Is earned bargaining power more fully exploited?

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Abstract

In past experiments involving a wide range of bargaining settings, individuals exploited their bargaining position less fully than standard theory predicts. Typically, these experiments allocated bargaining position randomly, so that bargainers, viewing their position as unearned, may have been reluctant to fully exploit it. We investigate the impact of *earned* bargaining power using theory and experiment. In our "Earned" treatment, disagreement payoffs – and hence bargaining power – are based on performance on a real–effort task. In our "Assigned" treatment, subjects perform the task but disagreement payoffs are randomly assigned. Our "Notask" treatment is like the Assigned treatment but without the task. Comparison of our Earned and Assigned treatments provides our main result: subjects are more responsive to changes in bargaining position when it is earned. Responsiveness is also often higher in our Assigned treatment than in our Notask treatment – suggesting a possible effect of merely including a status–irrelevant task – though these differences are usually insignificant.

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1 Background

Bargaining is pervasive. In markets, its role in determining prices is well recognised. Even in the formal sector of developed countries, where price posting by sellers has become the norm, bargaining is still used in trading many high–value goods (houses, cars, jewellery), as well as in labour markets (at the group level for unionised jobs, or the individual level for executives and professionals).[1] In developing countries, and in the informal sector more broadly, bargaining is even more prevalent. Bargaining is also common outside of market interactions. Political decision making within committees or legislatures is often modelled using a bargaining framework (e.g., Baron and Ferejohn 1989), as are pre–trial negotiations in legal disputes (Daughety 2000; Spier 2007). Bargaining has also been used to understand international treaty negotiations (see, e.g., Fisher and Ury's 1981 discussion of the role of bargaining during the 1973–1982 Third United Nations Conference on the Law of the Sea). Thus, understanding bargaining is essential to understanding not only markets, but many other phenomena.

Theoretical analyses of bargaining date back to before the systematic study of non–cooperative game theory (Edgeworth 1881; Zeuthen 1930), and work continues to this day (e.g., Karagözoğlu et al. 2019). However, theoretical work on its own has had limited success in characterising how people actually bargain. This is partly due to the very nature of the analytical techniques used: some typically yield a multiplicity of solutions (e.g., Nash equilibrium applied to the Nash demand game, or the core applied to unstructured bargaining). Even when a technique implies a unique solution (e.g., Nash's 1950 bargaining solution applied to unstructured bargaining), equally reasonable alternative techniques may imply different solutions (e.g., Kalai and Smorodinsky's 1975 solution). And even where there is broad consensus among theorists on a technique yielding a unique solution (e.g., subgame perfection applied to Rubinstein 1982 bargaining, or perhaps Harsanyi and Selten's 1988 risk–dominance applied to the Nash demand game), it may make strong and perhaps unwarranted assumptions regarding individuals' cognitive abilities, beliefs about others' abilities, and so on. Thus empirical testing of theoretical bargaining solutions is needed to complement theory.

Laboratory experiments are especially well suited to testing bargaining theories. First, the experimenter has much greater control over the decision–making environment, including the preferences of decision makers and the information they have available, than field researchers do. Second, relevant variables can be varied in a systematic way, meaning that they can be treated as exogenous when making inferences about their effects based on the results. Third, all decisions are collected and available for the researcher to view, in contrast to field data, where (for example) bargaining leading to agreement may be more visible than bargaining leading to disagreement, and proposals before the final accepted one may not be observable.

Bargaining experiments date back at least to Siegel and Fouraker (1960; see also Fouraker and Siegel 1963), and like the theory, continue to this day. A recurring result in this literature is that experimental subjects do not fully exploit their bargaining power: splits of the "cake" (the surplus available to be allocated between the bargainers) tend to skew toward 50–50, even when the bargaining setting structurally favours one of the bargainers. (See Section 2 for some examples.) As an implication, manipulations of the setting that imply a specific change in the predicted bargaining outcome, such as a change in the *disagreement payoffs* (the amounts received by the bargainers if they fail to reach agreement), typically produce a smaller–than–predicted change in the actual bargaining outcome. We refer to this as *under–responsiveness* to changes in bargaining position.

A potential explanation for under–responsiveness in bargaining experiments stems from one of the strengths

[1] As an example of the importance of bargaining in labour markets, some researchers have proposed sex differences in willingness or ability to bargain as a partial explanation for male–female wage gaps (Stuhlmacher and Walters 1999; Small et al. 2007; Fortin 2008).

of experimentation: random assignment. In most experiments, subjects are randomly allocated to treatments and roles, with the consequence that a favourable position (one that ought in principle to lead to relatively high money earnings for the subject) is typically gained on the basis of pure luck – and equally so for unfavourable positions. If such unearned status is viewed by the subjects as undeserved, then favoured subjects may be ill–disposed to take advantage of their position, and unfavoured subjects may be reluctant to let them do so. (Smith, 1991, also makes this point.) Such reluctance by either or both sides should push outcomes in the direction of equal splits, whether this is because "fair" outcomes are especially strong focal points when bargaining position is allocated by luck, or because individuals' preferences exhibit especially powerful aversion to inequity in such situations.

By contrast, bargaining position in settings outside the lab is likely to arise endogenously, via past decisions, intrinsic abilities, or effort. For example, in a professional labour market, a worker's bargaining power with her current employer may depend partly on market conditions outside her control, but it also depends on her suitability to other firms, which in turn depends on her education, previous job performance, etc. To the extent this is true, experiments with bargaining position assigned based on luck may have limited external validity.

In this paper, we connect these two notions – under–responsiveness in bargaining and the effects of earned rather than assigned status – using a bargaining experiment where both the distribution and the source of bargaining position are varied. Our main research question is simple: *how do bargaining outcomes change when bargaining power is earned rather than assigned?* A natural way to answer this question would be to have a baseline treatment similar to a typical bargaining experiment – with bargaining position determined randomly – and a second treatment with bargaining position based on information we had about subjects' intelligence, knowledge, work ethic, and so on. (Given the usual subject pool of university undergraduates, we might base bargaining position on standardised test scores or grade–point averages.) If even in the baseline treatment, it were made clear to subjects that this information is being collected (but in that case, nothing is done with it), then this experiment could identify the effect of earned–versus–assigned bargaining position.

However, we do not have access to pre–existing information of this kind. Instead, we follow a common approach and add our own task measuring skill and effort. We use a simple but tedious *real–effort task*: encoding nonsense words into strings of numbers. In our *Earned* treatment, subjects earn their disagreement payoffs, and hence their bargaining position, based on their task performance; subsequently, they bargain under complete information – i.e., knowing the cake size (which is always the same) and both disagreement payoffs. By contrast, our *Notask* treatment applies the more standard experimental methodology, with the same bargaining setting but with no task performed, and with disagreement payoffs randomly assigned (but with the underlying distributions chosen to mirror those in our Earned treatment).

At first glance, a comparison between our Notask and Earned results might appear to capture the effect of earned versus assigned bargaining position. However, this is just one of multiple differences between those two treatments. Importantly, they also differ in the very presence of the task, which could matter for several reasons (e.g., the mere expending of effort on the task may alter bargaining behaviour even without any impact on disagreement payoffs, or perhaps spending time on the task may inhibit learning about based on previous rounds' bargaining feedback). Because of these confounds, we have an third and intermediate treatment, *Assigned*. There, subjects need to encode a certain number of words in a round in order to take part in the subsequent bargaining stage, but once this is done, the disagreement payoff is randomly assigned as in our Notask treatment. Thus, comparison between the Earned and Assigned treatments comes closer to isolating the effect of earning bargaining position, while comparison between the Assigned and Notask treatments mainly captures the effects of adding a *status–irrelevant* pre–play task. Comparison between the Earned and Notask treatments, which corresponds to much of the relevant literature (see

Section 2), captures the resultant of these two effects: introducing a *status–relevant* pre–play task.

Standard theory predicts no differences across treatments. The previous literature comparing status based on task performance with status based on luck, while extensive, is also of limited use to us. We will discuss the reasons for this in detail in Section 2, but the important points are these. First, this literature is actually quite mixed in its results; there are many examples of real–effort tasks moving behaviour toward the theoretical predictions, but there are also a large number of findings of no significant effect, and even some where the task moves behaviour *away* from the prediction. Second, nearly all of this literature makes comparisons between analogues of our Earned and Notask treatments, rather than between our Earned and Assigned treatments. As noted already, these are therefore not pure tests of earned–versus–assigned status, but rather tests of introducing a status–relevant task. Third, of the few experiments that isolate the effect of earned status, most have used simple dictator–game–like settings. This gives them high power to identify a treatment effect, but often makes them unable to distinguish between earned status moving behaviour toward theoretical predictions and earned status giving people a property right (or a moral claim) to higher earnings in the experiment.[2] Those that do allow such a distinction turn out to yield ambiguous results (as we also discuss in Section 2).

Our main experimental results suggest that the source of bargaining power does matter. Subjects are indeed more responsive to changes in bargaining position when it is earned via the task than when it is assigned randomly irrespective of task performance (i.e., between our Earned and Assigned treatments). This result holds in two different bargaining settings (one unstructured and one highly structured), and it holds whether or not there is *dominant bargaining power* (a disagreement payoff so high that 50–50 splits are no longer individually rational). Importantly, it continues to hold when we control for selection bias via individual–subject fixed effects. The differences between our treatments with random assignment of disagreement payoffs (Assigned versus Notask), while usually insignificant, are also typically in the predicted direction, meaning that the true treatment effect (comparing the Earned and Assigned treatments) is often less than the apparent treatment effect from an uncontrolled comparison of our Earned and Notask treatments. These results suggest (i) having earned rather than assigned bargaining position does push results toward the theoretical prediction (and away from equal splits), and (ii) care should be taken when treating the effect of introducing a status–relevant task as identical to the effect of earned status.

2 Some relevant literature

This paper speaks to two strands of the experimental and behavioural economics literatures. Firstly, there have been many experimental tests of bargaining theories (for surveys, see Roth 1995 and Camerer 2003, pp. 151–198). While few of these have concentrated specifically on how bargainers respond to changes in disagreement payoffs, a larger number have varied some structural aspect of bargaining position in order to look at other research questions, and many of these provide sufficient information to allow the reader to assess responsiveness to bargaining position. This research is surveyed by Anbarci and Feltovich (2018), and we will not repeat here what they wrote. We will only note two stylised facts: under–responsiveness is prevalent, and there is substantial variation in its extent across studies.

Figure 1 illustrates both of these stylised facts. Each experimental cell from five well–known bargaining experi-

[2] We hasten to emphasise, however, that these remarks should not be construed as criticisms of this earlier literature, which includes several very important and highly cited works. Our point is not that these earlier papers are flawed; it is simply that they were typically addressing research questions different from ours, and a design that was optimised for one research question may not be well–suited for others. This, perhaps obvious, point means that the applicability of their results to our questions may be limited.

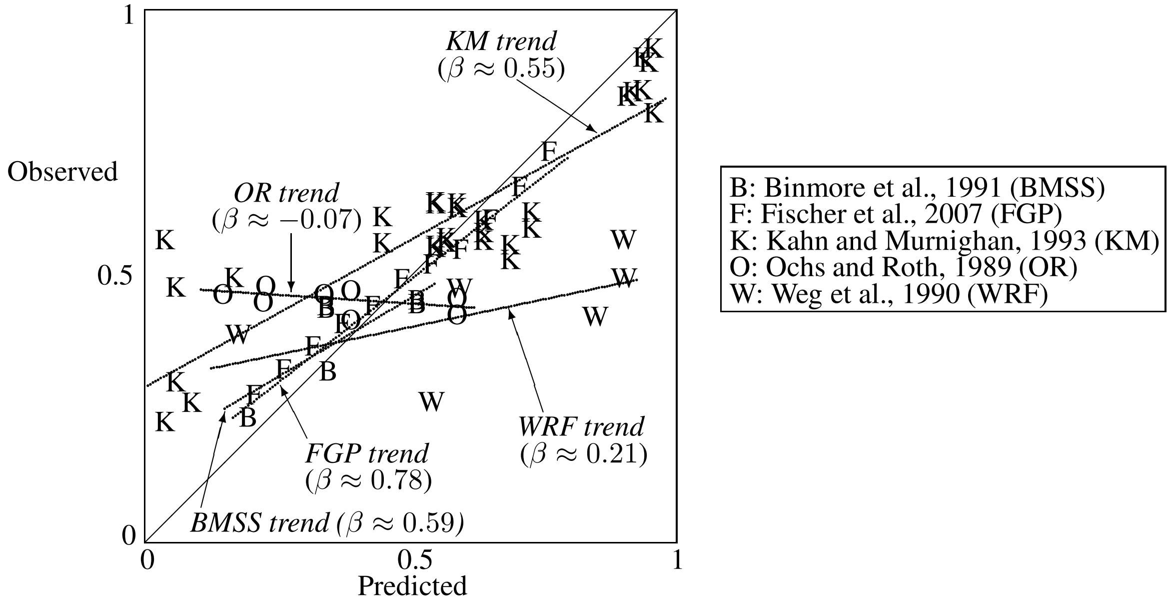

Figure 1: Predicted and observed bargaining outcomes, selected previous studies (treatment–level data)

ments is plotted on the unit square, with predicted bargaining outcome on the horizontal axis and observed outcome on the vertical axis.[3] Also shown are weighted–least–squares (WLS) trend lines for each study (with the number of observations in each cell as the weights). The slopes of these trend lines vary substantially – from essentially zero in Ochs and Roth (1989) (which they also note, on p. 362), to more than three–quarters in Fischer et al. (2007) – but are well below unity in each case.[4] Though this is admittedly a small sample of the literature, it is arguably representative in the sense of spanning the range of observed bargaining–position responsiveness. Also, these studies share an important feature with most of this literature: subjects' bargaining position is based on random draws rather than skill, effort, or related qualities.

The second relevant literature involves comparisons of behaviour when some aspect of one's status is earned versus behaviour in a similar setting when the same aspect is assigned by the experimenter.[5] While it may seem

[3]The outcome variable used is the one given the most prominence in the study: first–mover offer in Ochs and Roth (1989), first–mover payoff share in Binmore et al. (1991), first–mover demand in Weg et al. (1990) and Kahn and Murnighan (1993), and fallback demand in Fischer et al. (2007). The relevant variable is then divided by the cake size to yield values between zero and one. These studies were chosen because they are reasonably well known within the literature, the relevant predicted and observed variables could be calculated either directly from the paper or from raw data made available by the authors, and variation in the predicted values is sufficient to allow estimation of a meaningful within–study trend line.

[4]The overall WLS trend line of these five studies has slope 0.43. We note that this low slope is not driven by truncation when predicted values are close to zero or one, as dropping observations with predicted share less than 0.3 and more than 0.7 yields a slope of 0.48.

[5]Of course, this is only a subset of a larger literature involving real–effort tasks. These tasks can also be used for examining labour supply: how worker behaviour responds to changes in the reward per unit of the task (Dickinson 1999; Van Dijk et al. 2001; De Araujo et al. 2015; Azar 2019), imposition of an income tax (Kessler and Norton 2016), or allowing internet browsing as a substitute or complement for work (Koch and Nafziger 2016); or how workers trade off between quantity and quality of output in response to incentives (Rubin et al. 2018). Also, some studies use real–effort tasks because they postulate that subjects will treat earned status differently from assigned status (Anbarci and Feltovich, 2018), for reasons of external validity (Cason et al. 2010), or so that particular moral or ethical rules will apply to the subsequent setting (Konow 2000; Gächter and Riedl 2005; Birkeland 2013; Karagözoğlu and Riedl 2015; Bolton and Karagözoğlu 2016; Konow et al. 2019; see also Luhan et al. 2019, who use a combination of luck and effort). Still another set of studies uses real–effort tasks as a robustness check for results found when effort is chosen; see, for example, Erev et al.'s (1993) follow–up of Bornstein et al.'s (1990)

intuitively obvious that having subjects earn their status will push their behaviour closer to theoretical predictions based on standard preferences, the reality is more complex. This is illustrated by perhaps the earliest controlled test of real–effort tasks and bargaining: Hoffman and Spitzer's (1985) experiment. They replicated an earlier finding (Hoffman and Spitzer, 1982) that even when disagreement outcomes are very inequitable (in their experiment, 79–85 percent of the cake for one bargainer and zero for the other), equal splits were common in face–to–face bargaining when favoured status was randomly assigned via a coin flip – even though these required the favoured bargainer to agree to less than she could unilaterally guarantee. When favoured position was earned based on performance in a game of skill, however, outcomes still typically involved equal splits. It was only when both (i) favoured position was earned, and (ii) *instructions emphasised that exploiting bargaining power is moral behaviour*, that outcomes moved away from equal splits toward the theoretical prediction. Thus the effect of using a game of skill rather than a coin toss to allocate status was more complicated than simply earned status improving fit with the theory. Moreover, Hoffman and Spitzer's variation between coin toss and skill game varies not only the source of bargaining power, but also whether the skill game is played at all, how much time passes between the start of the experiment and when bargaining takes place, and so on. That is, Hoffman and Spitzer test the effect of introducing a status–relevant real–effort task (corresponding to the variation between our Earned and Notask treatments), rather than that of earned–versus–assigned bargaining power.[6]

Much of the literature comparing bargaining with and without earned status is similar to Hoffman and Spitzer (1985): a real–effort task is performed if and only if it is used to assign status. These experiments have found mixed results. In ultimatum and dictator games, offers to the recipient are *higher* when the recipient earns the cake via real effort, but *lower* when the proposer earns the cake, compared to when luck determines the cake size (Hoffman et al. 1994; Ruffle 1998; Cherry et al. 2002; Oxoby and Spraggon 2008; García-Gallego et al. 2008; Rode and Le Menestrel 2011; Mittone and Ploner 2012; though Sonnegård 1996 found no significant differences). Brekke et al. (2019) found that the division of surplus in alternating–offer bargaining with outside options was roughly the same irrespective of whether the outside options were randomly assigned or earned by performance in a letters–to–numbers encoding task. Fischer and Normann (2019) examined repeated asymmetric Cournot duopolies with and without pre–play communication: when there is communication, the game takes on the flavour of bargaining, with the stage–game Nash equilibrium as the disagreement point but collusion providing opportunities for Pareto improvements. Fischer and Normann found that without communication, outcomes were close to the stage–game equilibrium irrespective of whether favoured status (lower costs) was allocated based on luck or performance in an encoding task. With communication, real effort moved outcomes closer to the stage–game equilibrium (away from equal splits).

Tests of earned–versus–assigned status in other settings have also tended to compare analogues to our Earned and Notask treatments (though in some, the treatment with a real–effort task has it being status–irrelevant and performed by everyone, making the experiment a comparison between analogues to our Assigned and Notask treatments). In linear public–good games, Kesternich et al. (2018) find lower contributions when endowments are earned, while Cherry et al. (2005), Harrison and El Mouden (2011) and Dutcher et al. (2016) find no significant difference.

public–good experiment in which subjects picked oranges in an orange grove, or Gächter et al.'s (2016) replication of previous experimental results using a new real–effort task ("ball catching"). There are also alternatives to real–effort tasks for nudging subjects to take decisions more seriously, such as having have subjects put their own money at stake rather than relying on "house money"; see Clark (2002) and also Harrison's (2007) comment. Another alternative is to change the reward medium (e.g., Coursey et al. 1987, where some bargainers must taste a bitter sucrose octa–acetate solution, or Noussair and Stoop 2015, where subjects bargain over time – getting to leave the lab earlier – instead of money).

[6]Our Earned, Assigned and Notask treatments were described briefly in the introduction, but see Section 4 for additional details.

(Harrison and El Mouden also find no significant differences in donations to a charity.) In auctions, Jacquemet et al. (2009) find that earning one's endowment increases the prevalence of weakly dominant "sincere" bidding in second–price auctions, while Corgnet et al. (2015) find that it reduces the extent of price–bubble formation in double–auction asset markets. In games with punishment, Danková and Servátka (2015) and Sääksvuori et al. (2016) find that earning status results in more retaliation – and thus behaviour *farther* from the standard–theory prediction (though see our discussion below of Cox et al. 2017, for an opposite result).

Interestingly, nearly all of the previous work we know of comparing analogues to our Earned and Assigned treatments – and hence identifying the effect of earned versus assigned status – uses dictator games or minor modifications of these. Erkal et al. (2011), Barr et al. (2015) and Barr et al. (2016) have subjects receive an endowment determined either by luck or by real effort, after which each subject decides how much of it to donate to other subjects.[7] All three studies found less willingness overall to donate to other subjects when endowments were earned. Because of the dictator–game setting, this kind of result is consistent with earned status providing either a greater tendency toward equilibrium play or a greater "moral claim" (or, perhaps, perceived "property right") to high earnings.[8]

The next two studies deserve additional detail, as they are the closest to the current paper. Lefgren et al. (2016) have all subjects perform an encoding task, where a certain number of words must be successfully encoded in order to earn a certain–sized endowment. Their treatment 1 is an effort treatment, where half of subjects must encode 50 words to earn 4 US dollars and the other half must encode 25 words to earn 2 dollars. Thus differences in endowment are due to differences in performance (though the opportunity for performance is still randomly assigned). In their treatment 2, all subjects must encode 50 words to get their endowment, but half of them know their endowment will be only $2 while the other half will earn $4. Thus everyone is asked to work equally hard, and differences in endowment are due entirely to luck. In both treatments, after the task is finished and everyone knows their endowment, each votes for how much will be redistributed from the "richer" subjects to the "poorer" ones. Each subject simultaneously chooses either $0, $0.50 or $1, and the median vote determines how much is actually redistributed. Consistent with the results in the previous paragraph, richer subjects vote for more redistribution when differences are due to luck than when they are due to effort (29 cents versus 6 cents on average). Notably, the same is true for the poorer subjects (95 cents versus 75 cents), suggesting that having subjects earn their status does not simply increase everyone's sense of a moral claim to higher earnings, but rather intensifies the sense that the richer subjects "deserve" their higher earnings (and the poorer subjects their lower earnings).[9] However, inferring subjects' preferences is complicated by the fact that subjects were casting votes rather than making direct decisions for redistribution. If subjects believed they were unlikely to be pivotal, non–strategic factors such as expressive voting may have affected their observed decisions.

Subjects in Cox et al.'s (2017) study play a modified dictator game where the first mover chooses whether to take 10 or 15 out of a cake size of 20, with a second mover receiving the remainder. After being informed of the

[7]These studies are therefore similar to the dictator–game experiments mentioned above, except that here the framing emphasises that the dictators' money belongs to them but they can give some away, versus being told in standard dictator–game experiments that the money is yet to be allocated but giving them all of the bargaining power. We note here also that there are other studies in this strand of the literature that, unlike those highlighted in the text, make comparisons similar to that between our Earned and Notask treatments. These have typically found less redistribution when endowments are earned (Carlsson et al. 2013, Gee et al. 2017), though Rutström and Williams (2000) found no significant differences.

[8]As evidence of the latter, Erkal et al. also found that the effect of earned status depended on one's position, with the "richest" subjects donating far less under earned endowments, while the "poorest" subjects actually donated slightly more.

[9]While the experiment described here recruited subjects from Mechanical Turk, a replication of treatments 1 and 2 using university students and all money amounts doubled yielded qualitatively similar results.

decision, the second mover has the option to reward the first mover (raising the latter's payoff by 2 at a cost of 1) if she chose to take 10, or to punish the first mover (lowering her payoff by 6 at a cost of 2) if she chose to take 15; in both cases, the second mover also has the option to leave the payoffs unchanged. In their "random" treatment, the roles of first and second mover, as well as the "default" allocation of the cake, are randomly chosen. In a second "tournament" treatment, roles and the default allocation are chosen based on relative performance on a mathematics quiz. In a third "target" treatment, all subjects are given the quiz, but roles and the default allocation are assigned randomly. So, Cox et al.'s treatments 1 and 2 correspond to our Notask and Earned treatments, while their treatment 3 is similar to our Assigned treatment – so that comparing treatments 2 and 3 would identify the effect of earned–versus–assigned status. They find that compared to the target treatment, second movers in the tournament treatment punish more often (38 percent of the time versus 28 percent) and reward less often (24 percent versus 31 percent), while first movers in the tournament treatment are more likely to choose the 10–10 split (50 percent of the time versus 39 percent in the target treatment). While none of these differences is significant, they suggest that "poorer" subjects feel at least as much moral claim to high payoffs when status is earned compared to when it is assigned, and "richer" subjects take more seriously the possibility of retaliation when status is earned.

Finally, a recent study by Demiral and Mollerstrom (2018) compared analogues to our Earned and Assigned treatments in the ultimatum game. All subjects completed a real–effort task, which was used to assign the role of proposer in their "entitlement" treatment but not their "random" treatment. They observed no significant differences in either proposers' offers or responders' minimum acceptable offers. As their design was otherwise modelled after that of Hoffman et al. (1994), their results suggest that Hoffman et al.'s reported differences between analogues to our Earned and Notask treatments may have been due to the presence of the task, rather than to the effect of earned status.

To summarise, we do not observe evidence of any systematic effect of earned–versus–assigned status. Of those studies that vary this while keeping constant whether or not a task is performed, we have one study that suggests earned status intensifies the moral claim of the successful, one study that suggests nearly the opposite, one that finds no differences, and three studies with results consistent with either more equilibrium play or a stronger property right to one's endowment when this is earned. The multitude of studies that examine the effect of introducing a status–relevant real–effort task (rather than of earned–versus–assigned bargaining power) could in principle be indicative, but drawing conclusions from these is complicated by the co–existence of significant results in opposing directions, as well as insignificant results.

Our experiment contributes to this literature in a couple of ways. Our Earned and Assigned treatments provide a cleaner comparison of earned and assigned bargaining power, and the bargaining setting allows us to distinguish between a treatment effect that moves behaviour toward equilibrium (as the behavioural model we develop in the appendix implies) and one that intensifies perceived property rights to high payoffs. Additionally, our Assigned and Notask treatments allow identification of the effect of introducing a real–effort task but without it affecting status; a non–zero effect would suggest that care should be taken when drawing a conclusion about earned–versus–assigned status based on an experiment with one treatment involving a task used to allocate status and another treatment involving no task and random status.

3 The bargaining environment and standard–theory predictions

The bargaining environment is depicted in Figure 2. There is a fixed sum of money (a *cake*) available to the players; we normalise its size to unity. The *bargaining set* is $\{(x_f, x_u) : x_f \geq 0, x_u \geq 0, x_f + x_u \leq 1\}$; an *agreement* can be any member of this set. If bargaining is unsuccessful, the players receive *disagreement payoffs* that are typically asymmetric; the *favoured player* receives d_f and the *unfavoured player* receives d_u (as fractions of the cake size), with $d_f \geq d_u \geq 0$ and $d_f + d_u < 1$. All features of the environment, including the values d_f and d_u, are assumed to be common knowledge. We define the *surplus* to be the portion of the cake remaining after subtracting off the disagreement payoffs $(1 - d_f - d_u)$; this positive quantity represents the gains available to be made from successful bargaining. An element (x_f, x_u) of the bargaining set is *individually rational* for Player i if $x_i \geq d_i$, and the *individually rational set* is the portion of the bargaining set that is individually rational for both players: $\{(x_f, x_u) : x_f \geq d_f, x_u \geq d_u, x_f + x_u \leq 1\}$.

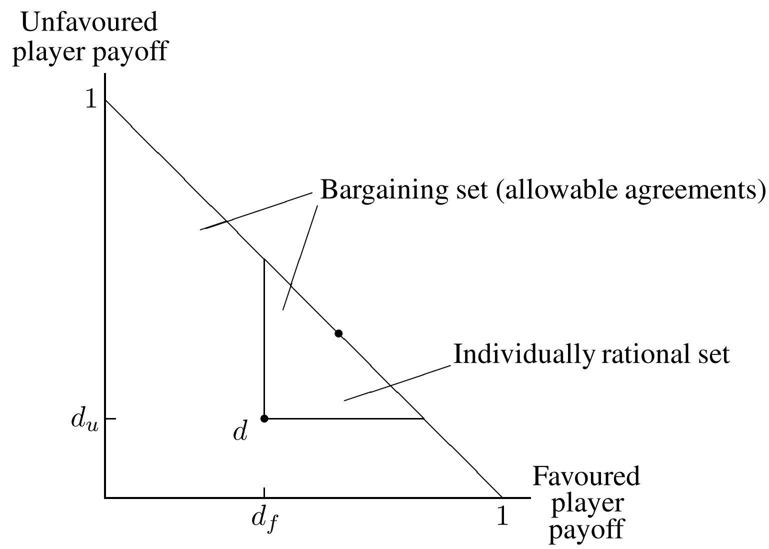

Figure 2: The bargaining environment (cake size normalised to 1)

Nash demand game (NDG)

In the Nash (1953) demand game, the bargainers make simultaneous demands x_f and x_u. If these are compatible $(x_f + x_u \leq 1)$, then each receives the amount demanded (with any remainder left "on the table"); if not, then they receive their disagreement payoffs. Nash equilibrium on its own does not yield a unique prediction for this game, but imposing an additional selection criterion such as either Harsanyi and Selten's (1988) *risk dominance* or their general solution concept for non–cooperative games does result in a unique prediction in which bargainers receive their disagreement payoffs plus half of the remaining surplus: $x_f = \frac{1}{2}(1 + d_f - d_u)$ and $x_u = \frac{1}{2}(1 - d_f + d_u)$.[10] The same prediction also results from combining either payoff dominance or efficiency with a notion of symmetry defined relative to the individually rational set.

Unstructured bargaining game (UBG)

In the unstructured bargaining game, players have a fixed, known amount of time available to negotiate a division of

[10] 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.

the cake. Either bargainer can propose an element of the bargaining set (see Figure 2) before the time expires, with no other constraint on the number, ordering and timing of proposals that can be made. The first proposal made by either bargainer and accepted by the other 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 be analysed by standard non–cooperative game–theoretic methods unless additional assumptions are imposed, so we use techniques from cooperative game theory.[11] 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 $x_f + x_u = 1$), 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. Under risk neutrality, the outcome of every well–known axiomatic bargaining solution (including the Nash and Kalai–Smorodinsky solutions) coincides, with $x_f = \frac{1}{2}(1 + d_f - d_u)$ and $x_u = \frac{1}{2}(1 - d_f + d_u)$.

Disagreement–payoff effects

Thus, for both games, the typically–used solution techniques imply not only certain agreement (and hence that a bargainer's demand is identical to her payoff) but also that the agreement will divide the surplus evenly after both bargainers are paid their disagreement payoffs:

$$x_f = \frac{1}{2}(1 + d_f - d_u) \quad \text{and} \quad x_u = \frac{1}{2}(1 - d_f + d_u). \tag{1}$$

This solution immediately implies

$$x_f - x_u = d_f - d_u \tag{2}$$

(the difference in the bargainers' payoffs from bargaining is equal to the difference between their disagreement payoffs). Equivalently, $g_f = g_u$, where $g_i = x_i - d_i$ is defined as the "gain" over one's disagreement payoff. Most importantly, (1) implies a sharp theoretical prediction concerning the relationship between the individual disagreement payoffs and the bargaining outcome:

$$\frac{\partial x_f}{\partial d_f} = \frac{\partial x_u}{\partial d_u} = 0.5 \tag{3}$$

$$\frac{\partial x_f}{\partial d_u} = \frac{\partial x_u}{\partial d_f} = -0.5. \tag{4}$$

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

Finally, it follows from (3) and (4) that

$$\left|\frac{\partial x_f}{\partial d_f}\right| + \left|\frac{\partial x_f}{\partial d_u}\right| = \left|\frac{\partial x_u}{\partial d_f}\right| + \left|\frac{\partial x_u}{\partial d_u}\right| = 1; \tag{5}$$

that is, the sum of these magnitudes (the "combined [disagreement–payoff] effect", abbreviated "CDPE") is one.[12]

[11] 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.

[12] Indeed, even some bargaining solutions that do not 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.

4 The experiment

The experimental design is summarised in Table 1 (see the appendix for additional session information). Most importantly, we varied the source of bargaining power. In our *Earned* treatment, a subject's disagreement payoff was determined entirely by the number of units of the real–effort task she successfully completed. (The task itself will be described in Section 4.1.) A baseline *Notask* treatment had disagreement payoffs randomly assigned as in a standard bargaining experiment, with no real–effort task performed or even mentioned.

Table 1: Experimental design and session information

Cell	Bargaining game	Real–effort task...	...used to assign disagreement payoffs	Matching groups	Subjects
NDG–earned (NE)	NDG	Yes	Yes	11	98
UBG–earned (UE)	UBG			11	102
NDG–assigned (NA)	NDG	Yes	No	11	108
UBG–assigned (UA)	UBG			11	102
NDG–notask (NN)	NDG	No	No	11	104
UBG–notask (UN)	UBG			10	102

Comparing results between our Earned and Notask treatments ought to provide some insight into the effects of earning bargaining power through a real–effort task. However, a design comprising only these treatments would not be sufficiently clean. Any difference observed between the treatments might in fact be due to disagreement payoffs being earned via the task, but it just as well may be attributable to having performed the task itself: perhaps raising the amount of effort required to take part in the experiment changes the way subjects bargain – through this effort making them take the bargaining more seriously, or giving them a sense of team spirit (since both have to succeed in order to bargain), or for some other reason. Or it may not be the task at all, but rather the time devoted to the task: perhaps putting more time in between bargaining rounds changes the way subjects learn from round to round. To remove these and other potential confounds, we include an intermediate treatment where subjects perform the real–effort task but the disagreement payoff is still assigned randomly; this is our *Assigned* treatment. The implementation of these treatments is discussed in detail below.

Besides our use of the real–effort task, we varied the bargaining institution (NDG or UBG). Our focus here is not on whether there are level–effect differences in disagreement–point responsiveness between the two games; evidence of such differences has been found previously by Anbarci and Feltovich (2013, 2018). Rather, we aim to investigate the robustness of the effects of earned bargaining power by using two bargaining settings – one entailing low strategic uncertainty and one entailing high strategic uncertainty (UBG and NDG respectively) – instead of a single setting. Finally, we need to vary the disagreement payoffs themselves in order to assess responsiveness to them. This variation is exogenous in our Assigned and Notask treatments, while it is partly endogenous and partly exogenous in our Earned treatment (as discussed below).

4.1 The real–effort task

The task used in the Earned and Assigned treatments is adapted from the one used by Anbarci and Feltovich (2018), which was based on one developed originally by Erkal et al. (2011). A sample screen–shot is shown in Appendix C.

The basic unit is the encoding of a string of letters (a "word") into numerals, based on a key displayed on the computer screen for the entirety of the task. Below each letter of the word was an empty box; to encode the word, the subject needed to type the numeral corresponding to each letter in the box below it, then click a button to submit the entire word. If the word was encoded correctly, it was accepted and a new word appeared. If the word was encoded incorrectly, an error message stated the first incorrectly–encoded letter, and the subject could go back and make changes as desired.

At the beginning of a round, a sequence of 400 letters was randomly drawn. 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 random sequences of letters means that most of our "words" were not actual words, reducing any advantage native English speakers might have relative to others. Drawing a new key and sequence of letters in each round reduces the importance of rote–memorisation ability. Additionally, previous research has found that this task does not systematically favour either males or females (Kuhn and Villeval, 2015).[13]

Variation in task difficulty within and across subjects was accomplished by varying the lengths of the words. At the beginning of a session, each subject was assigned a type (advantaged or disadvantaged) that remained fixed over all rounds. In the Earned treatment, subjects faced differing word lengths depending on their type, and both types faced "hard" and "easy" versions that varied across rounds as shown in Table 2.[14] Our within–subject variation of task difficulty was meant to ensure substantial variation in performance, and hence in disagreement payoffs. This is important because it allows us to include individual–subject fixed effects in some of our regressions (see Section 5.3) without losing too much power. In the Assigned treatment, all subjects faced the same task in a given round, identical

Table 2: Distributions of word lengths in real–effort task

Treatment and player type	Task version	Rounds	Probability of word length: 2	3	4	5	6	7	8	9	Expected word length
Earned treatment **and** advantaged player	Easy	2, 4, 5, 7, 9, 10	0.2	0.4	0.2	0.1	0.1	0	0	0	3.5
	Hard	1, 3, 6, 8, 11	0	0.2	0.4	0.2	0.1	0.1	0	0	4.5
Assigned treatment **or** disadvantaged player	Easy	2, 3, 5, 8, 9	0	0	0.1	0.1	0.2	0.4	0.2	0	6.5
	Hard	1, 4, 6, 7, 10, 11	0	0	0	0.1	0.1	0.2	0.4	0.2	7.5

to that faced by disadvantaged subjects in the Earned treatment.

This feature of the design deserves additional discussion. The reason for assigning different task difficulties to the two paired subjects in the Earned treatment is to increase the asymmetry in bargaining, giving us more power

[13] Furthermore, Charness et al. (2014) report that this task does not show strong learning–by–doing effects. We actually find a small but highly significant increasing time trend in task productivity – about 1.5 additional letters per round ($p < 0.001$) – in our own data (results available from the author). However, this is not inconsistent with Charness et al.'s result, as it may be that effort rather than skill is increasing with time.

[14] The word–length distributions were implemented as follows. At the beginning of the time allotted for the task, 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 the first k_i^1 letters 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 the next k_i^2 letters (i.e., 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 first k_i^j letters of the sequence that hadn't already been used. 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.

to detect treatment effects.[15] But a consequence is that even in our Earned treatment, favoured status (having the higher disagreement payoff) will be highly correlated with advantaged status (having the easier version of the task), which is randomly assigned. So if subjects view status as simply who has the higher disagreement payoff – and if they recognise the role luck plays in this, then they might question whether status is in fact earned in the Earned treatment.[16] However, once the task difficulty for a given subject in a given round is determined, the subject's disagreement payoff is determined wholly by the number of units of the task completed, and hence arguably is earned by the subject. This is in contrast to the Assigned and Notask treatments, where both favoured status and the disagreement payoff itself are entirely due to chance. So, whether or not subjects view bargaining position in the Earned treatment as fully earned, it it clearly earned to a greater extent there than in the other two treatments. To the extent that subjects view their status as randomly determined even in our Earned treatment, our results will understate the true effect of earned versus assigned bargaining power, making our experiment a conservative test of this question.

Subjects in the Earned and Assigned treatments were allotted five minutes in each round for the task. In the Earned treatment, a subject's disagreement payoff in Australian dollars was equal to $0.30 \cdot (m-5)$, where m was the number of units of the task completed; thus, a subject's bargaining position in the Earned treatment was determined completely by her own and her co–bargainer's respective performance on the task. In the Assigned and Notask treatments, disagreement payoffs were drawn randomly from one of the distributions shown in Figure 3 (according to the subject's type and the round, as shown in Table 2). These distributions were based on the realised distributions from the first four sessions of the Earned treatment – which were conducted before any sessions of the Assigned and Notask treatments – so as to make the distribution of disagreement payoffs, and resulting allocation of bargaining power between bargainers, roughly similar across all treatments.

While task performance did not affect subjects' disagreement payoffs in the Assigned treatment, it was important to make the task matter to them as well. Subjects in that treatment were (truthfully) informed that if either member of a pair failed to encode at least 12 words in a round, they would not bargain in that round, and the subject(s) encoding fewer than 12 words would earn a zero payoff. A subject who encoded at least 12 words, but whose would–be co–bargainer encoded fewer than 12, would earn a payoff of 30 cents for each word encoded. Out of 1155 bargaining pairs in the Assigned treatment, 35 (3.0 percent) failed to reach the bargaining stage, with about half of these failures occurring in the first round (only 1.7 percent of pairs failed to reach the bargaining stage in rounds 2–11).

Thus, unlike in the Earned treatment, there is no between–subjects variation of task difficulty in the Assigned treatment. This was in order to make it equally difficult for everyone to reach the 12–word "hurdle", so that all have equal incentives to take the task seriously. A potential drawback of this design choice is that there is likely to be less variation across subjects in task performance than in the Earned treatment (assuming task effort is similar between the two treatments). If the extent to which subjects perceive their bargaining power to be "earned" depends on task

[15] Symmetric bargaining is extremely likely to yield agreements on equal splits of the cake (Nydegger and Owen 1975; Roth and Malouf 1979). Indeed, in our experiment, of the roughly 1 percent of bargaining pairs with equal disagreement payoffs, almost 87 percent (33/38) reached agreement, with over three–quarters of these (25/33) agreeing on exactly a 50–50 split.

