Efficiency, Equality and Labelling: An Experimental Investigation of Focal Points in Explicit Bargaining

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We investigate Schelling's hypothesis that payoff-irrelevant labels ('cues') can influence the outcomes of bargaining games with communication. In our experimental games, players negotiate over the division of a surplus by claiming valuable objects that have payoff-irrelevant spatial locations. Negotiation occurs in continuous time, constrained by a deadline. In some games, spatial cues are opposed to principles of equality or efficiency. We find a strong tendency for players to agree on efficient and minimally unequal payoff divisions, even if spatial cues suggest otherwise. But if there are two such divisions, cues are often used to select between them, inducing distributional effects.

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In *The Strategy of Conflict*, Thomas Schelling (1960: 53, 67–74) proposes the hypothesis that the outcomes of bargaining problems can be systematically influenced by 'incidental details' or (as they would now be called) properties of framing or labelling. These labelling properties or *cues* have no direct relationship to payoffs and are not represented in standard game-theoretic models, but they nevertheless provide a means by which players can coordinate their expectations. Although much of his theoretical analysis and all of his informal experiments are concerned with simultaneous-move games without communication, Schelling claims that this hypothesis applies to 'explicit bargaining with full communication and enforcement'. He acknowledges that communication provides mechanisms for coordination that are not present in his simple experiments, but still argues that framing properties can remain significant in explicit bargaining. Most subsequent bargaining theory has not used this idea; but if real-world bargaining *is* substantially affected by payoff-irrelevant cues, a satisfactory theory of bargaining needs to take account of that fact. Our paper reports experimental tests of Schelling's hypothesis in a range of different bargaining games.

To date, there has been surprisingly little experimental investigation of the role of payoff-irrelevant cues in explicit bargaining games.[1] It is now well established that equilibrium selection in pure coordination games is strongly influenced by how strategies are labelled, and that players of these games often achieve high degrees of coordination by making use of concepts of salience (e.g. Judith Mehta, Chris Starmer, and Robert Sugden 1994; Michael Bacharach and Michele Bernasconi, 1997; Nicholas Bardsley et al., 2010). For games in which the players have a common interest in coordinating but have conflicting preferences between equilibria, the evidence is more mixed. Investigating matching games (that is, games in which each player sees the same set of labels and chooses one of them, with the aim of making the same choice as her co-player(s)), Vincent P. Crawford,

[1] Following Schelling, we distinguish between 'explicit' and 'tacit' bargaining. In explicit bargaining problems, the players have access to a rich message space, can communicate with one another in real time, and are able to reach binding agreements. In tacit bargaining problems, the players choose their strategies simultaneously without prior communication. Explicit bargaining problems can be modelled either as extensive-form non-cooperative games or as cooperative games; tacit bargaining problems are modelled as normal-form non-cooperative games.

Uri Gneezy, and Yuval Rottenstreich (2008) find a strong tendency for coordination on salient labels if the players' interests are fully aligned, but that these effects sometimes disappear when that alignment is less than perfect. In contrast, when mixed-motive games are framed as problems of tacit bargaining over the division of a stock of value, payoff-irrelevant cues have been found to have strong effects (Andrea Isoni et al., 2013). But it is still an open question whether conclusions about the role of payoff-irrelevant cues in tacit bargaining extend to explicit bargaining.

In a tacit bargaining game, the players have only one chance to reach agreement. By contrast, when bargainers can communicate with one another and make offers and counter-offers, additional mechanisms for the coordination of expectations are available, and payoff-based principles such as efficiency and equality might have more influence. Joseph Farrell's (1987) analysis of cheap talk shows that, for games with a Battle of the Sexes structure, pre-play communication can facilitate agreement on one of the two pure-strategy equilibria; the symmetry between the two players is broken by their probabilistic choice of messages in the symmetric mixed-strategy equilibrium, rather than by asymmetric cues.[2] These considerations might seem to support the intuition that payoff-irrelevant cues lose their power when bargaining is explicit.

However, Schelling offers a backward-induction argument against that intuition. This argument applies to bargaining that takes place over a finite interval of time, ending at a fixed deadline or 'midnight bell'. Because of the deadline, a 'perfectly move-symmetrical bargaining game' must have a final round of simultaneous play. Thus, a game with communication 'necessarily gives way, at some definite penultimate moment, to a *tacit* (non-cooperative) bargaining game.' Schelling then

[2] Tore Ellingsen and Robert Östling (2010) offer an alternative model of the role of communication in coordinating expectations. This model uses level-*k* theory and assumes that, if otherwise indifferent between strategies, players prefer to send honest messages.

argues: ‘Each player must be assumed to know this and may, if he wishes, by simply avoiding overt agreement, elect to play the tacit game instead’ (1960: 267–272, italics in original). If it is common knowledge that the tacit game, if reached, will be resolved in a particular way, then neither player will agree to an inferior payoff in the preceding negotiation. So if it is common knowledge that tacit bargaining problems are resolved by the use of payoff-irrelevant cues, explicit communication may be redundant.

Our experiments are designed to test the robustness of Schelling’s hypothesis in a range of cheap-talk settings in which payoff-irrelevant cues sometimes support and sometimes oppose efficiency and equality. More precisely, we test whether, in the presence of a variety of trade-offs between efficiency and equality, the existence of a payoff-irrelevant cue that suggests a particular solution to a bargaining problem tends to shift the actual outcome in the direction of that solution.

It is important to understand what we mean when we say that some feature of a game is *payoff-irrelevant*. In game theory, players are assumed to have common knowledge of the payoffs that would result from each possible profile of strategies. A payoff-irrelevant feature provides no additional information about the payoffs that are associated *with given strategy profiles*. However, it may influence players’ strategy choices (and hence the actual payoffs they derive from playing the game). In a game with two or more equilibria, a payoff-irrelevant feature may make one equilibrium particularly salient, thus suggesting a solution to the problem of equilibrium selection. A commonly recognized solution to such a problem (whether suggested by payoff-irrelevant or payoff-based features of the game) is a *focal point*.

If an experimental test of Schelling’s hypothesis is to be adequately controlled, the cues to be investigated must be payoff-irrelevant, not only with respect to traditional game-theoretic assumptions about players’ utility functions, but also with respect to other recognized theories of utility. In particular, since subjects might have preferences for equality in experimental earnings, as proposed by Ernst Fehr and Klaus Schmidt (1999) and Gary Bolton and Axel Ockenfels (2000), those cues should not provide information about the distribution of material payoffs between subjects for given strategy profiles – even if that information is irrelevant from the perspective of axiomatic bargaining theory under the assumption of self-interest. Thus, the design developed by Alvin E. Roth and Michael W.K. Malouf (1979) and by Roth and J. Keith Murnighan (1982), which investigates the effects on the outcomes of explicit bargaining games of varying information about the distribution of material payoffs, would not be suitable for our purposes. Nor should the cues to be investigated provide information about the players that might suggest differences in their moral entitlements to rewards. Thus, it would not be appropriate for us to use a design such as that of Simon Gächter and Arno Riedl (2005), which tests whether bargaining outcomes are influenced by players' perceptions of 'moral property rights', induced by relative performance in a general knowledge quiz. We need an experimental environment in which the cues that discriminate between alternative bargaining outcomes are perceived by the players *only* as a device for selecting between equilibria, and not as a source of information about the subjective values of those equilibria. Our experiment is designed so that the relevant cues meet this requirement.

Our main findings are the following. Communication is highly effective in coordinating players' expectations. Across all games, we find a strong tendency for players to settle on the least unequal of the efficient payoff divisions, whether or not that division is suggested by payoff-irrelevant cues. But when no equal and efficient division is feasible, those cues often influence which of the least unequal divisions is selected, and hence which player gets the larger payoff.

We begin by explaining the principal features of our design (in Section I) and by setting out formal hypotheses, in the spirit of Schelling's analysis, that this design can test (Section II). We then describe the details of the main treatment used in the experiment (Section III) and report the results of our hypothesis tests (Section IV). Using a more inductive approach, we investigate the dynamics of bargaining and various other aspects of the bargaining process (Section V). We briefly describe a control treatment which tests the robustness of our method of representing the absence of salient payoff-irrelevant cues (Section VI). Finally, we draw general conclusions (Sections VII and VIII).

I. The Bargaining Table Design

Our experimental design was chosen with the following considerations in mind. We wanted a bargaining protocol that had the 'feel' of a bargaining problem to the subjects who engaged in it. We wanted to allow bargainers to communicate with one another simply and freely, up to a fixed deadline, and to be able to make binding agreements during this period. We wanted to be able to introduce cues that were clearly payoff-irrelevant and yet would be perceived as a natural part of the bargaining problem, rather than merely as extraneous labels. And we wanted to use cues that were known to work as focal points in tacit bargaining and pure coordination games, so that if we were to find that those cues were ineffective in our explicit bargaining games, that finding could not be attributed to the cues being insufficiently salient.

Figure 1 shows the displays used in the 30 bargaining games that featured in our *main treatment*, labelled G1 to G30. Each game used a *bargaining table* format, similar to that used by Isoni et al. (2013) to investigate tacit bargaining. Each bargaining table was a 9×9 grid on which were superimposed two colored squares, representing the players' respective 'bases', and a number of 'discs'. On each disc there was a number, denoting its money value (in UK pounds). The players communicated anonymously with one another through computer terminals. Each player's screen showed the bargaining table, rotated so that her own base appeared at the bottom. On the screen, players were labelled as ‘you’ (with a red base) and ‘other’ (with a grey base) so that there was no commonly-known asymmetry between their descriptions. (For purposes of exposition, we will refer to the player whose base is on the left in Figure 1 as L, and the player on the right as R.) The players were invited to negotiate an agreement on how to divide the discs between them. The full text of the instructions is reproduced in the Online Appendix.[3]

[Figure 1 near here]

Negotiation took place continuously in real time, for a maximum of 90 seconds. At any time in this 90s interval, either player could ‘claim’ any disc by clicking on it with her mouse; any claim could be cancelled at any time by a further click. Each player could claim as many discs as she wished. Discs could be claimed again after claims had been cancelled. Each player’s screen continuously showed both players’ current claims. Discs on which there were no current claims were shown in white. On each player’s screen, the discs she claimed turned red and were connected to her base by a red line; the discs claimed by the other player turned grey and were connected to the other person’s base by a grey line. When a disc was claimed by both players, it turned yellow and started to blink, while still connected to both bases by colored lines.

At any time, either player could report that she was willing to agree to the claims currently made by her and her co-player. This was done by ticking an ‘accept box’. Throughout the negotiation time, each player could see both her own box and her co-player’s. An ‘accept’ tick could be cancelled at any time prior to an actual

[3] The displays in Figure 1 show the bargaining tables before any rotation was applied. The Figure contains the following features that were not part of the displays seen by participants. First, columns are numbered from –4 to 4 from left to right, and rows are numbered from –4 to 4 from top to bottom. Second, in each game discs are labelled from d_1 to d_n, where n is the number of discs in the game. These features are included to facilitate the reading of the datasets (*insert web address*). Third, the bases are labelled ‘L’ and ‘R’ rather than ‘you’ and ‘other’. Finally, disc values are shown with the omission of the ‘£’ symbol (which was displayed in the experiment).

agreement, and was automatically cancelled if either player changed her claims. An agreement was *sealed* if and when there was a tick in each player's accept box. At this stage, if any disc was claimed by both players, each player's payoff from the game was zero; otherwise, each player's payoff was the total value of the discs she had claimed. If no agreement had been reached after 90s, players' payoffs were calculated as if they had agreed to the set of claims that was current when the time allowed for bargaining expired. This design feature allows co-players to maintain conflicting claims right up to the final seconds of the game, while keeping open the possibility of achieving positive payoffs through a very late change of claims by one player. Such a concession would typically require only a few mouse clicks. Notice also that this bargaining protocol allows players to agree to leave some discs unclaimed by either of them, or to agree that both payoffs be zero.[4] (A screenshot of the bargaining table is shown in the online appendix.)

There are some significant differences between our bargaining protocol and those used by other researchers such as Roth and Murnighan (1982), Robert Forsythe, John Kennan, and Barry Sopher (1991), and Gächter and Riedl (2005).[5] One difference is that the mechanism for making claims in our design provides a structured language for non-binding communication between the players, prior to their using the agreement mechanism. Because these 'messages' are sent by mouse clicks and are immediately displayed on players' screens in an intuitive way, they can be exchanged very quickly. A more important difference is that, in our protocol, players make claims on specific objects rather than proposing

[4] Although the mechanisms of 'claiming' and 'accepting' do not provide a *direct* way of proposing that some discs should be claimed by neither player, there are indirect ways for a player to communicate that she wants an equal but inefficient agreement (for example, by claiming discs compatible with such an agreement and refusing to accept any unequal proposal from her co-player).

[5] In these designs, players submit proposals about how the available surplus should be divided; the sender of a proposal is committed to it if the other player accepts it. Players are also able to send free-form written messages, subject to certain constraints which require messages to be checked by an experimenter before being transmitted, thus requiring relatively long negotiation periods (ten minutes in Forsythe et al.'s, twelve minutes in Roth and Murnighan's, fifteen minutes in Gächter and Riedl's).

distributions of the total available payoff. One consequence of this feature is that a given profile of payoffs may be consistent with more than one set of claims, and so may set a coordination problem for two players who both want to arrive at those payoffs.[6] (In G1, for example, there are two distinct sets of claims which imply that each player receives £5.) However, given the communication opportunities provided by our protocol, one might expect such coordination problems to be easily solved. For our purposes, the principal advantage of linking claims with objects is that it allows us to use the spatial layout of discs as an intuitively natural cue.

We submit that these cues are payoff-irrelevant in the sense that is required for tests of Schelling's hypothesis. We recognize that the layout of discs on the table could prompt mental associations with real-world situations in which there are transport costs that increase with distance, or in which taking closer rather than more distant objects is an apparently natural heuristic. But mental associations such as these are exactly what Schelling's theory is about. Discussing the role of focal points in bargaining, Schelling says that what is perceived as the 'obvious' outcome depends 'on what analogies or precedents the definition of the bargaining issue calls to mind', and that the mechanism that lies behind focality is 'the power of *suggestion*' (1960, 69, 73, italics in original). From experiments on matching games, it is known that the labels that are focal are often those that are perceived as 'favorites' – in other words, that prompt mental associations with subjective value (Bardsley et al., 2010). In the classic matching game of choosing a meeting place in New York City, the focality of Grand Central Station for Schelling's respondents surely has something to do with associations of ideas with travel costs: for residents of New Haven, Connecticut in the 1950s, travelling to New York City by train, Grand Central Station was presumably a cost-minimizing meeting place. It seems

[6] Dorothea K. Herreiner and Clemens Puppe (2010) use a bargaining protocol in which players propose divisions of a set of specific objects (described as 'A', 'B', 'C' and 'D'), but in which given sets of objects can have different values for the two players. Values are assigned so that each division of the objects induces a unique payoff profile.

unavoidable that, in any fair test of Schelling's theory, payoff-irrelevant cues will have *associations with* real-world concepts of value, or with real-world precedents or habits. What is required (and our design achieves) is that the experiment uses labels that are payoff-irrelevant, that is, they do not provide information about the *actual* payoffs of the game that subjects play, and that this fact is transparent.

In choosing the spatial layouts of discs for our bargaining games, our working assumption was that players would classify locations on the table as 'closer to my base', 'closer to the other player's base', and 'equidistant from the bases' (i.e. in the central column). Thus, in any given game, a natural association of closeness suggests that each player should claim the discs that are closer to his base. To the extent that these suggestions are acted on, a player has an advantage relative to her co-player if the discs on her side of the table have a higher total money value than discs on the other player's side. In such a case, we will say that the first player is *favored*.

This assumption is supported by evidence from matching games and tacit bargaining games. Mehta et al. (1994) investigate a type of matching game in which two players each see the same diagram of a grid, similar to our bargaining tables, on which two squares and a number of circles are located. The players' problem is to coordinate on an assignment of circles to squares (each circle being assigned to exactly one square). Mehta et al. find high rates of coordination in these games, and a strong tendency for players to assign each circle to the square (if there is one) to which it is closer. Isoni et al. (2013) find that, when *tacit* bargaining games are displayed as bargaining tables, spatial cues act as powerful focal points; players are much more likely to claim discs at their own side of the table than discs at the other side, even when the layout of discs favors one player relative to the other.

In the light of this evidence, it is reasonable to expect that when explicit bargaining games are presented as bargaining tables, players will recognize (but not necessarily act on) spatial cues based on the relation of closeness between discs and bases. In any game in which there is at least one disc at each player's side of the table, we will say that the spatial cues *pick out* the solution in which each player takes the discs at her side of the table, and no others. Games in which all the discs are in the central column (*spatially neutral* games) will be used as controls.[7]

In G4, for example, the spatial cues pick out the solution in which player L takes the £6 disc and player R takes the £5 disc. Notice that spatial cues may pick out solutions in which some discs are assigned to neither player. For example, the spatial cues of G21 pick out the solution in which each player takes the £5 disc on her own side of the table, leaving the £1 disc in the central column unclaimed. The idea that cues can work in this way is consistent with Schelling's (1960: 295–297) discussion of games in which payoff-dominated equilibria are focal. It should also be remembered that if players have preferences for equality in money earnings, a solution that achieves equality by leaving some discs unclaimed can be non-dominated *in utility*.

Our objective is to investigate how far, and under what conditions, actual bargaining outcomes are skewed towards those solutions that are picked out by spatial cues. Such an investigation needs to take account of payoff-based factors that might complement or oppose spatial cues. Most theories of bargaining imply that bargaining outcomes are influenced by principles of efficiency and equality. We therefore consider how those principles apply to our games.

Consider any bargaining table game with players L and R. If we abstract from spatial properties, the game is defined by a collection D of disc values. For

[7] Notice that it is an intrinsic property of a bargaining table game that each disc has a distinct location. For this reason, it is impossible to remove every spatial cue from the game. But for the purposes of our tests of Schelling's hypotheses, what matters is that the most salient spatial cues are those defined by the closeness relation embedded in our design, and that these cues are absent from the games that are used as controls. As shown by Isoni et al. (2013), games in which all discs are equidistant from the two bases have much weaker spatial cues than games in which discs are split between the two halves of the table. An alternative form of control, in which spatial neutrality is defined by the absence of bases, will be considered in Section VI.

example, G1 and G2 are both defined by $D = \{5, 5\}$.[8] The specification of D determines which combinations of payoffs are feasible. We define an *allocation* (w_L, w_R) as a pair of payoffs to L and R respectively. Notice that an allocation is defined in terms of payoffs; when we need to refer to players' final claims on the discs themselves, defined by their spatial locations, we will use the term *assignment*. An allocation is *feasible* if it would result from some assignment of discs to players that is consistent with the rules of the game. (Thus, if $D = \{5, 5\}$, the set of feasible allocations is {(0, 0), (0, 5), (0, 10), (5, 0), (5, 5), (10, 0)}.)

Now consider any feasible allocation (w_L, w_R) in a game in which the total value of the discs is v. The *efficiency* of this allocation is $(w_L + w_R)/v$; we will say the allocation is *efficient* if $(w_L + w_R)/v = 1$, and *inefficient* otherwise. The allocation is *equal* if $w_L = w_R$. Its *worse consequence* is $\min(w_L, w_R)$. It is *least-unequal efficient* (LUE) if it is efficient and if no efficient allocation has a strictly greater worse consequence. It is *least-inefficient equal* (LIE) if it is equal and if no equal allocation gives greater payoffs to both players. It is *equal and efficient* (EE) if it is both LUE and LIE. Intuition suggests that, in the absence of payoff-irrelevant cues, LUE and LIE allocations are particularly likely to be perceived as focal. The games used in our experiment were chosen to represent different relationships between the allocation picked out by spatial cues (the *SC allocation*) and the LUE and LIE allocations.

Our bargaining table design imposes certain constraints on the sets of feasible allocations that can be represented, and hence on the efficiency/equality trade-offs that can be investigated. One constraint is that, for any payoffs x and y, (x, y) is feasible if and only if (y, x) is feasible: the payoff opportunities available to the players are always symmetrical. A second constraint is that if the LIE allocation is

[8] A 'collection', unlike a set, can contain two or more identical items. Thus, for example, {5, 5} and {5} are distinct collections.

inefficient, it must be weakly dominated by each of the LUE allocations. For example, consider a game with $v = 11$ in which (4, 4) is LIE. Then there must be one or more discs worth 3 in total that are 'left on the table' if (4, 4) is reached, which implies that (7, 4) and (4, 7) are also feasible. A third constraint is that if (x, y) and (y, x) are LUE with $x > 2y$, there must be a disc worth exactly x which is left on the table in any equal allocation, and so the LIE payoff to each player cannot be greater than y/2 (with the implication that the LIE allocation is *strictly* dominated by the LUE allocations).[9] Subject to these constraints, we investigate the effects of spatial cues in relation to a range of different efficiency/equality trade-offs.

II. Hypotheses

Table 1 summarizes the 30 games used in the main treatment, classifying them in relation to the hypotheses that are to be tested.

[Table 1 near here]

In this Table, and in the rest of the paper, we use the following compact notation to describe bargaining table games. For any given game, we list its disc values (in UK pounds). Two vertical lines are added to the list to show the main features of the spatial layout. Discs located in the left (right) columns of the bargaining table are listed to the left (right) of both vertical lines; discs in the central column are listed between the vertical lines. So, for example, G1 = |5,5| is a game in which there are two discs worth £5 each, both located in the central column. G18 = 2,1| |4, 4 is a game in which there are four discs, d_1 and d_2 on the left, worth £2 and £1

[9] Suppose (x, y) and (y, x) are LUE with $x > 2y$. First, suppose there is no disc worth exactly x. Then in any (x, y) allocation, player L must be assigned at least two discs, and the lowest-valued of these discs must be worth no more than x/2. But if this disc is transferred from L to R, the resulting (efficient) allocation is less unequal than (x, y), contradicting the assumption that (x, y) is LUE. So there must be a disc worth exactly x. Since this disc is worth more than all the others combined, it must be left on the table in an equal allocation.

respectively, and d_3 and d_4 on the right, both worth £4; in this game R is favored. G12 = 2,1,2|1|2,2,1 is a game in which there are seven discs, three on the left, worth £2, £1 and £2, one in the central column, worth £1, and three on the right, worth £2, £2 and £1.

Each row of Table 1 refers to a set of games that use a common disc collection, and therefore differ only in terms of the spatial layout of the discs. The entry in the first column of each row identifies the relevant disc collection. The entries in the second and third columns record the LUE and LIE allocations for this set of games. For each disc collection, there is one spatially neutral game, identified in the fourth column, and one or more games with spatial cues. The latter games are identified in the remaining four columns, classified according to the allocations that are picked out by those cues. Each of these games allows us to test some hypothesis about the role of spatial cues in explicit bargaining. These hypotheses are concerned with average or aggregate outcomes of given bargaining table games that are faced by many pairs of players.

The 'EE' column identifies games in which an EE allocation is picked out by spatial cues. These games allow us to test our background assumption that the spatial relation between discs and bases is perceived as salient. Because of the symmetry embedded in our design, any feasible EE allocation can be arrived at by at least two (and sometimes many more) different assignments of discs to players. If spatial cues were salient, one would expect the assignments suggested by those cues to be chosen more frequently than other assignments that implied the same allocations. This can be tested most cleanly in games in which payoff-based criteria make one allocation uniquely focal, and spatial cues pick out one particular assignment consistent with that allocation. We therefore test the following hypothesis:

Hypothesis 1: In any bargaining table game in which there is an EE allocation picked out by spatial cues, the disc assignment suggested by those cues occurs more frequently than any other assignment that implies the same allocation.

The 'LUE, not equal' and 'efficient, not LUE' columns of Table 1 respectively identify two types of game in which the spatial cues pick out unequal but efficient allocations. In games of the first type, there are two LUE allocations, differing according to which player gets the higher payoff, and the spatial cues pick out one of these allocations. These games span a range of different trade-offs between efficiency and equality. The difference between the two players' LUE payoffs varies from £1 in G4, G13, G17 and G20 to £5 in G6 and G29. In G4 and G6, the only equal allocation is that in which both players receive nothing. In G17, the LIE allocation gives positive payoffs but is strictly dominated by the LUE allocations. In G13, G23, G26 and G29, the LIE allocation is only weakly dominated by the LUE allocations, making the trade-off as favorable as possible to equality. Recall that if (x, y) is an LUE allocation and if $x > 2y$, the LIE allocation is strictly dominated by the LUE allocations. Thus, when the dominance is only weak, the x:y ratio cannot be greater than 2:1. This ratio ranges from 6:5 in G13 to the maximum value of 2:1 in G29. The 'efficient, not LUE' column identifies games in which the spatial cues pick out an efficient allocation that is more than minimally unequal.

For games in which spatial cues pick out unequal but efficient allocations, we test the following hypothesis:

Hypothesis 2: In any bargaining table game in which spatial cues pick out an unequal but efficient allocation, payoffs are higher for favored players than for unfavored players.

This hypothesis encapsulates a key difference between Schelling's analysis of explicit bargaining and analyses that take no account of payoff-irrelevant cues.

Whatever the number and value of discs on the table, the *only* asymmetries between the players are the result of spatial cues. (Recall that the players have not been given any commonly-known labels, and that each player is free to claim any disc.) Thus, if play is *not* influenced by spatial cues, there can be no systematic difference between the payoffs of favored and unfavored players. In contrast, it is fundamental to Schelling's analysis that payoff-irrelevant cues can confer a bargaining advantage on one player relative to another.

One might think that Schelling's approach also implies that, when spatial cues pick out an LUE allocation, bargaining outcomes will tend to be more efficient than in the corresponding spatially neutral game. However, the role of cues in explicit bargaining can be modelled in ways that imply Hypothesis 2 while *not* implying that the presence of cues increases efficiency. A simple model of this kind, based on the asymmetric Hawk–Dove game (John Maynard Smith and Geoffrey Parker, 1976; Sugden, 2004), is presented in the Online Appendix. The essential idea behind the model is that players may be *spatially aware* or *spatially blind*. Spatially blind players follow strategies that are independent of cues. Such players are of two types – tough bargainers ('hawks') and soft bargainers ('doves'). Spatially aware players act as hawks when they are favored and as doves when they are not. Whatever the mix of these three types, provided that there are at least some spatially aware players, average payoffs are strictly higher for favored than for unfavored players. For some parameter values, however, the interplay of the two types induces less agreement and hence lower efficiency in games with spatial cues than in spatially neutral games.

The final column of Table 1 identifies games in which spatial cues pick out an allocation that is LIE (with nonzero payoffs) but inefficient. The value of the discs that must be left unclaimed in order to achieve this allocation varies from £1 in G12 and G21 to £5 in G30. For these games, we test the hypothesis that bargaining outcomes are skewed in the direction of equality. In this case, the predicted effect of cues is symmetrical with respect to the players, and so 'skew' has to be defined by reference to spatially neutral games. Our final hypothesis is therefore:

Hypothesis 3: Let G be any bargaining table game in which the LIE allocation is inefficient, gives nonzero payoffs, and is picked out by spatial cues. Let G' be a spatially neutral game with the same disc collection as G. The probability that the outcome is the LIE allocation is higher for G than for G'.

III. Implementation of the Experiment

The main treatment was first run as a series of sessions in which subjects faced games G1–G18, which we call *Set I*. In the light of the results from those games, we extended the study to a wider class of games with a greater variety of trade-offs between efficiency and equality. We ran a further set of sessions in which subjects faced G19–G30 and (as a consistency check) G1–G6, making up *Set II*.

Each subject played the relevant eighteen games in random order, not always with the same opponent, taking the favored role in some games and the unfavored role in others.[10] Each participant was anonymously assigned to a *matching group* of four subjects, and for each game her opponent was chosen from the other three people in her group. This was known to the subject, but the matching sequence was varied so that subjects did not know which member of their group was their co-player in any given game. Given the number of games to be played, it was infeasible to match subjects in a way that would allow each pair in any given game to be treated as an independent observation. Thus, to allow clean statistical tests with an acceptable degree of power, it was necessary to match subjects within small fixed groups and to use matching groups as the units of observation. Partner

[10] For each of the games in which spatial cues favor one of the players, which player was favored was determined randomly. Given our matching protocol (see below), it would not have been possible for individual subjects to be assigned the same role for all of these games.

matching (i.e. a group size of two) would have introduced the possibility that pairs might try to equalize average payoffs over the eighteen games, for example by alternating which player would get the larger share of the surplus. We judged that, with a group size of four and with no opportunities for free-form communication, group-level cooperation across games would be difficult to sustain. However, it is possible that the use of matching groups could have induced subjects to bargain less aggressively than they would have done in one-shot games.

We used a variant of the random lottery incentive system. Each subject was paid his payoffs in two of the eighteen games, chosen at random by the computer. This selection procedure was subject to the constraints that both players in a pair were paid for the same game and that each player in a matching group appeared in two of the selected pairs.

The main treatment was run in June 2010 (for Set I) and November 2012 (for Set II) at the CBESS Experimental Laboratory at the University of East Anglia. Participants were recruited from the general student population using the ORSEE system (Ben Greiner, 2004), excluding individuals who participated in previous bargaining table experiments. There were 144 subjects in the Set I sessions and 168 in the Set II sessions; no one participated in both. The bargaining protocol was implemented using z-Tree (Urs Fischbacher, 2007). Subjects' understanding of this protocol and other key aspects of the experiment was checked using a computerized questionnaire (which can be found in the Online Appendix). Sessions lasted between sixty and eighty minutes; earnings ranged between £5 and £25 with an average of £15.12, including a £5 show-up fee.

IV. Results: Bargaining Outcomes

The outcomes of bargaining in the main treatment are summarized in Tables 2a (for Set I) and 2b (for Set II).

[Tables 2a and 2b near here]

In each Table, there is a row for each game in the relevant set. The horizontal lines separate groups of games that have a common disc collection. The first game in each of these groups is spatially neutral; the others have various kinds of spatial cues.

The first column describes the game in our compact notation. The second column reports *average efficiency* – that is, the average value of $(w_L + w_R)/v$ achieved by the 72 player pairs facing Set I or the 84 player pairs facing Set II. The following columns report the percentage of pairs whose payoffs constituted: an efficient allocation (column 3), an equal allocation with nonzero payoffs (column 4),[11] and an LUE allocation (column 5). For games with spatial cues, column 6 reports the percentage of pairs whose payoffs constituted the SC allocation. Since this allocation is implied by two or more different assignments of discs to players, the percentage of pairs in which each player's final claims were *exactly the discs* assigned to her by the spatial cues – the *SC claims* – is shown in parentheses. Column 7 reports the percentage of pairs whose interactions[12] ended with the allocation (0, 0). The percentage of cases in which this was the result of a sealed agreement is shown in parentheses. The final column reports an index of *normalized payoff asymmetry* (NPA). This is defined as $(w_F - w_U)/(w_F^* - w_U^*)$ where w_F and w_U are the average payoffs to favored and unfavored players respectively, and w_F^* and w_U^* are the payoffs implied by the SC allocation. Thus, normalized payoff asymmetry is equal to one if all pairs settle on the SC allocation,

[11] In every case in which a pair achieved an equal allocation with nonzero payoffs, that allocation was also LIE. We therefore do not report LIE outcomes separately.

[12] We use the term 'interaction' to refer to the course of play in a particular game (e.g. G1, G2, etc.) faced by a particular pair of players.

and has an expected value of zero if players' behavior is unaffected by spatial cues. The asterisks against entries in this column will be explained later.

It is immediately obvious from a comparison of the two Tables that, in each game that was common to Sets I and II, behavior was very similar in the two cases.[13] Thus, although our formal hypothesis tests will be carried out separately for the two sets, it is reasonable to interpret the results as if they had been generated by two samples from the same subject pool.

A. Spatially Neutral Games

We begin by looking at the outcome of bargaining in spatially neutral games. These provide a natural benchmark from which to assess the effect of payoff-irrelevant cues in other games.

In the games in which an EE allocation was feasible (i.e. G1 and G7), most pairs agreed on equal and efficient assignments of discs. Average efficiency in these games was very high, varying from 98.3 to 100 per cent. Clearly, when trade-offs do not have to be made between equality and efficiency, the type of communication allowed by our bargaining protocol enables players to solve the problem of coordinating their disc claims. For this, spatial cues are not needed.

In the games in which an EE allocation was *not* feasible, average efficiency was negatively related to the degree of inequality of the LUE allocations. When the LUE allocations were (5, 6) and (6, 5), average efficiency varied from 95.8 to 98.4 per cent. Its lowest value was 78.2 per cent in G5 in Set II, where the LUE allocations were (3, 8) and (8, 3). But in all games, a large majority of interactions ended with LUE allocations, even when equal allocations with nonzero payoffs

[13] For each of these games, we test the null hypothesis that the distributions of earnings do not differ between the Set I and Set II sessions by comparing the group-level averages with two-tail Mann-Whitney tests. For games with spatial cues, we conduct similar tests on the earnings of favored and unfavored players. The hypothesis is never rejected. See the Online Appendix for details.

were feasible. In G25, for example, the LUE allocations were (4, 7) and (7, 4), while (4, 4) was also feasible; 66 out of 84 pairs (78.6 percent) settled on one of the LUE allocations, while only six (7.1 percent) settled on (4, 4). The overall picture is clear: in the absence of spatial cues, there was a very strong tendency for players to agree on LUE allocations.

When the LUE allocations were (5, 6) and (6, 5), the percentage of interactions ending with zero payoffs to both players ranged from 1.4 to 4.2. When the LUE allocations were more unequal than this, that percentage ranged from 8.3 to 21.3. These figures are broadly in line with the disagreement rates found in other experimental investigations of unstructured bargaining.[14] It is difficult to make meaningful comparisons across experiments with very different designs and payoff parameters, but our bargaining protocol does not seem significantly more or less likely to induce agreement than those that have been used by other researchers.

B. Games in which Spatial Cues Pick Out Equal and Efficient Allocations

There are two games, G2 and G8, in which spatial cues pick out EE allocations. These games allow tests of Hypothesis 1. Given our findings for the corresponding spatially neutral games, it is not surprising that in G2 and G8, most pairs agreed on equal and efficient allocations. But we can ask whether, in reaching those allocations, players made use of spatial cues.

To test Hypothesis 1, we consider the number of pairs whose final claims were exactly those suggested by the spatial cues (corresponding with the entry in parentheses in column 6 of Table 2a or 2b for the relevant game) as a proportion of

[14] For example, Roth and Murnighan (1982, Table IV) find a disagreement rate of 16.7 per cent when (as in our experiment) players' payoffs are common knowledge. Forsythe, Kennan, and Sopher (1991, Table 2) investigate bargaining games with imperfect information. In the treatment ('IV') in which imperfect information is least likely to impede agreement, the disagreement rate is 9.9 per cent. Gächter and Riedl (2005, Table 3) find an average disagreement rate of 18.2 per cent in an experiment in which an unequal allocation is framed as respecting 'moral property rights'. Herreiner and Puppe (2010, Table 6) find an average disagreement rate of 4.7 per cent in games with 'cardinal' payoffs; in the game with the most severe efficiency/equality trade-off ('R3'), the rate is 10.4 per cent.

the number of pairs who achieved the SC allocation (the main entry in the same column). Our null hypothesis is that players are equally likely to make any set of final claims with the same total value; the alternative hypothesis is that the set of final claims suggested by the cues (the SC claims) is made with higher probability than other claims with the same total value. In G2, there are two possible ways in which the (5, 5) allocation can be achieved: when both players claim the disc closer to them, as suggested by the spatial cues, and when both claim the farther disc. In Set I, 61 of the 71 pairs who settled on a (5, 5) allocation selected the SC claims. In Set II, this was true for 74 out of 84 pairs. In G8, there are twelve possible ways in which the (5, 5) allocation can be achieved; 51 of the 63 pairs who settled on this allocation selected the SC claims. In each case, the null hypothesis is decisively rejected in a χ^2 test of goodness of fit for a binomial distribution ($p < 0.0001$).[15] This finding confirms our background assumption that the cues that were built into our experimental design would be perceived as salient by players.

C. Games in which Spatial Cues Pick Out Unequal LUE Allocations

There are eight games, G4, G6, G13, G17, G20, G23, G26, and G29, in which spatial cues pick out one of two unequal LUE allocations. Two of these games (G4 and G6) appeared in both Set I and Set II. In each of these ten cases, the level of average efficiency and the proportion of pairs who settled on LUE allocations were similar to the values observed in the corresponding spatially neutral game. In each case, there is no significant difference in average group earnings between the spatially neutral game and the game in which spatial cues pick out one of the unequal LUE allocations.[16]

[15] This test takes into account the fact that each of these games was played by two pairs in each matching group.

[16] These comparisons are based on two-tail Wilcoxon signed-rank tests which use the matching groups as the unit of observation.

For the reasons explained in Section II, the similarity in efficiency need not be interpreted as evidence against Schelling's hypothesis. It is more important to ask whether, as predicted by Hypothesis 2, payoffs were higher for favored than for unfavored players. For each of these ten cases, normalized payoff asymmetry is shown in the final column of Table 2a or 2b. NPA is positive in eight of these cases. To test for significant payoff asymmetry in the direction implied by Hypothesis 2, we use a one-tail Wilcoxon signed-rank test, comparing average payoffs to favored and unfavored players and using matching groups as the unit of observation. The results of this test are shown by the asterisks in the final columns of the Tables. There is significant payoff asymmetry in four cases – G4 in Set I ($p < 0.1$), G4 in Set II ($p < 0.01$), G17 ($p < 0.01$) and G26 ($p < 0.05$).

These results suggest that when spatial cues pick out one of two LUE allocations, there is some tendency for bargaining outcomes to be skewed in the direction suggested by those cues, but this tendency is relatively weak.

D. Games in which Spatial Cues Pick Out Efficient Allocations that are not LUE

There are four games, G9, G10, G14 and G18, in which spatial cues pick out an efficient allocation that is not LUE. Given the high frequency of LUE allocations in the corresponding spatially neutral games, these games provide a test of the power of spatial cues to induce *gratuitous* inequality – that is, inequality beyond the minimum necessary for efficiency. Efficiency levels are very similar to the spatially neutral games, and the distributions of earnings are not statistically different, with the exception of G14, in which a marginally significant *decrease* in earnings is observed ($p < 0.1$). The proportion of pairs who settled on LUE allocations was also not significantly different.[17]

[17] More details on these tests can be found in the Online Appendix.

Very few of these interactions ended with the *specific allocations* suggested by the spatial cues. In G9 only seven pairs settled on the suggested (4, 6) allocation, with two pairs settling on the opposite (6, 4) allocation. In all other cases, in which the suggested distributions were more unequal – (3, 7), (8, 3) and (3, 8) in G10, G14 and G18 respectively – none of the 72 pairs settled on the SC allocation.

However, this does not mean that spatial cues had no distributional effects in these games. Just as in the games considered in Section IV.C, we can measure NPA and can test Hypothesis 2. In all four games, NPA was positive. Payoff asymmetry was significant in G9 ($p < 0.05$), G18 ($p < 0.05$) and G14 ($p < 0.10$). In G14 and G18, virtually all agreements were LUE (65 out of 66 in G14, 68 out of 69 in G18); the effect of spatial cues was to discriminate between the two LUE allocations to the advantage of the favored player. In G9, in which an EE allocation was available, 57 of the 69 agreements were on that allocation, but the non-EE agreements were skewed to the advantage of the favored player.

These results suggest that spatial cues have little or no power to induce gratuitous inequality. But they provide additional evidence that, when efficiency is incompatible with equality, asymmetric spatial cues can influence the distribution of payoffs.

E. Games in which Spatial Cues Pick Out Inefficient LIE Allocations

There are six games, G12, G16, G21, G24, G27 and G30, in which spatial cues pick out an allocation that is LIE but not efficient. These games allow tests of Hypothesis 3 – that spatial cues of this kind increase the probability that players settle on the LIE allocation.

In all six games, LIE allocations (shown in column 6 of Tables 2a and 2b) were very infrequent. The highest percentage of LIE allocations was 13.1 per cent in G24; there were *no* such allocations in G12 or G16. In only one of the six games was the frequency of LIE allocations greater than in the corresponding spatially neutral game. (This is G30, in which 9 out of 84 pairs settled on the LIE allocation. In G28, the corresponding spatially neutral game, the LIE allocation was achieved by 5 out of 84 pairs.) Clearly, our results give no support to Hypothesis 3.

V. Results: Bargaining Dynamics, Cross-Group Variation, and Learning Effects

In this Section, we go beyond the testing of prior hypotheses about bargaining outcomes. Using an inductive approach, we seek insights into the causal mechanisms that generated those outcomes. Because of space constraints, we provide only a summary of our findings; further details are given in the Online Appendix.

A. Time Spent Reaching Agreement

We begin by looking at the time that pairs spent in trying to reach agreement.

For any given interaction, we define *bargaining duration* as the time that elapsed before an agreement was sealed, or as 90s if no agreement was sealed before the deadline. For each game G1–G6 that appears in both Set I and Set II, there is no significant difference between the cumulative distributions of bargaining duration in the two cases.[18] More surprisingly, there is very little evidence that bargaining duration was affected by the presence or absence of spatial cues.[19]

[18] Between-subject comparisons between cumulative distributions of bargaining durations are based on the Kolmogorov-Smirnov test. Since our matching protocol entails that the same individual may be part of two different pairs in the two games used for these comparisons, we conduct the tests on the average bargaining duration for each game computed over the two pairs belonging to the same group of four players. This reduces the sample size to 36 and 42 observations for each comparison in Sets I and II respectively, but ensures that each observation refers to the same group of individuals.

[19] Cross-game comparisons between cumulative distributions of bargaining durations are based on two-tail Wilcoxon signed-rank tests. Significant differences in bargaining duration between games with spatial cues and corresponding spatially neutral games were found only for G8, G21 and G26. In all three cases, the presence of spatial cues shortened bargaining duration. For each game with spatial cues, we also compare the frequency of 'profitable agreements' (i.e. interactions ending

While bargaining duration is a convenient metric to use in conducting statistical tests, a plot of the cumulative distribution of bargaining duration does not allow us to see some interesting aspects of the bargaining process. Recall that our bargaining protocol did not require players to tick their accept boxes in order for their payoffs to be determined by the claims on the table at 90s. In fact, it was not uncommon for interactions to end with no sealed agreement, but with claims that gave a nonzero payoff to one or both players. This was the case for 113 interactions (8.7 per cent) in Set I and 336 interactions (22.2 per cent) in Set II. Significantly, however, almost all these interactions (103 in Set I, 310 in Set II) were ones in which at least one player changed her claims in the final five seconds of the interaction. It is natural to interpret these cases as ones in which the players had effectively agreed, but the change of claims that immediately preceded that agreement occurred so close to the deadline that the players did not have time to tick their accept boxes. (This interpretation is supported by the fact that, as we will show later, a high proportion of sealed agreements were reached in the final seconds.) Bargaining duration does not distinguish between these cases and interactions that ended with the players making conflicting claims.

Since sealed agreements on (0, 0) allocations were very infrequent (see Tables 2a and 2b), little is lost by focusing on *profitable agreements*, defined as the outcomes of interactions in which at least one player's payoff was nonzero. We will say that such agreements were either 'implicit' or 'explicit'. A (profitable) agreement was *explicit* if it was sealed within the 90s available for bargaining, in which case the *agreement time* is the time, in seconds from the start of the interaction, when it was sealed. Thus, if an interaction ended without a sealed agreement but with a positive payoff for at least one player, this constitutes an *implicit agreement*. We arbitrarily

with a positive payoff for at least one player) with that in the corresponding spatially neutral game, and we find no significant difference.

assign an agreement time of 95s to implicit agreements to distinguish them from agreements sealed at exactly 90s.

Figures 2a and 2b report the cumulative distributions of agreement times for Sets I and II respectively, averaging across games with the same disc collection.[20] Because of our conventions, the last point of each curve represents the net effect of implicit agreements, while the vertical distance between the last point and the 100 per cent boundary represents the percentage of (0, 0) allocations (including the few cases in which such allocations resulted from sealed agreements).

[Figure 2a and Figure 2b near here]

Notice that, in both Figures, the graph for G1–G2 is an outlier: in these games, over 85 per cent of pairs sealed agreements in the first 10 seconds. It seems that players recognized that, when there are only two discs and both have the same value, neither player can credibly hold out for anything other than the EE allocation, and so the problem reduces to that of coordinating on who takes which disc. That most pairs were able to resolve this problem in a few seconds, even in the absence of spatial cues, is evidence of the transparency and flexibility of our bargaining protocol. Agreement times are also relatively short for G7–G10, the other set of games in which an EE allocation is feasible.

The remaining graphs all refer to games in which there are two unequal LUE allocations. These graphs show several suggestive regularities.

First, other things being equal, agreement times are longer for games in which LUE allocations are more unequal. (In Figure 2a, for example, the graphs for G3–G4, G11–G14, G15–G18, where the LUE allocations are (5, 6) and (6, 5), are almost everywhere above the graph for G5–G6, where the LUE allocations are (3,

[20] Distributions of agreement time are very similar for games with the same disc collection (compare footnote 18).

8) and (8, 3).) Given that most pairs settled on LUE allocations, this effect should not be surprising. When LUE allocations are unequal, agreement on one such allocation requires one player to concede the larger payoff to the other; the greater the inequality, the greater the concession that has to be made.

Second, other things being equal, agreement times are longer for games with more discs. This is as one would expect: the more discs there are, the more time needs to be spent making claims, and the greater the variety of messages that can be transmitted in the 'language' of claims.

Third, and we suggest most important, all the graphs show the same three phases of agreement behavior, occurring in the same time intervals. The rate at which agreements are made is relatively high from 0s to about 30s. From then to about 80s, this rate levels off, before increasing steeply in the final seconds of the interaction. Similar 'deadline effects' have been found in other unstructured bargaining experiments (e.g. Alvin E. Roth et al. 1988; Uri Gneezy et al. 2003; Gächter and Riedl 2005, 2006), and are consistent with the backward-induction logic of both Schelling's and Farrell's models of bargaining (see Introduction). The high rate of agreement in the first phase is more surprising, given that, in all phases of the game, most agreements were on one or other of the LUE allocations and that (as we will show later) failures to agree were usually the result of both players holding out for the higher LUE payoff. The implication is that any player could be reasonably certain of getting at least the lower LUE payoff by waiting until late in the game before conceding. Thus, a player who accepted the lower payoff early in the interaction passed up the chance of getting the higher payoff as a result of a concession by her opponent.[21]

[21] In a game with spatial cues, an unfavored player might use Schelling's backward-induction reasoning and conclude that her favored opponent would hold out to the end of the game. But this argument cannot explain the frequency of early agreements in spatially neutral games.

B. The Time Path of Claims

Further insight into the dynamics of bargaining can be gained by looking at the evolution of players' claims prior to the sealing of agreements. The relevant data are provided in the Online Appendix. These data reveal a striking regularity across all games with two unequal LUE allocations: during the second phase of play, the value of the claims made by players who had not yet agreed converged to the higher LUE payoff. For example, at 60s in G13 (= 2, 1, 2, 1| |2, 2, 1), 19 pairs had not yet agreed; 10 of the unfavored players and 13 of the favored players were claiming discs worth exactly £6. At 60s in G29 (= 5| |5, 5), 57 pairs had not yet agreed; 50 of the unfavored players and 49 of the favored players were claiming discs worth exactly £10.

It is clear from this regularity that, for most players who reached the final phase of play, that phase was essentially a game of Chicken. The first player to back down took the lower LUE payoff, allowing the other player to take the higher payoff. If neither had backed down when the deadline was reached, the payoff was zero for both. Notice the implication that, until the moment at which one player backed down, the two players were typically making claims of equal value, even if spatial cues favored one player relative to the other. Thus, if asymmetric spatial cues had any effect on agreements reached in the final phase, that effect would not be visible in players' claims in the run-up to the moment of agreement.

C. Comparisons between Early and Late Agreements

In the light of the regularities described in the previous subsections, it is natural to ask whether there were systematic differences between early and late agreements. To address this question, we divide our data into two categories – *early agreement interactions* and *deadline interactions*. Early agreement interactions are those in which an agreement was sealed in the first 60s; anything else is a deadline interaction. In the Online Appendix, we present the content of Tables 2a and 2b separately for the two kinds of interaction.

This disaggregation shows that inequality in final payoffs was mainly confined to early agreements. Recall that there are fourteen cases (identified in Sections IV.B and IV.C) in which spatial cues pick out efficient but unequal allocations. For early agreement interactions, NPA is positive in twelve of the fourteen cases, and the asymmetry is significant in eight of these ($p < 0.10$ for eight cases, $p < 0.05$ for six, $p < 0.01$ for four). For deadline interactions, NPA is positive in only nine of the fourteen cases, and the asymmetry is significant in only one (albeit with $p < 0.01$.)

This disaggregation reveals a further interesting regularity. Recall that interactions resulting in inefficient LIE allocations with nonzero payoffs were rare, and almost wholly confined to the three-disc games G19–G30. Comparatively, however, such outcomes were much more common in early agreement interactions than in deadline interactions. