How responsive are people to changes in their bargaining position? Earned bargaining power and the 50–50 norm

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Abstract

A recurring puzzle in bargaining experiments is that individuals under–respond to changes in their bargaining position, compared to the predictions of standard bargaining theories. Nearly all of these results have come from settings with bargaining power allocated exogenously, so that individuals may perceive it as having been "unearned" and thus be reluctant to exploit it. Also, equal splits have typically been equilibrium outcomes, leading to a powerful tendency toward 50–50 splits. We conduct a bargaining experiment in which subjects earn their bargaining power through a *real–effort task*. Treatments are based on the Nash demand game (NDG) and a related unstructured bargaining game (UBG). Subjects bargain over a fixed "cake" (amount of money), with disagreement payoffs determined entirely by the number of units of the real–effort task successfully completed. About one–fourth of the time, one player earns a disagreement payoff above half the cake size; in these cases, 50–50 splits are not individually rational, and thus not equilibrium outcomes.

We find that subjects are least responsive to changes in own and opponent disagreement payoffs in the NDG with both disagreement payments below half the cake size. Responsiveness is higher in the UBG, and in the NDG when one disagreement payment is more than half the cake, but in both cases it is still less than predicted. It is only in the UBG when a disagreement payment is more than half the cake that responsiveness to disagreement payoffs reaches the predicted level. Our results imply that even when real–life bargaining position is determined by past behaviour rather than luck, the extent to which actual bargaining corresponds to theoretical predictions will depend on (1) the institutions within which bargaining takes place, and (2) the distribution of bargaining power; in particular, whether the 50–50 norm yields a viable outcome.

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1 Introduction and background

Bargaining is pervasive. Even in Western societies, where haggling over small purchases has been de–emphasised, the selling prices of large goods (e.g., new and used cars, houses) are often determined at least partly by bargaining. Employees' compensation packages are also, in many cases, the result of a bargaining process at either the group level (for unionised jobs) or the individual level (in many professional labour markets). In order to understand how markets work – especially whether, and at what price(s), trade takes place – we must understand bargaining.[1]

A fundamental principle of bargaining is that outcomes depend on bargaining power. Even before bargaining settings were thought of as leading to precise predictions, it was generally understood that the division of surplus would depend on the two parties' relative bargaining positions (Edgeworth, 1881). Later, axiomatic bargaining solution concepts (e.g., Nash, 1950; Kalai and Smorodinsky, 1975) formalised this dependence and quantified it by specifying a precise outcome – the bargaining solution – based on the most important features of the environment. Even non–cooperative game–theoretic approaches to bargaining, which may yield a multiplicity of theoretical predictions, can often me made to have a unique theoretical implication if minor additional assumptions are made; if so, this unique prediction will also reflect the important features of the bargaining environment.

Any solution technique that yields a unique prediction will also imply a comparative–static relationship between features of the bargaining environment and that prediction. In particular, specified changes to the bargaining position of one player relative to the other will have well–defined implications for the bargaining outcome. Whether these theoretical implications are seen in real bargaining is, however, an empirical question, and the preponderance of previous experimental work suggests that bargainers are far less responsive to changes in bargaining position than predicted. (See Section 3 for a review of the relevant literature.)

The goal of the current paper is to improve understanding of how bargaining outcomes are shaped by players' bargaining positions. We use a laboratory experiment, allowing us to maintain, in two important ways, a high degree of control over the bargaining environment relative to observational studies from the field. First, we are able to standardise the rules under which bargaining takes place, in contrast to field studies that must aggregate bargaining outcomes from various, possibly imperfectly understood, bargaining institutions. Second, we have complete information about the *cake size* (the amount being bargained over, which we fix) and *disagreement payoffs* (the amounts they get if bargaining is unsuccessful, more of which below), so that we know exactly what the theoretical prediction is for any particular bargaining pair.

In the experiment, we implement differences in bargaining position by bargainers having different disagreement payoffs. This allows individuals' bargaining power to vary nearly continuously over a large range of possible levels, in contrast to many earlier studies that varied bargaining power in more discrete ways (e.g., first– versus second–mover, number of stages of bargaining, endogenous versus random breakdown). Additionally, for the bargaining environment we will use, all of the most common axiomatic bargaining solutions – as well as those non–cooperative techniques that yield unique solutions – have exactly the same consequence: a unit increase in one's own disagreement payoff implies a one–half unit *increase* in one's own payoff as a result of bargaining, while a unit increase in one's opponent's disagreement payoff implies a one–half unit *decrease* in one's own payoff.[2] We will call these the *own–disagreement–payoff effect* and *opponent–disagreement–payoff effect*, and their sum – a measure of overall responsiveness to bargaining position – the *combined disagreement–payoff effect*.

[1]As an example, many researchers have suggested that male–female wage gaps are at least partly due to sex differences in willingness or ability to bargain; see, e.g., Stuhlmacher and Walters (1999), Small et al. (2007) and Fortin (2008).

[2]Thomson (1987) analyses how various axiomatic bargaining solutions vary with the disagreement outcome for more general bargaining problems with two or more agents.

As already indicated, this prediction has typically not been observed in previous experimental results. Two recent studies (Fischer, Güth and Pull, 2007; Anbarci and Feltovich, 2013) have found combined effects between 40% and 60% of their predicted sizes. However, in both of these experiments, subjects' disagreement payoffs were exogenously chosen and assigned to them; more generally, the vast majority of the studies that vary subjects' bargaining power in any way do so exogenously. A frequent criticism of this experimental design aspect is that when subjects are endowed with bargaining power, they do not perceive this power to have been "earned", and thus do not fully internalise it. As a result, subjects with favourable bargaining position may be reluctant to make full use of it, and those with unfavourable bargaining position may be reluctant to allow their opponents to use theirs. Given such reluctance on both sides, the ubiquitous societal norm of 50–50 splits – irrespective of the distribution of bargaining power – serves as a powerful focal point. We attempt to improve on these previous experiments by making subjects *earn* their bargaining position. Subjects perform a simple but repetitive *real–effort task* (described in Section 4.1), with their disagreement payoffs in the subsequent bargaining game based solely on their own performance on this task. This means that a subject's task performance relative to that of the other bargainer determines not only the *allocation* of favourable bargaining position, but also the *size* of the bargaining advantage the favoured subject has.

We view our design aspect of earned bargaining power to be more realistic than exogenously–assigned bargaining power; a disagreement payoff in real bargaining – being the value of the best alternative opportunity to agreement with the current co–bargainer – is typically the result of past decisions, effort, and intrinsic abilities, with only a small part played by luck. More important than this realism, however, is the effect we expect earned disagreement payoffs to have on bargaining behaviour. We conjecture that subjects, having earned their bargaining power (or lack thereof), will internalise it more fully and thus make use of it to the extent that they can, reducing the attraction of the 50–50 norm and increasing responsiveness to the disagreement outcome.

An additional feature of our design is that subjects can earn disagreement payoffs that are *more than half* of the cake. In these situations, which we refer to as those where a subject has *dominant bargaining power*, equal splits cannot occur in equilibrium, so that the pull of the 50–50 norm ought to decrease still further. Our experimental design is novel, as there have been very few bargaining studies that vary bargaining power nearly continuously through a large range of levels, few in which bargaining power depends on skill and effort, few where individually rational 50–50 splits can be ruled out, and to our knowledge, none with all three of these features.

Subjects in our experiment bargain over a fixed, known cake, and with known disagreement payoffs determined by performance on the real–effort task. They bargain under one of two bargaining institutions, both commonly used in bargaining experiments and in theoretical modelling of the bargaining process: the single–stage Nash Demand Game (NDG), and a continuous–time analogue called the Unstructured Bargaining Game (UBG), which we describe in Sections 2.1 and 2.2.[3]

Our main result is that subjects' responsiveness to earned disagreement payoffs depends on two factors: (1) the bargaining institution used; and (2) the distribution of bargaining power, and in particular, whether either subject has dominant bargaining power. When bargaining is according to the NDG and neither subject has dominant bargaining power, subjects are under–sensitive to disagreement outcomes, to an extent that is in line with previous results. In the UBG with no dominant bargaining power, subjects are substantially more responsive to disagreement payoffs than in the NDG, but still less so than theory predicts. When one bargainer does have dominant bargaining power, by contrast, own– and opponent–disagreement–payoff effects are significantly larger. In the NDG, this increase still

[3]Feltovich and Swierzbinski (2011) conduct an experiment involving games very similar to our NDG and UBG, and find not only less frequent disagreements in the latter than in the former, but also a greater tendency in the former for agreements to split the cake evenly. They attribute these (and other) differences to the higher level of strategic uncertainty in the NDG than in the UBG.

leaves responsiveness below the theoretically predicted level. In the UBG, however, this increased responsiveness leads to levels in line with the theory. Thus, even with earned bargaining power, it takes the combination of a bargaining institution with low levels of strategic uncertainty and elimination of the 50–50 "security blanket" for bargaining behaviour to yield outcomes like those predicted by the theory.

2 The bargaining environment

There is a fixed sum of money (a *cake*) of size £M available to the players. Negative payments are not possible, so that set of feasible agreements is the set of non–negative pairs totalling M or less. The bargaining institution varies with the treatment, as described in Sections 2.1 and 2.2 below. If bargaining is unsuccessful, the players receive *disagreement payoffs* that are typically asymmetric, with the *favoured player* receiving D_f and the *unfavoured player* receiving D_u, such that $D_f > D_u \geq 0$ and $D_f + D_u < M$. The values of M, D_f and D_u (along with which player is the favoured one) are assumed to be common knowledge. We use the term *surplus* to mean the amount of cake remaining after subtracting the sum of the disagreement payoffs ($M - D_f - D_u$); this positive quantity represents the gains available to be made from successful bargaining.

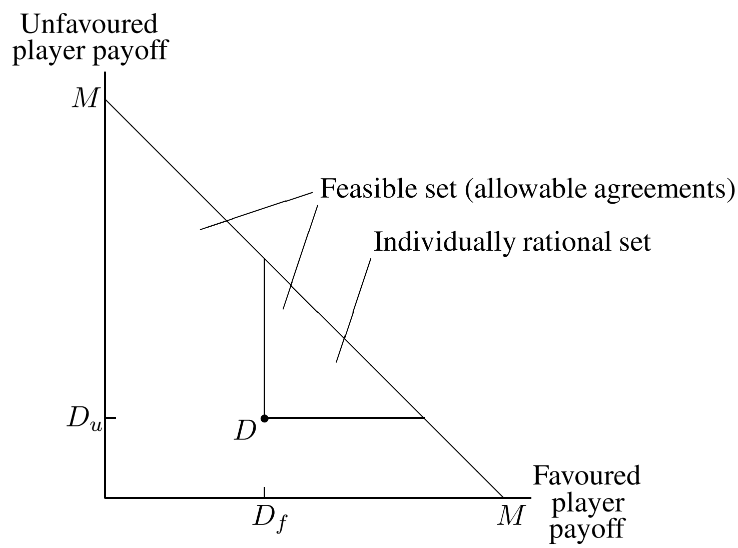

Figure 1: The bargaining environment

2.1 Nash demand game (NDG)

In the Nash demand game (Nash, 1953), bargaining consists of a single pair of simultaneously made demands X_f and X_u by the favoured and unfavoured players respectively. If the demands are compatible ($X_f + X_u \leq M$), then each player receives the amount demanded (any remainder is left "on the table"). If the demands are incompatible ($X_f + X_u > M$), then both receive their disagreement payoffs.

The NDG is simple enough to be analysed by standard non–cooperative techniques, but the result is not a unique prediction. Rather, if Nash equilibrium is the solution concept used, there is a large number of solutions, including (a) efficient pure–strategy equilibria in which $X_f \geq D_f$, $X_u \geq D_u$ and $X_f + X_u = M$, leading to payoffs (X_f, X_u); (b) inefficient pure–strategy equilibria in which $X_f > M - D_u$ and $X_u > M - D_f$, with resulting payoffs (D_f, D_u); and (c) inefficient mixed–strategy equilibria with expected payoffs totalling less than M but more than $D_f + D_u$.

Equilibrium selection criteria such as payoff dominance or efficiency can reduce the set of equilibria, eliminating the inefficient equilibria mentioned above. Harsanyi and Selten's (1988) criterion of *risk dominance*, as well as their general solution concept for non–cooperative games, go even further. Both make the unique prediction that players split the surplus evenly: $X_f = \frac{1}{2}(M + D_f - D_u)$ and $X_u = \frac{1}{2}(M - D_f + D_u)$.[4] The same prediction is entailed by combining payoff dominance (or efficiency) with symmetry, defined relative to the individually rational set (rather than the entire bargaining set).

2.2 Unstructured bargaining game (UBG)

In the unstructured bargaining game, players have a fixed, known amount of time available to negotiate a mutually agreeable division of M. Either player can make proposals, which take the form (X_f, X_u) with $X_f,\ X_u \geq 0$ and $X_f + X_u \leq M$. There is no constraint (other than the time available) on the number, ordering and timing of proposals that can be made, and the cake size remains the same until the time runs out (in contrast to shrinking cake models like Rubinstein's, 1982). Either player can accept any opponent proposal; the first accepted proposal is implemented. (In case both players accept proposals at the same instant, each is implemented with probability one–half.) If no proposal is accepted before the time limit, the disagreement outcome is imposed.

The UBG is too complex to allow the use of standard non–cooperative game–theoretic methods for its analysis, without the imposition of additional assumptions.[5] Instead, we make use of techniques from cooperative game theory. These techniques say little about the precise strategies used by the two players; rather, they have implications about what the outcome of bargaining is. The *core* predicts that the division of the cake corresponds to an efficient Nash equilibrium outcome ($X_f \geq D_f$, $X_u \geq D_u$ and $X_f + X_u = M$), but makes no sharper prediction. Axiomatic bargaining solution concepts can refine this multiplicity of predicted outcomes to a unique one; however, they require an assumption about the relationship between monetary payments and payoffs. If the relationship is proportional (risk neutrality), then the outcome of every well–known axiomatic bargaining solution (including the Nash and Kalai–Smorodinsky solutions) coincides, with $X_f = \frac{1}{2}(M + D_f - D_u)$ and $X_u = \frac{1}{2}(M - D_f + D_u)$.

2.3 Disagreement–payoff effects

We will find it convenient to normalise the bargaining problem by dividing through by the cake size. Define $x_f = \frac{X_f}{M}$, $x_u = \frac{X_u}{M}$, $d_f = \frac{D_f}{M}$ and $d_u = \frac{D_u}{M}$. Then, the prediction of Nash equilibrium (with the additional assumptions of either risk dominance or efficiency and symmetry) for the NDG, and the predictions of the well–known axiomatic bargaining solutions for both the NDG and the UBG – discussed in the previous two sections – all imply the same outcome: $x_f = \frac{1}{2}(1 + d_f - d_u)$ and $x_u = \frac{1}{2}(1 - d_f + d_u)$.

This solution implies that $x_f - x_u = d_f - d_u$ (the difference in the bargainers' payoffs from bargaining is equal to the difference between their disagreement payoffs). Also, it implies a sharp theoretical prediction concerning the

[4]Risk dominance (Harsanyi and Selten, 1988) formalises the intuitive notion that when players have little information about the choices others will make, they will prefer strategies that are (in some sense) less risky. In the simplest case of a symmetric game with exactly two symmetric strict Nash equilibria (s, s) and (t, t), (s, s) is risk dominant if the threshold probability of the opponent choosing s at which s becomes a best response is lower than the corresponding threshold probability for t. In their text, Harsanyi and Selten extend this intuition for general non–cooperative games. Their general solution concept (presented in the same text), while different in that payoff dominance is prioritised over risk dominance, yields the same result here, since the risk dominant outcome is efficient and hence not payoff dominated by any feasible outcome.

[5]See Simon and Stinchcombe, 1989; Perry and Reny, 1993, 1994; and de Groot Ruiz et al., 2010 for non–cooperative game–theoretic analyses of unstructured bargaining using various additional assumptions.

relationship between the individual disagreement payoffs and the bargaining outcome:

$$\frac{\partial x_f}{\partial d_f} = \frac{1}{2} = \frac{\partial x_u}{\partial d_u} \quad \text{and} \quad \frac{\partial x_f}{\partial d_u} = -\frac{1}{2} = \frac{\partial x_u}{\partial d_f}.$$

That is, both the "own–disagreement–payoff effect" and the "opponent–disagreement–payoff effect" have magnitudes of 0.5.

An obvious corollary is that the sum of the magnitudes of these effects is equal to one:

$$\left|\frac{\partial x_f}{\partial d_f}\right| + \left|\frac{\partial x_f}{\partial d_u}\right| = \left|\frac{\partial x_u}{\partial d_f}\right| + \left|\frac{\partial x_u}{\partial d_u}\right| = 1.$$

Indeed, even some bargaining solutions that don't imply own– and opponent–disagreement–payoff effects of one–half still imply that their combined effect is exactly one (e.g., the generalised Nash solution that maximises the expression $[x_f - d_f]^\theta [x_u - d_u]^{1-\theta}$ for given $\theta \in [0,1]$). Also, Anbarci and Feltovich (2013) show that when bargainers are risk averse with (possibly different) constant absolute risk aversion preferences, both risk dominance and the standard Nash solution imply a combined effect of exactly one, and if either has constant relative risk aversion preferences instead, the combined effect is at least one.

3 Related literature

We will not go into detail here regarding the huge literature on bargaining experiments in general (for surveys, see Roth, 1995 and Camerer, 2003, pp. 151–198). Despite this expansive literature, there has been very little research into the effects of disagreement outcomes in particular.[6] What previous work there has been has usually varied disagreement outcomes as a "side–treatment", in conjunction with some other manipulation intended to address the primary research question. Hoffman and Spitzer (1982) examined unstructured bargaining games with (in essence) a fixed, known cake size and one of two randomly chosen disagreement outcomes.[7] Disagreement outcomes were very asymmetric: approximately (0.79, 0) and (0, 0.83) in their "Decision 1" and (0.81, 0.08) and (0, 0.85) in their "Decision 2". Hoffman and Spitzer found a substantial frequency of equal splits of the cake – irrespective of which disagreement outcome was chosen – even though this means that some bargainers were agreeing to payments well below their disagreement payoffs. Their results may not have much implication for our experiment, however, as they are likely at least partly explained by lack of anonymity, as not only did their experiment use face–to–face bargaining, but subject reaching agreement were also required to jointly sign a record sheet.

[6]In addition to the research discussed here, there is also a small literature (Kahn and Murnighan, 1993; Binmore et al., 1998; Feltovich and Swierzbinski, 2011) looking into the effects of varying *outside options* in the NDG. Outside options differ from disagreement payoffs in that they must be chosen in lieu of bargaining, rather than being available afterwards in the event bargaining fails; they therefore represent less bargaining power than a comparable disagreement payoff. Despite this distinction, typical findings in this literature are fairly similar to those found in experiments with disagreement payoffs. Subjects often make minimal use of their bargaining power, with 50–50 splits common except when the outside option is larger than half of the cake, in which case the favoured player receives the outside option or slightly more (the "deal me out" outcome). To our knowledge, all of the papers in this literature have bargaining positions assigned exogenously, rather than being earned.

[7]In their setup, an agreement involved bargainers settling on one of a small number of payment pairs, but side–payments were allowed; thus, under some weak assumptions, one can consider their bargaining set to have a fixed cake size. Rather than directly implementing disagreement outcomes, Hoffman and Spitzer assigned one of the bargainers the role of "controller"; in the case of disagreement, the controller unilaterally imposed one of the payment pairs. Assuming that controllers in this situation would always choose the most favourable payment pair, this was equivalent to randomly choosing one of two disagreement outcomes.

With equal splits frequently occurring even in settings like Hoffman and Spitzer's, where they are not individually rational (and thus not an equilibrium outcome), it is perhaps not surprising that a strand of the literature has been devoted specifically to the focal nature of equal splits in bargaining. Besides the well–known phenomena of 50–50 splits in ultimatum and dictator games (Roth, 1995, pp. 254–292; Camerer, 2003, pp. 48–59) and in symmetric bargaining games (see Note 11), there have been several recent experiments showing large differences in behaviour according to whether 50–50 splits are available. Güth, Huck and Müller (2001) considered binary ultimatum games where proposers have only two pure strategies: a demand of 85% of the cake and, depending on the treatment, 45%, 50% or 55%. They found that demands of 85% were substantially less frequent when the alternative was 50% (a proposal of an equal split) than when the alternative was either of the other two demands. Falk, Fehr and Fischbacher (2003), also looking at binary ultimatum games, found that responders reject demands of 80% of the cake much more often when the proposer's alternative was a 50% demand than when the alternative was a 100% demand, a 20% demand or another 80% demand. Levati, Nicholas and Rai (2013) elicit preference orderings over outcomes for second movers in a trust game, and dictators in a dictator game, and find that when 50–50 splits are an available option, these subjects are much more likely to submit preferences that are non–single–peaked (and thus inconsistent with own–monetary–payoff maximisation as well as nearly all standard models of other–regarding preferences). Janssen (2006) discusses 50–50 splits as a focal point in both the ultimatum game and the Nash demand game, while Andreoni and Bernheim (2009) present examples of the 50–50 norm in the field and report experimental results suggesting that subjects conform to this norm more closely when deviations can be attributed to themselves (rather than luck).

There is also a rich theoretical literature examining the effects of introducing a small amount of incomplete information into the environment: namely, a low but positive probability that one's opponent is "irrationally" or "obstinately" unwilling to accept any offer below some threshold amount.[8] Myerson (1991, pp. 399–403) showed that even when both individuals are rational, the existence of these irrational types implies that each bargainer will receive no less than (approximately) the corresponding threshold amount, since rational bargainers in an unfavourable position can mimic the irrational types' behaviour. (See also Abreu and Gul, 2000, and Compte and Jehiel, 2002.) Such a model, applied to our bargaining environment, could predict a preponderance of roughly 50–50 splits (and more generally, lower responsiveness to disagreement payoffs than under common knowledge of rationality) as long as neither side has dominant bargaining power, since a few irrational types insisting on 50% shares of the cake will make it profitable for unfavoured players to mimic them.

The first experiment combining a bargaining environment with a task designed to let subjects "earn" their bargaining power was Hoffman and Spitzer's (1985) follow–up to their 1982 study. In some of their cells, favourable bargaining position was assigned randomly, while in others, it was assigned based on performance in a game of skill. Perhaps surprisingly, Hoffman and Spitzer found that on its own, whether favoured status was earned or randomly assigned had only a minor effect on bargaining outcomes. Rather, their data indicate that in order for subjects to exploit their bargaining position, it must be that both (1) bargaining power is perceived by subjects to be earned (their "game trigger" treatment), and (2) instructions make clear that exploiting bargaining power is acceptable behaviour (their "moral authority" treatment), with the latter more important than the former. However, even in their cell with *both* a game trigger and moral authority, the favoured player's average payoff from bargaining was still less than her disagreement payoff. Since this study, like their earlier one, used face–to–face bargaining, it is again unclear to what extent these results are driven by a lack of subject anonymity.

[8]Here, "irrational" is used in the popular sense, to indicate extreme obstinacy. It is not meant to imply that such behaviour is inconsistent with economic rationality, since clearly, refusal to accept less than a threshold amount can satisfy completeness and transitivity.

Since Hoffman and Spitzer, a substantial experimental literature has used effort and/or skill to allocate bargaining roles, typically for ultimatum or dictator games (Hoffman et al., 1994; Schurter and Wilson, 2007); these experiments have also found under–responsiveness to changes in bargaining position, though for ultimatum and dictator games, this result arises naturally from theoretical predictions being on opposite endpoints of the outcome space. There is also mixed evidence about the effect of earned bargaining power on responsiveness to bargaining power; for example, Schurter and Wilson, 2007 found dictator–game offers were lower when the dictator position is assigned via a quiz than via a die roll, but they were lower still when it was assigned on the basis of seniority (number of credit hours earned or taken). Other experiments have used effort and/or skill for reasons other than allocating bargaining power: e.g., to determine the cake size (Ruffle, 1998; Konow, 2000; Cherry et al., 2002; Parrett, 2006; Oxoby and Spraggon, 2008; Karagözoğlu and Riedl, 2010; Birkeland, 2011; Corgnet et al., 2011) or to give one bargainer "moral authority" to receive a larger share of the cake (Gächter and Riedl, 2005; Karagözoğlu and Riedl, 2010).

To our knowledge, no previous study has used real effort to determine disagreement outcomes, but we know of two studies that exogenously varied disagreement outcomes systematically through multiple values.[9] Fischer, Güth and Pull (2007) examine a variant of the NDG, where players simultaneously submit an ambitious demand x_i and a (typically smaller) fallback demand g_i. They receive their ambitious demands if they are compatible; if not, they each get their fallback demand if these are compatible, or disagreement payoffs d_i if not. (We alter their notation somewhat, to parallel ours.) Their main research question was how behaviour in this game compared with behaviour in an ultimatum game, but they also varied disagreement outcomes over eleven pairs: (0, .5), (.05, .45), (.1, .4), ... , (.5, 0) as shares of the cake. The perfect negative correlation between opposing players' disagreement payoffs does not allow for a distinction between own– and opponent–disagreement–payoff effects, but one can still compute the combined effect using their data. On average, the results they report imply that $|\partial x_i/\partial d_i| + |\partial x_i/\partial d_{-i}| \approx 0.38$ and $|\partial g_i/\partial d_i| + |\partial g_i/\partial d_{-i}| \approx 0.41$: that is, observed responsiveness to disagreement outcomes was 38% or 41% of the theoretical prediction.[10]

Anbarci and Feltovich (2013) had subjects bargaining in either the NDG or the UBG, with disagreement payoffs randomly drawn in each round, between 25% and 45% of the cake for the favoured player, and between 5% and 25% for the unfavoured player. Anbarci and Feltovich found that in the NDG, the own–disagreement–payoff effect $|\partial x_i/\partial d_i|$ was about 0.2 and the opponent–disagreement–payoff effect $|\partial x_i/\partial d_{-i}|$ was slightly less: roughly comparable to Fischer, Güth and Pull's (2007) finding. In the UBG, both effects were about 0.3 – significantly more than in the NDG, but still only about 60% of the predicted amount. Anbarci and Feltovich show that other–regarding preferences, along the lines of Fehr and Schmidt's (1999) "inequity aversion", can explain the low values of $|\partial x_i/\partial d_i|$ and $|\partial x_i/\partial d_{-i}|$, while either risk aversion or quantal response on its own cannot. Our experiment is designed to examine a complementary possibility: that sensitivity to disagreement payoffs increases when these disagreement payoffs are earned via a real–effort task, when 50–50 splits are not individually rational, or both.

[9]There is an earlier literature in which bargaining position was varied systematically; see, for example, Binmore et al. (1991) and Ochs and Roth (1989). Like Hoffman and Spitzer (1982), these studies tended to manipulate bargaining power only through a small number of discrete cells (though "small" is a relative term, since Kahn and Murnighan's (1993) 2x2x2x2x2 design had 16 different equilibrium payoff predictions). A consistent result is that subjects fail to fully exploit their bargaining power, though the extent to which they do so varies tremendously across experiments. For example, a one–unit increase in a subject's predicted payoff was associated with only a 0.07–unit increase in her observed payoff in Ochs and Roth's experiment, as compared to an increase of two–thirds of a unit in Binmore et al.'s.

[10]Harrison (1987) also varies disagreement payoffs in an unstructured bargaining game, but with perfect *positive* correlation between disagreement payoffs; his "Type 1 game" has a disagreement outcome of (0, 0), while in his "Type 3 game", both players receive equal positive payments in case of disagreement.

4 The experiment

In our experiment, subjects' disagreement payoffs were determined endogenously, based on performance on a real–effort task. The task itself is described next, with some details of its implementation and other aspects of the experimental procedures discussed in Section 4.2, followed by hypotheses in Section 4.3.

4.1 The real–effort task

The task, adapted from the one used by Erkal et al. (2011) to suit our design, was computerised, and was qualitatively similar for all subjects in the experiment. A sample screen–shot is shown in Figure 2. The basic unit of the task was the encoding of a sequence of 2–9 letters (a "word") into numerals, based on a key that was displayed on the computer screen for the entirety of the task. Below each letter of the word was a blank space; to encode the word, the subject needed to type the numeral corresponding to each letter in the space below it, then click a button once the entire word was encoded. If the word was encoded correctly, it was accepted and the word was replaced by a

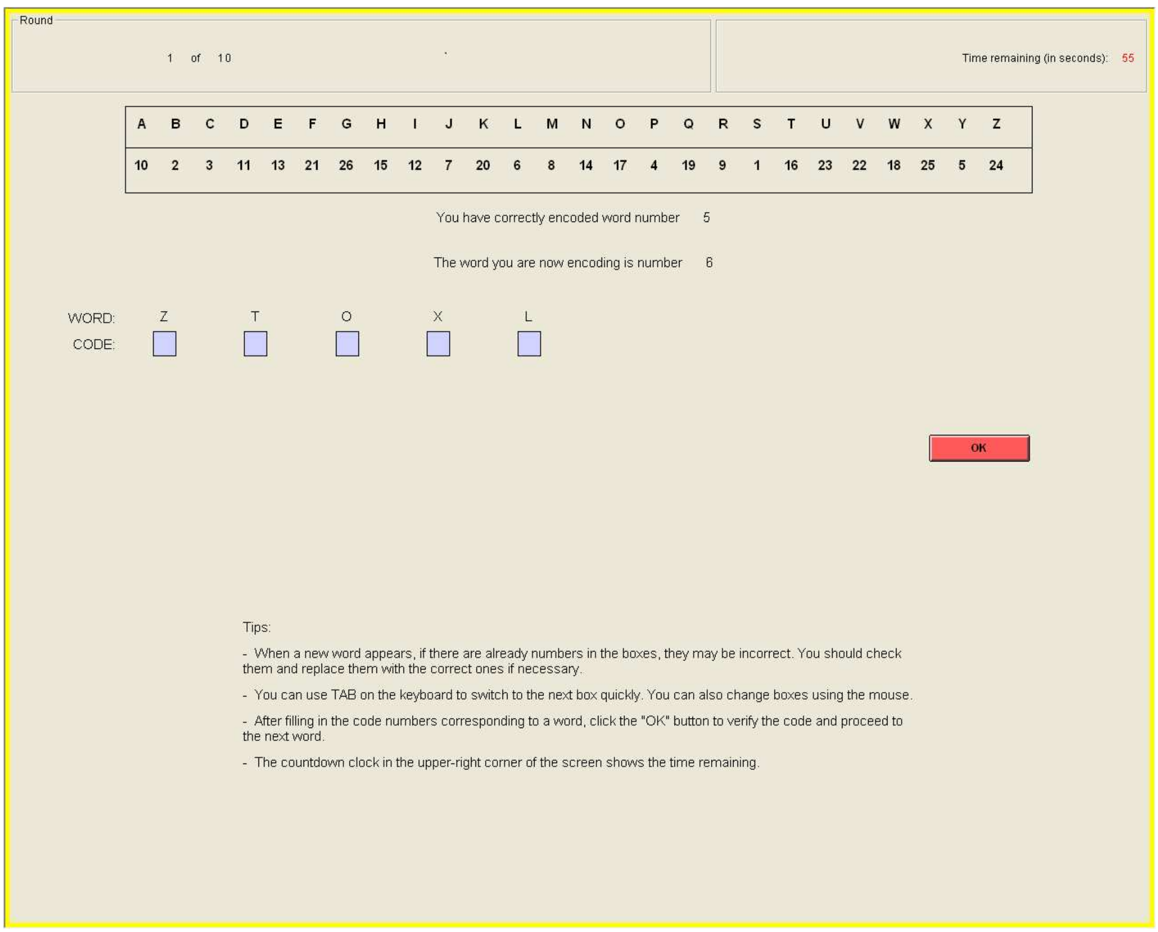

Figure 2: Real–effort task screen–shot

new word. If the word was encoded incorrectly, an error message stated the incorrectly–encoded letter (or the first, if there were more than one), and the subject could go back and change encodings as desired.

At the beginning of a round, a sequence of 400 letters was randomly drawn (with replacement); each letter had an equal chance of being chosen for any given place in the sequence. At the same time, the key – a permutation of

$(1, 2, ..., 26)$ – was randomly chosen from the set of all such permutations. Thus, both the key and the sequence of letters changed from round to round, though both were common to all subjects within a round of a session. Our use of a random sequence of letters means that the vast majority of our "words" were not actual words (in contrast with Erkal et al.'s task, which used a deterministic list of English words); this was done in order that subjects would not be privileged based on their ability to read English. Drawing a new key and sequence of letters in each round of each session was done in order to minimise differences across rounds due to memorisation.

Variation in task difficulty across rounds and subjects was accomplished by varying the lengths of the words. Each subject in each round was given words of changing lengths, drawn i.i.d. from a distribution that was fixed for that subject and round. At the beginning of a session, each subject was assigned a type (advantaged or disadvantaged) that remained fixed throughout the session. Both types of subject faced an easy and a hard version of the task; the four distributions are shown in Table 1. Note that for each type, the hard version of the task contained longer words,

Table 1: Real–effort task – distributions of word lengths

Type	Task	Probability of word length								Expected
	version	2	3	4	5	6	7	8	9	word length
Advantaged	Easy	0.2	0.4	0.2	0.1	0.1	0	0	0	3.5
	Hard	0	0.2	0.4	0.2	0.1	0.1	0	0	4.5
Disadvantaged	Easy	0	0	0.1	0.1	0.2	0.4	0.2	0	6.5
	Hard	0	0	0	0.1	0.1	0.2	0.4	0.2	7.5

in the sense of first–order stochastic dominance, than the easier version. However, the advantaged player was always advantaged; even the disadvantaged/easy distribution contained longer words than the advantaged/hard distribution (again, in the sense of first–order stochastic dominance).

Assigning an easier version of the task to one subject in a pair than the other serves an important function in the experiment: it decreases the likelihood that the subjects will have equal or nearly equal disagreement payoffs. The reason this matters is that many previous experiments have shown symmetric bargaining games to have an extremely strong tendency toward 50–50 splits of the cake, and even when a small amount of asymmetry is present, the 50–50 norm provides a powerful focal point that can affect what outcome the pair does reach.[11]

Relatedly, this design feature allows the difficulty of the task to be set so that one bargainer has a reasonable chance of earning a disagreement payoff more than 50% of the cake (so that equal splits are not individually rational, and hence not consistent with equilibrium) without a corresponding risk that both bargainers will do so (meaning that there would be no gains to agreement). We go even further by dividing the subjects into two fixed types, advantaged and disadvantaged players, with the former *always* having an easier version of the task, and thus likely to have a higher disagreement payoff than their opponents in *every* bargaining round. If subjects expected to have a favourable bargaining position in some rounds but not in others, they might be amenable to more equal splits of the cake in rounds where they have the better position, on the grounds that they expect to make up for this in rounds where they have the worse position.[12]

[11] See Nydegger and Owen (1975) and Roth and Malouf (1979) for experimental comparisons of symmetric and asymmetric bargaining.

[12] On the other hand, Binmore, Shaked and Sutton (1985) found the opposite effect – giving subjects experience in both bargaining roles can mitigate other–regarding preferences – while Bolton (1991) found no difference in behaviour between sessions with changing roles and those with fixed roles. Brandts and Charness (2011, pp. 393–394) provide a brief survey of "role reversal" in general experimental settings,

Except for an initial one–minute practice round, subjects were allotted five minutes in each round for the task. At the beginning of this time, a word length k_i^1 was drawn randomly from the relevant distribution for subject i. That subject's first word would be made up of letters 1 through k_i^1 of the sequence that had been drawn for that round. When that word was correctly encoded, a new word length k_i^2 was drawn, independently of the previous word length, and the subject's second word would comprise letters $k_i^1 + 1$ through $k_i^1 + k_i^2$ of the sequence. In a similar way, the subject's j–th word was made up of the next k_i^j letters of the sequence. Thus, all subjects in a session faced the same sequence of letters; it was only the way the sequence was broken up into words that varied across subjects.

Before continuing, we would like to remark that implementing disagreement outcomes via real–effort tasks rather than exogenously – while possibly having the advantage that subjects internalise them to a greater extent – is not costless. One obvious cost of having disagreement payoffs depend on performance in the task is that the experimenter loses a modicum of control over these disagreement payoffs. Fischer, Güth and Pull (2007) and Anbarci and Feltovich (2013) were able to specify the exact disagreement outcomes subjects faced (in the former paper) or the exact distribution from which these outcomes were drawn (in the latter). In the current paper, we are unable to do so, since the outcomes depend on subjects' behaviour. Indeed, even with the substantial difference in difficulty between the advantaged player's and disadvantaged player's tasks, we cannot guarantee that within a bargaining pair, the advantaged player will have the relatively better bargaining position.

A second disadvantage concerns the time subjects spend on the real–effort task, which is time they aren't spending on the bargaining game. The five minutes in each round allocated to the task itself, along with another half–minute for related aspects such as feedback, carry an opportunity cost of two to three rounds of bargaining in the UBG, or four to five rounds in the NDG, holding the length of the session constant. This has two negative implications: fewer data points of bargaining for a given length of session (and thus, roughly speaking, for a given budget for subject payments), and less opportunity for subjects to acquire experience in the bargaining task, leading to a higher variance in the data and more outliers (though on the plus side, the smaller number of rounds in our experiment implies higher stakes per bargaining round, so that subjects might be expected to take their decisions more seriously). Both of these implications complicate comparisons to previous studies with endowed bargaining power.

Finally, there is no guarantee that our use of a real–effort task will succeed in getting subjects to view their bargaining power as earned. As we will discuss in the next section, we made the differences in task difficulty (within and between subjects) as opaque as possible, in an attempt to get subjects to view their bargaining power as arising from skill and effort rather than luck. However, it is possible that especially clever subjects could infer the role that luck played in determining their disagreement payoffs. It is also possible that some subjects may have considered the task itself to be sufficiently contrived that (irrespective of the relative importance of luck vs. skill or effort) bargaining power based on performance on it would never be viewed as being earned. Either of these potential losses of experimental control would have the effect of pushing results toward those of a more typical experiment with endowed disagreement payoffs, making our experiment a conservative examination of earned bargaining power.

4.2 Experimental design and procedures

All experimental sessions consisted of ten rounds, plus an initial practice round (round 0) with no bargaining and no effect on subjects' payments. Rounds 1–10 were split into five two–round blocks; within each block, one round comprised five minutes of the task followed by bargaining, while the other consisted of the task only; except for the presence or absence of bargaining, the rounds within a block were identical (allowing for a comparison of task

and conclude that there is little evidence of any systematic effect.

performance with and without a subsequent bargaining round; see Appendix A). In half of the sessions, bargaining took place in the even–numbered rounds bar round 0, and in the other half of sessions, bargaining took place in the odd–numbered rounds (see Table 2). The cake size was always £10, and a subject's disagreement payoff in all

Table 2: Experimental design and session information

Cell	Bargaining game	Rounds in which bargaining took place	Groups	Subjects
NDG–odd	NDG	1, 3, 5, 7, 9	6	40
NDG–even		2, 4, 6, 8, 10	6	48
UBG–odd	UBG	1, 3, 5, 7, 9	6	46
UBG–even		2, 4, 6, 8, 10	6	42

bargaining rounds was £0.15 for each word correctly encoded in that round. (In rounds with no bargaining, this amount was simply the subject's payoff for the round.)

Each session, depending on its size, comprised one or more *groups*. A group contained equal numbers of advantaged and disadvantaged subjects – with these types randomly drawn before the practice round and fixed throughout the session – and was closed with respect to interaction. That is, advantaged subjects in a particular group interacted only with disadvantaged subjects in the same group, so that data from different groups can be considered statistically independent of each other.

Sessions took place at the Scottish Experimental Economics Laboratory (SEEL) at the University of Aberdeen. Subjects were primarily undergraduate and masters–level students from University of Aberdeen, and were recruited using the ORSEE software (Greiner, 2004) from a database of people expressing interest in participating in economics experiments. No one took part more than once. The experiment was run on networked personal computers, and was programmed using the z–Tree software package (Fischbacher, 2007). Subjects were asked not to communicate with other subjects except via the computer program.

At the beginning of a session, subjects were seated in a single room and given written instructions; these instructions were also read aloud in an attempt to make the rules of the game common knowledge.[13] Once the instructions were read and questions answered, subjects were given a few demographic questions to answer.[14] After all subjects had submitted their answers, the practice round began.

Subjects were randomly matched to opponents in each bargaining round, with each opposite–type subject in the same group equally likely (a two–population protocol). No identifying information was given about opponents, in an attempt to minimise incentives for reputation building and other supergame effects. Also, to reduce demand effects, rather than using terms like "opponent" or "partner" for the other player, we used the neutral though somewhat cumbersome "player matched to you" and similar phrases.

Each round began with a screen displaying instructions for the real–effort task, which were also shown during the task itself. After a few seconds, the task would begin. The task difficulties in each round are shown in Table 3. Notably, while we used no deception in the experiment, we withheld a lot of information about task difficulty from

[13]Sample instructions are shown in Appendix B. The remaining sets of instructions, as well as the raw data from the experiment, are available from the corresponding author upon request.

[14]We do not use the responses to these questions in our analysis of bargaining outcomes, but in Appendix A we look at their ability to explain behaviour in the real–effort task.

Table 3: Real–effort task difficulty by round

Round(s)	Task difficulty		Time allotted
	Advantaged player	Disadvantaged player	(seconds)
0 (practice)	Hard	Hard	60
1–2	Hard	Hard	300
3–4	Hard	Easy	300
5–6	Easy	Easy	300
7–8	Easy	Hard	300
9–10	Hard	Easy	300

the subjects; for example, they were not told that it varied over rounds or across subjects. The only information subjects received was the sequence of words they faced in each round, and whatever they could infer in bargaining rounds from the opponent's number of words encoded. This was done in order to reinforce subjects' perceptions that differences in disagreement payoff were due to skill and effort, not external factors.

Rounds without bargaining ended after the time allotted for the task had run out, with a feedback screen displaying the number of words the subject had correctly encoded, as well as the subject's payoff of £0.15 per word. In rounds with bargaining, after the task finished, subjects were informed of the number of words they and their opponent had encoded, and the corresponding disagreement payoffs of £0.15 per word; they were also reminded of the cake size of £10. After viewing their disagreement outcome, subjects in the NDG treatment were prompted to choose their demands. Demands were required to be whole–number multiples of £0.01, between zero and the cake size inclusive.[15] After all subjects had chosen their demands and clicked to continue, they received end–of–round feedback: own and opponent words encoded and disagreement payoffs, own and opponent demands, whether agreement was reached, and own and opponent payoffs. Previous results were also collected into a history table at the top of the computer screen; this could be reviewed at any time. After all subjects clicked a button on the screen to continue, the session proceeded to the next round.

In the UBG cells, subjects were given a 120–second "negotiation stage" (130 seconds in the first bargaining round) to reach agreement on a division of the cake. Figure 3 shows a sample screen viewed by subjects during this time. Subjects could make as many or as few proposals as they wished during the 120 seconds; a proposal consisted of a nonnegative multiple of £0.01 for the sender and one for the receiver, adding up to the cake size or less. There were no other constraints on proposals; e.g., later proposals did not have to be more generous than earlier ones, and it was not necessary to wait for the opponent to counter before making the next proposal. Proposals could not be withdrawn once made, and no messages were possible apart from the proposals.[16] Both the subject's own proposals

[15]Our restriction of NDG demands and UBG proposals to hundredths of a pound, necessitated by the discreteness of money, has minor effects on theoretical predictions. In particular, when the sum of disagreement payoffs is an odd number of pence, there is no longer a unique prediction according to symmetry, risk dominance and the axiomatic bargaining solutions; instead, there will be two distinct predictions, differing by one penny, and instead of each player receiving exactly half of the surplus, each receives half of the surplus plus/minus £0.005. For example, for a disagreement outcome of (£3.00, £3.15), all of these concepts predict agreements of either (£4.93, £5.07) or (£4.92, £5.08).

[16]Our prohibition of cheap talk, and the restriction of negotiation to computers rather than face–to–face interaction, were intended to maintain anonymity between bargainers in the experiment. This is important, as removing this anonymity opens up the possibility of side–payments or threats outside the laboratory. However, we acknowledge that lack of anonymity can be an important feature of some real bargaining situations. We also note that a side consequence of both of these design choices is they keep the level of social distance between

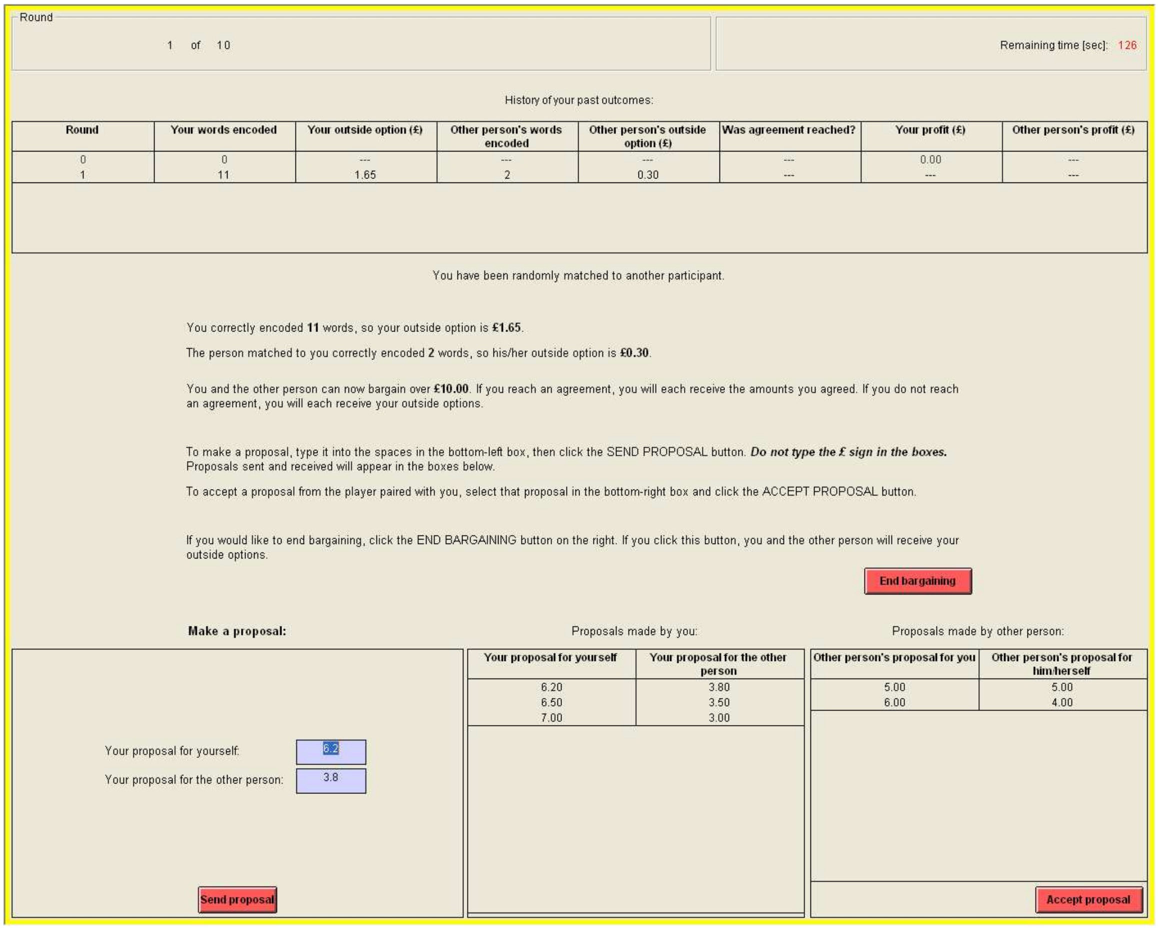

Figure 3: UBG negotiation stage screen–shot

and the proposals of the opponent were displayed on the subject's screen (in separate areas); it was not possible to view proposals for other pairs of subjects. As long as the negotiation stage hadn't ended, a subject could choose to accept any of the opponent's proposals, at which time that proposal would become binding. The opponent's proposals were listed in order of increasing payoff to the subject, so there was almost no cognitive effort required to determine the most favourable opponent proposal (it was always at the bottom of the list), though subjects were free to accept less favourable proposals if they wished. The negotiation stage ended if a proposal was accepted, if either subject in a pair chose to end it (by clicking a button on the screen), or if the time had expired without an accepted proposal; in these latter two cases, the disagreement outcome was imposed. In any case, after the negotiation stage ended, subjects received end–of–round feedback: own and opponent words encoded and disagreement payoffs, whether agreement was reached, and own and opponent payoffs. As in the NDG cells, subjects' previous results were also available in a history table.

At the end of round 10, the session ended and subjects were paid, privately and individually. For each subject, two bargaining rounds and two task–only rounds were randomly chosen, and the subject was paid his/her earnings in those four rounds. There was no show–up fee. Total earnings averaged £19.40 for favoured subjects (with a standard deviation of £2.96) and £13.34 for unfavoured subjects (with a standard deviation of £2.17), for a session

the bargainers relatively high. Some research (e.g., Hoffman, McCabe and Smith, 1996; Bohnet and Frey, 1999; Rankin, 2006) has found that lower levels of social distance are associated with a greater prevalence of other–regarding behaviour.

that typically lasted about 90–100 minutes (including about 10 minutes devoted to instructions, and another 10 minutes for other administration such as collecting consent forms).

4.3 Hypotheses

Our main hypotheses come from the implications of standard theory, as discussed in Section 2.3. All of the well–known axiomatic bargaining solutions, as well as Nash equilibrium combined with either efficiency and symmetry, risk dominance, or Harsanyi and Selten's (1988) solution concept, predict that the own– and opponent–disagreement–payoff effects will have magnitude 0.5. This implies the following null and alternative hypotheses (with the alternative in square brackets):

Hypothesis 1 *For both games and player types, a one–unit increase in a player's own disagreement payoff is associated with a one–half–unit [less than one–half–unit] increase in that player's payoff.*

Hypothesis 2 *For both games and player types, a one–unit increase in a player's opponent's disagreement payoff is associated with a one–half–unit [less than one–half–unit] decrease in that player's payoff.*

5 Experimental results

The experiment comprised twenty–four independent groups – six for each cell (NDG–odd and –even; UBG–odd and –even) – with a total of 176 subjects (see Table 2). Hence, our experimental design is perfectly counterbalanced at the group level, so we are able to pool the –odd and –even cells within each game when we use group–level data. However, due to variability in show–up rates across sessions, not all groups were the same size, so our design is not counterbalanced at the individual level, and we will have to include controls for game and ordering when we use individual–level data.

In our analysis, we will typically express quantities like demands, payoffs, and disagreement outcomes as fractions or percents of the cake size, as in Section 2.3. We will concentrate on four sub–samples of the experimental data–set. In the NDG, each subject makes a single demand, which may or may not lead to agreement. Our first sub–sample will comprise all bargaining pairs in the NDG (we will often call this "NDG–all") – giving us one demand for each member of the pair, for all NDG pairs over all rounds. Our second sub–sample is a subset of the first, comprising only pairs whose demands were compatible. In this "NDG–agreements" sub–sample, we will again have one demand for each member of the pair, for all NDG pairs in any round in which they reached agreement.

In the UBG, each subject may make a single proposal, multiple proposals, or no proposals at all. Each proposal made by a subject could be interpreted as a demand by that subject. Moreover, a subject's acceptance of an opponent proposal, and even failure to accept a proposal, can be construed as carrying information about demands. Inferring a single demand from all of a subject's behaviour in a UBG round carries a big subjective element. To minimise this subjectivity in analysing our data–set, we classify UBG demands based on unambiguous rules. Our "UBG–agreements" sub–sample is straightforward. We interpret each accepted proposal in the UBG as a pair of compatible demands; if a pair does not reach an agreement, it is left out of this sub–sample. Our "UBG–all" subsample includes these demands, but additionally, in cases where agreement was not reached, we examine the sequence of proposals and counter–proposals to find the *lowest* amount each bargainer proposed for him/herself. If this lowest amount exists (i.e., if the subject made at least one proposal), this was taken to be the demand. If a subject made no proposals and did not accept any opponent proposals, that subject was left out of this subsample. (This happened only once in 440

UBG observations.) It is worth noting that for both UBG and NDG, demands conditional on agreement are identical to payoffs conditional on agreement.

5.1 Preliminaries

Table 4 presents some statistics about the real–effort task. As already noted, it is possible for a subject with an

Table 4: Aggregate statistics – real–effort task

All non–practice rounds			Advantaged player		Disadvantaged player	
			Easy task	Hard task	Easy task	Hard task
Actual task difficulty		mean/std. dev.	3.50 / 0.20	4.51 / 0.26	6.51 / 0.28	7.52 / 0.28
		range	(2.92,4.35)	(3.78,5.67)	(5.71,7.33)	(6.69,8.24)
Task performance (words encoded)	task–only rounds	mean/std. dev.	33.5 / 6.0	25.7 / 5.5	18.8 / 3.2	15.5 / 2.7
		range	(19,48)	(3,39)	(11,27)	(8,23)
	NDG rounds	mean/std. dev.	32.7 / 7.5	25.5 / 6.0	18.6 / 4.0	15.2 / 3.3
		range	(2,46)	(0,40)	(0,29)	(0,24)
	UBG rounds	mean/std. dev.	33.1 / 6.6	25.1 / 5.6	18.9 / 3.1	15.5 / 2.8
		range	(5,46)	(3,38)	(11,27)	(7,22)

All bargaining rounds		Task difficulties (advantaged/disadvantaged player)				
		Easy/Easy	Easy/Hard	Hard/Easy	Hard/Hard	Total
Number of pairs	in total	88	88	176	88	440
	with d_i lower for advantaged player	3 (3.4%)	1 (1.1%)	15 (8.5%)	5 (5.7%)	24 (5.5%)
	with d_i lower or equal for advantaged player	3 (3.4%)	1 (1.1%)	22 (12.5%)	8 (9.1%)	34 (7.7%)
	with $d_f > 0.5$	43 (48.9%)	47 (53.4%)	12 (6.8%)	3 (3.4%)	105 (23.9%)
	with $d_f + d_u \geq 1$	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)

easier task (the advantaged player) nevertheless to encode fewer words (and thus be the unfavoured player in the subsequent bargaining game). Indeed, since the word–length distributions of the four versions of the task have overlapping support (see Table 1), it is not even possible a priori to rule out the possibility that within a bargaining pair, the disadvantaged player actually had the easier task than the advantaged player. However, the top portion of Table 4 shows that it was never the case that any advantaged player actually faced even a weakly more difficult task than any disadvantaged player (which of course is even stronger than showing that within a pair, the advantaged player always had the easier task).

The middle portion of the table shows that there was substantial heterogeneity in task performance (words encoded) within a type (advantaged or disadvantaged) and task level, leading to non–negligible overlap in the two types' performance. As an illustration, notice that an advantaged player who encodes one standard deviation less than the mean in the hard task would have performed worse than a disadvantaged player who encodes one standard deviation more than the mean in the easy task. If these two were paired in either bargaining game, therefore, the disadvantaged player would have the favourable bargaining position.

The bottom portion of the table shows that within a bargaining pair, the advantaged player usually, but not always, becomes the favoured player. In about 5.5% of pairs, the advantaged player earns a lower disagreement payoff than the disadvantaged player; in another 2.2%, the bargaining game is symmetric (disagreement payoffs are equal).[17] Also, in just under one–quarter of pairs, the advantaged player encodes enough words in a bargaining round to earn a disagreement payoff larger than £5 (dominant bargaining power); as mentioned previously, dominant bargaining power is of interest because 50–50 splits of the cake are no longer equilibrium outcomes. Finally, the last row portion of the table tells us that it was always the case that there were gains to be made from agreement: the bargainers' disagreement payoffs never totalled the cake size or more.

5.2 Aggregate behaviour

Some aggregate bargaining results are presented in Table 5. The first row shows that agreements (compatible demands in the NDG, accepted proposals in the UBG) are more frequent in the game (UBG) with less strategic uncertainty. The remainder of the table shows, for both games, the mean disagreement payoff for both advantaged and disadvantaged subjects; the fraction of observations with dominant bargaining power (a disagreement payoff larger than half the cake size); and both types' mean demands, both unconditional and conditional on agreement. The table also shows the corresponding statistics for favoured and unfavoured subjects. We can see that results are

Table 5: Aggregate statistics – bargaining outcomes

Game:	NDG		UBG	
Subject type:	advantaged/ disadvantaged	favoured/ unfavoured	advantaged/ disadvantaged	favoured/ unfavoured
Agreement frequency	0.618		0.800	
Mean disagreement payoff (d_i)	0.426/0.258	0.433/0.253	0.424/0.263	0.430/0.259
Frequency of $d_i > 0.5$	0.241/0.000	0.241/0.000	0.236/0.000	0.236/0.000
Mean demand, unconditional	0.563/0.482	0.563/0.483	0.562/0.453	0.569/0.446
Mean demand, given agreement	0.504/0.442	0.507/0.440	0.553/0.447	0.560/0.439

not extremely sensitive to whether we categorise by favoured/unfavoured or disadvantaged/advantaged status, so we will concentrate on the former.

One result suggested by Table 5 is that the favoured player makes some, but only limited, use of her better bargaining position. In the NDG, favoured subjects' disagreement payoffs are higher than their opponents' by an average of 17% of the cake (i.e., 17 percentage points), but overall, they demand only 8% more of the cake, and conditional on agreement, the difference is only about 6% of the cake. This under–exploitation of bargaining power is less stark in the UBG, but still present: on average, despite having a higher disagreement payoff by over 16% of the cake, the favoured player demands only 11% more of the cake, and the difference is the same in agreements.

[17] The ten observations of symmetric bargaining, out of 440 total observations, constitute too small a sample to draw any definitive conclusions, but they are consistent with our rationale for trying to impose unequal disagreement payoffs. All four of the symmetric pairs in the UBG treatment, and five of the six in the NDG treatment, reached agreement (well above the overall agreement frequencies reported in Table 5). Three of the four agreements in the UBG were for even splits of the cake, and seven of the twelve demands in the NDG were for 50% of the cake (with three of the remaining five between the subject's disagreement payoff and 50%, perhaps reflecting responses to the high degree of strategic uncertainty in the NDG).

Table 6 reports corresponding non–parametric test results.[18] The first row shows that subjects do take account of their bargaining power, as favoured players' demands are significantly higher than unfavoured players' – for all four sub–samples (two–tailed Wilcoxon signed–ranks test, group–level data, $p < 0.01$). On the other hand, the

Table 6: Aggregate statistics – p–values from non–parametric tests

Within–game tests	NDG (all)	NDG (agreements)	UBG (all)	UBG (agreements)
$x_f > x_u$	$p \approx 0.001$	$p \approx 0.005$	$p < 0.001$	$p < 0.001$
$x_f - x_u < d_f - d_u$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p \approx 0.002$
Between–game tests	NDG (all) vs. UBG (all)		NDG (agreements) vs. UBG (agreements)	
$(x_f - x_u) - (d_f - d_u)$ smaller in NDG	$p < 0.01$		$p \approx 0.02$	

Wilcoxon signed–rank test used for within–game tests, robust rank–order test used for between–game tests. Both use group–level data and two–tailed rejection regions.

second row shows that they significantly under–exploit their bargaining power; the difference is demands between favoured and unfavoured players is significantly less than the difference in their disagreement payoffs ($p < 0.01$ for all four samples). Finally, the bottom row gives evidence of systematic differences between the UBG and NDG treatments. Specifically, the difference $(x_f - x_u) - (d_f - d_u)$, the extent to which subjects exploit their bargaining power, is significantly less (larger in magnitude) in the NDG than in the UBG. This means that subjects are *more* under–responsive to disagreement payoffs in the NDG than in the UBG; that is, subjects more fully exploit their bargaining power in the UBG.

Next, to illustrate more detail about how subjects respond to their bargaining position, we disaggregate the data according to three characteristics. First, we classify the favoured subject's bargaining power according to which interval $d_f - d_u$ falls in: [0, 0.05), [0.05, 0.1), ... , [0.4, 0.45) or [0.45, 0.5]. Second, we classify the bargaining outcome according to which interval $x_f - x_u$ falls in: [–1, –0.15], (–0.15, –0.05], (–0.05, +0.05], ... , (+0.35, +0.45], (+0.45, +0.55] or (+0.55, +1]. Third, we note whether the favoured subject had dominant bargaining power. Then, for each (disagreement–outcome, bargaining–outcome) pair, we plot a white circle with area proportional to the total number of outcomes in that interval pair, and a black circle with area proportional to the number of outcomes with dominant bargaining power, so that the area of the region inside the white circle but outside the black circle represents the number of outcomes with $d_f < 0.5$. The result is shown in Figure 4. For comparison, each panel shows the horizontal line segment corresponding to a 50–50 split and the diagonal segment corresponding to the theoretical prediction of an equal split of the surplus. Finally, each panel shows two dotted OLS trend lines that illustrate the association between relative bargaining position and bargaining outcomes: one each for pairs with and without dominant bargaining power.

Each panel shows a considerable amount of heterogeneity in outcomes, even across pairs with similar relative bargaining positions. The large circles along the horizontal line segments highlight the focal nature of the 50–50

[18] See Siegel and Castellan (1988) for descriptions of the non–parametric statistical tests used in this paper, as well as for tables of critical values. Some critical values for the robust rank–order test are from Feltovich (2005). We note that in implementing these tests, we err on the side of conservatism in two ways. First, we use group–level data rather than more disaggregated data, so that we ignore the information that can be gained by looking at individuals separately. (While individuals within a group should not be assumed to be statistically independent of each other, neither are they perfectly correlated.) Second, we pool data from cells with bargaining in even– and odd–numbered rounds. We did this because we did not find systematic differences between these cells within either game, but to the extent that differences are indeed present, this will add a source of variance that will reduce the apparent significance of our test statistics.

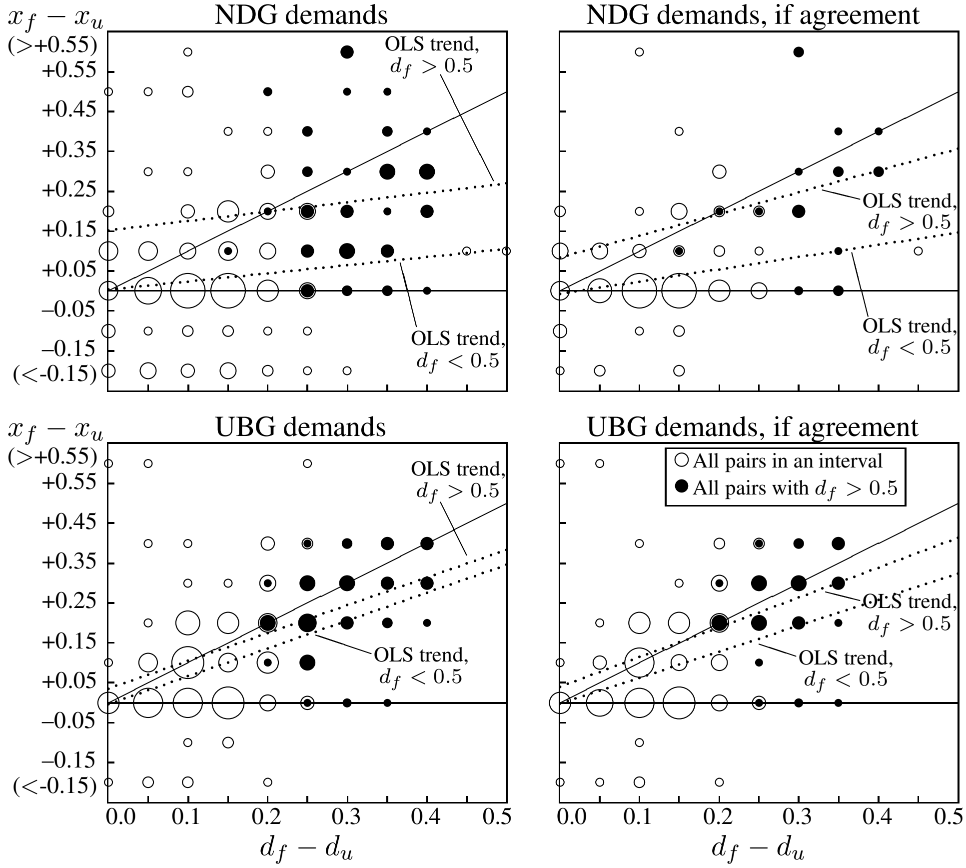

Figure 4: Bargaining outcomes, disaggregated by difference in disagreement payoffs. *Circles' areas are proportional to numbers of occurrences; horizontal lines represent equal splits of the cake; diagonal solid lines represent equal splits of the surplus; diagonal dotted lines represent least–squares fits to data for* $d_f > 0.5$ *and* $d_f < 0.5$.

norm, though the relative dearth of black circles here indicates that when equal splits aren't individually rational, they seldom occur. The trend lines labelled "$d_f < 0.5$" show that when neither subject in a pair has dominant bargaining power, outcomes only weakly reflect differences in bargaining power: these trend lines have intercepts near the origin and slopes that, while positive, are well below their theoretical predictions, especially in the NDG. The intercepts near the origin also imply that there is continuity in behaviour as $d_f - d_u$ approaches zero; i.e., there is no apparent effect due to either favoured status or symmetric bargaining per se. Comparing the $d_f < 0.5$ and $d_f > 0.5$ trend lines in each panel, we see some evidence that situations with dominant bargaining power are treated differently. The more apparent difference is a shift, which implies a discontinuity in behaviour at $d_f = 0.5$. There is also a (less discernible) difference in slopes between the two trend lines in each panel, with the $d_f > 0.5$ line always weakly steeper than the $d_f < 0.5$ line. The differences in slopes are particularly noteworthy; they mean that subjects are more responsive to disagreement payoffs when one of them is more than half the cake size, than when neither is.

5.3 Parametric statistical analysis

We next use parametric methods to disentangle the effect of the disagreement outcome from other various factors that might influence bargaining outcomes in our experiment. We estimate models with demands (as fractions of the cake) as the dependent variable, separately for all pairs and agreements only, with pooled NDG and UBG for both, and leaving out the observations with a symmetric bargaining game (and thus no favoured or unfavoured player). For each sub–sample, we estimate two panel Tobit models: an unrestricted model and a restricted model. The primary explanatory variables in each model are the disagreement payoffs (d_i and d_{-i}), a UBG indicator, and their interactions. To measure whether bargaining behaviour is different when there is dominant bargaining power, we also include interactions between all of these variables and an indicator for dominant bargaining power. Our restricted models include only these variables and a constant term.

Our unrestricted models use these variables and additional ones. We add the block number (1–5), an indicator for bargaining taking place in odd–numbered rounds, and a favoured–player indicator – along with its interaction with the dominant–bargaining–power indicator, to allow for a shift as well as a change in slope at 50% of the cake. Finally, we include two variables meant to capture skill and effort in the real–effort task unconnected with favoured status: own and opponent letters encoded (the total number of letters in all words encoded). Each model is estimated using Stata (version 12), and incorporates individual–subject random effects.

Table 7 presents marginal effects (taken at variables' means) and standard errors for each variable, and log likelihoods for each model. We note first that for all pairs of models, f–tests fail to reject the null that all of the extra coefficients in the unrestricted model are zero ($\chi^2_5 = 7.50, p \approx 0.19$ in Model 2; $\chi^2_5 = 2.63, p \approx 0.76$ in Model 4). Because of this, and since the results are fairly robust to which specification we use, we will focus on the restricted models.

Table 7: Tobit regressions: marginal effects, and standard errors in parentheses

Sub–sample:	NDG/UBG (all)		NDG/UBG (agreements)	
Equation:	[1]	[2]	[3]	[4]
Own disagr.	0.330^{***}	0.365^{***}	0.324^{***}	0.293^{***}
payoff (d_i)	(0.047)	(0.093)	(0.065)	(0.070)
Opp. disagr.	-0.184^{***}	-0.186^{**}	-0.270^{***}	-0.301^{***}
payoff (d_{-i})	(0.045)	(0.094)	(0.046)	(0.070)
UBG	–0.002	–0.002	0.033^{***}	0.034^{***}
	(0.012)	(0.012)	(0.010)	(0.011)
Dominant barg. power	–0.009	–0.007	–0.004	–0.005
	(0.019)	(0.019)	(0.016)	(0.016)
Block		-0.006^{**}		0.003
		(0.003)		(0.002)
Order (bargaining		0.006		–0.002
in odd rounds)		(0.008)		(0.009)
Favoured		–0.003		–0.003
		(0.018)		(0.013)
Letters encoded		–0.0001		0.0001
		(0.0003)		(0.0002)
Opp. letters encoded		0.0002		–0.0000
		(0.0003)		(0.0002)
Constant term?	Yes	Yes	Yes	Yes
N	859	859	606	606
$\|ln(L)\|$	686.44	690.17	756.90	758.21

* (**,***): *Marginal effect significantly different from zero at the 10% (5%, 1%) level.*

The table shows two important results. First, when we don't distinguish between situations with and without

Table 8: Point estimates and confidence intervals for combined disagreement–payoff effects, based on Models 1 and 3 from Table 7

		All bargaining pairs			Agreements		
		NDG	UBG	sig. diff.?	NDG	UBG	sig. diff.?
No d.b.p.	Point estimate	0.222	0.649	$p < 0.001$	0.300	0.709	$p < 0.001$
	95% conf. int.	(0.066, 0.377)	(0.483, 0.814)		(0.162, 0.437)	(0.560, 0.857)	
d.b.p.	Point estimate	0.690	0.824	$p \approx 0.17$	0.786	0.913	$p \approx 0.17$
	95% conf. int.	(0.560, 0.820)	(0.685, 0.963)		(0.649, 0.922)	(0.794, 1.032)	
Point estimates sig. diff.?		$p < 0.001$	$p \approx 0.09$		$p < 0.001$	$p \approx 0.01$	

Note: d.b.p. = dominant bargaining power.

dominant bargaining power, we replicate the finding from earlier work that subjects under–respond to disagreement payoffs. The own–disagreement–payoff effect, which is simply the marginal effect of the own–disagreement–payoff variable, varies across models from about 0.29 to 0.37, while the opponent–disagreement–payoff effect varies from about 0.18 to 0.30; all of these are clearly well below the predicted magnitude of 0.5, and comparable to results from previous studies. Second, neither the UBG indicator nor the dominant–bargaining–power indicator has much effect on the overall levels of demands, with the exception of a small but significant positive effect in the sub–sample conditional on agreements.

Table 8 shows additional results from Models 1 and 3. This table reports point estimates and 95% confidence intervals for the combined disagreement–payoff effect, taken at values of 0 and 1 for the UBG and dominant–bargaining–power indicators. This gives us the effects for all four combinations of NDG vs. UBG and with vs. without dominant bargaining power; reported both for all observations (Model 1) and for only agreements (Model 3). For both all observations and agreements, chi–square tests strongly reject the null hypothesis of equal effects in the four cases ($\chi^2_3 \approx 36.36$ for all observations and 53.76 for agreements, $p < 0.001$ in both); therefore we also report p–values for pair–wise tests between NDG and UBG and between with and without dominant bargaining power (in each case, holding the other variable constant).

There are few differences between the results for all observations and those for agreements only; responsiveness to changes in disagreement payoffs is slightly higher in the latter, but not significantly so. Importantly, for either model, the order relationships and significance between pairs of effects are very similar. Subjects are least responsive to their bargaining power in the NDG with no dominant bargaining power, and the combined effect of 0.222 (all) or 0.300 (agreements) is not only well below the theoretical prediction of 1, but neither corresponding confidence interval contains 1; indeed, the upper limit of the confidence interval is around 0.4, the level of responsiveness found in previous studies. Responsiveness is higher – significantly so – in the UBG without dominant bargaining power, and the point estimates of 0.649 and 0.709 are comparable to the roughly 0.6 found by Anbarci and Feltovich (2013). However, this is still well below 1, which again, is outside both of the corresponding confidence intervals.

When the bargaining situation is one with dominant bargaining power, responsiveness to bargaining power increases, sometimes drastically. In the NDG, the difference is especially striking – a rise from 22% to 69% (over all observations) or from 30% to 79% (over agreements) of the theoretically–predicted level – and is significant, though even so, this leaves disagreement–payoff responsiveness still well below 1. In the UBG, responsiveness increases from 0.649 to 0.824 (all observations), or from 0.709 to 0.913 (agreements), under dominant bargaining power compared to without it; these changes are also significant (though only at the 10% level in the case of all observations). In the case of agreements, the corresponding confidence interval contains 1, meaning that we cannot reject the null hypothesis that subjects fully respond to changes in their bargaining power, while in the case of all observations, 1 lies just above the upper limit of the corresponding confidence interval.

6 Discussion and concluding remarks

One of the simplest and most prevalent types of bargaining situation (and certainly the one most often incorporated into models by applied theorists) has a fixed, known cake size and a known disagreement outcome. All of the commonly used analytical techniques that yield a unique prediction – both axiomatic solutions and non–cooperative game–theoretic methods – have the same implication: when a bargainer's disagreement payoff increases by a unit, her payoff from bargaining should increase by one–half of a unit, and when her opponent's disagreement payoff increases by a unit, her payoff should decrease by one–half of a unit. Thus, the sum of these "own–" and "opponent–disagreement–payoff effects", called the "combined disagreement–payoff effect" – which measures overall responsiveness to bargaining power – is predicted to be unity.

Whether these quantitative properties hold in real bargaining settings is an empirical question, and previous experiments (see Section 3) have found that subjects severely under–react to changes in bargaining position. Nearly all of these experiments – including those with the greatest variation of bargaining position – have allocated bargaining position exogenously, and are thus open to the criticism that subjects may not perceive this endowed bargaining power to have been "earned", so that those subjects with a favourable position may be reluctant to exploit it fully, and those with an unfavourable position may use what power they have to prevent their opponents from exploiting theirs. Also, most of these experiments were designed so that equal splits were equilibrium outcomes. This matters, since the 50–50 norm has proved to be a prominent focal point in many bargaining experiments.

We conduct a laboratory experiment to measure bargainers' responsiveness to changes in bargaining position. We address the issue of bargaining position being viewed as unearned, with a design that requires subjects to *earn* it via a real–effort task. Each word encoded earns the subject £0.15 in disagreement payoff, so that within a bargaining pair, both favoured status and the degree of bargaining power are based entirely on task performance. Our conjecture was that having bargaining power depend on effort and skill, rather than purely luck, increases subjects' willingness to exploit their bargaining power and weakens the attractiveness of equal splits as a focal point. We vary task difficulty across rounds and subjects, without informing subjects about how difficulty is determined (so as to maintain the emphasis on effort and skill; see Section 4). In particular, we allow subjects to earn disagreement payoffs higher than 50% of the "cake"; in these cases of *dominant bargaining power*, where 50–50 splits are not an equilibrium outcome, we expect the focal nature of 50–50 to be further weakened.

Subjects bargain over a £10 cake, with both own and opponent disagreement payoffs announced prior to bargaining. Bargaining takes place under one of two institutions, both widely used in theoretical modelling and experimental tests of bargaining: the Nash Demand Game (NDG) and the Unstructured Bargaining Game (UBG). Our experimental design is novel, as there have been very few studies of how bargaining power is exploited when it is varied nearly continuously through a large range of levels, few in which bargaining power is earned via skill and effort, few in which individually rational 50–50 splits are ruled out, and to our knowledge, none with all three of these features.

Our main findings are shown in Table 8. For both all demands or for demands conditional on agreement, this table shows the estimated combined effect for the four possible combinations of game (NDG or UBG) and dominant bargaining power (no or yes). When there is no dominant bargaining power, we largely replicate what has been found in previous experiments, even quantitatively. Subjects in this case do respond to changes in their bargaining power, and this responsiveness is substantially and significantly higher in the UBG than in the NDG (65% or 71% of the predicted level in the former, versus 22% or 30% in the latter), but even in the UBG, these effects fall well (and significantly) short of the theoretical prediction.

Behaviour in pairs with dominant bargaining power is markedly different. In the NDG, the combined effect rises to 69% of the theoretical prediction for all demands, and 79% for agreements: significantly higher than without dominant bargaining power, though still less than full responsiveness. In the UBG, the combined effect under dominant bargaining power is 82% for all demands and 91% for agreements. In this last case, one can claim that subjects actually do fully exploit their bargaining power, as the corresponding 95% confidence interval includes the theoretical prediction of 1. We should point out that the grounds for a claim of full responsiveness to disagreement payoffs are still fairly weak: they are based on failure to reject a null hypothesis that the claim is true (the resulting p–value is about 0.15), and the corresponding confidence interval for all demands does not include 1. However, given that previous bargaining papers have considered even levels of responsiveness two–thirds that of the theoretical prediction as supporting the theory (Binmore et al., 1991), our finding of approximately full exploitation of bargaining power is significant.

The qualitative difference we find between situations with and without dominant bargaining power raises an obvious question: why should subjects be more responsive to disagreement payoffs in the former case? We conjecture that the crucial factor is the availability of equilibrium equal splits. We have noted that in many bargaining experiments, there is a strong tendency toward 50–50 agreements. The tendency appears overwhelming in symmetric bargaining games, but it exists in asymmetric settings as well, and even when bargainers don't actually coordinate on an equal split, agreements seem to gravitate toward 50–50 as long as it is an equilibrium outcome (see Figure 4). In this case, either the proclivity by some subjects to fixate on 50–50 irrespective of small changes in bargaining position (in line with the discussion of theoretical models with "obstinate types" in Section 3), or a general pull toward 50–50, will lower the average responsiveness to disagreement payoffs. In contrast, when one bargainer has a disagreement payoff high enough that equal splits are no longer individually rational, this "security blanket" disappears, possibly to be replaced by the alternative focal point of an equal split of the surplus – implying greater responsiveness to disagreement payoffs.

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A Do subjects consider under–responsiveness when choosing effort?

In our experiment, each non–practice block comprises two rounds, identical except that one has a bargaining stage and one is task–only. A natural question is whether subjects behave differently in task–only rounds, where the number of words encoded directly determines the payoff, compared with bargaining rounds, where payoff is based indirectly on words encoded, through the latter's effect on the bargaining outcome. Even if subjects fully exploit their bargaining power, an additional word encoded in a bargaining round will yield only half as much revenue as one from a task–only round (assuming bargainers reach agreement). Since subjects actually under–respond, the difference in revenue per word between bargaining and task–only rounds will be even larger. (In the extreme case where bargaining outcomes were completely insensitive to disagreement payoffs – for example, if bargainers always agreed on 50–50 splits – encoding words in bargaining rounds would confer no extra revenue at all.) If subjects recognise this difference in marginal benefits, they ought to expend less effort in bargaining rounds than in task–only rounds. But do they?[19]

We address this question with an additional regression, using letters encoded, rather than words, as our dependent variable. Our main explanatory variable is an indicator ("Bargain") for a bargaining round. Additional variables are indicators for UBG, advantaged player, hard task, and bargaining in odd–numbered rounds ("Order"), as well as the block number (1–5). We also include all interactions amongst these six variables. Finally, we include the following demographic variables: Female (indicator), Age (positive integer), Econ (integer number of economics classes taken at university, bounded at 0 and 4) and Residence (ordinal variable for number of years living in the United Kingdom, with categories 0–1, 1–2, 2–5, 5–10 and 10+ coded as 0, 1, 2, 3 and 4). We used Stata (version 12), and incorporated individual–subject random effects.

Table 9 displays the main results (the demographic variables were all insignificant and aren't shown). The negative overall marginal effect of "Bargain" is consistent with shirking, but its size is small. In the NDG, the effect is significantly different from zero, but its magnitude is only just over two letters per round, as compared to the average of 110 letters encoded per subject. In the UBG, the effect is even less – about half of a letter per round – and not significantly different from zero. The apparent, and weakly significant, difference in effect size between games makes sense, since subjects under–react more to disagreement payoffs in the NDG, yielding a lower expected payoff per word encoded and implying more shirking. However, their *absolute* magnitudes seem puzzling, as they suggest that subjects barely respond to the substantially lower expected returns in bargaining rounds compared to task–only rounds. There are several possible explanations for why this might be happening.

1. **Cognitive limits** Subjects may not understand that returns to encoding words are lower in bargaining rounds than task–only rounds, since the underlying logic is fairly subtle.

2. **Optimism/overconfidence** Subjects might overestimate the effect of their disagreement payoff on their bargaining outcome, either through overconfidence in their ability to negotiate, or through an optimistic assessment of the likelihood of being matched with a poor negotiator.

3. **Pessimism** Following a disagreement, subjects receive their disagreement payoffs, so in this case, the return to an additional word encoded is the same as in a task–only round. Overall, if α is a subject's subjective probability of reaching agreement in a bargaining round, the expected return to a word encoded in that round

[19] One subject in the NDG treatment clearly understood this logic. After encoding 14, 15 and 16 words in the first three bargaining rounds, he encoded zero in each of the last two – in both cases demanding £4 in the subsequent bargaining game and getting it. For sake of comparison, he encoded 12 and 16 words in the task–only rounds in those two blocks.

Table 9: Regression results (marginal effects and standard errors) – letters encoded, non–practice rounds

Marginal effects at means (std. errors)		*Marginal effects by game (std. errors)*	
Bargain (round with bargaining)	-1.392^{***}	Bargain (in NDG)	-2.268^{***}
	(0.498)		(0.704)
UBG	2.820	Bargain (in UBG)	–0.515
	(2.931)		(0.704)
Advantaged	-5.277^{*}	p–value, significance of differences	$p \approx 0.078$
	(2.955)		
Hard task	–0.797		
	(0.498)		
Order (bargain in odd–numbered rounds)	-6.086^{**}		
	(3.003)		
Block	3.467^{***}		
	(0.264)		
Constant term?	Yes		
Demographic variables?	Yes		
N	1760		
$-ln(L)$	6655.56		

* (**,***): *Coefficient significantly different from zero at the 10% (5%, 1%) level.*

is £0.15 multiplied by $1-\alpha+\alpha\frac{\partial x_i}{\partial d_i}$ $(i = f, u)$, compared with £0.15 in a task–only round. If the subject's α is close to zero (versus actual agreement frequencies of about 0.6 in NDG and 0.8 in UBG), then this multiplier will be close to one, so that the returns to encoding will be similar in bargaining and task–only rounds.

4. **Backward–bending labour supply** Some subjects' labour supply curves might be negatively sloped over the relevant range of per–word rates, so that on average, the effect of a change in this piece rate is no discernible change in effort. Charness and Kuhn (2011) note that the observation of a backward–bending labour supply curve in lab experiments is puzzling, since it would imply an income effect large enough to outweigh the substitution effect (which on its own would imply a positive slope), even though the amounts at stake in lab experiments are typically small compared to subjects' wealth levels. However, Andersen et al. (2011) have argued that subjects only partially integrate their lab earnings with their wealth outside the lab; if so, large wealth effects within the lab can be consistent with sensible behaviour outside the lab.

5. **Inelastic labour supply** It might simply be that subjects' labour supply curves are very inelastic, so that the effect of changes in the per–word rate is so small that even a halving of the per–word rate would lead to a barely perceptible change in behaviour. This could be due to a corner solution in the labour–supply problem; either because the disutility of effort is very low, or because intrinsic motivation raises the benefits of effort, subjects could run out of time allocated for the task before they get tired of performing it.

These explanations are not mutually exclusive, and still other explanations may exist. Our experiment was not designed to test among these, but future work may try to do so.

B Instructions from the experiment

Below is the text of instructions for our UBG treatment with bargaining in even-numbered rounds, followed by the text of instructions for our NDG treatment with bargaining in odd-numbered rounds. The instructions for the other two treatments are analogous, and available from the corresponding author upon request.

[UBG instructions]

You are about to participate in a study of decision making. Please read these instructions carefully, as your payment may depend on how well you understand them. If you have any questions, please feel free to ask the experimenter. We ask that you not talk with the other participants – or anyone else – during the experiment.

The main part of this experiment consists of ***ten*** rounds. In each round, you will perform an ***encoding task***. Your computer screen will display a "word" – a sequence of 1-10 letters of the alphabet – along with a key showing the number that corresponds to each letter. Below each letter on your screen will be a blank space. ***Inside each space, you should type the number that corresponds to the letter above it.*** Once you have encoded the entire word, click the "OK" button. If you have encoded the word correctly, the computer will accept it and display a new word. The key will stay the same for all words during a round, but may change from round to round.

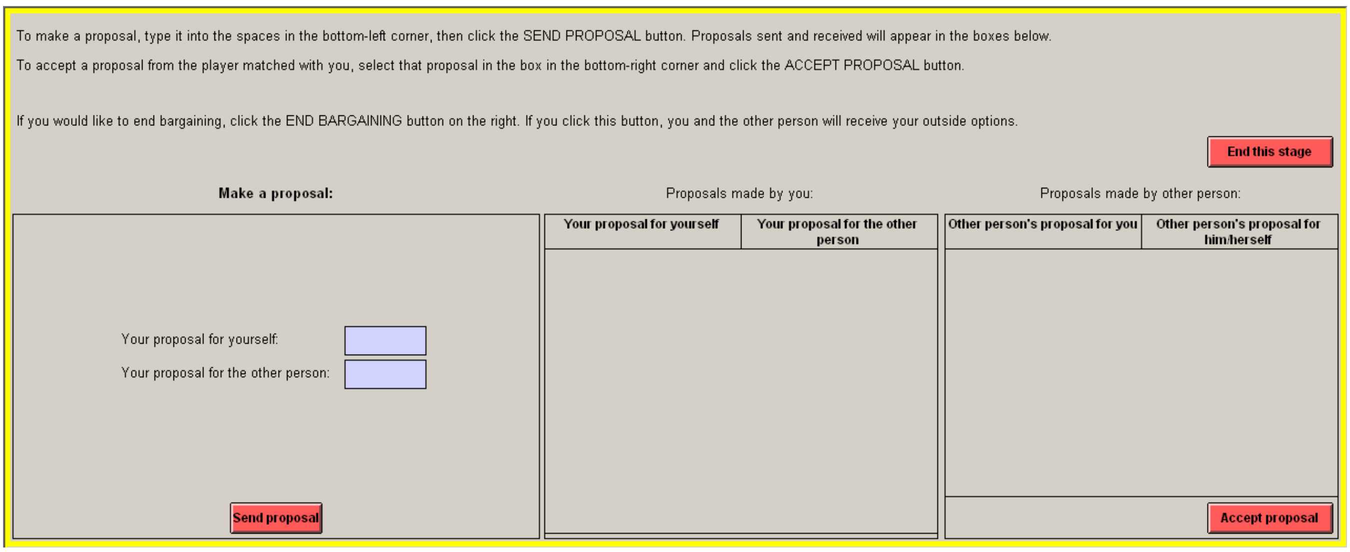
Example: Suppose the key is as follows (this is just an example; the actual key might be different):

A	B	C	D	E	F	G	H	I	J
1	9	5	4	10	6	7	3	8	2

Suppose the word you are given is G – A – J – F – J. The code for G is 7 (below G in the table), and the codes for A, J, F and J are 1, 2, 6 and 2. So, you would encode this word as 7 – 1 – 2 – 6 – 2, then click "OK".

In each round of the experiment, you will have ***five minutes*** to encode as many words as you wish. In ***rounds 1, 3, 5, 7 and 9***, the round ends after the five minutes have finished, and your profit is *15p (£0.15)* for each word you correctly encoded.

In ***rounds 2, 4, 6, 8 and 10***, there is another stage after the encoding task is over. ***In this stage, you have the opportunity to bargain with a randomly chosen participant, over a prize of £10***. The way you bargain is by ***sending and receiving proposals*** for dividing the £10. Below is an example of how the bottom portion of your computer screen will look during this bargaining stage.



To send a proposal to the other person, type the amounts for yourself and the other person in the "Make a proposal" box, then click "Send proposal". ***The amounts you enter must be between 0 and 10 (inclusive), and can have 0, 1 or 2 decimal places.*** The two amounts together must add up to £10 or less. All of your proposals will appear in the box in the bottom-centre of your screen, and all of the proposals made by the other person will appear in the box in the bottom-right. The person paired with you will see these proposals as well, but no one else will be able to see your proposals, nor will you be able to see theirs.

You may accept ***any one*** of the proposals from the person paired with you, ***or none*** of them. To accept a proposal, highlight the one you wish to accept and click "Accept proposal". ***If either you or the other person accepts a proposal, then you have reached an agreement, and the prize is divided according to the accepted proposal.***

The bargaining stage lasts for up to ***2 minutes***; you may send as many or as few proposals as you wish during that time. You may end the bargaining stage before the 2 minutes are over, by clicking on the button labelled "End this stage" on the right of your screen. ***Once you or the person matched with you has clicked this button, it is not possible to send or accept proposals.***

If you or the other person ends the bargaining stage early, or if the time available for proposals ends without you reaching an agreement, then you receive an ***outside option*** equal to ***15p*** for each word you had encoded, and the other person receives an ***outside option*** equal to 15p for each word he/she had encoded. Both you and the person paired with you are informed of ***both*** outside options at the beginning of the bargaining stage.

Organisation of the experiment: This experimental session has three parts.
(1) We will begin with a short questionnaire, where you will answer some demographic questions.
(2) Next, there will be a ***one-minute practice round***, so that you can familiarise yourself with the encoding task. After the one minute has ended, the computer screen will show the number of words you correctly encoded. The results of this practice round will ***not*** affect the amount you are paid.
(3) Then, the main part of the experiment will begin. Each round proceeds as follows:
(a) You perform the encoding task. In odd-numbered rounds, you earn 15p for each word correctly encoded. In even-numbered rounds, this amount becomes your outside option for the bargaining stage.
(b) If it is an even-numbered round, the computer randomly pairs you with another participant, and your screen displays both of your outside options. You can send proposals for dividing the £10.00 prize. The other person can also send proposals for dividing the £10.00 prize; you can accept one of these proposals or none of them.
(c) The round ends. Your computer screen displays the number of words you encoded and your profit. In even-numbered rounds, it also displays whether or not you reached an agreement, and the other person's profit.

Payments: The amount you are paid will depend on the results of the experiment. The computer will randomly select ***four rounds*** out of the ten you played. You will be paid the total of your profits from those selected rounds. Payments are made privately and in cash at the end of the session.

[NDG instructions]

You are about to participate in a study of decision making. Please read these instructions carefully, as your payment may depend on how well you understand them. If you have any questions, please feel free to ask the experimenter. We ask that you not talk with the other participants – or anyone else – during the experiment.

The main part of this experiment consists of ***ten*** rounds. In each round, you will perform an ***encoding task***. Your computer screen will display a "word" – a sequence of 1-10 letters of the alphabet – along with a key showing the number that corresponds to each letter. Below each letter on your screen will be a blank space. ***Inside each space, you should type the number that corresponds to the letter above it.*** Once you have encoded the entire word, click the "OK" button. If you have encoded the word correctly, the computer will accept it and display a new word. The key will stay the same for all words during a round, but may change from round to round.

Example: Suppose the key is as follows (this is just an example; the actual key might be different):

A	B	C	D	E	F	G	H	I	J
1	9	5	4	10	6	7	3	8	2

Suppose the word you are given is G – A – J – F – J. The code for G is 7 (below G in the table), and the codes for A, J, F and J are 1, 2, 6 and 2. So, you would encode this word as 7 – 1 – 2 – 6 – 2, then click "OK".

In each round of the experiment, you will have ***five minutes*** to encode as many words as you wish. In ***rounds 2, 4, 6, 8 and 10***, the round ends after the five minutes have finished, and your profit is *15p (£0.15)* for each word you correctly encoded.

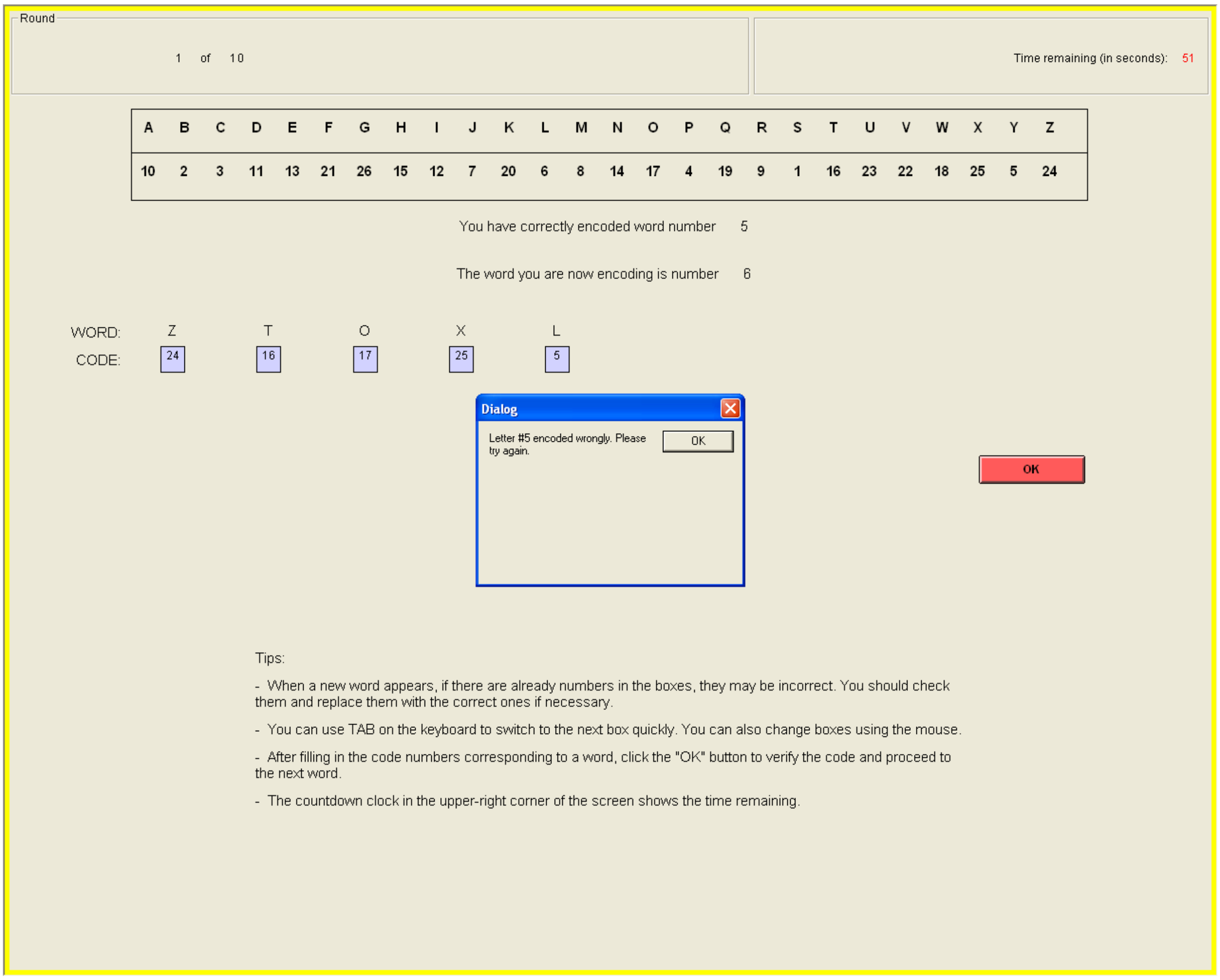
In ***rounds 1, 3, 5, 7 and 9***, there is another stage after the encoding task is over. ***In this stage, you have the opportunity to bargain with a randomly chosen participant, over a prize of £10.*** The way you bargain is by each making ***simultaneous*** claims for shares of the £10.
- If your claim and the other person's claim add up to ***£10 or less***, you receive your claim, and the other person receives his/her claim.
- If the claims add up to ***more than £10***, you receive an ***outside option*** equal to ***15p*** for each word you had encoded, and the other person receives an outside option equal to 15p for each word he/she had encoded. Both you and the person paired with you are informed of ***both*** outside options before choosing your claims.

Organisation of the experiment: This experimental session has three parts.
(1) We will begin with a short questionnaire, where you will answer some demographic questions.
(2) Next, there will be a ***one-minute practice round***, so that you can familiarise yourself with the encoding task. After the one minute has ended, the computer screen will show the number of words you correctly encoded. The results of this practice round will ***not*** affect the amount you are paid.
(3) Then, the main part of the experiment will begin. Each round proceeds as follows:
  (a) You perform the encoding task. In even-numbered rounds, you earn 15p for each word correctly encoded. In odd-numbered rounds, this amount becomes your outside option for the bargaining stage.
  (b) If it is an odd-numbered round, the computer randomly pairs you with another participant, and your screen displays both of your outside options. You choose a claim for your share of the £10.00 prize. The other person chooses a claim for his/her share. ***Your claim can be any multiple of 0.01, between 0.00 and 10.00 inclusive.*** Be sure to enter your claim before the allotted time runs out, or the computer will enter a claim of zero for you.
  (c) The round ends. Your computer screen displays the number of words you encoded and your profit. In odd-numbered rounds, it also displays your claim, the claim made by the person paired with you, and the other person's profit.

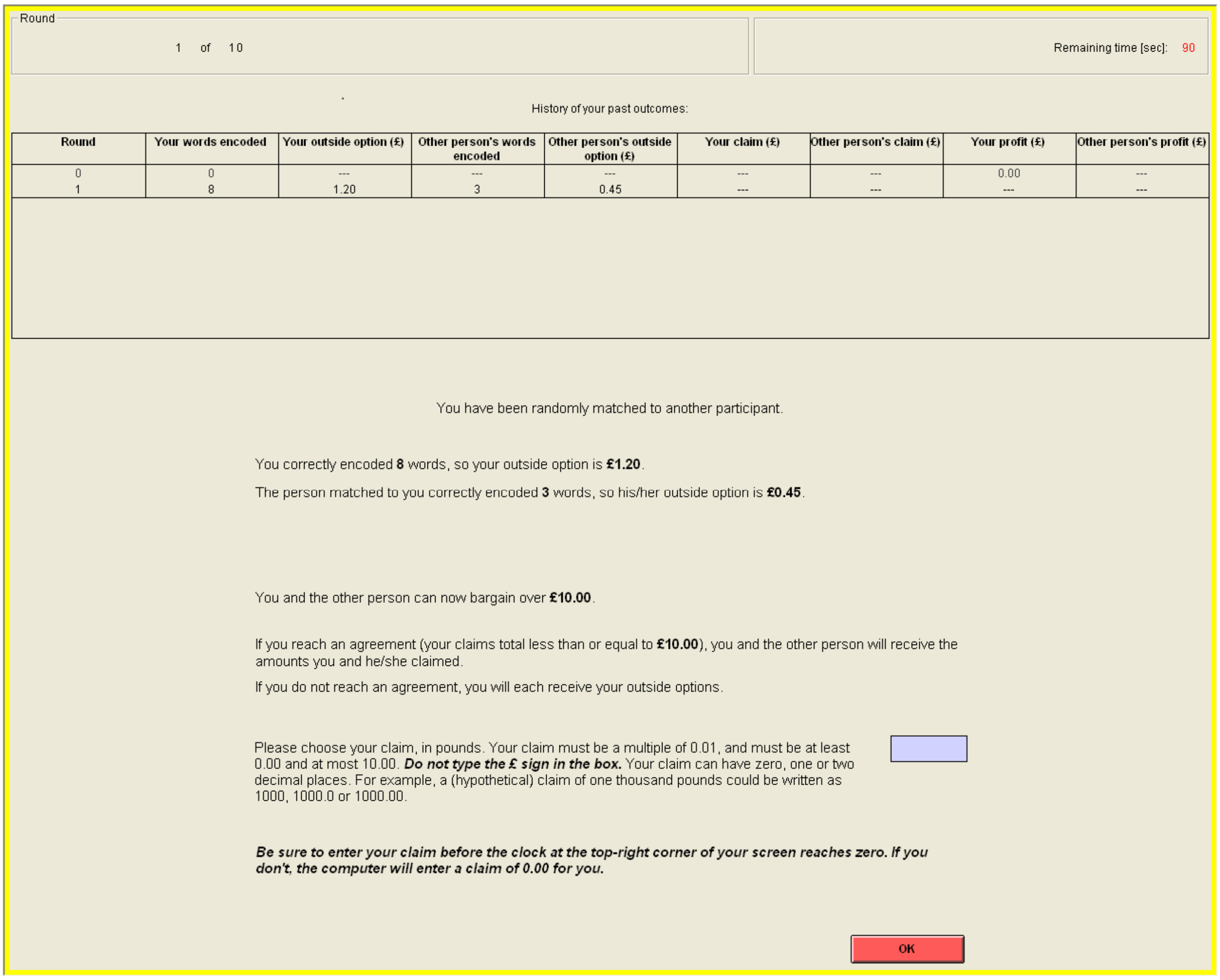
Payments: The amount you are paid will depend on the results of the experiment. The computer will randomly select ***four rounds*** out of the ten you played. You will be paid the total of your profits from those selected rounds. Payments are made privately and in cash at the end of the session.

C Additional screenshots from experiment

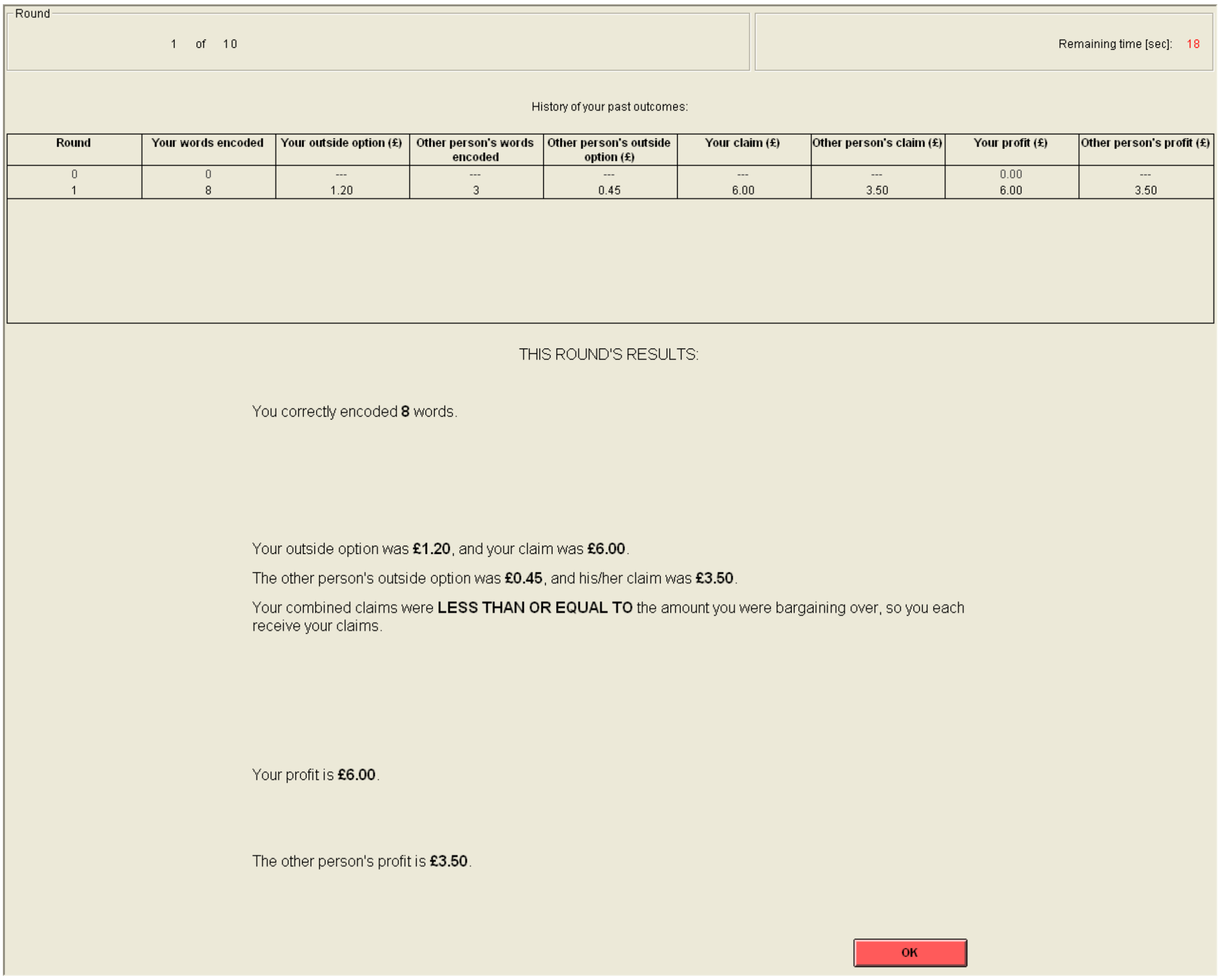
Error screen from real-effort task:



Decision screen, NDG treatment:



Feedback screen, NDG treatment:



Feedback screen, UBG treatment:

Round

1 of 10

Remaining time [sec]: 15

History of your past outcomes:

Round	Your words encoded	Your outside option (£)	Other person's words encoded	Other person's outside option (£)	Was agreement reached?	Your profit (£)	Other person's profit (£)
0	0	---	---	---	---	0.00	---
1	11	1.65	2	0.30	YES	6.20	3.80

THIS ROUND'S RESULTS:

You correctly encoded **11** words.

Your outside option was **£1.65**, and the other person's outside option was **£0.30**.

You and the other person **DID** reach an agreement.

Your profit is **£6.20**.

The other person's profit is **£3.80**.

OK