[16] Notably, while we did not use deception in the experiment, we did not provide specific information to subjects in our Earned treatment about how task difficulty varied within– or between–subjects; in particular, subjects were not told that some were advantaged and others disadvantaged. The only mention of task difficulty in the instructions was the wording, "[t]he sequence of words you face may differ from those faced by others". This reduces the likelihood that subjects recognise the role that luck plays in determining who has the better bargaining position in the Earned treatment, though clever subjects might eventually have inferred information about others' task difficulty by comparing their disagreement payoffs with their co–bargainers'. Similarly, subjects in the Assigned and Notask treatments were not told anything about the underlying distributions from which disagreement payoffs were drawn.

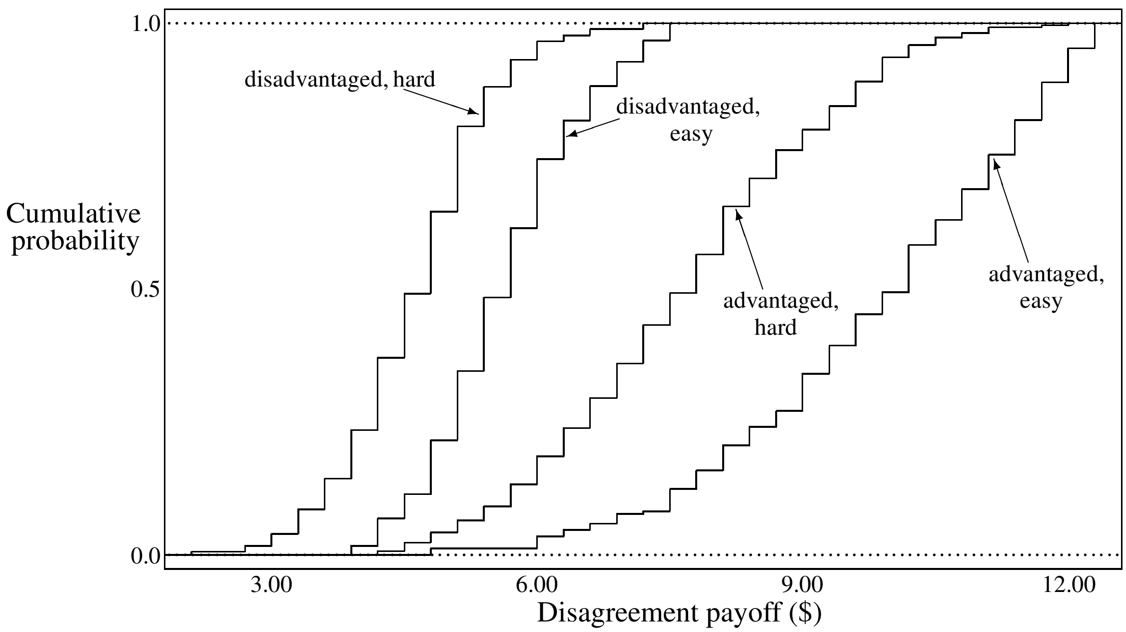

Figure 3: Disagreement payoffs in Assigned and Notask treatments (cumulative distribution functions)

performance – even when this performance does not affect the disagreement payoffs – this reduced variation could potentially impact the bargaining outcomes.[17]

4.2 Additional information about experimental procedures

Sessions took place at Monash University's experimental economics lab (MonLEE). Subjects were primarily undergraduate and masters–level students, recruited using the ORSEE software (Greiner, 2015) from a database of people expressing interest in participating in economics experiments; there were no exclusion criteria.[18] 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.

Each session, depending on its size, comprised one or more *matching groups*. A matching group contained equal numbers of advantaged and disadvantaged subjects, and was closed with respect to interaction. That is, advantaged subjects in a particular matching group interacted only with disadvantaged subjects in the same matching group, so that data from different matching groups can be considered statistically independent of each other. Subjects were randomly paired in each bargaining round, with each opposite–type subject in the same matching group equally likely (a "two–population strangers matching"). No identifying information was given about opponents, in an at-

[17] That is, bargainers' may view their "moral claims" to shares of the cake as determined by their task performance, and if these do not vary much, we may only see agreements close to 50–50 splits, regardless of subjects' relative bargaining positions. However, evidence from the experiment suggests that subjects did not treat their relative task performances as moral claims. Specifically, some of the regressions (models 3 and 4) discussed in Section 5.3, and additional ones done as robustness checks, included measures of task performance as an explanatory variable for bargaining outcomes. These variables turn out never to be significant.

[18] A natural exclusion criterion would have been people who had participated in previous bargaining experiments; however none had been conducted at MonLEE before the current experiment. The author had conducted some bargaining experiments, but these were at a different university. Similarly, even though one of the authors of Erkal et al. (2011) is at Monash University, no sessions of that experiment were conducted at MonLEE. Hence, subjects in the current experiment were likely to have been inexperienced at both the real–effort task and the bargaining setting.

tempt 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.

At the beginning of a session, subjects were seated in a single room and given written instructions.[19] These instructions were also read aloud in an attempt to make the rules of the game common knowledge, and questions were answered, before play began. There were eleven rounds, preceded by a practice round (one minute of the task, with no bargaining) in the Earned and Assigned treatments, and followed by a set of demographic questions in all treatments. In the Earned and Assigned treatments, each round began with a screen showing some information about the task, followed by the task itself. They then were shown a screen with the number of units of the task completed, and the number completed by the co–bargainer.[20]

After this task feedback – or to begin the round in the Notask treatment – came the bargaining stage, with subjects being reminded of the cake size (which was always $20) and informed of their disagreement payoffs. Demands in the NDG treatment, and proposals in the UBG treatment, were required to be non–negative–integer multiples of $0.05 (the denomination of the smallest circulating coin in Australia), between zero and the cake size inclusive. In the NDG treatment, after all subjects had chosen their demands, the round ended and they received feedback: both bargainers' words encoded and disagreement payoffs, both bargainers' demands, whether agreement was reached, and both bargainers' payoffs. Previous results were also collected into a history table at the top of the computer screen; these 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 two rounds) to agree on a division of the cake. Subjects could make as many or as few proposals as they wished during the allotted time; a proposal comprised both bargainers' payments, and these had to add 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, or even different from, 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.[21] No messages were possible apart from the proposals ("tacit" rather than "explicit" bargaining).[22] Both the subject's own proposals and those of the co–bargainer were displayed on the subject's screen (in separate areas); it was not possible to view other pairs' proposals. As long as the negotiation stage had not ended, a subject could accept any of the co–bargainer's proposals. These were listed in order of increasing payoff to the subject, so little cognitive effort was required to determine the most favourable

[19] Sample instructions are shown in Appendix B, and some screen–shots are shown in Appendix C. Other materials, including the raw data from the experiment, are available from the corresponding author upon request.

[20] Our giving subjects information about their relative performance may motivate them to take the task more seriously (see, e.g., Charness et al. 2014), even in the Assigned treatment.

[21] Strategically, this has a similar effect to requiring later proposals to be concessions (i.e., more favourable to the co–bargainer) relative to earlier proposals, though the effect is not exactly the same, as our subjects had the option to send an extreme proposal – or repeat the same proposal – as a form of cheap talk. We acknowledge that in real bargaining, it is possible to remove offers from the table. However, such behaviour is rare in practice, as once an offer is made, subsequent claims that the bargainer is unable or unwilling to re–offer it are likely to be viewed by the co–bargainer as either incredible or evidence that the bargainer is not bargaining in good faith.

[22] 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 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, which in a bargaining setting may push outcomes across–the–board toward 50–50 splits of the cake.

one, 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.[23] In any case, after the negotiation stage ended, subjects received end–of–round feedback: both bargainers' words encoded and disagreement payoffs, whether agreement was reached, and both bargainers' payoffs. As in the NDG cells, subjects' previous results were also available in a history table.

At the end of a session, subjects were paid, privately and individually. Each subject received the sum of their payoffs in five randomly chosen rounds, with no show–up fee. Total earnings averaged \$51.92 for advantaged subjects (with a standard deviation of \$6.93) and \$37.77 for disadvantaged subjects (with a standard deviation of \$7.11), for a session that typically lasted about 90–120 minutes (30–45 minutes for the Notask treatment).

4.3 Hypotheses

Our first hypothesis is an implication of standard bargaining theory:

Hypothesis 1 *Responsiveness to changes in bargaining position is unaffected by whether bargainers' status comes about from performance in the real–effort task.*

This is a weakening of the implication we would normally make from the standard theory: that a unit increase in a player's *own* disagreement payoff is associated with a half–unit increase in that player's demand and payoff, and a unit increase in the player's *opponent's* disagreement payoff is associated with a half–unit decrease in that player's payoff (see Equations 3 and 4 in Section 3) – irrespective of where these disagreement payoffs came from. The under–responsiveness results from previous bargaining experiments discussed in the first part of Section 2 suggest that a hypothesis of full responsiveness is likely to be a "straw man". Our Hypothesis 1 is consistent with these previous results, however, as long as under–responsiveness is similar across our treatments.

The previous research on real–effort tasks discussed in the second part of Section 2 has little clear implication for our experiment. It is certainly plausible based on those results that favoured players benefit (and unfavoured players suffer) when favoured status is earned, but there is little to suggest *how* that benefit changes with the disagreement payoff. For example, if favoured players receive a lump–sum benefit from an increased moral claim to payoffs, responsiveness to changes in the disagreement outcome (conditional on favoured/unfavoured status) may not differ from when disagreement payoffs are assigned. Nonetheless, we conjecture – based on our own intuition and that which seems to underlie much of this literature – that earning any aspect of one's bargaining power will move behaviour more in line with standard theory. In the appendix, we present a behavioural model that motivates our conjecture, but here we simply state the hypotheses derived from it.

Our Earned and Assigned treatments differ in whether disagreement payoffs for the two bargainers were determined based on task performance (Earned) or randomly assigned (Assigned). We conjecture that earning bargaining position matters:

Hypothesis 2 *Responsiveness to changes in the disagreement outcome will be* higher *when disagreement payoffs are earned than when they are assigned.*

Also, our Assigned and Notask treatments differ in whether the opportunity to bargain is based on task performance (Assigned) or given to everyone (Notask). This difference could have an effect for multiple reasons. One

[23] Just under 22 percent of disagreeing pairs (i.e., 3.4 percent of all bargaining pairs) in the UBG ended bargaining by either subject clicking the button, with the remaining disagreements (12.3 percent of all bargaining pairs) due to time running out in the bargaining stage.

is similar to the conjecture behind Hypothesis 2: earning the right to bargain could matter. Alternatively, it could simply be that performing a task changes subsequent behaviour, even if there were no connection at all between the task and the later stages (e.g., as if the effort spent on the task concentrates the mind, or gives the subject a sense of a moral claim). We have:

Hypothesis 3 *Responsiveness to changes in the disagreement outcome will be* higher *when the opportunity to bargain is earned than when it is assigned.*

5 Experimental results

In our analysis, we express quantities like demands, payoffs, and disagreement outcomes as fractions or percents of the cake size, thus normalising the cake size to unity as in Section 3. We focus on two sub–samples for each combination of treatment (Earned, Assigned or Notask) and game (NDG or UBG). In the three NDG cells, each subject makes a single demand, which may or may not lead to agreement. One sub–sample is simply the full set of demands over all rounds in each cell, which we will call "NE–all", "NA–all" and "NN–all" corresponding to the Earned, Assigned or Notask bargaining–power sources. A second sub–sample in each of the cells is a subset of the first, comprising only pairs whose demands were compatible. In the "NE–agreements", "NA–agreements" and "NN–agreements" sub–samples, we will again have one demand for each subject and round, conditional on that subject's pair reaching agreement.

In the UBG, each subject may make a single proposal, multiple proposals, or no proposals at all. Any of the proposals made by a subject can be construed as a demand by that subject. Moreover, a subject's acceptance of an opponent proposal, and even failure to accept a proposal, could be understood as carrying information about demands. Inferring a single demand from all of a subject's behaviour in a UBG round thus necessarily carries a subjective element. To minimise this subjectivity in analysing our data–set, we classify UBG demands based on unambiguous rules. Our "UE–agreements", "UA–agreements" and "UN–agreements" sub–samples are straightforward. We interpret each accepted proposal in the UBG as a pair of compatible demands (irrespective of who made the proposal and who accepted it); if a pair does not reach an agreement, it is left out of this sub–sample. Our "UE–all", "UA–all" and "UN–all" sub–samples include 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 in about 1 percent of UBG observations.) Note that in both games, demands conditional on agreement are identical to payoffs conditional on agreement.

5.1 Preliminaries

Before concentrating attention on bargaining behaviour, we look briefly at some other results. First, Table 3 displays some statistics relating to task performance (in the Earned and Assigned treatments), participation in the bargaining stage (in the Assigned treatment), and the assignment of disagreement payoffs based on either the task (in the Earned treatment) or random draws (in the Assigned and Notask treatments). Recall that the terms "advantaged" and "disadvantaged" refer to having the easier or harder version of the task, while "favoured" and "unfavoured" refer to having the higher or lower disagreement payoff.

Table 3: Results involving task performance and allocation of bargaining power

Cell:	NN	NA	NE	UN	UA	UE	Total
Number of potential bargaining pairs	561	594	539	572	561	561	3388
Mean words encoded (advantaged)		18.1	32.1		19.2	33.1	
(disadvantaged)		19.1	18.3		19.2	19.3	
Pairs not reaching bargaining stage	0	20	0	0	15	0	35
(after first round)	0	7	0	0	11	0	18
Pairs with no surplus available	0	0	0	0	0	1	1
Remaining bargaining pairs...	561	574	539	572	546	560	3352
...with d_i lower for advantaged player	8	15	24	17	5	12	81
...with d_i equal for both players	7	4	9	4	5	9	38
...with $d_f > 0.5$ (dom. barg. power)	136	128	105	127	131	123	750
	(24.2%)	(22.3%)	(19.5%)	(22.2%)	(24.0%)	(22.0%)	(22.4%)
...with $d_f \geq 0.6$	29	28	30	36	20	35	178
	(5.1%)	(4.7%)	(5.6%)	(6.3%)	(3.6%)	(6.2%)	(5.3%)
Mean d_f, % of cake	42.4	42.5	40.7	41.9	42.3	42.1	42.0
Mean d_u, % of cake	21.0	20.8	20.0	20.7	20.8	21.5	20.8

N, U* = NDG, UBG games. *N, *A, *E = Notask, Assigned, Earned treatments.*

One important result from this table is that our Assigned treatment seems to have succeeded in making subjects take the task seriously, since the average number of words encoded there is almost identical to that for disadvantaged subjects in the Earned treatment (recall that these groups faced the same task difficulty). Relatedly, there was a small fraction of subject pairs that failed to progress to the bargaining stage, and at the other extreme there was one pair so successful with the task that their sum of disagreement payoffs was more than $20, leaving no surplus to bargain over.[24] Combined, these two groups comprise approximately 1 percent of pairs, and are dropped from the sample in our subsequent analysis. Another 1 percent of pairs were in symmetric bargaining situations; these pairs are typically left in the sample, but in parts of the analysis where we distinguish between favoured and unfavoured players, this group will also be dropped. In about 2.4 percent of pairs, the advantaged player ended up with a lower disagreement payoff than her co–bargainer, thus becoming the unfavoured player in the bargaining; these observations present no difficulty for us and are included in the sample.

Table 3 also shows that just under a quarter of the sample comprises pairs in which one bargainer has dominant bargaining power, and in about a quarter of these, the favoured player's disagreement payoff d_f is at least 60 percent of the cake. Thus, most of our data–set involves cases either without dominant bargaining power, and when there is dominant bargaining power, d_f is fairly close to one–half, so that the assumptions underlying Hypotheses 2 and 3 ought to be satisfied (see the last section of Appendix A.1 for details). Finally, the fraction of pairs with dominant bargaining power is roughly similar across cells, as are average disagreement payoffs for both favoured and unfavoured players. Indeed, there are no significant differences in disagreement payoffs across treatments.[25]

Moving on to agreement frequencies, 75.8 percent of pairs reached agreement overall, but there were pronounced

[24]This last pair, who earned disagreement payoffs of 0.795 and 0.21 as fractions of the cake size, did not reach an agreement.

[25]Kruskall–Wallis tests, Notask versus Assigned versus Earned matching–group–level data, $p > 0.20$ for either favoured or unfavoured subjects. Robust rank–order tests also yielded $p > 0.20$ for every pairwise comparison. See Siegel and Castellan (1988) for descriptions of the non–parametric tests used in this paper, and Feltovich (2005) for critical values of the robust rank–order test.

differences according to the game (67.3 percent in NDG, versus 84.3 percent in UBG) and the presence of dominant bargaining power (63.6 percent with, versus 79.3 percent without).[26] Table 4 presents some information about the factors affecting agreement frequencies, based on a panel probit regression on pair–level data with agreement as the dependent variable. Right–hand–side variables are dummies for the UBG, the Assigned and Earned treatments, and dominant bargaining power; continuous variables for the difference in disagreement payoffs (the favoured bargainers' minus the unfavoured bargainers'), the available surplus (the cake size minus the sum of the bargainers' disagreement payoffs) and the round number; and a constant term. The positive effect of the UBG is in keeping

Table 4: Factors affecting agreement frequency (average marginal effects, standard errors in parentheses)

Dependent variable: agreement (panel probit, $N = 3352$)							
Notask treatment	Earned treatment	UBG	Dominant barg. power	Difference in disagreement payoffs	Available surplus	Round number	$\|ln(L)\|$
–0.007	–0.018	0.173^{***}	-0.046^{*}	0.515^{***}	0.123	0.012^{***}	1660.06
(0.027)	(0.027)	(0.022)	(0.027)	(0.084)	(0.083)	(0.002)	

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

with previous experimental comparisons of structured and unstructured bargaining (e.g., Feltovich and Swierzbinski, 2011). The negative effect of dominant bargaining power – weakly significant even after controlling for the difference in disagreement payoffs – is consistent with the behavioural model developed in the appendix (which only allows disagreements in equilibrium when there is dominant bargaining power).[27] The significant negative effect of the difference in disagreement payoffs itself is probably a reflection of a tendency toward agreements on equal splits when bargaining power is nearly symmetric.

5.2 Treatment effects – descriptive statistics

We now move to our main research questions. Cell–level aggregates showing the relationship between bargaining position and bargaining outcomes (demands and payoffs) are presented in Figure 4. The left panel shows that on average, subjects do exploit their bargaining power to some extent: favoured players demand substantially more on average than unfavoured players, illustrated by all of the plotted points lying well below the 45–degree line. The right panel shows that the extent of this exploitation is well below what is predicted by standard theory.As in Section 3, Player i's *gain* g_i is the difference between her demand and her disagreement payoff ($g_i = x_i - d_i$). Favoured players' gains are *lower* than those of unfavoured players (recall that (2) in Section 3 implies they should be equal). Non–parametric tests on the matching–group–level data confirm these within–cell results: higher average demands and lower average gains for favoured versus unfavoured bargainers (Wilcoxon signed–ranks test for paired samples, $p \leq 0.005$ in all 24 comparisons involving either demands or gains).

Closer examination of the figure also suggests the presence of treatment effects along the lines of the "lump–sum benefit" to favoured bargainers discussed in Section 4.3. In particular, under–exploitation of bargaining power

[26] Feltovich and Swierzbinski (2011) attribute the difference in agreement frequencies between Nash and unstructured bargaining to different levels of strategic uncertainty in the two bargaining settings. Some additional analysis of unstructured bargaining can be found in the appendix.

[27] However, the insignificant effect of the source of bargaining power is at odds with our model, since if aversion to disadvantageous agreements is lower under earned disagreement payoffs, disagreements should be less frequent in that case, not (slightly) more frequent as the table shows.

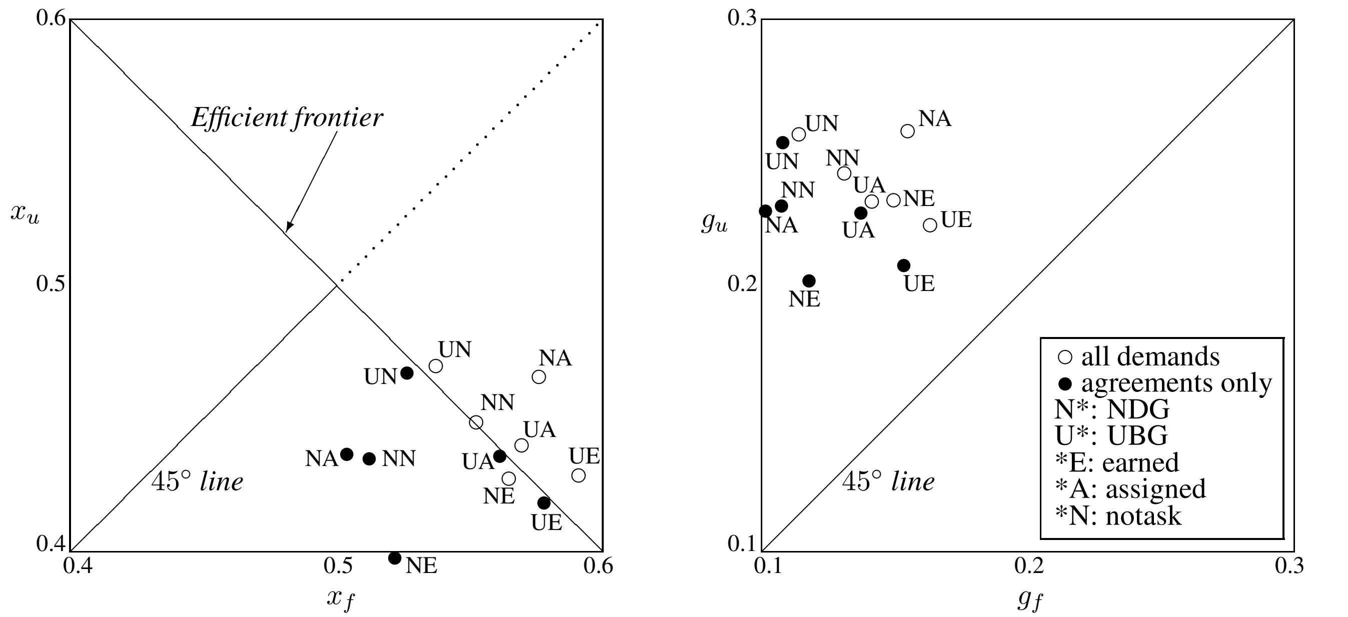

Figure 4: Treatment–level average bargaining outcomes. Left panel: favoured and unfavoured player demands (x_f, x_u). Right panel: favoured and unfavoured player "gains" ($g_f = x_f - d_f$, $g_u = x_u - d_u$)

appears to be least severe (i.e., plotted points in the right panel are closest to the 45–degree line) in the Earned treatment and most severe in the Notask treatment. Non–parametric tests partly confirm these differences. Using pooled NDG and UBG sessions, differences between the Earned treatment and either the Assigned or Notask treatment are significant, both for all demands and for agreements (robust rank–order test, $p < 0.05$), and the same is true for the UBG alone ($p < 0.05$). However, tests using only the NDG do not reach significance at conventional levels ($p > 0.10$), and none of the differences between the Notask and Assigned treatments are significant ($p > 0.10$), though they are in the direction consistent with our Hypotheses 2 and 3.

In Figure 5, we disaggregate to the level of the individual bargaining pair. Each panel shows all of the individual bargaining outcomes in a given treatment (from left to right: Notask, Assigned, Earned), plotted as dots with the difference between favoured– and unfavoured–player disagreement payoff as the horizontal coordinate (we drop those observations with symmetric bargaining), and the difference between favoured– and unfavoured–player demands as the vertical coordinate. To reduce clutter in the figure, we pool NDG and UBG data, bargaining pairs with and without dominant bargaining power, and agreements and disagreements.[28] (This pooling is only temporary; our regression analysis in the next section will allow different effects depending on the game and other factors.) As benchmarks, each panel also includes a dashed horizontal line at $x_f - x_u = 0$ (corresponding to a 50–50 split), and another dashed line corresponding to the standard–theory prediction ($x_f - x_u = d_f - d_u$, which would be a 45–degree line if we used the same scales on both axes). Finally, OLS trends are shown for each treatment.

The figure shows substantial heterogeneity within each treatment, with non–trivial fractions of demands at or near a 50–50 split (horizontal line), close to the theoretical prediction (45–degree line), and strictly between these two benchmarks, with a small but not negligible number of observations beyond the 45–degree line (e.g., agreements where the shares of the cake are proportional to the disagreement payoffs) and below the 50–50 split line (i.e.,

[28] Also, to avoid multiple observations coinciding in the figures, we perturb each dot with uniform noise over [–0.03, +0.03] (as fraction of the cake) in both the horizontal and vertical coordinates.

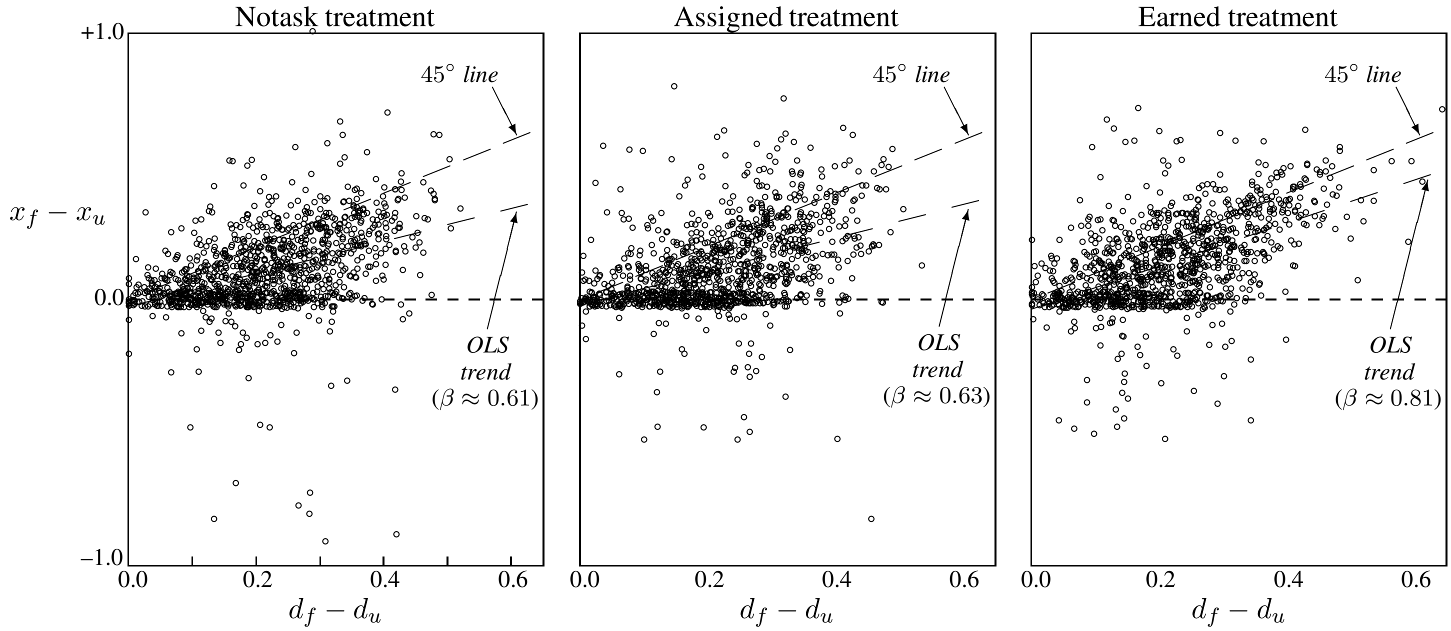

Figure 5: Individual bargaining outcomes (pooled data)

favouring the unfavoured player). The clouds of outcomes appear similar between the left and middle panels (Notask and Assigned treatments), while the right panel (Earned treatment) has more outcomes near or above the 45–degree line and fewer near or below the equal–split line. By the same token, the OLS trend lines in the left and centre panels are nearly identical, indicating just under two–thirds of the predicted level of responsiveness to the disagreement outcome in the Notask and Assigned treatments, with only slightly higher responsiveness in the latter. The OLS trend in the right panel is steeper, implying substantially higher responsiveness (over four–fifths of the predicted level) in the Earned treatment.

5.3 Treatment effects – regression analysis

We use panel regressions to investigate these treatment effects further. In each specification, demand is the dependent variable, and we use the combined NDG–all and UBG–all data, consisting of all demands irrespective of whether agreement was reached.[29] Our main explanatory variables are the disagreement payoffs (d_i and d_{-i}); the marginal effects of these variables correspond to our own– and opponent–disagreement–payoff effects, and therefore combined responsiveness is the sum of their magnitudes. Since we are interested in how this responsiveness differs across our treatments, we include indicators for the Earned and Notask treatments (so that Assigned is the baseline), as well as their products with the disagreement–payoff variables. Model 1 is a Tobit with only these variables, subject random effects, and a constant term on the right–hand side.

Model 2 corresponds roughly to Model 1. The most important difference is that Model 2 includes individual–subject fixed effects, in order to control for any unobserved characteristics that might affect both an individual subject's bargaining ability and her skill at the real–effort task. To the extent that these two talents are correlated, Model 1 will yield biased coefficient estimates, but including the fixed effects in Model 2 addresses this problem. The use of subject fixed effects necessitates a few additional minor differences between Models 1 and 2. Because

[29] See Section 5.1 for details of these samples, and see the appendix for corresponding regressions using the combined NDG–agreements and UBG–agreements sample, which yield similar results to those reported here.

our variation of Earned/Assigned/Notask was between–subjects, we have to drop the Earned and Notask indicators from Model 2 to avoid perfect collinearity with the fixed effects (but we keep their interactions with the disagreement payoffs, of course). Also, we estimate linear models instead of Tobits, though given the dearth of observations with demands close to either zero or the entire cake, this is unlikely to have much of an effect. An extra advantage of using linear models is that we can cluster standard errors, which we do (at the matching–group level). Finally, since we are using fixed effects, we drop the random effects that were in the Tobits.

Model 3 corresponds to Model 1, but with some additional variables: indicators for the UBG and for dominant bargaining power (both of which have been found previously to impact bargaining outcomes); the round number as a continuous variable (to control for any learning over time); all of the possible two– and more–way interactions of either disagreement payoff, either treatment dummy, the UBG and dominant bargaining power dummies, and the round number; and the own and opponent's total number of letters encoded (to control for the possibility that pure success at the task – beyond its connection with disagreement payoff – may affect bargaining ability). Model 4 corresponds to Model 2 in a similar way. The interaction terms in Models 3 and 4 give us a great deal of flexibility in allowing disagreement–point responsiveness not only to vary across treatments, but to do so in ways that potentially differ between games, over time, and so on.[30] All of the models are estimated using Stata (version 15).

The results are shown in Table 5. Marginal effects are shown for the own– and opponent–disagreement–payoff variables, both unconditional (average marginal effects) and conditional on each of the three treatments (e.g., for the Notask treatment, the Notask and Earned dummies are set to one and zero respectively).[31] Standard errors are shown in parentheses, significance of differences from the predicted level of one–half at the 10–, 5–, or 1–percent level is represented by stars, and the sample size and log–likelihood are displayed for each specification. Overall, own–disagreement–payoff responsiveness ranges from roughly 60–80 percent of the predicted level, and opponent–disagreement–payoff responsiveness from 45–60 percent of the predicted level, depending on which specification we use. Similarly, the combined disagreement–payoff effect varies from 55–70 percent of the predicted level, which is not unusual for this literature (see Figure 1 and surrounding discussion).

The nature of the differences in results across specifications merits some attention. Including the extra controls (moving from Model 1 to 3 or from 2 to 4) *reduces* estimates of responsiveness to the own and opponent disagreement payoff: both overall (as seen in the average marginal effects) and in each treatment (as seen in the marginal effects conditional on a given treatment). Including fixed effects (moving from Model 1 to 2 or from 3 to 4) has a more ambiguous effect. Mostly, it tends to *increase* estimated responsiveness, with the effect most pronounced in the Earned treatment (holding constant the pair of models being compared), which is exactly what we would expect given the potential for selection in that treatment. The increased responsiveness when fixed effects are included suggests that individual subjects' skill at the real–effort task is *negatively* correlated with their bargaining ability.

This last point is worth emphasising further. The rationale for including fixed effects is the concern that skill in the task and skill at bargaining might be *positively* correlated. If this had turned out to be true, then at least some of the apparent increase in disagreement–payoff responsiveness from the Assigned treatment (where random assignment means bargaining ability uncorrelated with disagreement payoffs) to the Earned treatment (where potentially better

[30] Additional specifications for robustness include models also containing all of the demographic variables collected after the last bargaining round, and models using either only favoured players or only unfavoured players. No qualitatively different results obtained from any of these other specifications. The results with favoured and unfavoured players separately are presented and briefly discussed in the appendix; the others are available from the author upon request.

[31] Marginal effects incorporate the main effect of the variable along with any interaction terms containing that variable. Thus it is possible to present marginal effects of a variable like Earned, even in those models that do not include the corresponding indicator, based on the interaction terms containing it.

Table 5: Factors affecting subjects' demands (marginal effects from regressions, with standard errors in parentheses)

	[1]	[2]	[3]	[4]
Own disag. payoff	0.355^{***}	0.392^{***}	0.293^{***}	0.304^{***}
	(0.012)	(0.017)	(0.016)	(0.019)
...Earned treatment	0.418^{***}	0.492	0.340^{***}	0.406^{***}
	(0.023)	(0.029)	(0.032)	(0.032)
...Assigned treatment	0.317^{***}	0.327^{***}	0.284^{***}	0.261^{***}
	(0.020)	(0.035)	(0.025)	(0.034)
...Notask treatment	0.330^{***}	0.359^{***}	0.255^{***}	0.249^{***}
	(0.020)	(0.026)	(0.025)	(0.023)
Opp. disag. payoff	-0.263^{***}	-0.292^{***}	-0.234^{***}	-0.236^{***}
	(0.012)	(0.018)	(0.015)	(0.023)
...Earned treatment	-0.310^{***}	-0.348^{***}	-0.291^{***}	-0.301^{***}
	(0.020)	(0.035)	(0.030)	(0.048)
...Assigned treatment	-0.259^{***}	-0.277^{***}	-0.219^{***}	-0.227^{***}
	(0.020)	(0.025)	(0.025)	(0.032)
...Notask treatment	-0.222^{***}	-0.250^{***}	-0.192^{***}	-0.180^{***}
	(0.020)	(0.033)	(0.025)	(0.035)
	panel Tobit with REs	panel linear with FEs and clustering	panel Tobit with REs	panel linear with FEs and clustering
Treatment dummies?	Yes	Yes (in interactions)	Yes	Yes (in interactions)
Additional variables?	No	No	Yes (UBG, dominant barg. power, round number, own and opp. letters encoded)	
Interaction terms?			Yes (see text for details)	
N	6670	6670	6670	6670
$\|ln(L)\|$	6374.64	7416.75	6523.50	7503.17

* (**,***): *Marginal effect significantly different from 0.5 at the 10% (5%, 1%) level.*

bargainers would have higher disagreement payoffs) would have been due to this selection rather than due to earning one's status, and our Tobit results would exaggerate the true effect of earned bargaining power. However, we actually find the opposite result: *negative* correlation between skill in the task and bargaining ability means that selection works in the opposite direction in the Earned treatment (better bargainers are worse at the task and hence have lower disagreement payoffs). Thus the true effect is *understated* in the Tobit results.

Figure 6 shows – for each of the specifications used in Table 5 – point estimates for the own–, opponent–, and combined disagreement–payoff effects for each treatment). For the combined effect, 95–percent confidence intervals are displayed as error bars, and p–values are shown for tests of significance of differences between treatments (labelled "A/E" for Assigned versus Earned, etc.). While there are some minor differences across specifications, two results are apparent. First, the combined disagreement–payoff effect – not only the point estimate, but even the upper bound of the 95–percent confidence interval – is below one throughout the figure (and indeed in Figure 7 below as well), meaning that subjects are under–responsive to changes in their bargaining power – even in the Earned treatment.

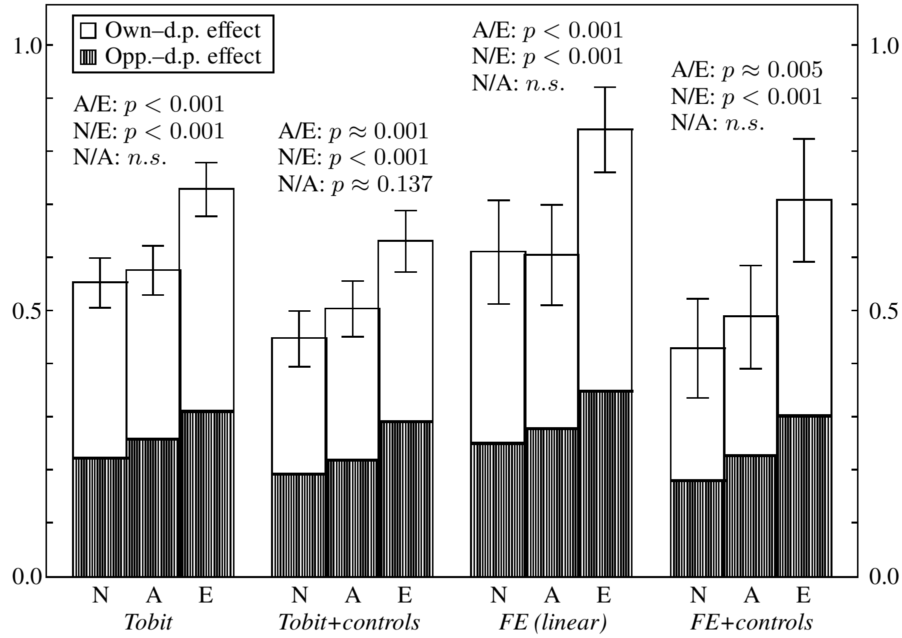

Figure 6: Estimated responsiveness to bargaining position by treatment, based on models in Table 5 (N=Notask, A=Assigned, E=Earned, d.p.=disagreement payoff, d.b.p.=dominant bargaining power, n.s.=not significant (p–value above 0.20)). Error bars illustrate 95–percent confidence intervals for *combined* effect.

Result 1 *In all three treatments, subjects are under–responsive to changes in their bargaining position.*

However, this responsiveness is significantly higher in the Earned treatment than in the Assigned treatment in all of the specifications. This provides fairly strong evidence of the effect of earned bargaining position (Hypothesis 2).

Result 2 *Subjects are significantly more responsive to changes in their bargaining position in the Earned treatment than in the Assigned treatment.*

By contrast, differences between the Assigned and Notask treatments are not significant at conventional levels (thus not supporting Hypothesis 3). However, the *direction* of the difference is in the anticipated direction in all models except for the linear model without controls, and the corresponding p–value approaches significance in one case. As a result, differences between the Earned and Notask treatments (corresponding to the typical test of real effort in the literature, without our intermediate Assigned treatment) are usually larger than those between Earned and Assigned, suggesting that the former comparison overstates the true effect of earned status. Since the Earned–Assigned difference is always significant, this overstatement is fairly harmless: limited to an exaggeration of the effect size.

Result 3 *While responsiveness to changes in bargaining position is typically higher in the Assigned treatment than in the Notask treatment, these differences are not significant.*

Finally, in Figure 7 we examine responsiveness in subgroups: by either game (NDG or UBG) or whether one bargainer had dominant bargaining power. We focus on Models 3 and 4, since the relevant interaction terms were not included in Models 1 and 2, making them unsuitable for subgroup analysis. The left panel shows that the combined effect is significantly higher in the Earned than in the Assigned treatment in the NDG according to either the Tobit

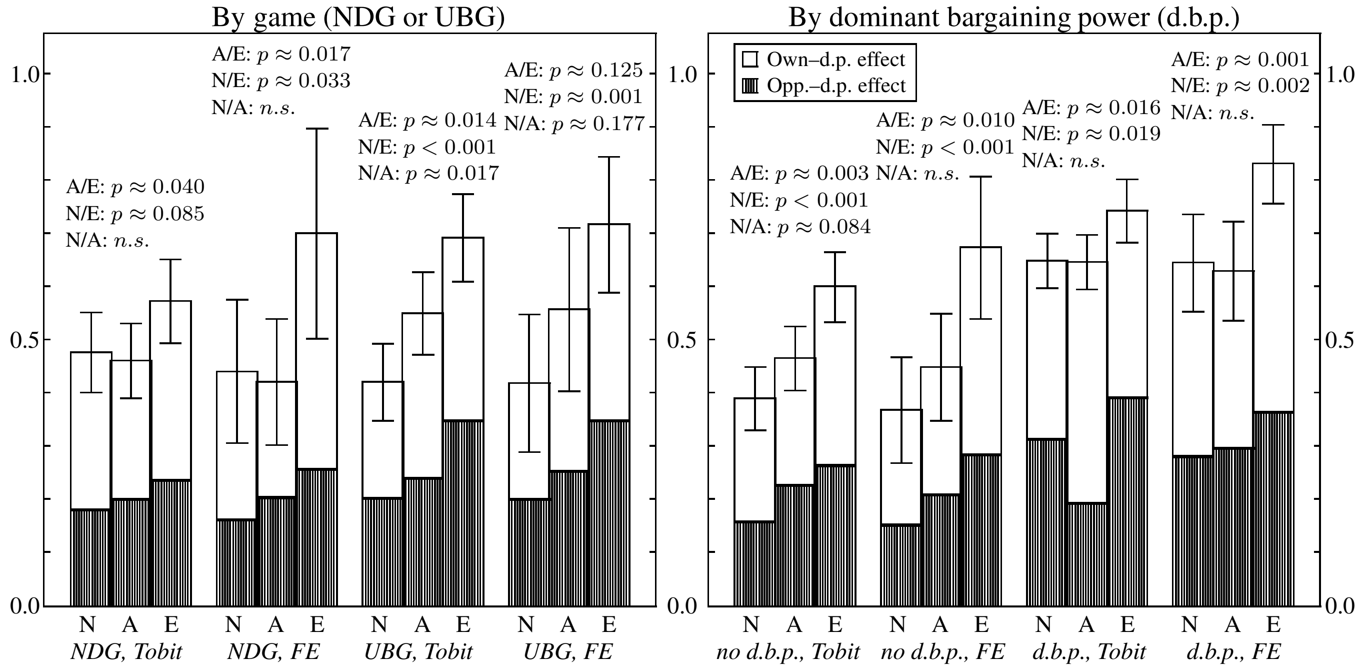

Figure 7: Estimated responsiveness to bargaining position by treatment, conditional on the game or presence of dominant bargaining power, based on Regressions 3 (Tobit) and 4 (fixed–effects linear with clustering) in Table 5 (N=Notask, A=Assigned, E=Earned, d.p.=disagreement payoff, d.b.p.=dominant bargaining power, n.s.=not significant (p–value above 0.20)). Error bars illustrate 95–percent confidence intervals for *combined* effect.

(Model 3) or the fixed–effects (Model 4) model. In the UBG, the difference is significant in the Tobit but not the fixed–effects model. However it is worth noting that the point estimate of the difference is nearly identical in the two specifications. So, the lack of significance in Model 4 is likely not due to any correction for selection bias, but rather to the increased standard errors relative to Model 3. Thus, even though this experiment has a large number of subjects (616) by the standards of economics experiments, an even larger number would have been needed for the power to detect a difference in the UBG.

The right panel shows that both with and without dominant bargaining power, the combined effect is significantly higher in the Earned than in the Assigned treatment according to both specifications.[32] In both panels, differences between the Assigned and Notask treatments are either negligible or in the direction predicted by Hypothesis 3, and some of these differences are at least weakly significant, but they are unlikely to survive any of the common adjustments for multiple statistical comparisons.[33] As with the corresponding results from Figure 6, therefore, we consider these results to be suggestive of differences between our Assigned and Notask treatments, rather than conclusive. Also as before, however, the higher responsiveness in the Assigned treatment than in the Notask treatment means

[32]The strong result in the case of dominant bargaining power is noteworthy, since according to Proposition 1 in the appendix, reducing α (the parameter of aversion to disadvantageous agreements) is only guaranteed to increase disagreement–payoff responsiveness when disagreement payoffs are less than half of the cake. As noted there, when one bargainer has dominant bargaining power, additional conditions on parameter values would have to hold for the theoretical result to go through. To the extent that our behavioural model is true, rather than simply a useful source of predictions, our experimental results would suggest those additional conditions do hold.

[33]For example, according to Benjamini and Hochberg's (1995) correction, none of the eight differences between Notask and Assigned treatments displayed in Figure 7 remains significant at even the 10 percent level. By contrast, the corresponding differences between Earned and Assigned treatments largely survive this correction: six of the eight remain significant at the 5 percent level, while the remaining two have nearly–significant adjusted p–values of 0.069 and 0.126.

that comparisons between Earned and Notask treatments will overstate the actual effect of earned status.

Result 4 *The higher responsiveness in the Earned treatment (versus Assigned treatment) is observed in both games, and both with and without dominant bargaining power, and the difference is nearly always significant.*

6 Discussion and concluding remarks

Our main result is Result 2: bargainers are more responsive to changes in earned bargaining position than when it is randomly assigned. This result confirms our Hypothesis 2, and is robust: we observe significant differences between our Earned and Assigned treatments not only in our full sample, but in both bargaining games (NDG and UBG) and both with and without dominant bargaining power (and even separately for subjects with strong and weak bargaining position, as noted in footnote 30). Importantly, it survives the use of subject fixed effects to overcome potential selection bias. This result cannot be explained by standard theory, though it is consistent with the predictions of a behavioural model developed in the appendix.

We also find some evidence of increased responsiveness in our Assigned treatment compared to our Notask treatment, which both assign bargaining position randomly, but differ in the former's presence of a *status–irrelevant* real–effort task. However, these differences are smaller than those between Earned and Assigned, and are typically not significant. As usual for insignificant results that are in the expected direction, drawing broad conclusions is difficult. One potential interpretation is that because the differences between Assigned and Notask treatments are not significant, the distinction between the two is irrelevant. If so, tests of earned–versus–assigned status can safely use the standard approach in which there is one treatment with no task and randomly assigned status, and a second treatment with a task used to assign status. An alternative interpretation follows from the finding that the differences are typically in the predicted direction, so that the effect size of the Earned–versus–Assigned comparison is typically smaller than that of the Earned–versus–Notask comparison. This implies that the standard approach may over–state the size of the treatment effect between earned and assigned status. Our view is closer to the latter interpretation, but this is clearly a question of taste. Future research may be necessary to establish whether – and in what settings – behaviour is affected by status–irrelevant tasks, and if it is, what the mechanism is behind their effects.

Bargaining theorists may be particularly interested in our finding of greater responsiveness when bargaining position is earned, as it suggests their theories might perform better in the field than they have heretofore in the lab. As mentioned in Section 2, while bargaining experiments have tended to find substantial under–exploitation of bargaining position relative to the theory, they have typically allocated roles and other aspects of the setting randomly. To the extent that bargaining position outside the lab reflects bargainers' qualities such as skill or effort, our results suggest that evidence from the lab may understate the degree of exploitation of bargaining position we would observe in the field; that is, the theory may explain behaviour in the lab better as the lab environment becomes more like the outside world. Even in our Earned treatment, however, observed responsiveness continues to be less than predicted, suggesting that replacing assigned bargaining position with earned bargaining position may not fully reconcile the theory with observed bargaining behaviour.

Our results may also be of interest to other experimenters considering using real–effort tasks in their procedures. Consistent with much of the previous literature, we find that simply including a real–effort task has a relatively minor effect on behaviour. The task "works" best when performance is tied to a strategically relevant component of the setting – like disagreement payoffs in the current experiment.

We close with a discussion of some of the limitations of this experiment. Perhaps the most important one arises from the use of the task in the Earned treatment to earn subjects' disagreement payoffs, rather than favoured status per se. While a subject's disagreement payoff in a given round is indeed determined by task performance, favoured status itself is highly correlated with being advantaged in the task (i.e., having an easier version of the task than one's co–bargainer), which is randomly assigned. This was done in order to increase bargaining asymmetry. Since subjects were not informed about how task difficulty varied, it is unclear to what extent subjects perceived their bargaining position as being randomly assigned rather than earned. To the extent that they did, this should have made behaviour in the Earned treatment more similar to that in the Assigned treatment. Relatedly, if subjects viewed the task itself as contrived (since they expend labour but produce nothing of value), they may not have treated performance in the task as reflective of any form of quality; this would likely also have reduced the difference between Earned and Assigned treatments (and perhaps between Assigned and Notask). Also, subjects in both Earned and Assigned treatments were told both own and co–bargainer's task performance before bargaining began. Since performance is equivalent in the Earned treatment to disagreement payoff, it was necessary to give subjects performance information in the Assigned treatment as well to avoid a confound, but a consequence of this is that subjects may have used the relative performances to establish moral claims to larger or smaller shares of the cake. This could add noise in the Assigned treatment, though it is unclear how it would change the treatment effect between Earned and Assigned. Another unclear effect is that of our use of between–subjects variation in task difficulty in the Earned treatment but not the Assigned treatment. Finally, to the extent that behaviour differs between the Assigned and Notask treatments, it might be due to our use of a hurdle in the former (subjects had to encode 12 words to reach the bargaining stage). This was done in order to ensure subjects took the task seriously in the Assigned treatment (to avoid a confound with the Earned treatment), but also means that subjects reaching the bargaining stage had clearly indicated their willingness to bargain, while in the Notask treatment (and for that matter, the Earned treatment) subjects were required to bargain. In a recent paper, Smith and Wilson (2019) report ultimatum–game behaviour closer to theoretical predictions when subjects bargain voluntarily; such an effect would move behaviour in our Assigned treatment away from that of the Notask treatment and toward that of the Earned treatment. We also hasten to acknowledge that this experiment involved one particular real–effort task and two closely–related bargaining paradigms, and generalisation to other tasks and subsequent settings should be done with caution. We encourage future research to improve understanding of how status earned via real–effort tasks affects subsequent behaviour, how the effects depend on features of the tasks (the cognitive, physical, or time cost of effort; whether effort influences strategic factors like bargaining position, non–strategic factors like cake size, or both; whether effort is productive; etc.), and how their impact varies with the post–task setting.

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A Additional results

A.1 Alternative predictions from a simple behavioural model

As an alternative to standard bargaining theory, we present a simple behavioural model. Our model departs from standard theory in that bargainers are assumed not only to care about their money payment, but also to dislike agreements they view as "unfair" to themselves. Using the notation from the previous section, and continuing to take the cake size as unity, Player i's utility function is given by

$$U_i(x_i) = \begin{cases} x_i - \alpha(x_i - 0.5)^2 & \text{if agreement and } x_i < 0.5 \\ x_i & \text{if agreement and } x_i \geq 0.5 \\ d_i & \text{if disagreement} \end{cases} \tag{6}$$

where d_i is her disagreement payoff and $\alpha \geq 0$ is an index of aversion to agreements involving disadvantageous inequity.[34] These preferences are inspired by inequity–aversion models (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002), though these models also specify an aversion to *advantageous inequity*. (We assume away the latter in order to keep the model as tractable as possible.) Note that the utility function in (6) is increasing and weakly concave everywhere, continuous and differentiable at one–half, and reduces to standard preferences when $\alpha = 0$ (see Figure 8 for examples). We assume that α is the same for all bargainers within a

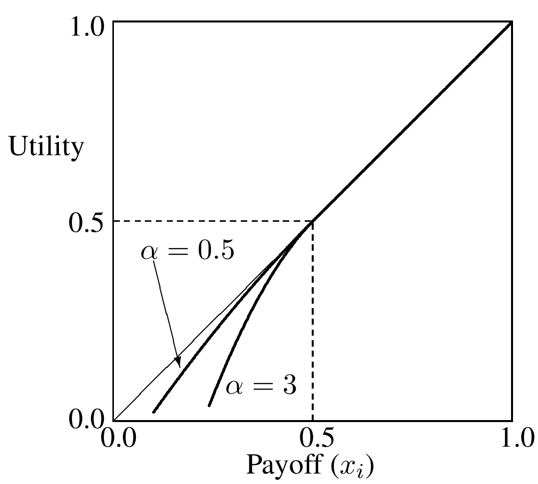


Figure 8: Sample utility functions U ($\alpha = 0.5$ and $\alpha = 3$)

particular bargaining setting, and that this is common knowledge, though we will allow α to change across bargain-

[34]This specification makes some assumptions about the nature of fairness judgements that are worth noting. First, in the event of agreement, bargainers compare their payoffs to one–half of the cake rather than to the opponent's payoff; an alternative specification would use the latter. In our equilibrium analysis, these specifications are equivalent, but this will not be true in general when behaviour is out of equilibrium: especially in the Nash demand game, where inefficient agreements are often observed. Second, in the event of disagreement, players care about the absolute disagreement payoff rather than comparing it to the opponent's disagreement payoff (or to one–half). We do not view any of these alternatives as manifestly superior to the others. We chose our specification partly because it turns out to be more tractable than the obvious alternatives. Also, regarding the assumption about disagreements, we had originally conjectured that agreeing to an inequitable outcome might lead to a more intense distaste than simply having an inequitable outcome imposed – analogous to the difference between "sins of commission" and "sins of omission".

ing settings. Thus α is a characteristic of the bargaining setting – construed broadly to include the way in which bargaining parameters are determined – and not of the bargainers themselves.

Although this change to preferences is fairly minor, it entails an important change to the analysis: in general, the various solution techniques of bargaining theory no longer coincide. We proceed using the Nash bargaining solution, due to its being the most widely used of the axiomatic solution concepts, as well as its tractability. By construction, the Nash solution of the UBG will continue to coincide with the risk–dominant Nash equilibrium of the NDG.

Bargaining outcomes in the behavioural model

Recall that $d_f \geq d_u$ and $d_f + d_u < 1$. Clearly, when $d_f = d_u$, we will have $x_f = x_u = 0.5$; symmetric bargaining will lead to an equal split as before. So assume $d_f > d_u$. Also, suppose at least one feasible split of the cake Pareto dominates disagreement, so that bargaining is potentially productive. This is guaranteed when $d_f < 0.5$ – that is, there is no dominant bargaining power – since in that case, a 50–50 split will Pareto dominate disagreement. When $d_f \geq 0.5$, the split $(d_f + \epsilon, 1 - d_f - \epsilon)$ will Pareto dominate disagreement for some sufficiently small ϵ as long as α is low enough: specifically, if and only if

$$\alpha < \frac{1 - d_f - d_u}{(d_f - 0.5)^2}. \tag{7}$$

If (7) does not hold, then no agreement will Pareto dominate disagreement.

Our first result confirms that two important features of the Nash bargaining solution are preserved under our generalisation of preferences: efficiency and monotonicity, both with respect to monetary payoffs.

Lemma 1 *Suppose $d_f > d_u$ and that at least one feasible split of the cake Pareto dominates disagreement. Then in the Nash solution, the outcome (x_f, x_u) will maximise the Nash product*

$$(x_f - d_f)[x_u - d_u - \alpha(x_u - 0.5)^2]$$

with $x_f + x_u = 1$ and $x_f \geq x_u$.

Proof: First, it is clear that $x_f + x_u = 1$, since $x_f + x_u > 1$ is not feasible and $x_f + x_u < 1$ implies that an additional ϵ could be given to both bargainers, increasing their monetary payoffs and with disadvantageous inequity either decreasing or remaining unchanged.

Second, suppose that $x_f < x_u$. Since $x_f + x_u = 1$, we must have $x_f < 0.5 < x_u$. Then the Nash product is $(x_u - d_u)[x_f - d_f - \alpha(x_f - 0.5)^2]$. Since $x_f + x_u = 1$, maximisation yields the first–order condition

$$\begin{aligned} 0 &= -[x_f - d_f - \alpha(x_f - 0.5)^2] + (x_u - d_u)[1 - 2\alpha(x_f - 0.5)] \\ &= 1 - 2x_f + d_f - d_u + \alpha(x_f - 0.5)[(x_f - 0.5) - (1 - x_f - d_u)], \end{aligned}$$

which implies

$$x_f = \frac{1 + d_f - d_u}{2} + \frac{\alpha}{2}(x_f - 0.5)[(x_f - 0.5) - (1 - x_f - d_u)]. \tag{8}$$

But $\frac{1+d_f-d_u}{2} > 0.5$ because $d_f > d_u$, $x_f < 0.5$ because $x_f < x_u$ by assumption, and $x_f + d_u \leq 1$ since $d_u \leq x_u$. So, the second term of (8) is positive, and thus $x_f > 0.5$. This last inequality contradicts our assumption that $x_f < x_u$, meaning that we must have $x_f \geq x_u$.

Finally, since $x_f \geq x_u$, the relevant Nash product is

$$(x_f - d_f)[x_u - d_u - \alpha(x_u - 0.5)^2],$$

with $x_f + x_u = 1$. $\square$

Given Lemma 1, the first–order condition derived from the Nash product can be written implicitly as

$$(x_f - 0.5)[2 + \alpha(3x_f - 2d_f - 0.5)] = d_f - d_u. \tag{9}$$

It is straightforward to show that $\frac{\partial x_f}{\partial d_f} > 0 > \frac{\partial^2 x_f}{\partial d_f^2}$ – that is, x_f is increasing and concave in d_f – as long as either $d_f \leq 0.5$ or α is not large (see footnote 35 below for details). Note that if $\alpha = 0$, we have $x_f = (1 + d_f - d_u)/2$ as in Section 3. Note also that for any α, if $d_f = d_u$ then $x_f = x_u = 0.5$, while for $\alpha > 0$ and $d_f > d_u$, we have

$$\frac{1}{2}(1 + d_u - d_f) < x_u < \frac{1}{2} < x_f < \frac{1}{2}(1 + d_f - d_u).$$

This first–order condition leads us quickly to our main theoretical result.

Proposition 1 *Suppose $d_f > d_u$ and that x_f is given by (9), and define the combined (disagreement payoff) effect as in Section 3:*

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

Then

(i) If $\alpha > 0$, then $CDPE < 1$.

(ii) If $d_f < 0.5$, then $\frac{\partial CDPE}{\partial \alpha} < 0$.

Proof: For (i), take the total derivative of (9) with respect to x_f, d_f and d_u:

$$dd_f - dd_u = dx_f[2 + \alpha(3x_f - 2d_f - 0.5)] + (x_f - 0.5)\alpha(3dx_f - 2dd_f),$$

which after collecting terms yields

$$dx_f[2 + 2\alpha(3x_f - d_f - 1)] + [1 + 2\alpha(x_f - 0.5)]dd_f - dd_u,$$

so that

$$\frac{\partial x_f}{\partial d_f} = \frac{1 + 2\alpha(x_f - 0.5)}{2 + 2\alpha(3x_f - d_f - 1)}$$

and

$$\frac{\partial x_f}{\partial d_u} = -\frac{1}{2 + 2\alpha(3x_f - d_f - 1)}.$$

Since $x_f \geq 0.5$ and $x_f \geq d_f$, and therefore $3x_f \geq d_f + 1$, we have $\frac{\partial x_f}{\partial d_u} < 0 < \frac{\partial x_f}{\partial d_f}$, so that

$$\begin{aligned} C.E. &= \left|\frac{\partial x_f}{\partial d_f}\right| + \left|\frac{\partial x_f}{\partial d_u}\right| \\ &= \frac{\partial x_f}{\partial d_f} - \frac{\partial x_f}{\partial d_u} \\ &= \frac{1 + \alpha(x_f - 0.5)}{1 + \alpha(3x_f - d_f - 1)} \\ &= 1 - \frac{\alpha(2x_f - d_f - 0.5)}{1 + \alpha(3x_f - d_f - 1)}. \end{aligned}$$

Since the second term of this last expression is positive for any $\alpha > 0$, we have that $CDPE < 1$.

For (ii), first solve (9) explicitly for x_f:

$$x_f = \frac{-2 + 2\alpha(1 + d_f) + [4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u]^{1/2}}{6\alpha}.$$

Then

$$\frac{\partial x_f}{\partial d_f} = \frac{1 - \alpha(1 - 2d_f) + [4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u)]^{1/2}}{3[4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u)]^{1/2}}$$

and

$$\frac{\partial x_f}{\partial d_u} = -\frac{1}{[4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u)]^{1/2}},$$

so that

$$CDPE = \frac{4 - \alpha(1 - 2d_f) + [4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u)]^{1/2}}{3[4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u)]^{1/2}}$$

and

$$\frac{\partial CDPE}{\partial \alpha} = -\frac{4(1 - 2d_u) + \alpha(1 - 2d_f)(1 - d_f - d_u)}{[4 + \alpha^2(1 - 2d_f)^2 + 4\alpha(1 + d_f - 3d_u)]^{3/2}}. \tag{10}$$

Since $d_u < d_f$ and therefore $d_u < 0.5$, so that $3d_u < 1 + d_f$, the denominator of the right–hand side of (10) is positive. Since $d_f + d_u < 1$, the numerator is also positive if $d_f < 0.5$, in which case $\frac{\partial CDPE}{\partial \alpha} < 0$. □

Part (i) of the proposition means that as long as bargainers have distaste for unfavourable agreements, they will under–exploit their bargaining position. As noted in Section 2, this is the result typically observed in the experimental bargaining literature.

Part (ii) means that if neither player has dominant bargaining power, then exploitation of bargaining position will increase as fairness tastes weaken. However, as the proof in the appendix makes clear, this no–dominant–bargaining–power condition is sufficient but not necessary for the consequent comparative static to hold; even if one of the players does have dominant bargaining power, disagreement–payoff responsiveness increases as fairness tastes weaken as long as either this disagreement payoff or α is not too high.[35] Thus, decreasing α will increase disagreement–payoff responsiveness when there is no dominant bargaining power, but is likely also to do so when dominant bargaining power is present.

Behaviour in the task

To complete the analysis, we present a sketch of behaviour in the pre–bargaining stage of our Earned treatment, where players earn their bargaining position. While this stage is tangential to our main research questions involving bargaining behaviour, our experimental design assumes that when subjects "produce" their disagreement payoffs through a real–effort task, they treat these as being earned. This would seem to imply that other things equal, higher disagreement payoffs are associated with more effort, greater ability, or both.

[35] Indeed, a grid search finds no combinations of α and the disagreement outcome (d_f, d_u) that yields $\partial C.E./\partial\alpha > 0$ for any α less than 16, and if $d_f \leq 0.6$ – as is typically true in the experiment even when one side does have dominant bargaining power – there is no combination yielding $\partial C.E./\partial\alpha > 0$ for any α less than 25. To get a sense of how extreme such values of α are, note that in the ultimatum game, with a cake size of 1 and disagreement payoffs of 0 for both players, a responder with $\alpha = 16$ would reject an offer of 35 percent of the cake, and a responder with $\alpha = 25$ would reject an offer of 37 percent of the cake; rejections of offers this substantial are rare in experienced subjects. Moreover, cases with $\partial C.E./\partial\alpha > 0$ are often knife–edge in the sense that for only moderately higher α, there no longer exists an individually rational split of the cake. For example, suppose $d = (0.62, 0)$. When $\alpha = 21$, we have the usual $\partial C.E./\partial\alpha < 0$. If α is between 22 and 26, we have $\partial C.E./\partial\alpha > 0$, but if α is 27 or higher, no agreement Pareto dominates the disagreement outcome.

Suppose player i's disagreement payoff is given by $d_i = f(e_i, a_i)$, where $e_i \geq 0$ is effort (chosen by the bargainer) and a_i is exogenous ability.[36] The function f has the typical features of production functions: $f(0, a_i) = 0$, $f_e > 0 > f_{ee}$, $f_a > 0$ if $e_i > 0$, and $f_{ea} > 0$. We assume f and all of its relevant derivatives are continuous, and that $f(e_i, a_i) < 1$ (i.e., there is insufficient time to earn a disagreement payoff equal to the entire cake, no matter the ability and effort). Effort has a fixed per–unit cost $c > 0$, and its benefit is solely through its impact on the bargaining outcome (there is no intrinsic motivation).

We can then write the player's objective function as $E[U_i(x_i(d_i(e_i, a_i), d_{-i}))] - ce_i$, where U_i is as given by (6) and the expectation is over the player's subjective distribution of d_{-i}. From (6), U_i is strictly increasing and weakly concave in x_i, and from (9), x_i is strictly increasing and strictly concave in d_i under the conditions for which the conclusion of part (ii) of Proposition 1 holds.

Proposition 2 *Suppose $e_i^* > 0$ maximises $E[U_i(x_i(d_i(e_i, a_i), d_{-i}))] - ce_i$, and let $d_i^* = f(e_i^*, a_i)$. Then $\frac{\partial d_i^*}{\partial a_i} > 0$.*

Proof: Suppose $e_i^* > 0$ maximises $E[U_i(x_i(d_i(e_i, a_i), d_{-i}))] - ce_i$. Then

$$\begin{aligned} c &= \frac{\partial}{\partial e_i} E[U_i(x_i(d_i(e_i, a_i), d_{-i}))] \\ &= \frac{\partial E[U_i]}{\partial d_i} \cdot \frac{\partial d_i}{\partial e_i} \\ &= f_e \cdot \frac{\partial E[U_i]}{\partial d_i}. \end{aligned}$$

Since $f_{ea} > 0$, increasing a_i means increasing f_e, so we must have $\frac{\partial E[U_i]}{\partial d_i}$ decreasing when a_i increases.

Now,

$$\frac{\partial U_i}{\partial d_i} = \frac{\partial U_i}{\partial x_i} \cdot \frac{\partial x_i}{\partial d_i},$$

so that

$$\begin{aligned} \frac{\partial^2 U_i}{\partial d_i^2} &= \frac{\partial}{\partial d_i}\left[\frac{\partial U_i}{\partial x_i} \cdot \frac{\partial x_i}{\partial d_i}\right] \\ &= \frac{\partial U_i}{\partial x_i} \cdot \frac{\partial^2 x_i}{\partial d_i^2} + \frac{\partial^2 U_i}{\partial x_i^2} \cdot \left[\frac{\partial x_i}{\partial d_i}\right]^2, \end{aligned}$$

and since $\frac{\partial U_i}{\partial x_i}$ and $\left[\frac{\partial x_i}{\partial d_i}\right]^2$ are strictly positive, $\frac{\partial^2 x_i}{\partial d_i^2}$ is non–positive, and $\frac{\partial^2 U_i}{\partial x_i^2}$ is strictly negative, we have $\frac{\partial^2 U_i}{\partial d_i^2} < 0$. Finally, if $G(d_{-i})$ is the cumulative distribution function of d_{-i},

$$\begin{aligned} \frac{\partial^2 E[U_i]}{\partial d_i^2} &= \frac{\partial^2}{\partial d_i^2} \int_0^1 U_i(d_i, d_{-i}) dG(d_{-i}) \\ &= \int_0^1 \frac{\partial U_i^2}{\partial d_i^2} dG(d_{-i}) \\ &= < 0. \end{aligned}$$

Since $\frac{\partial^2 E[U_i]}{\partial d_i^2} < 0$, it must be that $\frac{\partial E[U_i]}{\partial d_i}$ decreases when d_i increases. So, d_i^* increases when a_i increases. □

Proposition 2 implies that other things equal, a player will perform better on the task and earn a higher disagreement payoff the higher her ability is. So, if all players have the same beliefs about the distribution of d_{-i} in

[36] In the experiment, a_i contains a "natural" component and an "experimenter–assigned" component. Treating these as separate parameters complicates the analysis but does not change the implications substantially.

the population, those with higher ability will earn higher disagreement payoffs. Thus, players are justified in reasoning backwards: viewing disagreement payoffs as reflecting ability and hence "earned". Also, beliefs about the distribution of d_{-i} are isomorphic to beliefs about the distribution of abilities in the population.[37]

Formulation of Hypotheses 2 and 3

In our behavioural model, the parameter α measures the intensity of (all) bargainers' aversion to agreements yielding payoffs less than one–half of the cake.

Assumption 1 *Earning any aspect of one's status in the bargaining setting will reduce α, relative to when that same aspect is assigned.*

That is, earning one's status weakens the per–se attractiveness of equal–split agreements, and makes bargainers' preferences more like those of standard money–payment maximisation.

The real–effort task is involved in our experimental design in two distinct ways. First, our Earned and Assigned treatments differ in whether disagreement payoffs for the two bargainers were determined based on task performance (Earned) or randomly assigned (Assigned). Applying Assumption 1 and Proposition 1 above, we have our Hypothesis 2:

Responsiveness to changes in the disagreement outcome will be higher *when disagreement payoffs are earned than when they are assigned.*

Second, our Assigned and Notask treatments differ in whether the opportunity to bargain is based on task performance (Assigned) or given to everyone (Notask). Applying Assumption 1 and Proposition 1 again, we have our Hypothesis 3:

Responsiveness to changes in the disagreement outcome will be *higher* when the opportunity to bargain is earned than when it is assigned.

We note finally that these hypotheses rely on a further assumption: either there is no dominant bargaining power (i.e., d_f is not more than one–half) in the setting, or if there is, its effect is not strong (essentially, either d_f is close to one–half, or bargainers' aversion to disadvantageous inequity is not extreme). As we can see in the results section, a substantial majority – though not all – of bargaining pairs will not involve dominant bargaining power. Also, while we do not have direct information about our subjects' preferences for equality, we note that our subject pool (mainly university undergraduates) is similar to the kind that has been used in many other experiments, and ultimatum–game experiments with these subject pools do not find the high frequency of rejections of substantial positive offers that an extreme aversion to disadvantageous inequity would imply (see Footnote 35 for some details).

[37] Note that we have not assumed beliefs about the distribution of disagreement payoffs (and hence abilities) are correct – just that they are the same across individuals. The final step in a full analysis of task performance would be specifying a distribution of abilities μ such that given $f(\cdot)$, optimal behaviour given beliefs μ leads to μ being the resulting distribution. Such an analysis is beyond the scope of this paper, given our focus on bargaining outcomes.

A.2 Additional session information

Table 6: Session information

Session	Cell	Matching–group size(s)	Session	Cell	Matching–group size(s)
1	UE	14	19	NA	8, 8
2	NE	14	20	UA	8, 8
3	NE	8, 10	21	NE	8, 8
4	UE	8, 8	22	UE	8, 8
5	NA	8, 8	23	NN	14
6	UA	14	24	UN	14
7	UA	8, 8	25	UE	10, 12
8	NN	8, 8	26	NA	10, 12
9	NA	14	27	UN	10, 12
10	UN	10	28	NN	10, 10
11	UN	8, 10	29	UA	10, 10
12	NN	8, 8	30	NE	8, 10
13	UE	8, 8	31	UA	10, 10
14	NE	8, 8	32	NE	8, 8
15	NA	8, 10	33	NA	10, 12
16	UA	8, 8	34	NN	8, 10
17	UN	10, 10	35	UE	8, 10
18	NN	8, 10	36	UN	10, 10

A.3 Additional regression results

Table 7 and Figure 9 correspond to Table 5 and Figure 6 in the main text, but using the agreements–only data rather than the all–demands data. These results are largely similar, so are left out of the main text for reasons of brevity and decluttering.

Table 7: Marginal effects (SEs in parentheses) from regressions like Table 5, but using agreements–only data

	[1a]	[2a]	[3a]	[4a]
Own disag. payoff	0.315^{***}	0.344^{***}	0.257^{***}	0.266^{***}
	(0.012)	(0.018)	(0.014)	(0.019)
...Earned treatment	0.428	0.468^{***}	0.333^{***}	0.354^{***}
	(0.022)	(0.029)	(0.029)	(0.034)
...Assigned treatment	0.249^{***}	0.266^{***}	0.215^{***}	0.215^{***}
	(0.020)	(0.035)	(0.023)	(0.030)
...Notask treatment	0.271^{***}	0.301^{***}	0.222^{***}	0.230^{***}
	(0.020)	(0.028)	(0.022)	(0.032)
Opp. disag. payoff	-0.292^{**}	-0.320^{***}	-0.237^{**}	-0.225^{**}
	(0.011)	(0.019)	(0.014)	(0.024)
...Earned treatment	-0.310^{***}	-0.348^{***}	-0.261^{***}	-0.234^{***}
	(0.019)	(0.039)	(0.027)	(0.050)
...Assigned treatment	-0.299^{***}	-0.313^{***}	-0.255^{***}	-0.248^{***}
	(0.020)	(0.032)	(0.023)	(0.032)
...Notask treatment	-0.268^{***}	-0.301^{***}	-0.200^{***}	-0.197^{***}
	(0.019)	(0.026)	(0.022)	(0.035)
	panel Tobit with REs	panel linear with FEs and clustering	panel Tobit with REs	panel linear with FEs and clustering
Treatment dummies?	Yes	Yes (in interactions)	Yes	Yes (in interactions)
Additional variables?	No	No	Yes (UBG, dominant barg. power, round number, own and opp. letters encoded)	
Interaction terms?	Yes (see text for details)			
N	5082	5082	5082	5082
$\|ln(L)\|$	5937.05	6924.74	6230.23	7089.60

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

Table 8 shows some results from regressions similar to those reported in Table 5, but for favoured and unfavoured players individually (top and bottom blocks, respectively) instead of pooled. For each of the 8 models we had estimated, and for each of favoured and unfavoured players, the table shows point estimates and 95–percent confidence intervals for the combined disagreement–payoff effect.

In all of the models, total responsiveness is higher in the Earned treatment than in the Assigned treatment for both favoured and unfavoured subjects. The difference is significant at the 5–percent level or better for 6 of the 8 comparisons of favoured players (and at the 10–percent level for all of them), but only for one (Model 2) for unfavoured players (though additionally Model 5 yields significance at the 10–percent level). By contrast, while responsiveness is usually higher in the Assigned treatment compared to the Notask treatment, these differences are

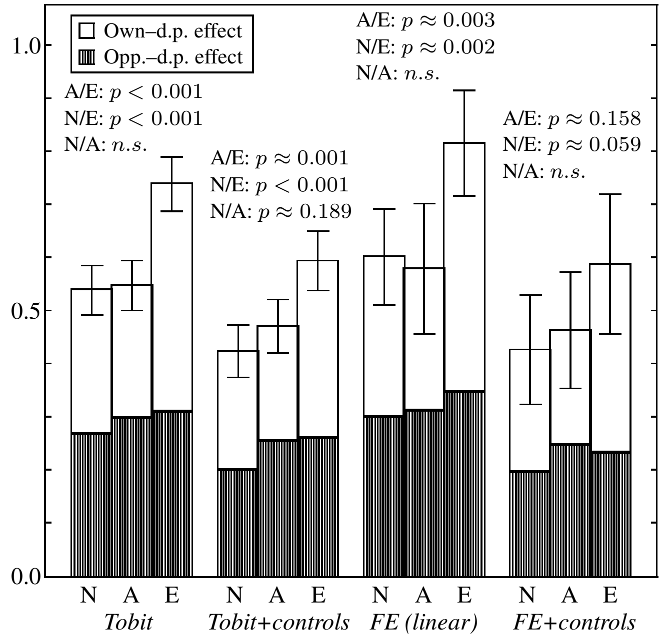


Figure 9: Estimated responsiveness to bargaining position by treatment: like Figure 6 but with agreements–only data

Table 8: Combined disagreement–payoff effects (point estimates and 95–percent CIs) from Models 1–8, but estimated separately for favoured and unfavoured subjects

	Restricted models				Unrestricted models			
	All demands Tobit/REs	Agreements Tobit/REs	All demands linear/FEs	Agreements linear/FEs	All demands Tobit/REs	Agreements Tobit/REs	All demands linear/FEs	Agreements linear/FEs
Favoured subjects								
Earned	0.802	0.675	0.852	0.671	0.760	0.594	0.816	0.592
	(0.680,0.923)	(0.570,0.780)	(0.697,1.007)	(0.527,0.815)	(0.599,0.920)	(0.464,0.724)	(0.612,1.019)	(0.453,0.731)
Assigned	0.600	0.484	0.599	0.475	0.542	0.408	0.518	0.391
	(0.491,0.710)	(0.393,0.575)	(0.463,0.735)	(0.297,0.652)	(0.407,0.676)	(0.301,0.515)	(0.396,0.640)	(0.231,0.552)
Notask	0.633	0.480	0.651	0.488	0.498	0.373	0.497	0.377
	(0.522,0.744)	(0.387,0.573)	(0.530,0.751)	(0.394,0.582)	(0.365,0.631)	(0.266,0.480)	(0.403,0.592)	(0.283,0.470)
Unfavoured subjects								
Earned	0.614	0.836	0.630	0.808	0.571	0.674	0.567	0.637
	(0.480,0.748)	(0.704,0.968)	(0.450,0.810)	(0.616,0.999)	(0.406,0.736)	(0.515,0.833)	(0.403,0.732)	(0.459,0.815)
Assigned	0.515	0.655	0.523	0.652	0.383	0.568	0.437	0.579
	(0.411,0.619)	(0.551,0.758)	(0.393,0.652)	(0.511,0.792)	(0.258,0.509)	(0.446,0.694)	(0.295,0.579)	(0.422,0.735)
Notask	0.474	0.579	0.482	0.589	0.357	0.475	0.362	0.456
	(0.369,0.579)	(0.474,0.683)	(0.380,0.583)	(0.456,0.722)	(0.231,0.482)	(0.353,0.597)	(0.267,0.458)	(0.336,0.575)

never significant. Thus, our main results largely hold true for favoured and unfavoured players individually, though they are arguably stronger for the former than the latter.

A.4 Unstructured bargaining in our UBG cells

In this section, we present additional results from the UBG cells. Experiments with unstructured bargaining, like this one, yield a rich set of data involving the bargaining process itself, and while regularities from these data are only tangentially relevant to our main research questions, some of them are interesting in their own right.

We begin with some descriptive statistics, in Table 9. Initial demands are simply the own–payoff component of

Table 9: UBG individual–level means and standard deviations, rounds 3–11, asymmetric bargaining

Bargainer:	Favoured			Unfavoured		
Treatment:	Notask	Assigned	Earned	Notask	Assigned	Earned
Initial demand (frac. of cake)	0.637	0.636	0.655	0.518	0.505	0.505
	(0.120)	(0.120)	(0.097)	(0.111)	(0.090)	(0.107)
Initial demand (frac. of surplus)	0.570	0.582	0.629	0.908	0.848	0.872
	(0.392)	(0.421)	(0.252)	(0.534)	(0.292)	(0.436)
Time remaining, initial demand	110.09	108.19	110.15	110.45	108.59	109.09
	(12.00)	(14.34)	(9.94)	(11.30)	(12.81)	(11.95)
Number of proposals	3.35	4.31	4.30	3.70	4.81	3.65
	(3.53)	(7.81)	(4.72)	(4.42)	(7.07)	(3.36)
Number of proposals, given at least one	3.72	4.75	4.50	4.01	5.32	3.77
	(3.53)	(8.07)	(4.73)	(4.47)	(7.25)	(3.35)
Total concession (frac. of cake)	0.055	0.054	0.047	0.055	0.049	0.049
	(0.073)	(0.093)	(0.060)	(0.090)	(0.078)	(0.070)
Accepted opponent proposal	0.425	0.422	0.364	0.431	0.467	0.471
Time of accepted opponent proposal	49.08	47.95	38.96	47.92	41.70	38.31
	(40.99)	(44.36)	(41.40)	(44.49)	(45.66)	(40.88)

a bargainer's first proposal, in those UBG rounds where the bargainer made at least one proposal; these are shown both as fractions of the cake and as fractions of the available surplus (the amount remaining after subtracting off both disagreement payoffs). The time remaining when an initial demand is made is fairly self–explanatory, but since there were 120 seconds for bargaining in rounds 3–11 and 130 seconds in rounds 1–2, we drop the first two rounds from the table to ensure our data are comparable. The number of proposals is also self–explanatory, but we include this both for all bargainers and for those who made at least one proposal. The total concession is the difference between the initial offer (the opponent–payoff component of the first proposal) and the highest offer during the bargaining stage. "Accepted opponent proposal" is a binary variable taking a value of one if the bargainer accepted a proposal by the opponent, and the time (remaining) of the acceptance is also shown.

Initial demands look similar to agreements, and to demands in the NDG, in that favoured players demand more than unfavoured players, but not to the extent predicted by bargaining theory (which would imply equal demands as fractions of the surplus). Favoured players' demands are slightly higher in the Earned treatment than the other two treatments. There do not appear to be any systematic differences across treatments or player types in when initial demands are made, or in total concessions, while there are more proposals in the Assigned treatment than in the other two. Unfavoured bargainers are more likely than favoured bargainers to accept the opponent's proposal, and agreements tend to occur later in the Earned treatment.[38]

Figure 10 shows the distribution of agreement times (pooled over all three treatments), conditional on an agreement being reached. Consistent with previous results from the unstructured–bargaining literature (e.g., Roth et al. 1988, Karagözoğlu and Kocher 2019), we see a substantial fraction of last–moment agreements, as evidenced by the steepness of the cdf on the left side of the figure. Interestingly, there is a smaller spike in very early agreements (right

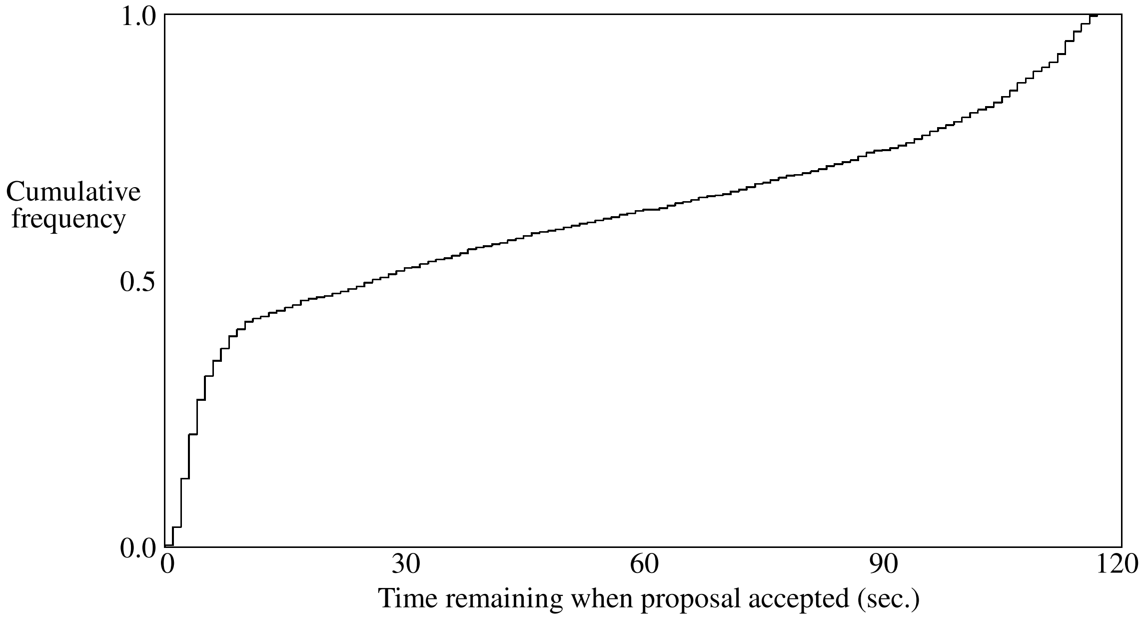


Figure 10: Time remaining when agreement reached, rounds 3–11 (cumulative distribution function)

side of the figure). It is unclear why this would occur, but one conjecture is that bargaining is unpleasant to some subjects, and when they expect to split the surplus nearly evenly, they do so quickly. Some suggestive evidence for this conjecture is that the difference in payoffs between bargainers averages 6.4 percent of the cake for agreements reached in the first 30 seconds, 14.1 percent of the cake for agreements reached after this but before the last 15 seconds, and 17.3 percent of the cake for agreements reached in the last 15 seconds, meaning that early agreements are closer to equal splits than later agreements.

Table 10 shows some correlations in bargaining behaviour after the initial proposals are made. The sum of initial offers is a measure of how much total concession is needed between the two bargainers in order to reach agreement. The total number of proposals (between the two bargainers) and agreement (binary variable) are fairly self–explanatory. The total concession for a bargainer is the difference between the initial offer and the highest offer, and the sum of total concessions is these summed for the two bargainers. The table shows significant (Bonferroni corrected) correlations between a lower sum of initial offers, more proposals during the bargaining process, greater total concessions, and a lower frequency of agreement.

Finally, we show results from panel regressions, similar to those in the main text (models 1–4) but with initial demand as the dependent variable, and using only the UBG data. As in the main text, we use two Tobit models with random effects and two fixed–effects linear models with clustering, and for each pair, one has a reduced set of right–hand–side variables and the other has a larger set. The results are shown in Table 11.

[38] Given the large number of comparisons we examine here, and the lack of ex–ante hypotheses, we do not discuss statistical significance. Rather, any differences we observe should be viewed as suggestive for future research.

Table 10: Pairwise correlations (rounds 3–11, asymmetric bargaining)

	Total number of proposals	Sum of total concessions (frac. of cake)	Agreement
Sum of initial offers (frac. of cake)	-0.260^{***}	-0.575^{***}	$+0.173^{***}$
Total number of proposals		$+0.344^{***}$	-0.069^{*}
Sum of total concessions (frac. of cake)			-0.068^{*}

* (**,***): *Marginal effect significantly different from 0.5 at the 10% (5%, 1%) level (Bonferroni corrected p–values).*

The main results from these regressions correspond roughly to their counterparts in the main text. First, initial demands are under–responsive to changes in bargaining position, as was true for final demands and agreed demands. Second, this responsiveness is higher in the Earned treatment than in the Assigned treatment, and the difference is significant in two of the four regressions. Third, there are no significant differences in responsiveness between the Assigned and Notask treatments. These results suggest that the effect of earned bargaining power on bargaining behaviour is expressed in initial proposals rather than the pattern of subsequent concessions. Since we did not have an ex–ante hypothesis regarding this, we treat it as a conjecture worthy of examination in future research.

Table 11: Factors affecting initial UBG demands (marginal effects from regressions, standard errors in parentheses)

	[9]	[10]	[11]	[12]
Own disag. payoff	0.254^{***}	0.255^{***}	0.215^{***}	0.220^{***}
	(0.019)	(0.026)	(0.025)	(0.028)
...Earned treatment	0.278^{***}	0.288^{***}	0.239^{***}	0.277^{***}
	(0.037)	(0.049)	(0.051)	(0.045)
...Assigned treatment	0.252^{***}	0.241^{***}	0.232^{***}	0.207^{***}
	(0.033)	(0.044)	(0.041)	(0.041)
...Notask treatment	0.230^{***}	0.235^{***}	0.179^{***}	0.172^{***}
	(0.031)	(0.040)	(0.039)	(0.053)
Opp. disag. payoff	-0.307^{***}	-0.309^{***}	-0.320^{***}	-0.314^{***}
	(0.018)	(0.023)	(0.024)	(0.033)
...Earned treatment	-0.361^{***}	-0.361^{***}	-0.406^{***}	–0.404
	(0.030)	(0.042)	(0.039)	(0.063)
...Assigned treatment	-0.290^{***}	-0.289^{***}	-0.243^{***}	-0.225^{***}
	(0.032)	(0.045)	(0.041)	(0.063)
...Notask treatment	-0.267^{***}	-0.275^{***}	-0.300^{***}	-0.305^{***}
	(0.031)	(0.031)	(0.046)	(0.056)
Combined disagreement–payoff effect...				
Notask versus Assigned	+0.045	+0.019	–0.004	–0.045
	(0.058)	(0.101)	(0.067)	(0.122)
Assigned versus Earned	+0.096	+0.119	$+0.170^{**}$	$+0.248^{**}$
	(0.062)	(0.087)	(0.074)	(0.116)
	panel Tobit with REs	panel linear with FEs and clustering	panel Tobit with REs	panel linear with FEs and clustering
Treatment dummies?	Yes	Yes (in interactions)	Yes	Yes (in interactions)
Additional variables?	No	No	Yes (dominant barg. power, round number, own and opp. letters encoded)	
Interaction terms?	Yes (see text for details)			
N	3115	3115	3115	3115
$\|ln(L)\|$	2883.90	3489.00	2937.92	3509.47

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

B Sample instructions

Below are instructions from 3 of the 6 experimental cells; the others are similar and available from the author upon request. Horizontal lines represent where page breaks occurred in the actual instructions. The first page of each set of instructions is labelled with the cell in square brackets; these notations were not in the actual instructions.

[Notask/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 each other during the experiment, and please put away your mobile phones and other devices.

The main part of this experiment consists of ***eleven*** rounds. In each round, ***you will have the opportunity to bargain over $20 with another randomly chosen person.*** The way you bargain is by each making ***simultaneous*** claims for shares of the $20.
- If your claim and the other person's claim add up to ***$20 or less***, then you have reached an ***agreement***. You receive your claim, and the other person receives his/her claim.
- If the claims add up to ***more than $20***, then you have reached ***disagreement***. You receive your ***outside option*** and the other person receives his/her outside option. Your outside option is between 0 and $20 inclusive, is chosen randomly by the computer and may be different from the other person's outside option. You and the other person are informed of ***both*** outside options ***before*** choosing your claims.

Organisation of the experiment: Today's session has two parts.
(1) You play the eleven bargaining rounds. Each round proceeds as follows:
  (a) The computer pairs you with another person, and your screen displays both outside options.
  (b) You choose a claim for your share of the $20. The other person chooses a claim for his/her share. ***Your claim can be any multiple of 0.05, between 0.00 and 20.00 inclusive.*** (Do not type the dollar sign.) 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 your outside option and that of the person paired with you, both of your claims, and both of your profits.
(2) After the 11^{th} round, you will be asked to answer some demographic questions.

Payments: The amount you are paid will depend on the results of the experiment. The computer will randomly select ***five rounds*** out of the eleven 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.

[Assigned/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 each other during the experiment, and please put away your mobile phones and other devices.

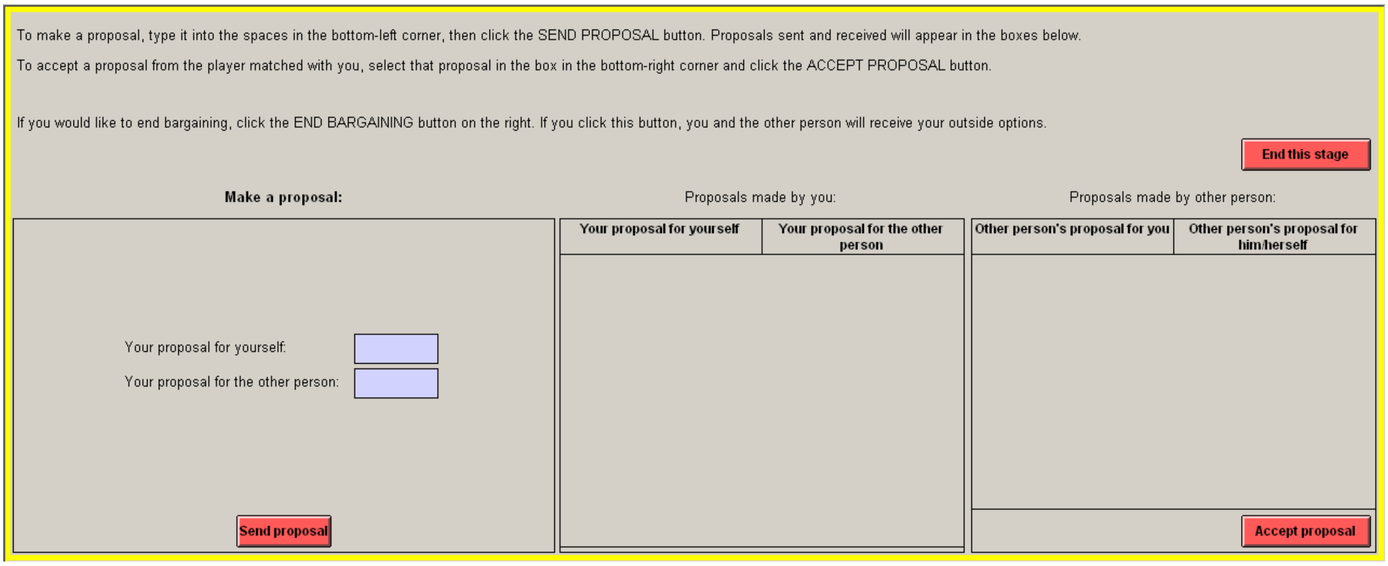
The main part of this experiment consists of ***eleven*** rounds. Each round starts with an ***encoding task***. Your computer screen will display a "word" – a sequence of 1-10 letters of the alphabet – along with a key showing the number corresponding to each letter. Below each letter will be an empty box. ***Inside each box, 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. If you have made any mistakes, the computer will inform you, and you can make changes. The key will stay the same for all words during a round, but may change from round to round. The sequence of words you face may differ from those faced by others.

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, you will have ***five minutes*** to encode as many words as you wish. After the encoding task ends, there is another stage, where ***you may have the opportunity to bargain over $20 with another randomly chosen person.*** The way you bargain is by ***sending and receiving proposals*** for dividing the $20. 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, without any dollar signs, in the “Make a proposal” box, then click “Send proposal”. ***The amounts you enter must be multiples of 0.05 between 0 and 20 (inclusive), must add up to 20 or less, and can have 0, 1 or 2 decimal places.*** 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.

[Please turn over]

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, click “Accept proposal”, then verify by clicking “Yes”. ***If either you or the other person accepts a proposal, then you have reached an agreement, and the $20 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. ***If you or the person matched with you clicks this button, it is no longer 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 have reached ***disagreement***. You receive your ***outside option*** and the other person receives his/her outside option. Your outside option is between 0 and $20 inclusive, is chosen randomly by the computer and may be different from the other person's outside option. You and the other person are informed of ***both*** outside options at the beginning of the bargaining stage.

You will reach the bargaining stage as long as ***both*** you and the other person correctly encode ***12 words or more***. If you encode fewer than 12 words, your profit for the round will be ***zero***. If you encode 12 words or more, but the other person encodes fewer, your profit will be ***30 cents*** for each word you correctly encoded. In either of these cases, you will not take part in bargaining.

Organisation of the experiment: Today's session has three parts.
(1) First, there is a ***one-minute practice round***, to let you familiarise yourself with the encoding task. After the one minute has ended, the computer screen will show the number of words you correctly encoded. There is no bargaining in the practice round, and its results will ***not*** affect the amount you are paid.
(2) Then, the main part of the experiment will begin. Each round proceeds as follows:
(a) You perform the encoding task.
(b) The computer pairs you with another person. If you reach the bargaining stage, your screen displays both outside options. You can send proposals for dividing the $20. The other person can also send proposals for dividing the $20; you can accept one of these proposals or none of them.
(c) The round ends. Your computer screen displays your profit and that of the person paired with you, and if you bargained, both of your outside options and whether or not you reached an agreement.
(3) After the 11th round, you will be asked to answer some demographic questions.

Payments: The amount you are paid will depend on the results of the experiment. The computer will randomly select ***five rounds*** out of the eleven 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.

[Earned/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 each other during the experiment, and please put away your mobile phones and other devices.

The main part of this experiment consists of ***eleven*** rounds. Each round starts with 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 corresponding to each letter. Below each letter will be an empty box. ***Inside each box, 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. If you have made any mistakes, the computer will inform you, and you can make changes. The key will stay the same for all words during a round, but may change from round to round. The sequence of words you face may differ from those faced by others.

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".

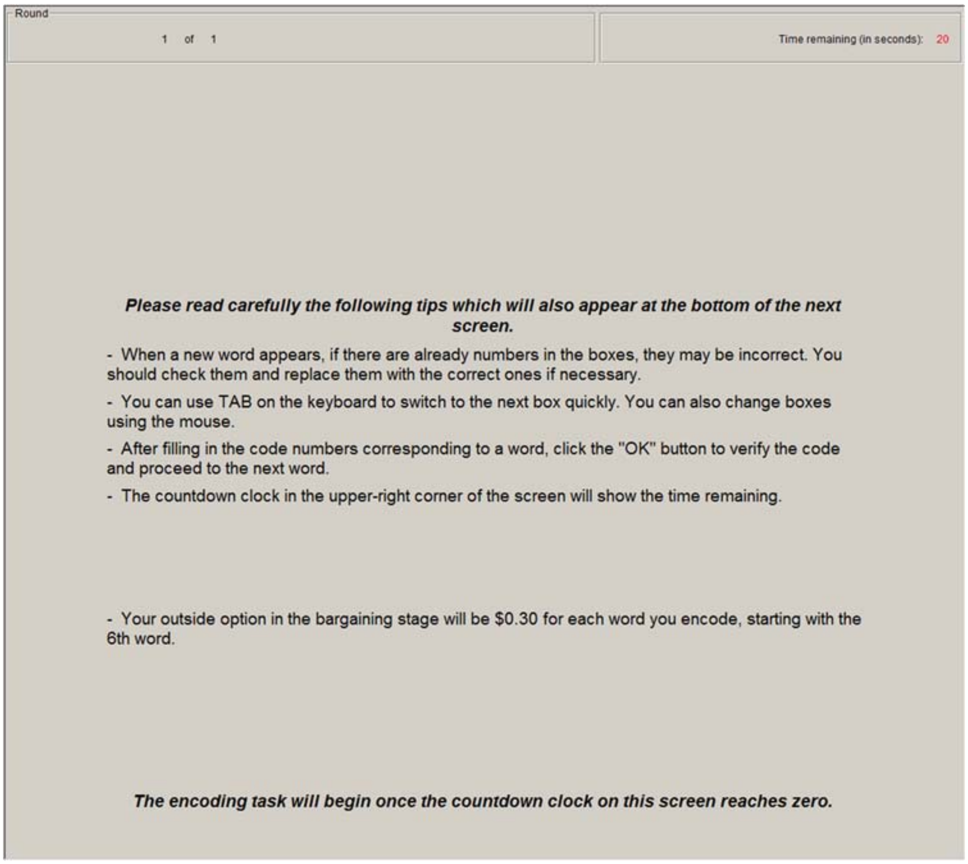
In each round, you will have ***five minutes*** to encode as many words as you wish. After the encoding task ends, there is another stage, where ***you have the opportunity to bargain over $20 with another randomly chosen person.*** The way you bargain is by each making ***simultaneous*** claims for shares of the $20.
- If your claim and the other person's claim add up to ***$20 or less***, then you have reached an ***agreement***. You receive your claim, and the other person receives his/her claim.
- If the claims add up to ***more than $20***, then you have reached ***disagreement***. You receive an ***outside option*** equal to ***30 cents*** for each word you had encoded, ***starting with your sixth word***, and the other person's outside option is calculated in the same way from his/her words encoded.
Both you and the person paired with you are informed of ***both*** outside options ***before*** choosing your claims.

Organisation of the experiment: Today's session has three parts.
(1) First, there is a ***one-minute practice round***, to let you familiarise yourself with the encoding task. After the one minute has ended, the computer screen will show the number of words you correctly encoded. There is no bargaining in the practice round, and its results will ***not*** affect the amount you are paid.
(2) Then, the main part of the experiment begins. Each round proceeds as follows:
  (a) You perform the encoding task. Your outside option in the bargaining stage will be 30c for each word encoded after the fifth word.
  (b) The computer pairs you with another person, and your screen displays both outside options. You choose a claim for your share of the $20. The other person chooses a claim for his/her share. ***Your claim can be any multiple of 0.05, between 0.00 and 20.00 inclusive.*** (Do not type the dollar sign.) 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 your outside option and that of the person paired with you, both of your claims, and both of your profits.
(3) After the 11th round, you will be asked to answer some demographic questions.

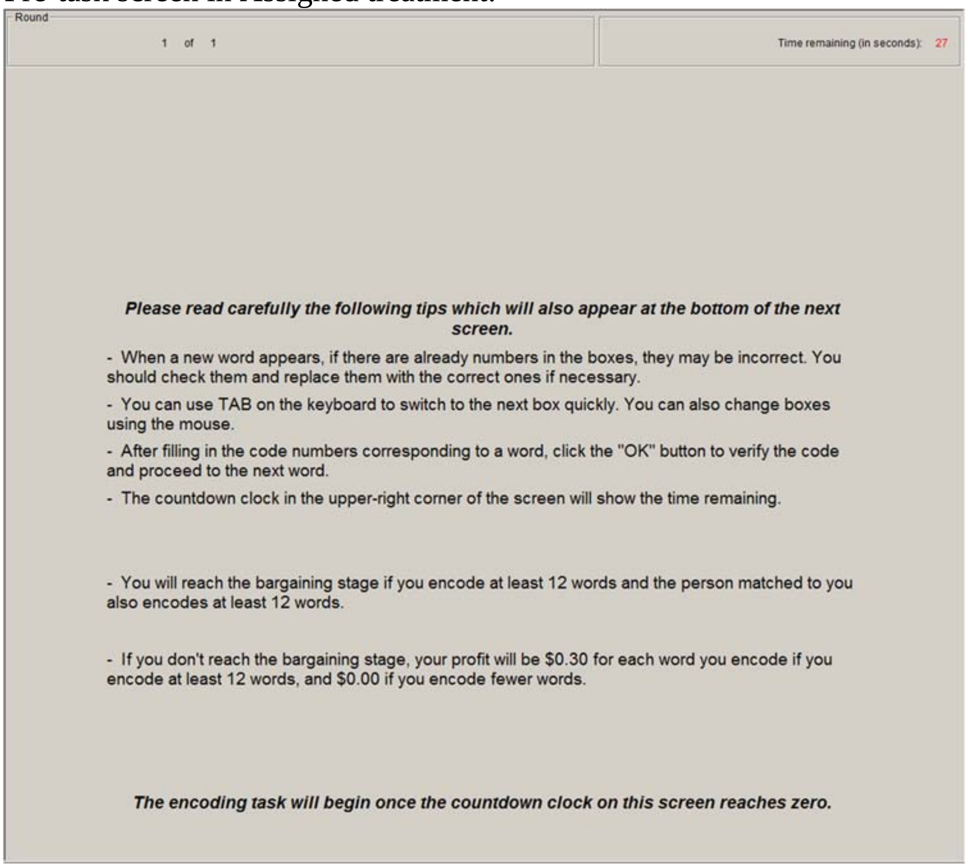
Payments: The amount you are paid will depend on the results of the experiment. The computer will randomly select ***five rounds*** out of the eleven you played. You will be paid the total of your profits from those selected rounds. Payments are made privately and in cash at the end of the session.

C Sample screenshots

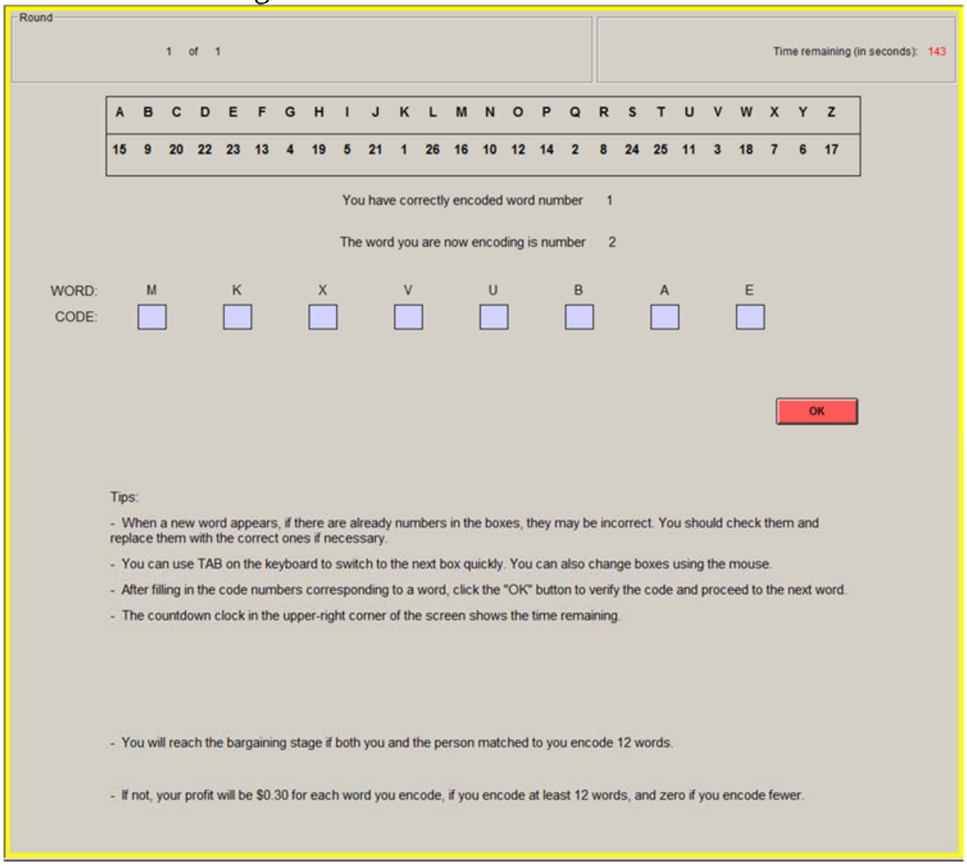
Pre-task screen in Earned treatment:



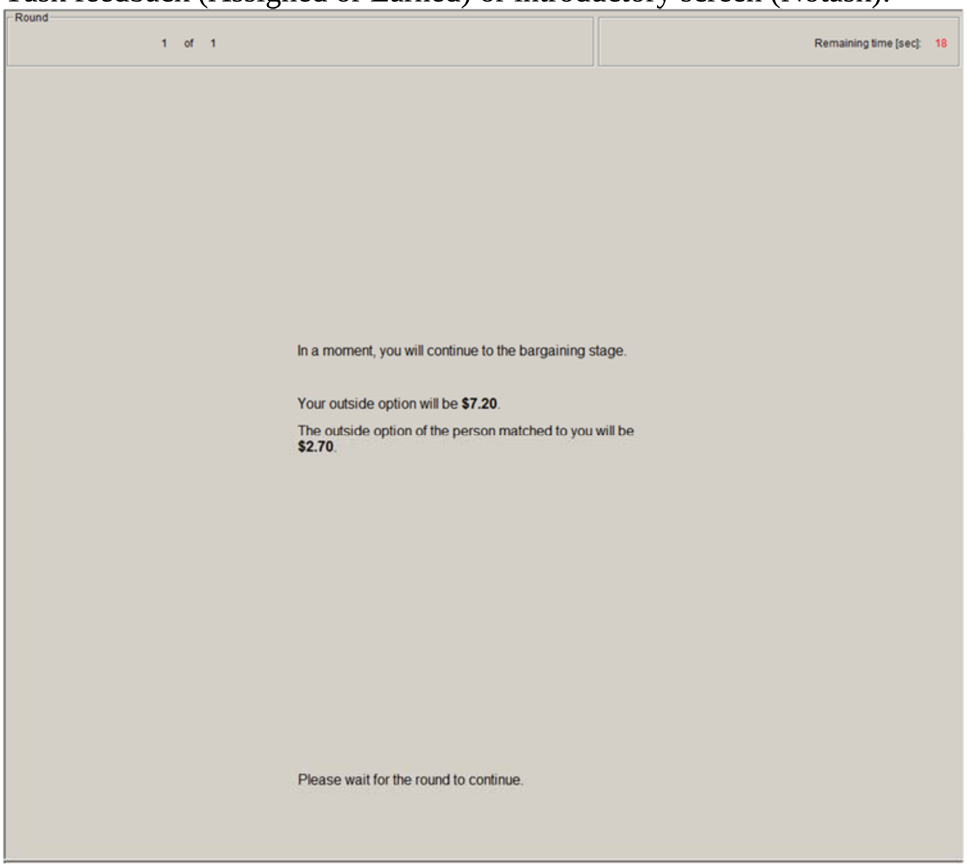
Pre-task screen in Assigned treatment:



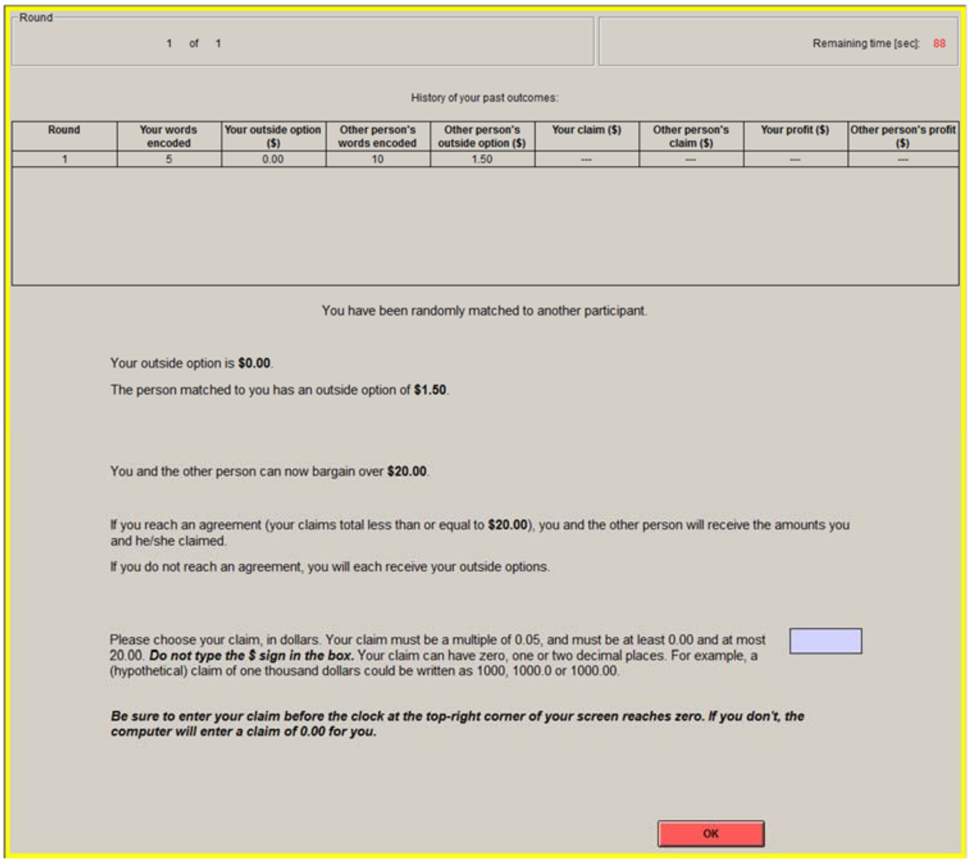
Task screen in Assigned treatment:



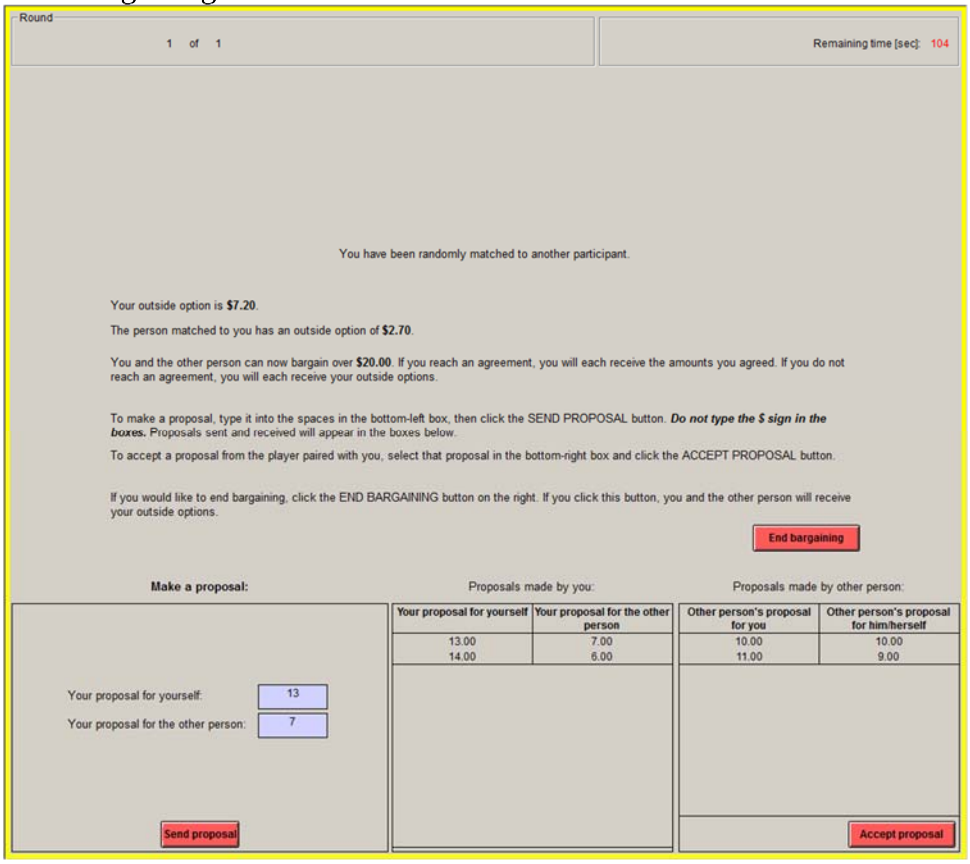
Task feedback (Assigned or Earned) or introductory screen (Notask):



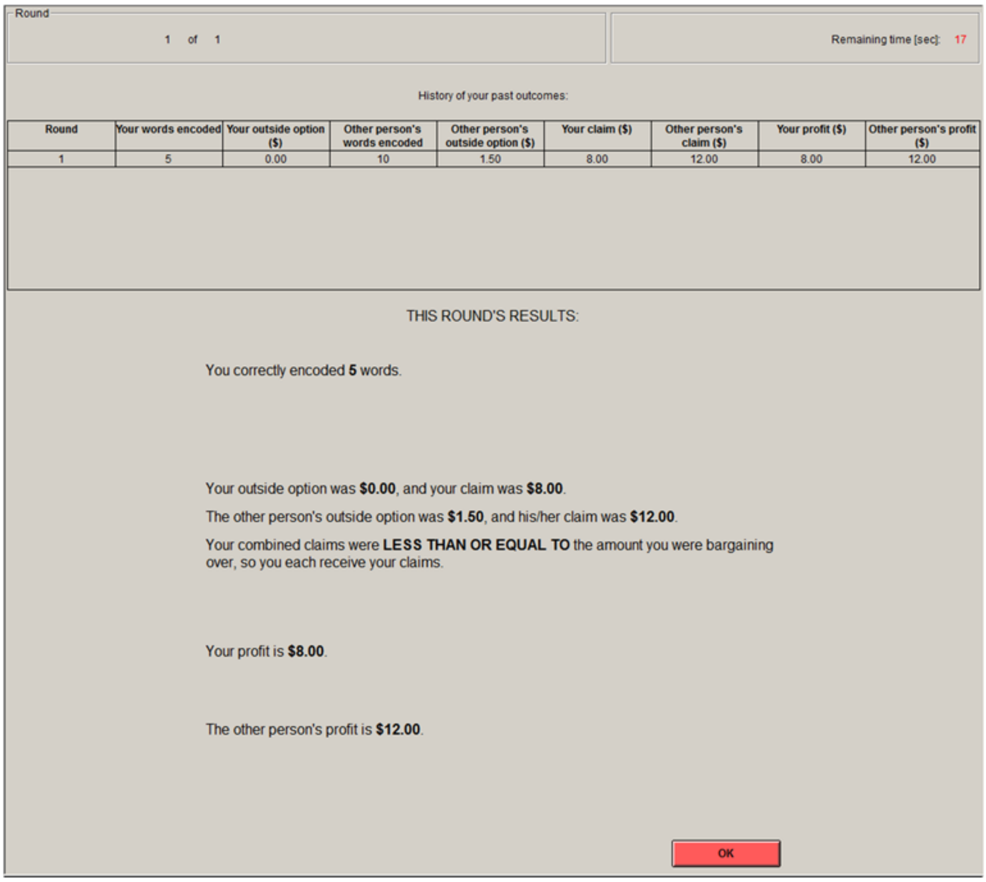
NDG demand choice:



UBG bargaining screen:



NDG feedback:



Round

1 of 1

Remaining time [sec]: 17

History of your past outcomes:

Round	Your words encoded	Your outside option ($)	Other person's words encoded	Other person's outside option ($)	Your claim ($)	Other person's claim ($)	Your profit ($)	Other person's profit ($)
1	5	0.00	10	1.50	8.00	12.00	8.00	12.00

THIS ROUND'S RESULTS:

You correctly encoded **5** words.

Your outside option was **$0.00**, and your claim was **$8.00**.

The other person's outside option was **$1.50**, and his/her claim was **$12.00**.

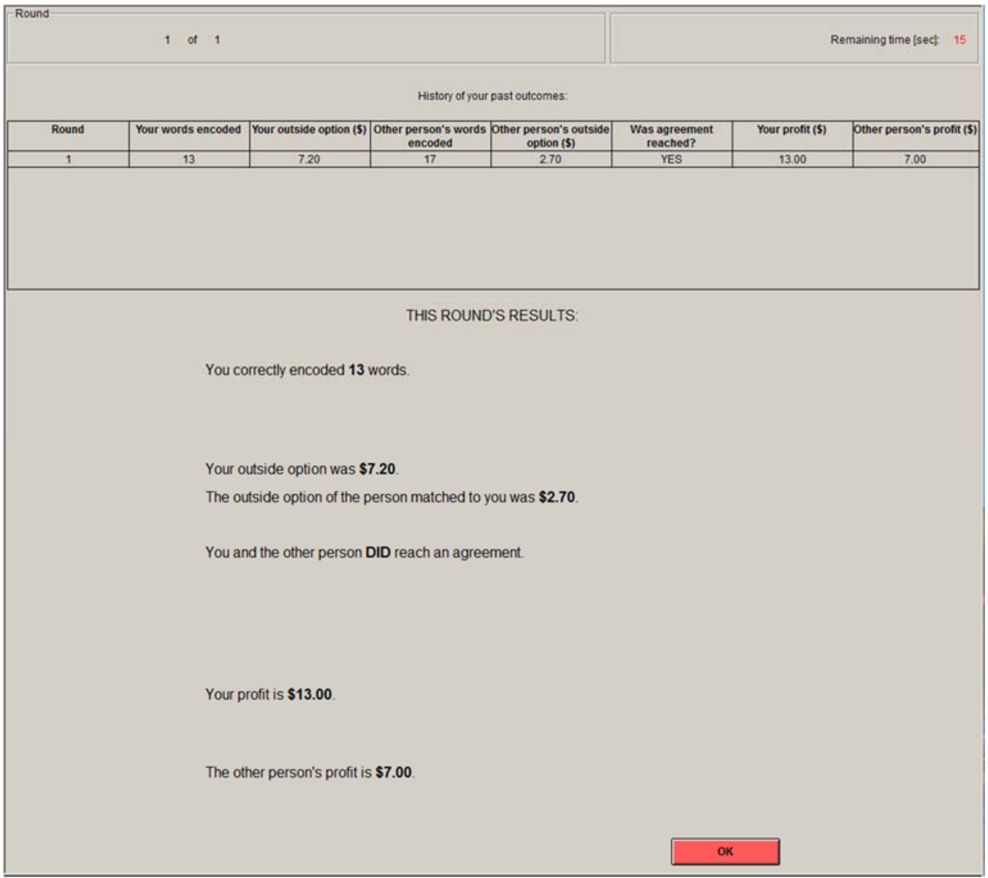
Your combined claims were **LESS THAN OR EQUAL TO** the amount you were bargaining over, so you each receive your claims.

Your profit is **$8.00**.

The other person's profit is **$12.00**.

OK

UBG feedback:



Round

1 of 1

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 ($)	Other person's profit ($)
1	13	7.20	17	2.70	YES	13.00	7.00

THIS ROUND'S RESULTS:

You correctly encoded **13** words.

Your outside option was **$7.20**.

The outside option of the person matched to you was **$2.70**.

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

Your profit is **$13.00**.

The other person's profit is **$7.00**.

OK

Post-experiment questionnaire: