The role of strategic uncertainty in games: an experimental study of cheap talk and contracts in the Nash demand game

Nick Feltovich*
University of Aberdeen Business School
Edward Wright Building
Aberdeen AB24 3QY, UK
n.feltovich@abdn.ac.uk

Joe Swierzbinski
University of Aberdeen Business School
Edward Wright Building
Aberdeen AB24 3QY, UK
j.swierzbinski@abdn.ac.uk

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Abstract

We utilise results from a human–subjects experiment to examine the connection between strategic uncertainty and outcomes in games. Our basic game is a Nash demand game where one player has an outside option available. A “chat” treatment allows bargainers to send cheap-talk messages prior to playing the basic game, and in a “contracts” treatment, they can additionally propose and accept binding contracts. We propose that strategic uncertainty comprises at least two facets: “coordination–type”, which is lower in the chat game than in the basic game, and “rationality–type”, which is lower in the contracts game than in the chat game. We find that both types of strategic uncertainty impact bargaining outcomes: moving from the basic game to the chat game, and thence to contracts, improves several aspects of outcomes, such as higher efficiency, less opting out and less under–demanding. Other results include a treatment effect on the types of agreements that are reached.

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1 Introduction

One of the most frequently made assumptions by applied microeconomic theorists is that bargaining under complete information ought to lead to efficient outcomes. This view was perhaps best articulated by Coase (1960), whose eponymous theorem stipulated that under certain conditions (no barriers to transacting, well-defined and enforceable property rights, complete information about each bargaining party’s incentives), an efficient outcome will be reached. In stark contrast to this benign theoretical prediction is the ubiquitous finding in the empirical bargaining literature (in both experiments and field studies) that a sizeable fraction of bargaining situations results in inefficient outcomes such as disagreements and costly delays. (See Roth, 1995; and Camerer, 2003 for surveys, and see Crawford, 1982; and Fernandez and Glazer, 1991 for theoretical treatments in which disagreements or costly delays can arise even in equilibrium.) The frequency of these inefficient outcomes depends on the particular bargaining situation, but it is rarely close to zero.

One likely cause for these inefficient outcomes is the presence of strategic uncertainty – the uncertainty arising from not knowing how other decision makers will behave – in the bargaining environment. Because of the large number of Nash equilibria present in many bargaining games, it may be quite difficult for bargainers who don’t know each other well to coordinate on a single equilibrium. Such coordination problems are common in games with multiple equilibria, particularly if there is not a unique self-evident focal point (see Ochs (1995) for a survey). Additionally, if bargainers admit even a small probability of an irrational opponent (or even that the opponent’s preferences are not fully captured by the amount of money received), then the amount of strategic uncertainty could be high indeed, in which case the incentives to avoid bargaining entirely in favour of other alternatives would be strong.

The connection between strategic uncertainty and bargaining outcomes is not just of academic interest. Fisher and Ury (1981), in their best-selling guide to “principled negotiation” that grew out of the Harvard Negotiation Project, stress the importance of communication during negotiations to avoid missing out on opportunities for mutual gain. They provide a cautionary example, from the negotiations taking place at the 1973–1982 Third United Nations Conference on the Law of the Sea:

At one point, representatives of the developing countries expressed keen interest in an exchange of technology; their countries wanted to be able to acquire from the highly industrialized nations advanced technical knowledge and equipment for deep-seabed mining.

The United States and other developed countries saw no difficulty in satisfying that desire — and therefore saw the issue of technology transfer as unimportant. In one sense it was unimportant to them, but it was a great mistake for them to treat [emphasis in original] the subject as unimportant. By devoting substantial time to working out the practical arrangements for transferring technology, they might have made their offer far more credible and far more attractive to the developing countries. By dismissing the issue as a matter of lesser importance to be dealt with later, the industrialized states gave up a low-cost opportunity to provide the developing countries with an impressive achievement and a real incentive to reach agreement on other issues [pp. 26–27].

Well-known negotiation texts aimed at business students also emphasise the consequences of strategic uncertainty. Raiffa (1982), for example, discusses the advantages of bringing in an intervenor (mediator or arbitrator) instead of breaking off negotiations: “[b]oth you and your adversaries might be willing to disclose more confidential information to an intervenor than to each other” [p. 130]. The implication, of course, is that the resulting exchange of information would benefit both parties.

Despite the prevalence of strategic uncertainty in economic models and in the outside world, there has been little systematic study of the topic. The most thorough theoretical treatment of strategic uncertainty is probably that of Roth (1977a,b), who extended the notion of the expected utility function to decisions between a safe alternative
and entry into a coalition–function game whose payoff depended on the agreement that was subsequently made – thus establishing a connection between strategic uncertainty and objective risk. Previous experimental studies have focussed on testing for associations in decision making behaviour between strategic situations (involving strategic uncertainty) and individual decision problems involving objective risk – generally with positive results. (See, for example, Murnighan, Roth and Schoumaker, 1988; Goeree, Holt and Palfrey, 2003; Bohnet and Zeckhauser, 2004; Schechter, 2007; and Heinemann, Nagel and Ockenfels, 2009.)

We believe it is quite reasonable to expect individuals’ risk attitudes to be positively associated across decisions under uncertainty, whether the uncertainty in question involves known probabilities, unknown probabilities in an individual decision problem, or unknown probabilities in a game against other individuals. However, an equally interesting issue – one that has gone unaddressed by these previous studies – is what happens to individual behaviour when the amount of strategic uncertainty in a game changes.¹

This is the hole that our paper seeks to fill. Unlike these previous studies, we do not attempt to relate strategic uncertainty and objective risk, nor do we make specific assumptions about their relationship. Rather, we study the effects of varying the amount and character of strategic uncertainty in a strategic environment, with an experiment designed to differentiate between two distinct forms of strategic uncertainty. We use the term “coordination–type” strategic uncertainty to refer to the uncertainty involved when there are multiple Nash equilibria, in which case players may have difficulty coordinating on one of them. This facet of strategic uncertainty was noted by Crawford (1990, p. 214) in his survey of bargaining theory and experiments: “...the bargaining game is likely to generate a great deal of uncertainty about how players will respond to its multiplicity of equilibria. Unless bargainers find a way to resolve this strategic uncertainty, they may not realize any of the gains from reaching an agreement”. By contrast, “rationality–type” strategic uncertainty refers to players’ uncertainty about whether their opponents actually will carry out a verbal agreement when unilateral deviation would result in a lower monetary payoff (such a deviation could be explained either by a breakdown of common knowledge of rationality or by payoffs that are not fully captured by monetary earnings).²

Our experiment begins with a bargaining game designed to yield a high frequency of inefficient outcomes: a variant of a two–player Nash demand game where exactly one of the players has an outside option available which, if exercised, leaves the other player with nothing.³ The size of this outside option varies, but in some cases is substantially more than 50% of the “cake” (the surplus being bargained over). This game, like many bargaining games, has a large number of Nash equilibria, but it has the added feature that it can be difficult to identify one as being a compelling focal point. In particular, when the size of the outside option is more than 50% of the cake, the most compelling focal point – both sides demand half – is no longer an equilibrium, and even theorists seem to disagree about which of the remaining equilibria is the most reasonable prediction (see Section 2). In such a situation, it is hard to imagine that ordinary people playing the game will be able easily to reach efficient outcomes.

In our experiment, human subjects take on the roles of bargainers in this game. Subjects play repeatedly against

¹There is a plethora of experimental evidence from a wide range of strategic environments that the amount and type of feedback given to subjects in experiments can have an effect on behaviour (e.g., Wilson and Sell, 1993; Duffy and Feltovich, 1999; Binmore, Swierzbinski and Proulx, 2001; Offerman, Potters and Sonnemans, 2002) and Weber, 2003) in public good games, ultimatum games, games with unique equilibria in mixed strategies, oligopoly games and beauty contest games, respectively). To the extent that better feedback about others’ previous decisions leads to better information about their current actions – and thus less strategic uncertainty – these experiments could be viewed as tests of varying the amount of strategic uncertainty. However, this claim should not be taken too far, since increasing the amount or quality of end–of–round feedback can have other effects, such as increasing incentives for reputation building and other supergame effects. We also note that while previous studies of strategic uncertainty (such as Heinemann et al. (2009)) often vary aspects of the game that are relevant to players’ decisions, it’s not clear that these manipulations actually change the level of strategic uncertainty – in the same way that changing the probability of winning a binary lottery does not change its level of objective risk, even though it may well affect behaviour.

²Myerson (1991, pp. 394–399) and Abreu and Gul (2000) present theoretical treatments of bargaining with players who with some small probability are irrationally stubborn.

³This game has been studied experimentally by Binmore et al. (1998).
changing opponents, and with varying outside options. In order to focus on the effects of strategic uncertainty on outcomes, we consider three communication treatments that we believe capture varying degrees of strategic uncertainty. In our baseline treatment, subjects simply play the basic bargaining game, and have no additional contact with each other. In our “chat” treatment, we allow subjects a two–minute round of unstructured costless nonbinding communication (cheap talk) prior to playing the basic game. In our “contracts” treatment, we allow not only cheap talk, but also mutually agreed binding contracts, in the pre–play round.

It seems reasonable to assert that our contracts treatment lessens the amount of strategic uncertainty in the game relative to our chat treatment, which in turn reduces the level of strategic uncertainty relative to the baseline treatment. More specifically, the difference between the chat and baseline treatments is in the likelihood that in the former, through communication, players can reach an agreement on a division of the cake. While any such agreement is not formally binding, if the agreement is a Nash equilibrium, it should be de facto binding. This suggests that comparison of our chat and baseline treatments will serve as a test of the effects of the “coordination–type” strategic uncertainty discussed above. By contrast, both chat and contracts treatments allow communication, suggesting that there is little difference in this “coordination–type” strategic uncertainty between these two treatments. Instead, the difference is in players’ ability in the contracts treatment to turn a verbal agreement into a binding written agreement, rendering moot any question about whether the other player will in fact carry out the agreement. If the verbal agreement was a Nash equilibrium, there would be no incentive for the other player to renege; however, if there is any doubt about the other player’s rationality (or belief that the other player may have tastes for non–monetary aspects of outcomes), the ability to make a contract binding could be valuable. Thus, comparison of our chat and contracts treatments will serve as a test of the effects of the “rationality–type” strategic uncertainty discussed earlier. Such an interpretation of our treatments suggests that both types of strategic uncertainty are widely believed to be relevant in bargaining in the outside world, as well as in the lab: the pure fact of the existence of a market for professional mediation services suggests that some negotiations can be improved simply by non–binding communication, while the fact that some negotiations require binding arbitration suggestions that in some cases, communication is not sufficient.

The results of our experiment show a close relationship between strategic uncertainty and bargaining outcomes. With the size of the outside option held constant, moving from our baseline treatment to our chat treatment and thence to our contracts treatment (that is, decreasing the level of strategic uncertainty) is associated with increases in the frequency of efficient outcomes, decreases in opt–outs and higher payoff efficiency – usually with average payoffs increasing for players both with and without an outside option available. Both comparisons – chat versus baseline and chat versus contracts – show some significant differences, suggesting that both forms of strategic uncertainty have effects on behaviour. We also find that the verbal agreements reached in our chat treatment are largely, but not completely, self–enforcing, as a small but persistent fraction of these agreements are subsequently broken. In addition to these results, we find that demand pairs, conditional on reaching an efficient outcome, add up to closer to the cake size in either the chat or the contracts treatment compared to the baseline, reflecting less need in these treatments for “hedging” by under–demanding. Finally, we find that even the types of agreements that are reached can be dependent on the level of strategic uncertainty. In the treatments with communication possible (with or without binding contracts), we find support for at least three focal points: equal–split (even when this is not an equilibrium outcome), “split–the–difference” (the player with the outside option receives its value plus half of the remainder) and “deal–me–out” (the player with the outside option receives either the option or 50% of the cake, whichever is larger). In our baseline treatment, on the other hand, efficient outcomes cluster around equal–split (which nearly always coincided with deal–me–out in those cases where efficient outcomes were reached in the baseline).

4To avoid confusion, we will abuse terminology somewhat by using “verbal agreement” for the nonbinding agreements that can be reached in our chat treatment, and “written agreement” for the binding agreements that can be reached in the contracts treatment – even though in the experiment, both types of agreements will be made by writing.
2 The games used in the experiment

The games used in the experiment are modifications of Nash’s (1953) demand game, in which two players simultaneously make demands for shares of a “cake” whose value of 100 points is known by both players. Demands are constrained to be integers between 0 and 100 inclusive. If the demands are compatible – that is, they total 100 or less – both players receive their demands, plus one–half of any leftover amount. (Thus, any compatible pair of demands results in an efficient split of the cake.) If the demands are incompatible, both earn zero. In either case, the game ends with no opportunities for renegotiation. Our basic game adds one complication: an outside option for one of the players, which can be chosen in lieu of making a demand. (Following Binmore, Shaked and Sutton (1989) and others, we will henceforth refer to the player without the outside option as “Player 1” and the player with the option as “Player 2”.) The size of the outside option varies in our experiment – taking on values of 9, 25, 49, 64, or 81 – but within a game is known by both players. If Player 2 opts out, he receives his outside option and Player 1 receives zero.

Our basic game, while obviously a simplification of real–life bargaining situations, is nevertheless appropriate as a model of real–life bargaining. As Binmore (2007) has pointed out, in situations where bargainers are able to commit to demands, but neither is favoured relative to the other, the Nash demand game is the limiting case where both bargainers “rush to get a take–it–or–leave–it demand on the table first” (p. 496), resulting in simultaneous irrevocable demands.5 Relatedly, Skyrms (1996) notes that in studying bargaining, “[o]ne might imagine some initial haggling...but in the end each of us has a bottom line” (p. 4); focussing on these bottom lines results in the Nash demand game. Our basic game, which adds the outside option, captures the notion that bargaining parties often must incur sunk costs in order to produce the surplus over which they subsequently bargain. We assume that only one of the parties (Player 2) needs to incur such a cost, though the theory is very similar when both parties must do so.

Our basic game is also especially well suited for the purpose of studying strategic uncertainty, as it has two advantageous features not present in the ordinary Nash demand game, both of which act to decrease the likelihood of compatible demands (and thus increase the likelihood of finding treatment effects if any exist). First, giving an outside option to only one of the players makes the game asymmetric. A typical result in experiments using symmetric bargaining games is that equal splits of the cake are very common; if 50–50 splits are an obvious focal point, then the game doesn’t have much actual strategic uncertainty, so there is little room to improve outcomes by adding measures designed to reduce strategic uncertainty. In contrast, games that are asymmetric in some way – such as ours – have usually led to substantially lower frequencies of compatible demands, and more variation in what outcomes occur.6 The set of outside options we use ranges from one so small (9) as to make the game nearly symmetric, to one so large (81) as to place Player 2 in an extremely advantageous position in the game.

Second, introducing the asymmetry in the form of an outside option (rather than, say, as a modification to the disagreement outcome or to the set of feasible agreements) combines being simple to explain to experimental subjects with a degree of theoretical ambiguity, in the sense that intuitive criteria for equilibrium selection may not make a clear–cut prediction. Binmore, Shaked and Sutton (1989) suggest two fairly appealing focal points for games with outside options: “split–the–difference”, where Player 2 receives his outside option plus half of the remainder, and “deal–me–out”, where Player 2 receives the maximum of his outside option and 50% of the cake (with Player 1 receiving the rest in both cases). As Binmore et al. (1998) point out, both of these focal points – which are distinct as long as the outside option is strictly between 0 and 100 – can be justified by an appeal to Nash’s (1950) bargaining solution, with the outside option treated as either a change to the disagreement outcome or a constraint

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5 In the alternative case, when one bargainer has more ability to commit than the other, the ultimatum game is the natural model. See Fischer et al. (2006) for an experiment that nests the ultimatum and Nash demand games.

6 See, for example, Nydegger and Owen (1975) and Roth and Malouf (1979) for experimental comparisons of symmetric and asymmetric bargaining games. Also, Malouf and Roth (1981) find that symmetry is more useful than a measure of “conflict” (proposed by Axelrod, 1967) in explaining the likelihood of reaching agreement in bargaining.
on the set of feasible agreements. Sutton (1986) argues that “deal–me–out” is the more reasonable prediction when there is an outside option, but judging by the applied bargaining literature, there is no clear consensus. Experimental evidence has thus far leaned toward “deal–me–out” (Binmore, Shaked and Sutton, 1989; Binmore et al., 1998), though evidence of “split–the–difference” has also been observed (Ellingsen and Johannesson 2004b), and in all of these studies, inefficient outcomes such as disagreements and opt–outs are common. We note in particular the study done by Binmore et al. (1998), whose experiment uses our basic game with the same set of outside options. In addition to finding stronger support for “deal–me–out” than “split–the–difference” in outcomes, Binmore et al. (1998) found nonzero frequencies of inefficient opt–outs for every size of outside option except for the smallest (representing 9% of the available surplus), with the frequency of opting out rising as the size of the outside option increases.

2.1 Experimental treatments

One plausible explanation for Binmore et al.’s (1998) results, and other findings of inefficient outcomes in bargaining, involves strategic uncertainty. For any of the outside options we consider, many pairs of compatible claims exist that are both efficient and consistent with equilibrium; as a result, it could be nearly impossible for one bargaining party (even with an understanding of game theory) to know with certainty what claim the other will choose to make. In particular, a player with an outside option, faced with the worry that the other player might make a relatively large claim and leave both parties with zero, might prefer the safe outside option to the riskiness of bargaining, forgoing those potential Pareto improvements.\(^7\)

If inefficient outcomes occur at least partly because of strategic uncertainty, then it stands to reason that reducing strategic uncertainty ought to lead to better outcomes. We thus turn to changes to the basic game that we expect to lower the level of strategic uncertainty. One modification we consider is allowing communication between the bargaining parties. In our “chat” game, players are able to send nearly unrestricted cheap–talk messages via a computer chat room, prior to play of the basic bargaining game, so that (nonbinding) verbal agreements can be reached. In our “contracts” game, we go a step further. Players during the pre–play stage not only have access to the same chat room as in the chat game, but also can make proposals via the computer for splits of the cake which, if accepted by the other player, become binding written agreements. (See Section 3 for details of how we implement these modifications in the experiment.) In both of these games, the cake size remains constant throughout: there are no monetary costs to delay in either game or failure to reach a written agreement in the contracts game (in that case, players simply play the basic game – with the same cake size – after the pre–play stage ends).

2.2 Theoretical implications

Like many bargaining games, our basic game has a large number of Nash equilibria. These include efficient equilibria in which demands total exactly 100 (the cake size) and Player 2 demands at least his outside option, as well as inefficient equilibria (e.g., a class of equilibria in which Player 2 opts out and Player 1 demands more than 100 minus Player 2’s outside option). Standard game theory says little about which particular equilibrium should be selected, though some equilibrium selection criteria make more precise predictions. For example, both risk dominance and Harsanyi and Selten’s (1988) equilibrium selection procedure select the deal–me–out agreement discussed in the previous section. On the other hand, payoff dominance implies merely that one of the efficient equilibria will be chosen, but not which one. Security predicts that Player 2 will choose his outside option, guaranteeing that amount, though it makes no prediction for Player 1, who cannot guarantee more than zero with any strategy.

\(^7\)As evidence of this last claim, consider Wickelgren (2004), who argues in favour of “deal–me–out”, but lists several applied bargaining articles that use “split–the–difference” instead.

\(^8\)To the extent that bargaining agreements are consistent with “deal–me–out”, so that Player 2 receives only a small amount more than his outside option even when bargaining is successful, this temptation may be particularly strong when a large outside option is available.
Adding either unrestricted communication or unstructured bargaining to the basic game vastly increases the size of the strategy space, making a full noncooperative game-theoretic analysis (as well as use of Harsanyi and Selten’s technique) impossible. However, we will note that according to standard theory, neither of these treatments need have any nontrivial effect on outcomes. The addition of cheap talk in the pre-play stage has no effect on the set of Nash equilibrium outcomes of the subsequent bargaining stage, and allowing binding contracts before the bargaining stage adds only the possibility that an equilibrium outcome is agreed sooner rather than later. There are additional solution concepts that become relevant when unrestricted communication or unstructured bargaining are possible, but these also shed little light. The core of this game corresponds exactly to its set of efficient Nash equilibria, so no further selection occurs (other than eliminating the inefficient equilibria). Axiomatic bargaining solutions, such as Nash’s (1950) solution, typically predict a unique efficient equilibrium, but again are silent as to whether behaviour in our basic game should be different from that in either the chat or the contracts game. Finally, theoretical models of cheap talk in games often imply a link between communication and increased efficiency, though they seldom yield sharp predictions.

2.3 Implications from previous experiments

A better sense of the possible effects of our chat and contracts treatments can be found in the experimental literature. In contrast to the theoretical literature, there is some experimental evidence suggesting that both cheap talk and the possibility of binding contracts can improve outcomes. We don’t know of any experimental studies of unrestricted cheap talk in complete-information bargaining games such as ours, but Ellingsen and Johannesson (2004b) show that allowing a single one-way message substantially raises the frequency of compatible demands and lowers that of opt-outs in a game similar to ours with an outside option representing 60% of the cake. Some results from other types of games are also relevant. Many coordination game experiments have found that communication (including unrestricted cheap talk) nearly always improves the likelihood of successful coordination. In his discussion of communication in experiments, Crawford (1990) conjectured that this improvement is due to a reduction in strategic uncertainty. Crawford (1998), in a more extensive survey of cheap talk in experiments, extended this conjecture by discussing two specific ways in which cheap talk might improve coordination — both of which are relevant to the bargaining game we use. First, in games with multiple equilibria that can’t be Pareto ranked (like Battle of the Sexes), communication allows players to settle on one of these. Second, in games with equilibria that Pareto dominate other equilibria, communication plays a “reassurance role...reducing their uncertainty about each other’s decisions” (p. 294), allowing mutual gains to be realised.

Communication also tends to improve outcomes in social dilemmas, though the effect can be sensitive to what kind of communication is permitted. Sally’s (1995) meta-analysis of prisoners’ dilemma experiments found that

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9 For example, Rabin (1994) proposed a model of pre-game communication which predicts that players can do no worse than getting payoffs arbitrarily close to their worst Pareto efficient Nash equilibrium payoffs. In our game, this refines away the inefficient Nash equilibria, but cannot discriminate amongst the many efficient ones. (See also Costa-Gomes (2002), who applies Rabin’s model to characterise several results from previous bargaining experiments.) Farrell and Rabin (1996) suggest criteria under which cheap-talk messages are likely to be truthful. In their nomenclature, all proposals of Nash equilibrium divisions of the cake are “self-committing” (the sender prefers to carry out the signalled action if she expects the receiver to choose a best response), but none are “self-signalling” (the sender wants the receiver to choose a best response if and only if the signal is truthful); as a result, these concepts have little ability for selecting among equilibria in our game. More recently, Demichelis and Weibull (2008) consider the effect of cheap talk in games with Pareto ranked pure-strategy Nash equilibria. They find that under fairly weak conditions, only a slight aversion to lying is required in order for selection criteria to favour the payoff dominant equilibrium (and truth telling). Again, however, their setup doesn’t distinguish amongst the multiple efficient equilibria in our game.

10 There has also been some research into the effects of cheap talk on bargaining under incomplete information; see, for example, Hoffman and Spitzer (1982) or Valley et al. (2002).

11 Crawford (1998) concentrated on the special case of symmetric two-player games with asymmetric pure-strategy equilibria, in which case coordination necessarily required “symmetry breaking”.

allowing verbal communication has a significant positive relationship on the frequency of cooperation, but he found no significant relationship between cooperation and written communication. On the other hand, Bochet, Page and Putterman (2006) found that unrestricted written communication led to more cooperation and higher payoffs in a public–good game, compared with no communication. To the extent that cheap talk increases the likelihood of Pareto efficient outcomes in public–good games – where they are not Nash equilibria – we would have reason to believe that cheap talk may be even more likely to improve outcomes in our setup, where the efficient outcomes are equilibria.

As the above results indicate, there is plenty of evidence that cheap talk improves outcomes. However, the reasons why cheap talk works are less well understood. Ellingsen and Johannesson (2004a) propose that individuals often have tastes for consistency: that is, they have an aversion to going back on their word. Clearly, such a taste would lead to them following through on their promised actions. Similarly, Charness and Dufwenberg (2006) suggest a model of guilt aversion, where individuals receive disutility from making choices that (they believe) hurt others relative to those others’ payoff expectations. Given such preferences, reaching verbal agreement on a good outcome should be expected to raise players’ expectations about their eventual payoffs, hence raising the cost of dashing these expectations by reneging on the agreement. Since either tastes for consistency or guilt aversion is likely to increase the credibility of verbal agreements, bargainers in our chat treatment might be more likely to find it worthwhile to make such agreements.

There has been much less experimental research into the effects of allowing binding contracts, once cheap talk is possible. A sense of these effects can be found in the work by Van Huyck, Battalio, and Walters (1995, 2001) on peasant–dictator games. In the standard peasant–dictator game, the peasant first chooses a level of effort, then the dictator chooses a tax rate; in equilibrium, the peasant exerts minimum effort, knowing that the dictator will confiscate all of the proceeds. Van Huyck, Battalio, and Walters (1995) find that allowing the dictator to make a binding commitment prior to the peasant’s decision results in sizeable increases in efficiency. On the other hand, Van Huyck, Battalio, and Walters (2001) find that cheap–talk promises made by dictators before the peasant’s choice are largely incredible. As a result, we conjecture that our contracts treatment – by allowing binding commitments – similarly improves outcomes compared to our chat and baseline treatments, where such commitments are not possible.

2.4 Hypotheses

The above discussion leads us to several testable hypotheses about behaviour in the baseline, chat and contracts treatments. To save space, we only state the alternative hypotheses here (the corresponding null hypotheses should be clear from the context). These hypotheses will help us to organise our analysis of the experimental results in Section 4.

Our first hypothesis arises from our conjecture (based on the theoretical and experimental results described above) that in general, lower levels of strategic uncertainty should be associated with better outcomes.

**Hypothesis 1** Moving from the basic game to the contracts game is associated with improved outcomes – specifically higher likelihood of compatible demands, fewer opt–outs and higher total payoffs.

Our next hypothesis refines the previous one, and arises from our distinction between coordination–type and rationality–type strategic uncertainty. The literature on cheap talk, and the peasant–dictator results of Van Huyck, Battalio, and Walters (1995, 2001), imply that both allowing chat and allowing binding contracts should have effects.

**Hypothesis 2** Moving either from the basic game to the chat game, or from the chat game to the contracts game, is associated with improved outcomes.

Behavioural theories based on experimental results, such as guilt aversion (Charness and Dufwenberg, 2006) and tastes for consistency (Ellingsen and Johannesson, 2004a), imply that subjects should be reluctant to renege on verbal agreements in the chat treatment. We conjecture that this should be especially true in our experiment, as verbal
agreements are likely to correspond to efficient Nash equilibria (in contrast to previous work which typically involved social dilemmas, so that messages often pointed toward high-payoff, but non-equilibrium, outcomes). As a result, we have:

**Hypothesis 3** Verbal agreements should be self-enforcing; that is, when subjects reach a verbal agreement, their subsequent demands should be compatible.

Our last hypothesis is based on previous experimental results (such as Binmore et al. 1998) involving simultaneous-move bargaining with outside options. These experiments have tended to find more support for deal–me–out than for split–the–difference (see the beginning of Section 2 for a discussion of these focal points and the relevant literature). As our baseline treatment is similar to Binmore et al.’s design, we expect for this result to continue to hold; that is, divisions of the cake are expected to be consistent with deal–me–out more often than with split–the–difference.

**Hypothesis 4** When subjects achieve an efficient outcome, the distribution of the cake between Player 1 and Player 2 will be close to the distribution implied by the deal–me–out focal point more often than that implied by split–the–difference.\(^\text{12}\)

### 3 Experimental design and procedures

All sessions lasted for thirty rounds, beginning with ten rounds of our basic bargaining game (with outside option), but varying the game that was used in the remaining twenty rounds. In our baseline treatment, these rounds were also of the basic game. In our chat treatment, the last twenty rounds were of the chat game, and in our contracts treatment, these rounds were of the contracts game. In addition to varying the level of strategic uncertainty, we also varied the ordering of the outside options. Following Binmore et al. (1998), our “increasing” treatment used the ordering 9-9-25-25-49-49-64-64-81-81 for each block of ten rounds and our “decreasing” treatment used the reverse ordering. We thus have a 3 × 2 × 5 factorial design, with three communication treatments (baseline, chat, contracts) and two ordering treatments (increasing, decreasing) varied between-subjects, and five outside-option amounts (9, 25, 49, 64, 81) varied within-subject.

The experimental sessions took place at the Scottish Experimental Economics Laboratory (SEEL) at the University of Aberdeen. Each session involved between 10 and 20 subjects. Subjects were primarily undergraduate students from University of Aberdeen, and were recruited from a database of people expressing interest in participating in economics experiments. No one took part more than once.

At the beginning of a session, subjects were seated in a single room and given written instructions for the first ten rounds.\(^\text{13}\) They were informed then that the experiment would comprise three parts totalling thirty rounds, but details of the later parts were not announced until after the first part had ended. These instructions were also read aloud to the subjects, in an attempt to make the rules of the game common knowledge. Then, the first round of play began. After the tenth round was completed, each subject was given a copy of the instructions for rounds 11–20. This was done even in the baseline treatment, where the game did not change. (In this case, the new instructions were quite short – stating essentially just that the game was not changing.) These instructions were also read aloud, before round 11 was played. After the twentieth round was completed, each subject was given a copy of the instructions for rounds 21–30, even though the game was never changed at this time. These instructions were also read aloud, before the final ten rounds were played.

The experiment was run on networked computer terminals, using the z–Tree experiment software package (Fischbacher, 2007). Subjects were asked not to communicate with other subjects except via the computer program.

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\(^{12}\)In Section 4.4, we will define “close” to mean within 5 percentage points of the theoretical prediction.

\(^{13}\)A sample set of instructions (those used in the chat treatment) and some screenshots are shown in Appendix A. The full instructions used in the baseline and contracts treatments, as well as the raw data from the experiment, are available from the corresponding author upon request.
Subjects were randomly matched to opponents. In most sessions, each subject was equally likely to be the opponent in a given round. In a few larger sessions, however, we partitioned the subjects into two “subsessions” – so that subjects were matched only to other subjects in the same subsession – in order to increase the number of independent observations. Within each matched pair of subjects, both were equally likely to have the outside option in that round. No identifying information was given to subjects about the opponent (in an attempt to limit incentives for reputation building and other supergame effects). Rather than using potentially biasing terms like “opponent” or “partner” for the other player, we used the neutral though somewhat cumbersome “player matched to you” and similar phrases.

Each round of the game began with an introductory screen, telling each subject whether he/she had an outside option for that round, and what the size of the outside option was. In the basic game, after all subjects clicked a button on the screen to continue, subjects were prompted to make their choices for that round. Subjects with outside options chose whether to opt in or out and, if they opted in, their demand for the round. Subjects without outside options chose only a demand for the round. After all subjects had made their choices, each was given the following feedback: own choice(s), opponent choice(s), own payoff, opponent payoff. A subject’s previous results were also collected into a history table at the top of the computer screen; these could be referred to at any time. After all subjects clicked a button on the screen to continue, the session proceeded to the next round.

The chat and contracts games used similar procedures, but with a pre–play stage added after the introductory screen and before the choices of demands and opting in or out. In the chat game, this pre–play stage was a chat stage, during which subjects were able to send as many or as few messages as they chose. Messages were sent via a computer “chat room” (see Figure 1); once sent, they were visible by the sender and receiver (but no other subjects) until the stage ended. Subjects were instructed not to send messages containing (a) personal or identifying information or (b) physical threats, but messages were otherwise unrestricted. The chat stage normally lasted for two minutes, but either subject in a pair could end communication early by clicking a button, in which case no more messages could be sent, though the previously–sent messages were visible until the stage ended. Once the two minutes were complete or all subject pairs had ended communication, play continued to the demand stage as in the basic game.

The contracts game was similar, except that the chat room screen allowed subjects to make, view and accept proposals for written agreements, as well as being able to send and receive messages (see Figure 2). A proposal consisted of a nonnegative integer amount for the sender and one for the receiver, adding up to 100 (the cake size) or less. These contracts could be proposed by either player in a bargaining pair, and aside from the two–minute time limit for the stage, there was no restriction on how many proposals could be made, and the content of a proposal imposed no constraints on future proposals (e.g., there was no requirement that later proposals had to be more favourable to the receiver than earlier ones). Once a proposal was made, it stayed “on the table” until the end of the stage (proposals could not be revoked). Both the subject’s own proposals and the proposals of the opponent were shown on the subject’s screen, in different places. As long as the stage hadn’t ended, a subject could choose to accept any of the opponent’s proposals, at which time that proposal would become binding. The opponent’s proposals were listed in order of decreasing payoff to the subject, so there was almost no cognitive effort required to determine the most favourable opponent proposal (it was always at the top of the list), though of course a subject could accept a less favourable proposal if desired. The contracts stage ended if a proposal was accepted, if either subject in a pair chose to end it (by clicking a button as in the chat game), or after the two minutes had expired. Once all pairs had finished, play continued to the demand stage as in the basic game, though pairs that had agreed to a contract would not have any decisions to make in this later stage.

At the end of the thirtieth round, the experimental session ended. For each subject, two rounds from each set of ten were randomly chosen, and the subject was paid his/her earnings in those rounds. The exchange rate was £1 per

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14 Some researchers (for example, Binmore, Shaked and Sutton, 1985) have found that giving subjects experience in both bargaining roles can mitigate other–regarding preferences, though Bolton (1991) found no difference between sessions with changing roles and those with fixed roles.
50 points in the first ten rounds, and £1 per 10 points in the last twenty rounds. Additionally, all subjects received a £2 show–up fee. Total earnings for subjects participating in a session averaged about £20. The length of a session was typically 45–60 minutes in the baseline treatment, and 90–120 minutes in the other treatments.

4 Experimental results

The experiment comprised 15 sessions with 206 subjects (see Table 1). As mentioned in the previous section, some larger sessions were partitioned into two subsessions, so that the subjects in one subsession did not interact with those in the other. (This was done in five of the sessions, so that there are a total of 20 subsessions.) The subsession is thus the smallest independent unit in our experiment, and will often be our unit of analysis.

The presentation of results is organised as follows. In Section 4.1, we address Hypotheses 1 and 2 by examining how aggregate–level features are affected by chat and contracts, by comparing session–level results. In Section 4.2, we consider Hypothesis 3, with a look at how “success” in our chat and contracts treatments (defined as reaching an agreement, whether binding or not) is associated with good outcomes in the subsequent bargaining stage. In Section 4.3, we analyse the disaggregated results, focussing on how individuals adjust their choices in response to the differences in strategic uncertainty across treatments. In Section 4.4, we look at how the distribution of surplus
Table 1: Session and treatment information

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Cheap talk possible?</th>
<th>Contracts possible?</th>
<th>Ordering of outside options</th>
<th>Number of subsessions</th>
<th>Number of subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>No</td>
<td>No</td>
<td>Increasing</td>
<td>3</td>
<td>30</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Decreasing</td>
<td>4</td>
<td>34</td>
</tr>
<tr>
<td>Chat</td>
<td></td>
<td>No</td>
<td>Increasing</td>
<td>3</td>
<td>34</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Decreasing</td>
<td>3</td>
<td>28</td>
</tr>
<tr>
<td>Contracts</td>
<td>Rounds 11–30</td>
<td>Rounds 11–30</td>
<td>Increasing</td>
<td>4</td>
<td>36</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Decreasing</td>
<td>3</td>
<td>44</td>
</tr>
</tbody>
</table>

Figure 2: Sample screen–shot from contracts stage of experiment

In this period, you **DO** have an outside option. Your outside option is 9.

Table 1: Session and treatment information

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Cheap talk possible?</th>
<th>Contracts possible?</th>
<th>Ordering of outside options</th>
<th>Number of subsessions</th>
<th>Number of subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>No</td>
<td>No</td>
<td>Increasing</td>
<td>3</td>
<td>30</td>
</tr>
<tr>
<td>Chat</td>
<td></td>
<td>No</td>
<td>Increasing</td>
<td>3</td>
<td>34</td>
</tr>
<tr>
<td>Contracts</td>
<td>Rounds 11–30</td>
<td>Rounds 11–30</td>
<td>Increasing</td>
<td>4</td>
<td>36</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Decreasing</td>
<td>3</td>
<td>44</td>
</tr>
</tbody>
</table>

varies across our treatments, in order to provide anecdotal evidence regarding Hypothesis 4. Finally, in Section 4.5, we present and discuss some evidence of other–regarding behaviour in our data.
4.1 The effect of chat and contracts on aggregate–level outcomes

We begin our analysis of the results with a look at aggregate outcome frequencies. The game has three possible types of outcome: efficient outcomes (compatible demands) and the inefficient outcomes of opting out by Player 2 and disagreement (incompatible demands). Table 2 shows the frequencies of efficient outcomes and opt–outs for rounds 11–30, broken down according to communication treatment and the size of the outside option.15

Table 2: Frequencies of efficient outcomes and opt–outs by treatment and outside option, rounds 11–30

<table>
<thead>
<tr>
<th>Outside option (as % of cake)</th>
<th>Efficient outcomes</th>
<th>Opt–outs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline Chat Contracts</td>
<td>Baseline Chat Contracts</td>
</tr>
<tr>
<td>9%</td>
<td>0.867 0.900 0.950</td>
<td>0.055 0.073 0.013</td>
</tr>
<tr>
<td>25%</td>
<td>0.805 0.855 0.931</td>
<td>0.102 0.055 0.050</td>
</tr>
<tr>
<td>49%</td>
<td>0.453 0.809 0.850</td>
<td>0.508 0.145 0.113</td>
</tr>
<tr>
<td>64%</td>
<td>0.148 0.782 0.756</td>
<td>0.781 0.191 0.231</td>
</tr>
<tr>
<td>81%</td>
<td>0.063 0.627 0.769</td>
<td>0.930 0.345 0.231</td>
</tr>
</tbody>
</table>

We see in the table that outcomes in our baseline treatment are broadly comparable to those observed by Binmore et al. (1998), whose experimental design was similar to this treatment in many ways. We find more opt–outs across the board than Binmore et al., but we observe the same effect of the outside option: as it increases, opt–outs become more likely. We are primarily interested, however, in the effects of our communication treatments. Holding constant the size of the outside option, the table shows that efficient outcomes tend to become more likely and opt–outs less likely as we move from the basic game to the chat game and thence to the contracts game. The size of the effect is relatively small when the size of the outside option is small, so that outcomes even in the basic game are sufficiently good that there is little room for improvement. When the outside option is large, reducing strategic uncertainty can lead to a striking improvement in outcomes. For example, when the outside option is 81% of the available surplus, allowing cheap talk results in a nine–fold increase in efficient outcomes, and nearly a two–thirds reduction in opt–outs, while allowing binding contracts results in a ten–fold increase in efficient outcomes and a three–quarters reduction in opt–outs. These relationships are largely confirmed by nonparametric statistical tests. Jonckheere tests for ordered alternatives, which test the null hypothesis of no difference in frequency across the three communication treatments versus an ordered relationship from baseline to chat to contracts (increasing efficient outcomes or decreasing opt–outs), nearly always reject the null hypothesis at the 5% level or better for both efficient outcomes and opt–outs using subsession–level data; exceptions are for opt–outs when the outside option is 9 (where the test fails to reject the null), and for efficient outcomes when the outside option is 9 and opt–outs when the outside option is 25 (where rejection is only at the 10% level).16

Figure 3 shows some additional features of the aggregate results. Shown here are average payoffs for each player type in rounds 11–30 according to the communication treatment and outside option. Also shown is the line segment corresponding to the efficiency frontier; the resulting triangle corresponds to the set of feasible payoff pairs in these bargaining games. Like Table 2, this figure shows sizeable differences across communication treatments (as well

15Not shown are frequencies broken down according to increasing or decreasing outside–option orderings, as nonparametric tests fail to find significant order effects. We acknowledge that failure to reject a null of no effect can be weak evidence that no effect is actually present, but we also note that our qualitative results are unchanged by disaggregating by outside–option ordering, and point out that to the extent that any order effects do exist in our data, this will add a source of variation that will make our statistical tests more conservative. Finally, we also found qualitative results to be robust to using changes from rounds 1–10 to rounds 11–30, instead of frequencies from rounds 11–30 as in Table 2.

16See Siegel and Castellan (1988) for descriptions of this and the other nonparametric statistical tests used in this paper, as well as for most tables of critical values. Some critical values for the robust rank–order tests used below are from Feltovich (2005).
as across outside option amounts). In the basic game, average payoff pairs are well below the efficiency frontier, reflecting sizeable losses due to opt–outs and disagreements. Moving from here to the chat game, and thence to the contracts game, leads to substantial gains in efficiency; in particular, for the contracts game, average payoff pairs are close to (though not on) the efficient frontier. Indeed, Jonckheere tests reject the null hypothesis of no difference in joint payoff across the three treatments in favour of an increase from basic game to chat game to contracts game for all outside option values except 9 (session–level data, $p > 0.10$ when the outside option is 9, $p < 0.01$ for the other four values).

While reducing strategic uncertainty tends to lead to larger total payoffs, Figure 3 shows that the gains are shared differently depending on the size of the outside option. When the outside option is small, decreasing strategic uncertainty leads to roughly equal gains in payoffs for both player type. As the outside option increases, gains to reducing strategic uncertainty accrue more and more to Player 1s, and for the largest outside option, Player 2s actually become slightly worse off as strategic uncertainty is reduced. Jonckheere tests usually reject the null hypothesis of no difference in payoff across the three treatments in favour of an increase from basic game to chat game to contracts game at least at the 5% level; the exceptions are Player 2 payoffs when the outside option is 81 ($p > 0.10$), as expected, but also Player 1 payoffs when the outside option is 9 ($p > 0.10$).

Based on Table 2 and Figure 3, we conclude:

**Result 1** Reductions in strategic uncertainty (from the basic game to the chat game to the contracts game) result in improved outcomes according to several measures: higher likelihood of compatible demands, fewer (inefficient) opt–outs, higher total payoffs and often higher payoffs for both types of player. These improvements are often not only statistically significant, but large in a numerical sense.

That is, our data support Hypothesis 1.

Having ascertained that less strategic uncertainty tends to be associated with better outcomes, we next examine the relative abilities of chat and binding contracts to improve upon outcomes in the baseline treatment. We will look at five statistics – frequencies of efficient outcomes and opt–outs, Player 1 payoff, Player 2 payoff and their sum (which we call efficiency) – and all five of the outside options. For 24 of the 25 possible combinations of statistic and outside option, the outcome is better in our contracts treatment than in our baseline treatment. Noting that in our contracts
treatment, both chat and binding contracts are possible, a natural question to ask is what portion of the improvement from the baseline treatment to the contracts treatment can be attributed to allowing chat on its own (with the remainder attributable to allowing binding contracts, once chat is possible).

We can address this question by comparing results from the chat treatment – where chat is possible but binding contracts are not – to the corresponding results from the baseline and contracts treatment. Roughly speaking, when the value of a statistic from the chat treatment is close to that from the contracts treatment, then one can conclude that most of the improvement (from the baseline to the contracts treatment) is due to allowing chat, with little additional improvement due to allowing binding contracts. On the other hand, if the value from the chat treatment is close to that from the baseline treatment, then one can conclude that allowing chat on its own does not lead to much of an improvement, so that most of the improvement is due to allowing binding contracts. Finally, if the value from the chat treatment is strictly between those of the baseline and contracts treatments, but not close to either, then one can conclude that both allowing chat and allowing binding contracts result in improved outcomes.

To quantify this notion, we take the total amount of improvement of contracts over the baseline, and calculate the share of this improvement that is attributable to chat, for each combination of outside option and statistic. Namely, if $x_{\text{baseline}}$, $x_{\text{chat}}$ and $x_{\text{contracts}}$ are the values of some statistic $x$ (in the baseline, chat and contracts treatments respectively) for a particular outside option, then this share (in percent terms) is given by $100 \cdot \frac{x_{\text{chat}} - x_{\text{baseline}}}{x_{\text{contracts}} - x_{\text{baseline}}}$. (In the one case where the outcome was not better under contracts than in the baseline, this share is undefined.)

According to this normalisation, a share of 0 means that chat does not improve upon the baseline; 100 means that allowing binding contracts adds nothing once chat is possible; and 50 means that chat and binding contracts contribute equally toward improving outcomes from the baseline.

These shares, for each combination of statistic and outside option, are shown in Table 3. Also shown are the results of robust rank–order tests of significance of pairwise differences between the chat treatment and the other two treatments (subsession–level data, not normalised): asterisks indicate significant improvements from the baseline to the chat treatment, while daggers indicate significant improvements from the chat to the contracts treatment. The

Table 3: Improvement on baseline results due to chat alone, as % of improvement due to chat and contracts combined, with significance results, by statistic and outside option – rounds 11–30

<table>
<thead>
<tr>
<th>Size of outside option</th>
<th>9%</th>
<th>25%</th>
<th>49%</th>
<th>64%</th>
<th>81%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Freq. of efficient outcomes</td>
<td>39.6</td>
<td>39.4†</td>
<td>89.7*</td>
<td>104.2*</td>
<td>80.0†</td>
</tr>
<tr>
<td>Freq. of opt–outs</td>
<td>–43.0</td>
<td>91.2</td>
<td>91.7*</td>
<td>107.3*</td>
<td>83.6†</td>
</tr>
<tr>
<td>Efficiency</td>
<td>43.6</td>
<td>33.5</td>
<td>87.8*</td>
<td>99.9*</td>
<td>65.1†</td>
</tr>
<tr>
<td>Player 1 payoff</td>
<td>57.2</td>
<td>47.8</td>
<td>94.0*</td>
<td>107.3*</td>
<td>61.1†</td>
</tr>
<tr>
<td>Player 2 payoff</td>
<td>36.4</td>
<td>25.9†</td>
<td>69.6</td>
<td>82.7*</td>
<td>‡</td>
</tr>
</tbody>
</table>

*: chat significantly better than baseline at 5% level.
†: chat significantly worse than contracts at 5% level.
‡: contracts treatment does not improve upon baseline.

shares in this table are almost always strictly between 0 and 100, meaning that it is nearly always the case that some, but not all, of the improvement of the contracts treatment over the baseline treatment is due to subjects’ ability to communicate, with the remainder of the improvement due to the ability to agree to a binding contract. Also, the table shows examples of significant differences between baseline and chat, as well as between chat and contracts. In sum, based on this table, we conclude:

Result 2 Both types of strategic uncertainty affect behaviour, as reductions in either “coordination–type” strategic
uncertainty (moving from the basic game to the chat game) or in “rationality-type” strategic uncertainty (from the chat game to the contracts game) can improve outcomes.

That is, our data support Hypothesis 2.

4.2 Effects of verbal and written agreements on subsequent behaviour

If the improvement of the chat treatment over the baseline treatment is indeed due to the usefulness of cheap talk for coordinating on equilibrium outcomes, then we would expect to see a substantial difference in the likelihood of an efficient outcome depending on whether or not such coordination took place: that is, whether subjects reached a verbal agreement. (As mentioned in Note 4, we use “verbal agreements” to mean the nonbinding agreements made in the chat treatment, and “written agreements” to mean the binding agreements possible in the contracts treatment.) We test this conjecture by classifying individual bargaining pairs in the chat treatment, as well as the contracts treatment, according to whether the pair reached an agreement prior to play of the basic game, and then noting whether the pair achieved an efficient outcome in that round. The results are shown in Figure 4.

Figure 4: Frequency of efficient bargaining outcomes, by treatment (chat or contracts), outside option and the result of the pre–play stage (agreement reached or no agreement reached) – rounds 11–30

As the figure shows, after a verbal agreement has been made, subsequent efficient outcomes are likely but not certain, as roughly 10% of pairs (88 of 878) fail to make compatible demands after a verbal agreement, with about two-thirds of these failures due to opt-outs (and the remainder due to incompatible demands). The fact that efficient outcomes occur only about 90% of the time after a verbal agreement, rather than 100% of the time, suggests that there is more to strategic uncertainty than simply the possibility of coordination failure. Indeed, this result suggests both that players believe their opponents might renege on a verbal agreement (since some players opt out when they have the chance), and that some players actually do renege on the agreement (since some disagreements occur). By contrast, in the contracts treatment, there is no possibility of subsequent opting out or incompatible demands following a written agreement. In both chat and contracts treatments, when an agreement (verbal or written) is not reached, results are

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17There is some room for interpretation in classifying pairs in the chat treatment. We determined whether a pair reached a verbal agreement based on readings of the “conversation” by the pair. We considered a verbal agreement to have been reached if one bargaining party proposed a division of the cake, the other party accepted, and neither made any subsequent proposals or expressed a change of mind about the accepted proposal; in any other case, we classified the pair as not having reached a verbal agreement. This is a conservative criterion – leaving out (for example) pairs in which one made a proposal and the other implied but did not state assent – but has the advantage that it leaves little room for subjective judgment by the researcher. To the extent that pairs reaching an agreement were not classified as such, Figure 4 will underestimate the difference between pairs reaching agreement and pairs not reaching agreement.
similar in nature to the baseline treatment, with efficient outcomes substantially less likely – though their frequency varies from over two-thirds when the outside option is 9 to below one-third for outside options of 64 and 81. Wilcoxon signed–ranks tests confirm that in both chat and contracts treatments, the frequency of efficient outcomes is higher when an agreement is reached than when one is not (subsession–level data, \( p < 0.05 \)).

Thus, we have:

**Result 3** Verbal agreements are largely, though not completely, self–enforcing: compatible demands subsequently occur roughly 90% of the time, compared with 100% of the time following written agreements. When subjects do not reach agreements in the pre–play stage, they are substantially and significantly less likely to make compatible demands than when they do reach agreements.

That is, our data support Hypothesis 3.

### 4.3 Effects of our treatments on individual behaviour

We continue our examination of how strategic uncertainty affects outcomes with a look at behaviour at a more disaggregated level. Figures 5, 6 and 7 show the results of every individual bargaining pair in rounds 11–30 of the experiment, grouped according to outside option (but with the two lowest outside options pooled, as results are similar in these two cases) and communication treatment. Each pair of demands is represented by a circle, the area of which is proportional to the number of times in which that pair of demands was observed. Outcomes in which Player 2 opts out are also shown in these figures, as circles above the dashed line; in these cases no Player 2 demand is made, but Player 1’s demand can be seen. These circles also have areas proportional to the number of times in which that Player 1 demand and an opt–out by Player 2 was observed. For reference, the segment corresponding to demands summing to 100 is also shown (so that demand pairs above this segment correspond to disagreements), as are the outcomes corresponding to the split–the–difference, deal–me–out and equal–split focal points.

In Figure 5 we see, consistent with earlier results (see Table 2), a fairly substantial proportion of inefficient outcomes (opt–outs and disagreements) in the baseline treatment. Moreover, we can see another effect of strategic uncertainty: even when the players in this treatment do reach an efficient outcome, their demands often add up to substantially less than the cake size, reflecting “hedging” by one or both players against the risk that they’ve underestimated their opponent’s demand. Hedging by a player has no effect on efficiency (under the rules, any amount left on the table is split equally between the two bargaining parties), but is costly to the player individually (at least ex post), since each decrease of one point in a player’s demand results in a loss of one–half of a point. By contrast, much less hedging is apparent in Figure 6 and especially Figure 7.

The varying extent of hedging across these treatments is summarised in Table 4, which shows the number and proportion of efficient outcomes in which the sum of the two players’ demands is less than 90%, between 90% and 95% and more than 95% of the cake, disaggregated by treatment and outside option (but again, with results from the two lowest outside options pooled).

Even for the cases of outside options of 9, 25 and 49 – in which the 50–50 split is a fairly compelling focal point (and as visible in the left two panels of Figure 5, a common outcome) – about two–fifths of efficient outcomes in the baseline treatment involve demands totalling 95% of the cake or less, and a fifth involve demands totalling less than 90%. When the outside option is large, nearly all efficient outcomes involve a substantial amount of hedging (though sample sizes are small here). By contrast, an overwhelming majority of efficient outcomes in the chat and contracts treatments involve demands summing to nearly the whole cake, irrespective of the outside option amount. Nonparametric pairwise tests confirm that frequencies of demands totalling at least 96% of the cake (conditional on an efficient outcome) are significantly higher at the 5% level or better in both chat and contracts treatments compared to the baseline treatment for all five outside option values (one–tailed robust rank–order test, session–level data).
In contrast, there is a much smaller apparent decrease in hedging as we move from the chat treatment to the contracts treatment in Table 4, and what differences can be seen are seldom significant. At first glance, this may seem surprising, since one would think giving subjects the opportunity to make binding written agreements would eliminate the need for hedging that would otherwise exist. However, as the table shows, allowing even verbal agreements already eliminates much of this need – though as mentioned previously, Player 2s are still usually more likely to opt out in the chat treatment than in the contracts treatment, which of course is another way of ruling out the need to hedge.

We therefore have:

**Result 4** When strategic uncertainty is high (the basic game), subjects often reach agreement by hedging – reducing demands to avoid disagreement – so that the demands of a pair often add up to substantially less than the available surplus. When strategic uncertainty is lower (the chat and contracts games), subjects are less likely to hedge, so that the demands of a pair, if compatible, add up to nearly all of the available surplus. These decreases in the frequency of hedging are both statistically significant and large in a numerical sense. There is a small, but usually insignificant,

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Figure 6: Individual outcomes in chat treatment (Player 1 demand, Player 2 demand or opt–out) in comparison with theoretical predictions, by outside option, rounds 11–30

Notes: Areas of circles are proportional to the number of observations of the outcome.
SD = split–the–difference prediction; DO = deal–me–out prediction; ES = equal–split prediction

difference in hedging between the chat and contracts treatments.

4.4 Effects of chat and contracts on the division of surplus between bargainers

Finally, we examine the way the surplus is divided between players reaching efficient outcomes. Recall from Section 2 the two commonly discussed focal points in bargaining games with outside options: “split–the–difference” (Player 2 gets his outside option plus half of the remainder) and “deal–me–out” (Player 2 gets the larger of his outside option and half of the cake). We add to these a third potential focal point, “equal–split”, and we attempt to classify efficient outcomes – specifically, the payoff of Player 2 (who has the outside option) – according to whether they are consistent with one or more of these focal points. We limit classification to those pairs of compatible demands summing to 90% or more of the cake; as Table 4 showed, this category comprises roughly 95% or more of compatible demands in the chat and contracts treatment, and about 75% of compatible demands in the baseline treatment.\footnote{Our conclusions here are qualitatively unaffected either by narrowing our definition to those with demands totalling at least 96% of the cake, or by broadening it to including all efficient outcomes.} Within these pairs, we define an outcome to be consistent with a given focal point if Player 2’s payoff is within 5 units (5% of the cake)
Figure 7: Individual outcomes in contracts treatment (Player 1 demand, Player 2 demand or opt–out) in comparison with theoretical predictions, by outside option, rounds 11–30

Notes: Areas of circles are proportional to the number of observations of the outcome.
SD = split–the–difference prediction; DO = deal–me–out prediction; ES = equal–split prediction

of that focal point. For example, when the outside option is 49, split–the–difference implies a division of 25.5% for Player 1 and 74.5% for Player 2; so, we consider any outcome in which Player 2 earns between 69.5% and 79.5% to be consistent with split–the–difference.

Figure 8 charts the distributions of Player 2 payoffs for all three treatments and for the outside options 25, 49, 64 and 81. Note first that outcomes that give Player 2 less than about half of the cake or more than split–the–difference are rare in all treatments and for all outside options. Also, efficient outcomes in the baseline treatment nearly always involve an equal division of the cake, which is also the deal–me–out outcome (as well as the prediction of both risk dominance and the Harsanyi and Selten (1988) equilibrium selection criterion); this happens over 95% of the time when the outside option is 25 and 90% of the time when it is 49. (There are only five baseline–treatment observations when the outside option is 64, and none when it is 81.) There is little or no evidence of efficient equilibria consistent with split–the–difference in the baseline treatment, possibly indicating that in the absence of communication, Player 2s

---

2⁰We omit the outside option of 9, for which it is difficult to distinguish the focal points; both deal–me–out and equal–split predict a payoff of 50, and split–the–difference predicts 54.5. For the remaining four outside options, the intervals consistent with these three focal points either coincide or are mutually exclusive.
are reluctant to demand such high amounts, understanding how unlikely it is that Player 1 will make a correspondingly low demand.

Distributions of Player 2 payoffs under the chat and contracts treatments are similar to each other. When the outside option is either 25 or 49, the predictions of equal–split and deal–me–out coincide, so it is not possible to distinguish between them. For both of these cases, more than half of efficient outcomes are consistent with their common prediction, though a sizeable minority (10–20%) is consistent with split–the–difference. For both of these cases, more than half of efficient outcomes are consistent with their common prediction, though a sizeable minority (10–20%) is consistent with split–the–difference. For the larger outside options (64 and 81), equal–split, deal–me–out and split–the–difference all make distinct predictions. In these cases, there are fairly substantial frequencies consistent with both deal–me–out and split–the–difference. Somewhat surprisingly, there are nonnegligible frequencies of equal–split outcomes here as well (more so in the contracts treatment than in the chat treatment), even though this requires Player 2 turning down a higher monetary payoff from opting out. Finally, when the outside option is 49 or 64, there is a substantial fraction of efficient outcomes in between the deal–me–out and split–the–difference predictions, suggesting some bargainers are compromising between a Player 1 preference for deal–me–out and a Player 2 preference for split–the–difference. This is not seen when the outside option is 25.

Hoffman and Spitzer (1982) and Harrison and McKee (1985) also find substantial numbers of players accepting equal divisions of the cake even when this involves forgoing more lucrative outside options. Hoffman and Spitzer (1985) consider the possibility that subjects choose fair divisions in these situations because they don’t believe that being randomly assigned the outside option gives them the “entitlement” necessary to allow them to take advantage of it. However, they find that having players “earn” the outside option by winning a game of skill does not in fact lead to significantly fewer equal splits, nor does giving Player 2s “moral authority” by framing the instructions differently, though the combination of these two treatments does substantially (and significantly) reduce the frequency of equal splits.

When the outside option is 64, a large fraction of these “in–between” agreements are 70–30 splits, which are (just) outside the deal–me–out interval [59,69], but which arguably might nevertheless be considered consistent with deal–me–out. We left these observations outside the deal–me–out interval in Figure 8 for the sake of uniform definitions across all outside options and focal points. We do note that including 70–30 splits in the deal–me–out interval would change the frequencies of efficient outcomes consistent with deal–me–out greatly: from 25.9%

### Table 4: Sum of players’ demands, as % of cake size, by treatment and outside option (all efficient outcomes, rounds 11–30)

<table>
<thead>
<tr>
<th>Size of outside demand (% of cake size)</th>
<th>Number of occurrences (frequency)</th>
<th>Size of outside demand (% of cake size)</th>
<th>Number of occurrences (frequency)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Sum of players’ demands (as % of baseline contracts option)</td>
<td>Sum of players’ demands (as % of baseline contracts option)</td>
<td>Sum of players’ demands (as % of baseline contracts option)</td>
</tr>
<tr>
<td></td>
<td>Baseline treatment</td>
<td>Chat treatment</td>
<td>Contracts treatment</td>
</tr>
<tr>
<td></td>
<td>0–89</td>
<td>90–95</td>
<td>96–100</td>
</tr>
<tr>
<td>9% or 25%</td>
<td>38 (17.8%)</td>
<td>10 (5.2%)</td>
<td>9 (3.0%)</td>
</tr>
<tr>
<td>49%</td>
<td>13 (22.4%)</td>
<td>5 (5.7%)</td>
<td>6 (4.4%)</td>
</tr>
<tr>
<td>64%</td>
<td>13 (68.4%)</td>
<td>4 (4.7%)</td>
<td>0 (0.0%)</td>
</tr>
<tr>
<td>81%</td>
<td>8 (100.0%)</td>
<td>3 (4.3%)</td>
<td>3 (2.5%)</td>
</tr>
<tr>
<td>Total</td>
<td>72 (24.2%)</td>
<td>22 (5.1%)</td>
<td>18 (2.7%)</td>
</tr>
</tbody>
</table>
Figure 8: Distribution of Player 2 share of payoffs as % of the cake size, in comparison with theoretical predictions (all efficient outcomes with demands totalling at least 90% of the cake, by treatment and outside option excluding 9% – rounds 11–30)

However, and when the outside option is 81, there is not enough room between the predictions of deal–me–out and split–the–difference for such compromises to be observable.

We therefore have:

Result 5 In the baseline treatment, the vast majority of efficient outcomes are consistent with the equal–split or deal–me–out focal points, with nearly none consistent with split–the–difference. In the chat and contracts treatments, there are sizeable fractions of efficient outcomes consistent with all three of these focal points, and for some outside options, there is also a substantial frequency of efficient outcomes between the predictions of deal–me–out and split–the–difference.

That is, the data from our baseline treatment support Hypothesis 4, while the support from the data in our chat and contracts treatments is more equivocal.

4.5 Evidence of other–regarding preferences

A few of the results discussed above suggest that at least some aspects of behaviour in our experiment are difficult to explain under the standard assumption of own–payoff maximisation. Chief among these is the finding mentioned earlier (see Figure 8) that equal and near–equal splits occur a nonnegligible proportion of the time, even when a player has an outside option appreciably larger than half of the cake. More generally, we find that sometimes a bargainer (in the role of Player 2) either demands less than he could guarantee by simply opting out, or agrees to a contract with the same implication, in the process raising the other player’s payoff substantially. Such behaviour suggests that at least of efficient outcomes to 61.7% in the chat treatment, and from 26.4% to 52.9% in the contracts treatment, with corresponding decreases for the interval of “in–between” outcomes (from 55.6% to 19.8% in the chat treatment and from 39.7% to 13.3% in the contracts treatment). There would be little effect on our qualitative results, though: deal–me–out would look even better compared to split–the–difference, but there would still be nonnegligible proportions of split–the–difference and in–between outcomes.
some of the subjects have a dislike for inequality, even when is in their favour – as in the inequity aversion model of Fehr and Schmidt (1999).

Our experiment is not well designed for examining other–regarding preferences in detail (in the sense of being able to distinguish between competing specifications for such preferences, or estimating parameters for a given specification), since nearly all of Player 1 actions and (depending on the outside option) many of Player 2 actions are consistent with own–payoff maximisation. We also suspect that these other–regarding preferences are to some extent situation–dependent – as opposed to arising purely from innate tastes – since the frequency with which Player 2s demand less than the outside option varies with the treatment: 9.6% of observations in the baseline treatment with an outside option of more than half of the cake, compared with 14.0% in the chat treatment and 20.0% in the contracts treatment. This association between opportunities for communication and other–regarding behaviour is consistent with dictator–game results observed by Bohnet and Frey (1999) and Rankin (2006). Bohnet and Frey found that allowing information to pass between dictators and recipients led to increased offers by the dictators, and the size of the effect depended on the amount and form of the information. Rankin (2006) found that requests by the recipients for specific shares of the cake tended to lead to increased offers if interaction was anonymous, but decreased offers if the requests were hand–delivered by the recipient to the dictator (implying a modicum of face–to–face interaction). The authors of both papers conjecture that their treatments affect the amount of social distance between dictator and recipient, and lower levels of social distance are associated with higher dictator game offers (i.e., increased other–regarding behaviour).

It follows naturally that in our experiment as well, allowing chat or binding contracts might have the effect of decreasing the amount of social distance between the bargainers, making Player 2s reluctant to utilise all of their bargaining power, and leading to demands less than outside options. However, we note that such an effect only seems to apply for a minority of subjects. Figure 9 shows some information about the distribution of other–regarding behaviour in the experiment. As before, our measure of a subject’s other–regarding behaviour is the frequency with

![Figure 9: Other–regarding behaviour – individual subjects’ observed frequencies of demands less than the outside option (outside options of 64% and 81%, all rounds) as Player 2, cumulative distribution functions, by treatment](image)

which that subject demanded less than the outside option (including agreeing to a share smaller than the outside option in the contracts treatment).

The figure shows three important characteristics of other–regarding behaviour in our experiment. First, as mentioned earlier, the prevalence of this behaviour is sensitive to the treatment: the c.d.f. for the contracts treatment lies almost entirely to the right of that for the chat treatment, which in turn lies almost entirely to the right of that for the baseline treatment. Indeed, a Jonckheere test on the subsession–level data allows us to reject the null hypothesis that
this measure of other–regarding behaviour is equally prevalent in all three treatments, in favour of the alternative that it is most prevalent in the contracts treatment and least so in the baseline treatment ($p < 0.01$).

Second, other–regarding behaviour is not seen at all in a large fraction of individual subjects, as the proportion who never demand less than their outside option in this situation varies from 44% of subjects in the contracts treatment to 69% in the baseline treatment. Third, even for subjects who do display such behaviour with positive frequency, this frequency varies tremendously from individual to individual: over three–quarters of all subjects do so either only once or not at all, while just over a tenth do so in more than half of the opportunities available to them.

In summary:

**Result 6** There is evidence of other–regarding preferences in all three treatments. Other–regarding behaviour varies significantly across treatments, with its prevalence increasing from the baseline to the chat to the contracts treatment. There is also substantial heterogeneity across subjects, with over half never showing such behaviour.

5 Discussion

Empirical tests of bargaining models often find, contrary to much applied theoretical work, a sizeable fraction of inefficient outcomes such as disagreements or costly delays. Strategic uncertainty might be one of the primary causes of inefficiency in bargaining; indeed, in one–shot complete–information bargaining, it might be the main cause. If so, then changes to the environment that reduce the level of strategic uncertainty should improve matters. In our experiment, we consider two ways of reducing strategic uncertainty in a Nash demand game where one of the players has an outside option. In our “chat” treatment, the bargaining parties are able to send nearly unrestricted cheap–talk messages to each other prior to playing the bargaining game, though they cannot write binding contracts. In our “contracts” treatment, the two parties can additionally propose and accept binding contracts – possibly obviating the need to play the subsequent bargaining game. We argue that the chat treatment reduces the amount of strategic uncertainty in the game relative to our baseline treatment (where players simply play the bargaining game, with no communication beforehand) by facilitating coordination on one of the many Nash equilibria of the game (thus lessening “coordination–type” strategic uncertainty). The contracts treatment, for its part, reduces strategic uncertainty relative to the chat treatment by allowing verbal agreements to become binding, so rendering moot any worry that the other player subsequently reneges on the agreement (lessening “rationality–type” strategic uncertainty).

Our results show a striking connection between changes to the level and nature of strategic uncertainty in the game and changes in several aspects of outcomes. Less strategic uncertainty is associated with more efficient outcomes, fewer inefficient opt–outs and less hedging (reducing demands to avoid disagreement). Nontrivial (and statistically significant) gains are found when moving from the baseline treatment to the chat treatment and thence to the contracts treatment, suggesting that both “coordination–type” and “rationality–type” strategic uncertainty have effects. Moreover, we find that verbal agreements in the chat treatment are typically, but not always honoured, leading to efficient outcomes roughly 90% of the time – less than the corresponding 100% in the contracts treatment (highlighting the impact of “rationality–type” strategic uncertainty), but substantially more often than when an agreement is not reached. We also find that the way the surplus is divided between the bargaining parties varies systematically across these treatments. In the baseline, equal–split and deal–me–out are predominant. In the chat and contracts treatments, we find support for all three of the common focal points: equal–split, deal–me–out and split–the–difference, as well as many outcomes in between these last two.

Several remarks are worth making at this point. First, even our baseline treatment satisfies the conditions laid out by Coase (1960) and elaborated by Hoffman and Spitzer (1982) – primarily, complete information, property rights and zero transaction costs – that are supposed to ensure efficient bargaining outcomes. Our results show, however, that efficient outcomes are most definitely not assured. The obvious implication is that these conditions are in fact
not sufficient; rather, it is additionally necessary for the bargaining situation to give parties ample opportunities for communication.\textsuperscript{23} Without such opportunities, strategic uncertainty imposes real costs on the parties – either in aggregates (lower efficiency from disagreements or opting out), or simply borne by individuals (lower individual payoffs due to hedging demands to avoid disagreements). These costs tend to be relatively small when the game is nearly symmetric (low outside option amounts) but grow large as the game becomes increasingly asymmetric. Relatedly, our results suggest that it is not innocuous to assume that a complex bargaining situation can be reduced to a simple one by concentrating attention solely on bargainers’ final offers, and leaving out potentially valuable information that can be gleaned from earlier communication (even when it is nonbinding).

Second, our results speak to the conjecture by Binmore et al. (1998) that efficiency can be sensitive to whether bargainers are able to commit to binding contracts before the outside option must be forgone. Comparison of the data from our contracts and chat treatments – which differ only in whether the outside option is still available when a binding agreement can be signed (yes and no respectively) – suggests that the timing of the outside option does indeed have the effect they predicted: higher efficiency in the former case. However, we recall that the difference between these two treatments is often smaller than between either and the baseline treatment – reflecting the substantial gains to be made from simply allowing bargaining parties to communicate and thus reach an accord, even if it is not binding. Examination of the power of chat, relative to contracts, to improve outcomes might also improve our understanding of negotiations in the outside world as well as in the laboratory; for example, environments where chat does fairly well on its own might correspond to situations where mediation (as opposed to binding arbitration) is likely to be observed.

Finally, we acknowledge that there is plenty of room for further research into strategic uncertainty. Crawford (1990) writes about the opportunity for a “theoretical and practical understanding of how strategic uncertainty and cheap talk interact” (p. 218). Two decades later, this opportunity remains available. We suggest our findings as a starting point. Based on these, it seems that any treatment of strategic uncertainty would need to be grounded in standard theory, as comparison of our baseline and chat treatments suggests that a substantial part of strategic uncertainty may be due simply to the coordination issues inherent in games with multiple equilibria. However, it seems that a behavioural game theoretic component is also required, as comparison of our chat and contracts treatments (for example, the fraction of verbal agreements that are not honoured) suggests that at least some of strategic uncertainty derives from an (accurate) imperfect confidence that the assumptions underlying the common knowledge of rationality are satisfied. We hope that our paper provides a useful, if small, step towards a better understanding of strategic uncertainty and its effects.

\textsuperscript{23}One could alternatively argue that Coase’s condition of “zero transaction costs” entails limitless opportunities for communication, though this requirement would seem to eliminate the possibility that this condition is ever satisfied in any actual bargaining situation, thus relegating Coasian bargaining to the status of a theoretical curiosity.
References


Appendix A: instructions and screenshots from the experiment

Shown here is a sample demand/opt-out screen from all treatments, followed by the full instructions from the chat treatment (which includes a sample screenshot). The instructions from the other treatments are available from the corresponding author upon request.
Instructions: first part of experiment

You are about to participate in an experiment in the economics of decision-making. If you follow these instructions carefully and make good decisions, you might earn a considerable amount of money that will be paid to you in cash at the end of the session. If you have a question at any time, please feel free to ask the experimenter. We ask that you not talk with the other participants during the experiment.

This part of the experimental session is made up of 10 rounds. Each round consists of one play of a simple bargaining game, played between two players via the computer. In every round, you will be randomly matched with another player, with whom you will play this bargaining game. You will not be told the identity of the person you are matched with in any round, nor will they be told your identity—even after the end of the session. Your score in each round will depend on your choice, and in some cases, the choice of the person you are matched with.

The basic bargaining game is as follows. Each player makes a claim for his/her own share of a prize worth 100 points. These claims are made simultaneously, so neither player knows the other’s claim when making his/her claim. If these claims total 100 or less, each gets the amount of his/her claim, plus half of any remaining amount. (For example, if the claims total 78 points, then each gets his/her claim, plus 11 more points—half of the remaining 22.) If the players’ claims total more than 100, then each gets zero.

The game you will be playing has one additional detail: one of the two players has an “outside option”—an amount of money available, which he/she can choose instead of playing the basic bargaining game. If the outside option is chosen, that player receives the amount of the outside option, and the other player receives zero. (For example, if you have an outside option worth 37 points, and you choose it, you will earn 37 points and the player matched to you will earn 0.) In each round, either you or the player matched to you—but not both—will have an outside option available. At the beginning of a round, the computer randomly chooses which of you will have the outside option, and this will be announced to both of you. At this time, the computer will also announce to both of you what the size of the outside option is.

Sequence of Play: The sequence of play in a round is as follows.
(1) The computer randomly matches you to another player, and randomly determines which of you will have an outside option available. You and the player matched with you are both informed of who has the outside option and what the size of the outside option is.
(2) The player with the outside option available chooses whether to take it, and if not, chooses a claim for a share of the 100 points. The player without the outside option chooses a claim for a share of the 100 points.
(3) The round ends. You receive the following information: your own choice(s), the choice(s) made by the player matched with you, your own payoff for the round, the payoff of the player matched with you.

After this, you go on to the next round.

Payments: At the end of these ten rounds, two of the rounds are chosen randomly for each participant. Each participant is paid the total number of points he/she earned in those two rounds, at an exchange rate of 1 point = £0.02. In addition, each participant will receive £2 for completing the session, and there will be additional opportunities for payments in the rest of the session. Payments are made in cash at the end of the session.
Instructions: second part of experiment

The procedure in this part of the experiment is similar to that in the first part. You will play a bargaining game ten times, and the participant matched with you will still be chosen randomly in every round. However, you now have an opportunity to communicate with the player matched to you in a computer “chat room” before playing the bargaining game.

A sample chat room screen is shown here. To write a message, make sure your cursor is active in the blue rectangle in the bottom-right corner of your screen, and type normally using the keyboard. To send the message, press the Enter key. Sent messages will appear in the box on the right side of your screen and on the screen of the player matched to you. If the player matched to you sends a message, this will also appear in the box on your screen. Other players in the experiment will not be able to see your messages, and you will not be able to see theirs.

There is a total of 2 minutes available for chat in each round. You may send as many or as few messages as you wish during that time. We ask that you do NOT send messages containing:

(a) personal or identifying information about yourself;
(b) physical threats.

Any other kinds of messages are acceptable, including messages about the game you are playing.

You may end the chat session before the 2 minutes are over, by clicking on the button labelled “End this stage” in the bottom-left corner. Once you or the player matched with you has clicked this button, no more messages can be sent in the current round, though you will still be able to view all of the messages that were sent. Once all participants have ended the chat session, or when the 2 minutes are over, the bargaining game will begin.

At the end of this part of the experiment, two of these ten rounds will be chosen randomly for each participant, but now, each participant will be paid the total number of points he/she earned in those two rounds, at an exchange rate of 1 point = £0.10 instead of 1 point = £0.02. Your earnings from this part of the experiment will be added to your earnings from the first part.
Instructions: third part of experiment

The procedure in this part of the experiment is exactly the same as that in the second part. You will play the same bargaining game as in the second part, for ten rounds. The participant matched with you will still be chosen randomly in every round. At the end of this part of the experiment, two of these ten rounds will be chosen randomly for each participant, and each participant will be paid the total number of points he/she earned in those two rounds at an exchange rate of 1 point = £0.10. Your earnings from this part of the experiment will be added to your earnings from the previous parts.