribution of MAOs in Baseline is not different from any of the treatments (T40: p-value=1; T20: p-value=.28; p-value=.59).

indicate that offers are lower in T20 than they are in T40. Despite being small in magnitude, the difference in medians is significant (Fisher's exact, p-value$<.05$). This finding is consistent with the alternative hypothesis based on the assumption that the non-responder's payoff affects the definition of what constitutes a fair offer. It is also consistent with the behavior of the responders. All other pairwise comparisons fail to yield significant differences using a Fisher's exact test.

The fact that offers are not significantly different in T5 and T40 (p-value=.3) could be taken as evidence that proposers correctly anticipate the responders' reaction to the presence of a non-responder. However, as we will see shortly, this is not the case. The median offer in Baseline is not significantly different compared to T5 (p-value=.18) and T40 (p-value$>.3$), but is higher than the median offer in T20 (p-value$<.01$).

Figure 3 illustrates the cumulative distribution of offers for each treatment. Offers in T20 appears to be lower than they are in T40. However, the difference in distributions is not significant (Kolmogorov-Smirnov, p-value=.19), and neither is the difference of T40 and T5 (p-value=.76). Similar conclusions can be drawn by looking at Figure A2 in the appendix.[6]

Next, we examine the impact of social comparisons on rejection rates.

Result 3: *The non-responder's payoff has a dramatic effect on rejection rates.*

SUPPORT: Table 1 presents rejection rates in each treatment. It is clear that non-responders have a dramatic impact on bargaining outcomes. Rejection rates are relatively low in the Baseline treatment (10.7%) and comparable to those in previous studies (Camerer, 2003; p.50-55). This indicates that the real-effort task did not impact greatly on the ability of proposers and responders to coordinate in our experiment.

The introduction of a non-responder increases rejection rates by 19.9 percentage points, from 10.7% to 27.6%, even though the non-responder's payoff in T40 was chosen to be close to the equal split.[7] Further distancing the payment to the non-responder from the 50-50 split increases rejection rates by 21.6 percentage points (T20) 47.4 percentage points (T5) relative to the Baseline treatment. In T5 the majority of subjects (58.1%) did not reach an agreement and left the experiment with zero earnings (and a tram ticket).

[6] The distribution of offers in Baseline is not different from T40 and T5 (T40: p-value=.94; T20: p-value=.27), but is different than T20 (p-value$<.05$).

[7] We chose $y = 40$ rather than $y = 50$ as we thought proposers who earn their role in the ultimatum game would be more likely to offer 40 ECU rather than 50 ECU (Hoffman et al., 1994).

Given the random pairing of individuals, arguably a better way of examining rejection rates is to calculate the likelihood that the median offer in a treatment would be rejected. In Baseline, all responders stated an MAO equal to or less than 50 ECU. Therefore, the median offer of 50 ECU would never be rejected. In T40, T20 and T5 the median offer would be rejected 21%, 26% and 49% of the times, respectively.

6 Discussion

Social comparisons have implications for a range of economic issues such as wage determination (Babcock and Loewenstein, 1997), labor supply (Neumark and Postlewaite, 1998), saving decisions (Abel, 1990) and optimal taxation (Frank, 1985). However, evaluating the impact of social comparisons using only data collected in the field is problematic (Clark et al., 2008).

This paper presents the results from a novel three-player game investigating the impact of social comparisons on ultimatum bargaining. The proposer in the ultimatum game is given an endowment and makes a take-it-or-leave-it offer to the responder about the dividision of the endowment. If the offer is rejected both parties are left with zero earnings. The third player, the non-responder, is a bystander and makes no decisions. He receives a fixed payment which is independent of the proposer's endowment and the other players' actions.

We find that the presence of a non-responder has a pronounced effect on the bargaining outcome. The high rejection rates in our experiment (sometimes higher than 50%) leave little doubt about the impact that social comparisons have on ultimatum bargaining. As far as we are aware, these are the highest rejection rates ever observed in an ultimatum game. It is, therefore, important to understand how the non-responder's payoff may affect bargaining.

To this end, it is useful to reexamine behavior in T5 where the non-responder's payment is 5 ECU. Figure 3 shows that 35.5% of proposers offer 30 ECU or less (compared to 22.6% in T20, 20.7% in T40, and 14.3% in Baseline). While the behavior of proposers is consistent with our alternative hypothesis in individuals are assumed to take into account the non-reponder's payoff in defining what constitutes a fair offer, it is inconsistent with the behavior of responders, the majority of whom demands *at least* 50 ECU. The behavior of responders in T5 is in line with our null hypothesis which is derived on the assumption that responders do *not* consider non-responders as being part of their reference group; hence, the non-responder's payment does not affect responders' definition of a fair offer. At the same time, responders clearly take into account the non-responder in stating their minimum acceptable offers when the latter receives a payment of 20 or 40 ECU (recall that in T20, the median MAO -25 ECU- is only slightly over the payoff to the non-responder, while in T40 the median MAO perfectly coincides with the non-responder's payoff and is slightly higher than in the Baseline treatment).

One explanation for our results is that at least some responders exhibit a self-serving bias in the way they form their reference groups. Babcock and Loewenstein (1997; p.110) define self-serving bias as a tendency to "conflate what is fair with what benefits oneself." There is considerable evidence from psychology and, more recently, behavioral economics for the existence of such bias (Babock and Loewenstein, 1997). Self-serving bias has been offered as an explanation for bargaining impasse. For example, Babcock et al. (1996) find that teachers and school boards engaged in negotiations over teachers' salaries use different districts as "comparable" leading to an impasse. In our case and the hypotheses we derived using Rabin's (1993) model, self-serving bias would imply that responders base their judgement of what is fair (partly) on the non-responder's payoff when doing so is expected to make the responder better off, but not when the non-responder's payoff is very low. Similarly, one could argue that some proposers also behave in a self-serving way as they make low offers in T5 believing that they could secure a higher payoff for themselves.[8]

In summary,.the evidence from our experiments shows that social comparisons can indeed affect bargaining outcomes and suggests that reference groups are formed in a self-serving way. The latter contributes to an important debate in economics about what constitutes the relevant reference group against which an individual compares himself (see Fehr and Schmidt, 2006). However, it is important to emphasize that not all of our subjects exhibit such a bias. Therefore, agent heterogeneity should be taken into consideration when modeling individual behavior. An advantage of our experimental design relative to those in previous studies is the fact that the reference group is exogenously altered. This permits us to examine instances in which self-serving bias is more likely to have an impact on outcomes and test mechanisms for overcoming this problem.

One important question to ask in light of the high rejection rates is whether rejection

[8]This suggests that one might usefully consider an extension of Rabin's model in which the weight placed on an individual's payoff is a function of the payoff itself.

rates in our experiment would be reduced after a number of repetitions of the one-shot game. There are a lot of difficulties with repeating the stage game. Perhaps the most important obstacle is the fact that repetitions would likely introduce past outcomes as additional focal points. This would confound the analysis. However, evidence suggests that rejection rates are likely not to be greatly reduced. First, self-serving assessments tend to occur when there are competing focal points. These focal points in our experiment would be the 50-50 split between proposer and responder, and the non-responder's payoff. As Babcock and Loewenstein (1997, p.110) point out, self-serving assessments can rule out any chances of agreement by eliminating any commonly acceptable outcomes. In turn, this implies that even allowing for multiple rounds of negotiation might not completely eliminate rejections. Second, Knez and Camerer (1995), who also attribute their high rejection rates (47%) to a self-serving bias, find that rejection rates do not decline when the game is repeated five times. We leave analysis of a repeated version of our experiment for future research.

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Table 1 – Summary of experimental results

Treatment	Non-Responder's Payoff	Number of participants	Number of observations	Median MAO	Median Offer	Modal MAO	Modal Offer	Average* MAO	Average* Offer	Rejection Rates (%)
T5	5	93	31	33	40	50	50	27.8	37.2	58.1
T20	20	93	31	25	40	30	40	25.2	37.4	32.3
T40	40	87	29	40	45	40	50	32.1	41.7	27.6
Baseline	n.a.	56	28	32.5	50	40	50	30.7	45.5	10.7

*The calculation of the average excludes one observation for MAO with m=95, and two for Offer (x=80, x=85). MAO stands for minimum acceptable offer.

Figure 1 – Screen shot of effort stage

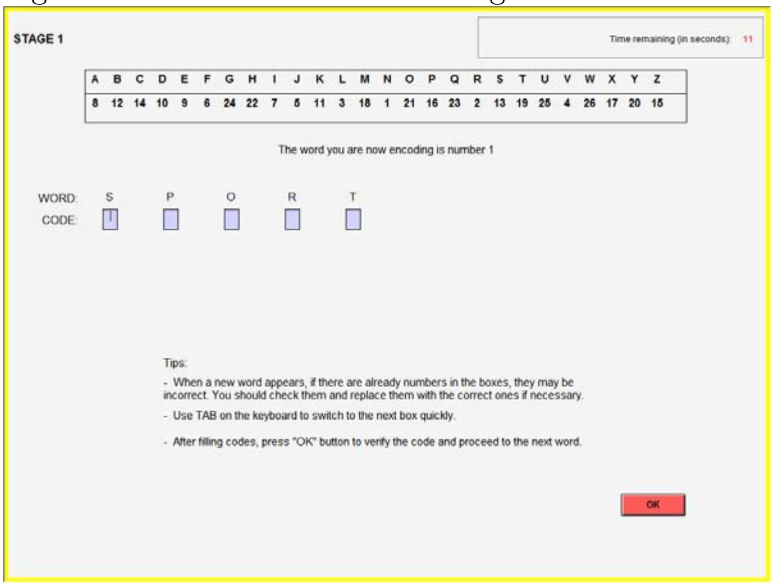


Figure 2 – Cumulative distribution of MAOs by treatment

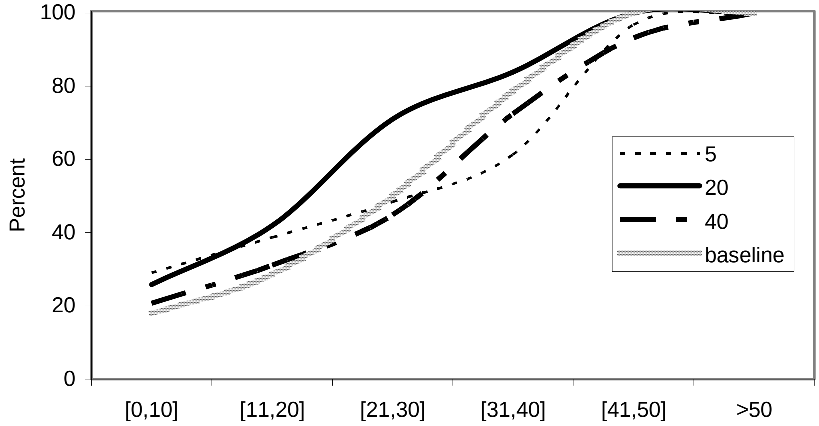


Figure 3 – Cumulative distribution of Offers by treatment

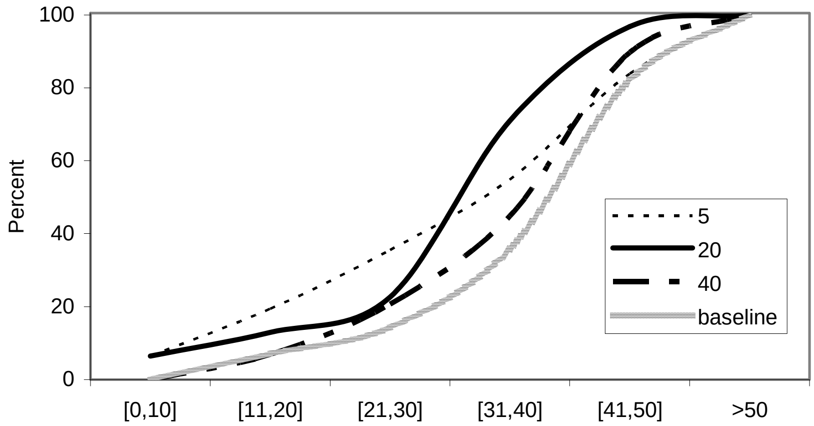


Figure A1 – Distribution of Minimum Acceptable Offers by treatment

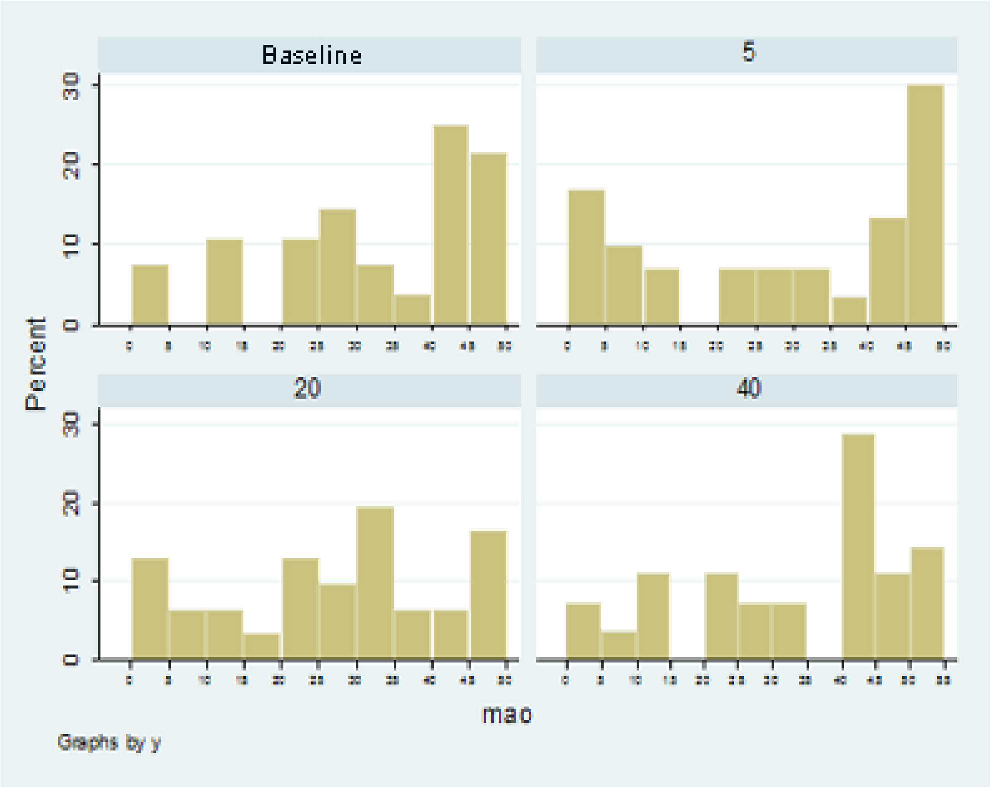
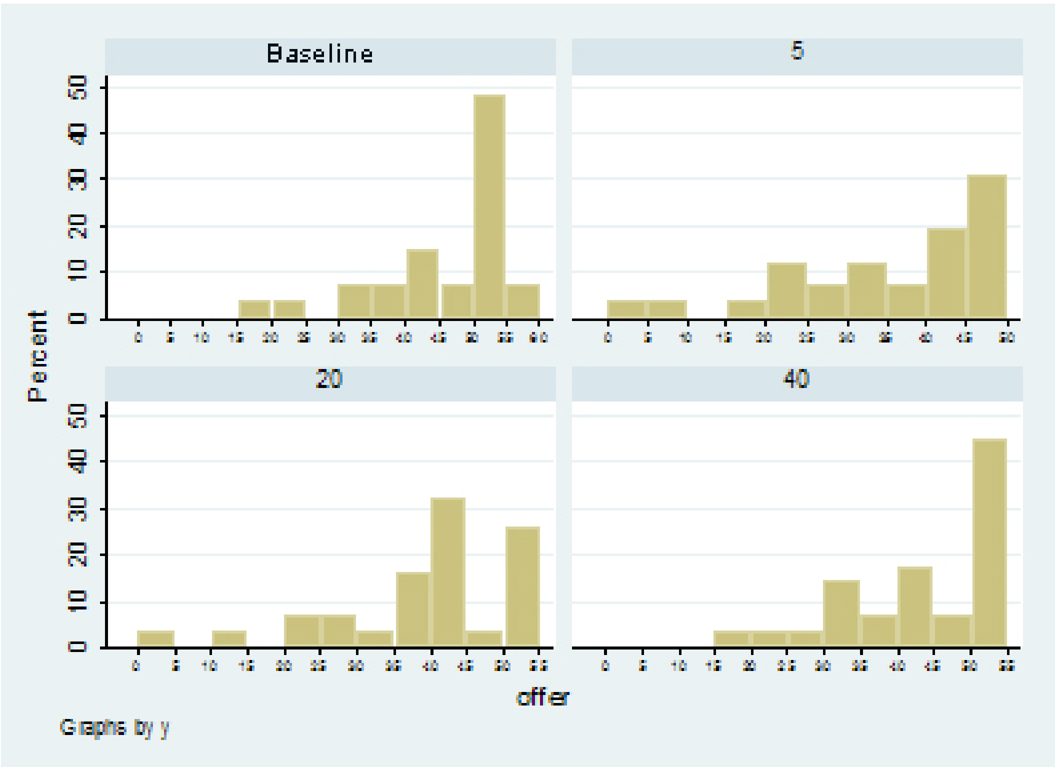


Figure A2 – Distribution of Offers by treatment



Appendix: Impact of the Payment to the Non-Responder on Equilibrium when Players exhibit 'Reciprocal Kindness'

In this appendix we apply Rabin's (1993) model of reciprocal fairness to the game described in section 3. In the analysis below we restrict our attention to equilibrium involving acceptable offers, i.e. offers for which $X \geq M$. In the application of Rabin's theory it seems intuitively plausible to focus on acceptable offers. It seems unlikely that participants would enter the game with the intention of causing a rejection ($X<M$). Similarly it seems unlikely that participants would expect other participants to act in such a way as to cause a rejection. In this event we have:

> Proposition 1: (i) If participants exhibit reciprocal kindness there is a unique, trembling hand perfect Nash equilibrium. In this equilibrium the offer, X^K, is equal to the MAO. (ii) Further, X^K is increasing with the payment to the non responder.

Proof: Let P be the pie, X be the offer, M be the MAO and X^R be the reference level. Further let $\tilde{M}$ be the proposer's belief about the MAO set by the responder and $\tilde{\tilde{X}}$ be the proposer's belief about the responder's belief about the offer. Further let $\tilde{X}$ be the responder's belief about the offer set by the responder and $\tilde{\tilde{M}}$ be the responder's belief about the proposer's belief about the MAO.

Rabin (1992) incorporates reciprocal kindness into the preferences of players by transforming the payoffs of the game. In this paper the transformation of payoffs utilises the following formula:

$$U_i = \pi_i + \rho_i \tilde{f}_i [1 + f_i]$$

where U_i is they payoff to player i, i=P, R and y_i is player i's payoff, $\tilde{f}_i$ is player i's belief of player j's kindness to i and f_i is player i's kindness to player j. The parameter ρ_i is the player i's weighting of reciprocation. The introduction of the parameter ρ_i captures a number of previous treatments. Rabin's original formulation can be recovered by writing α_R=0.5 and replacing ρ_2 with 1/P. The treatment by Dickinson (2000) is recovered by defining weightings α_i where $\rho_i = (1-\alpha_i)/(P\alpha_i)$.

The proof proceeds in the following steps:

A. First the responder's utility is specified when they exhibit reciprocal kindness.

B. The optimal strategies for the responder are determined.

C. The proposer's utility is specified.

D. The optimal strategies for the proposer are determined.

E. Pure strategy Nash equilibrium are found (summarised in lemma 1).

F. The Perfect Nash equilibrium acceptable offers and MAOs are found (summarised in lemma 2).

A. Responder's utility. First consider the responder's preferences. These depend on the responder's interpretation of kindness. Assuming that $\tilde{X} \geq \tilde{\tilde{M}}$ then:

1. If $\tilde{X} \geq M$ then

$$f_2 = \frac{(P-\tilde{X})-(P-X^R)}{P} = \frac{X^R-\tilde{X}}{P}$$

and:

$$\tilde{f}_1 = \tilde{X}-X^R$$

2. If $\tilde{X} < M$ then:

$$f_2 = \frac{0-(P-X^R)}{P} = \frac{X^R-P}{P}$$

and:

$$\tilde{f}_1 = \tilde{X}-X^R$$

Using the above specification of kindness the Responder's utility function can be written as:

$$V_R = \begin{cases} \tilde{X}+\rho_2(\tilde{X}-X^R)\left(1+\frac{X^R-\tilde{X}}{P}\right) & \text{for } \tilde{X} \geq M \\ 0+\rho_2(\tilde{X}-X^R)\left(1+\frac{X^R-P}{P}\right) & \text{for } \tilde{X} < M \end{cases}$$

<u>B. Responder's optimal strategy.</u> The responder chooses $\tilde{X} \geq M$ if:

$$\tilde{X}+\rho_2(\tilde{X}-X^R)\left(1+\frac{X^R-\tilde{X}}{P}\right) \geq \rho_2(\tilde{X}-X^R)\left(1+\frac{X^R-P}{P}\right)$$

Let $\tilde{\theta} \equiv \tilde{X}/P$ and $\tau \equiv X^R/P$. Then:

$$-\rho_2\tilde{\theta}^2 + [1+\rho_2(1+\tau)]\ \tilde{\theta} - \tau\rho_2 \geq 0$$

This inequality is satisfied for all $\tilde{\theta} \in [\underline{\theta}^K,1]$ where:

$$\underline{\theta}^K \equiv \frac{1+\rho_2(1+\tau)-\sqrt{(1+\rho_2(1+\tau))^2--4\tau\rho_2^2}}{2\rho_2}$$

Note that $0<\underline{\theta}^K< \frac{1+\rho_2(1+\tau)}{2\rho_2}$. Intuitively the responder will interpret an offer as fair, and thus acceptable, if it is sufficiently high (i.e. above $P\underline{\theta}^K$).

C. Proposer's utility. Consider the proposer's measures of kindness. Assuming that $\tilde{\tilde{X}} \geq \tilde{M}$:

1. If $X \geq \tilde{M}$:

$$f_1 = \frac{X\text{-}X^R}{P}$$

and:

$$\tilde{f}_2 = (P\text{-}\tilde{\tilde{X}})\text{-}(P\text{-}X^R) = X^R\text{-}\tilde{\tilde{X}}$$

2. If $X < \tilde{M}$:

$$f_1 = \frac{0\text{-}X^R}{P} = \frac{\text{-}X^R}{P}$$

and:

$$\tilde{f}_2 = X^R\text{-}\tilde{\tilde{X}}$$

The Proposer's utility function can now be presented.

$$V_P = \begin{cases} P\text{-}X+\rho_1(X^R-\tilde{\tilde{X}})\left(1+\frac{X\text{-}X^R}{P}\right) & \text{for } X \geq \tilde{M} \\ 0+\rho_1(X^R-\tilde{\tilde{X}})\left(1-\frac{X^R}{P}\right) & \text{for } X < \tilde{M} \end{cases}$$

D. Proposer's optimal strategy. The proposer chooses $X \geq \tilde{M}$ if:

$$P\text{-}X+\rho_1(X^R-\tilde{\tilde{X}})\left(1+\frac{X\text{-}X^R}{P}\right) \geq \rho_1(X^R-\tilde{\tilde{X}})\left(1-\frac{X^R}{P}\right)$$

Equilibrium beliefs require $\tilde{\tilde{X}} = \tilde{X} = X$. Hence the above inequality becomes:

$$\rho_1\theta^2+(1-\rho_1\tau)\theta-1 \leq 0$$

This inequality is satisfied for all $\theta \in [0, \overline{\theta}^K]$ where:

$$\overline{\theta}^K = \frac{1-\rho_1\tau+\sqrt{(1-\rho_1\tau)^2+4\rho_1}}{2\rho_1}$$

If the reciprocation parameter ρ_1 is sufficiently low, the material payoff P-X will dominate the utility from reciprocation. Thus the proposer will set the lowest offer that would result in a payoff. Specifically if $\frac{\rho_1(X^R-\tilde{\tilde{X}})}{P} < 1$ then the proposer's optimal offer is $X = \tilde{M}$. If however $\frac{\rho_1(X^R-\tilde{\tilde{X}})}{P} > 1$ then the reciprocation term dominates, and the proposer's optimal offer is X=P. However this cannot be an equilibrium, as in equilibrium this would require $\tilde{\tilde{X}} = X = P < X^R$ (which is a contradiction). Intuitively the proposer believes the responder is treating them kindly, by accepting an unfair offer. Because the proposer is a strong reciprocator they respond by making a generous offer. Thus in equilibrium, it is required that $\theta > \frac{\rho_1 \tau - 1}{\rho_1} \equiv \underline{\theta}^{\kappa}$.

E. Nash Equilibria

In equilibrium $\tilde{\tilde{X}} = \tilde{X} = X$ and $\tilde{\tilde{M}} = \tilde{M} = M$. Using the above analysis we obtain:

> Lemma 1: X= M is a Nash equilibrium if
> $X/P = \theta \in \Omega^K$ where $\Omega^K = \left[\max(\underline{\theta}^K, \underline{\theta}^{\kappa}), \overline{\theta}^K\right]$.

There are thus multiple Nash equilibrium acceptable offers (with X=M). In the standard non-reciprocal kindness game $\rho_i = 0$. In this case $\underline{\theta}^K = 0$ and $\overline{\theta}^K = 1$, and hence any offer can represent a Nash equilibrium acceptable offer.

F: Perfect Nash Equilibria

We now impose the requirement that the Nash equilibrium be (trembling hand) perfect. The following lemma establishes proposition 1(i):

> Lemma 2: Suppose that $\underline{\theta}^K \geq \underline{\theta}^{\kappa}$ and thus $\Omega^K = \left[\underline{\theta}^K, \overline{\theta}^K\right]$. Then there is a unique (trembling hand) perfect Nash equilibrium with offer $X^K \equiv P\,\underline{\theta}^K$.

Proof: (i) Consider a Nash equilibrium for which $M^*/P = X^*/P \in \left[\underline{\theta}^K, \overline{\theta}^K\right]$.

Assume there is a small non-zero probability, $\mu(X)$, that the proposer makes an offers $X \neq X^*$. Then the expected payoff to the responder if they set MAO equal to M is:

$$V_R(M,\mu,X^*)=\int_M^P X+\rho_2(X-X^R)(1+\frac{X^R-X}{P})\mu(X)dx + \int_0^M \rho_2(X-X^R)(1+\frac{X^R-P}{P})\mu(X)dx +$$

$$+\left(1-\int_0^P \mu(X)dx\right)\left(X^*+\rho_2(X^*-X^R)\left(\frac{X^*-X^R}{P}\right)\right)$$

It is readily shown that $\partial V_R/\partial M= 0$ when $M/P = \underline{\theta}^K$. Hence the only trembling hand perfect Nash equilibrium offer in the set Ω^K is $\underline{\theta}^K$. Note that it is also required that $\underline{\theta}^K \geq \underline{\theta}^\kappa$ if $\underline{\theta}^K P$ is to be a Nash equilibrium offer.

Finally, to establish proposition 1(ii) we relate the equilibrium offer to the reference level.

> Lemma 3: The (trembling hand perfect Nash) equilibrium offer, X^K, is increasing in the reference level.

Proof: By differentiation of the definition of $\underline{\theta}^K$:

$$\partial\underline{\theta}^K/\partial\tau = \frac{\rho_2(1-\underline{\theta}^K)}{1+\rho_2(1+\tau)-2\underline{\theta}^K\rho_2} > 0 \text{ as } \underline{\theta}^K < \frac{1+\rho_2(1+\tau)}{2\rho_2}$$

Hence the equilibrium offer, X^K, increases with X^R.

Instructions

These are the instructions from treatment T5. Instructions for the other treatments were appropriately adjusted.

You are now taking part in an economic experiment. If you read the following instructions carefully, you can, depending on your decisions and of those made by the others, earn a considerable amount of money. It is therefore important that you take your time to understand the instructions.

The instructions which we have distributed to you are for your private information. Please do not communicate with the other participants during the experiment. Should you have any questions please ask us.

During the experiment we shall not speak of Dollars, but of Experimental Monetary Units (EMU). Your entire earnings will be calculated in EMUs. At the end of the experiment the total amount of EMUs you have earned will be converted to Australian Dollars at the rate of **1 EMU = 40 cents** and will be immediately paid to you in cash. Payments will be private and none of the other participants will know how much money you earned in the experiment. Every participant in the experiment will also receive a daily zone 1 concession MetCard.

At the beginning of the experiment the participants will be randomly divided into groups of three. You will therefore be in a group with two other participants. The experiment is divided in two stages.

The effort stage

The first stage is common for all participants and we will refer to it as the 'effort stage'. In the effort stage, all participants will be given a task that will determine the role that they will play in their group during the experiment. The task in the effort stage is the same for everyone. You will be presented with a number of words and your task is to code these words by substituting the letters of the alphabet with numbers using Table 1 on page 4. The effort stage decision screen is seen in Figure 1.

Example: You are given the word FLAT. The letters in Table X show that F=6, L=3, A=8, and T=19.

Once you code a word correctly, the computer will prompt you with another word to encode. Once you encode that word, you will be given another word and so on. **This process will continue for 7 minutes** (420 seconds). All group members will be given the same words to encode in the same sequence.

Allocation of roles

There are three roles in this experiment: Proposer, Responder and Non-Responder. The role of the Proposer is allocated based on the relative performance of the individuals in your group. The person who encodes the highest number of words in will be assigned the role of a Proposer. The participants with the second and third highest number of encoded words in their group will be randomly assigned the role of either the Responder or the Non-Responder. If two or more participants tie in the first place, the computer will determine the roles randomly.

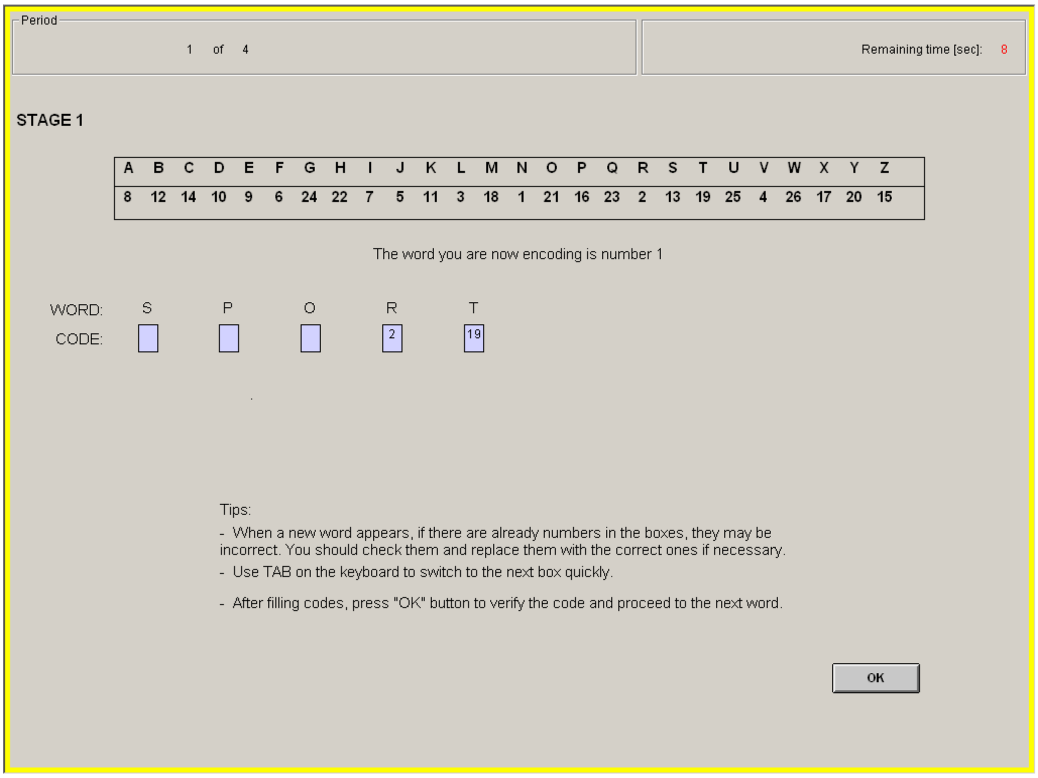


Figure 1

At the end of the effort stage you will be informed about whether you encoded the highest number of words or not. If you have not encoded the highest number of words there will be a random draw that will determine whether you will have the role of the Responder or the Non-responder. You will not be informed about the exact number of words that each group member encoded.

The decision stage

The task of each participant in the decision stage depends on the role they are assigned. The Non-Responder has no decision to make in this stage and will receive 5 EMU at the end of the experiment. The Proposer will be given an endowment of 100 EMU. S/he must then propose a division of the 100 EMU by making an offer to the Responder. The amount can be *any* integer number from 0 to 100 (inclusive). The Responder can accept or reject the offer. If the offer is accepted then the suggested division is implemented. If the offer is rejected then both Proposer and Responder receive 0 EMU. Note that Responders will have to make a decision about the offer they are willing to accept *before* they see the Proposer's actual offer. To do this the

Responder will be prompted to state the *minimum* amount s/he is willing to accept from the Proposer.

If the Proposer's offer is an amount higher or equal to the Responder's minimum stated amount, then the Proposer's offer will be accepted. The Responder will receive the offer and the Proposer will earn 100 EMU minus his/her offer. If the Proposer's offer is lower than the Responder's minimum stated amount, then the offer will be rejected. In that case, both the Proposer and the Responder will receive 0 EMU. The earnings of the Non-Responder are independent of the decisions of the Proposer and the Responder and equal to 5 EMU.

Each decision will be made only once.

Note that if the minimum offer that the Responder is willing to accept is greater than the Proposers offer then both the Proposer and the Responder will have zero earnings from the experiment. Therefore, make sure you take your time to make your decisions.

Control Questions

Please answer the following questions. If you have any questions or have answered all the questions, please raise your hand and one of the experimenters will come to you.

1. What does the effort stage determine? (Tick the correct answer)

□ the roles of each participant
□ who will have the role of the Proposer
□ who will have the role of the Responder
□ who will have the role of the Non-Responder

2. The role of the Responder is (Tick the correct answer)

□ assigned randomly to one of the participants who ranked 2nd and 3rd in the effort stage
□ assigned to the person who ranked 2nd in the effort stage
□ assigned to the person who ranked 3rd in the effort stage

3. Assume that the Proposer offers an amount X to the Responder which is larger than the Responder's minimum stated amount. How much will

(i) the Proposer earn? ....
(ii) the Responder earn? ....
(iii) the Non-Responder earn? ……………………………………

4. Assume that the Proposer offers an amount X to the Responder which, however, is smaller than the Responder's minimum stated amount. How much will

(i) the Proposer earn? ....
(ii) the Responder earn? ....
(iii) the Non-Responder earn? ………………………………….

Table 1

Letters	Numbers
A	8
B	12
C	14
D	10
E	9
F	6
G	24
H	22
I	7
J	5
K	11
L	3
M	18
N	1
O	21
P	16
Q	23
R	2
S	13
T	19
U	25
V	4
W	26
X	17
Y	20
Z	15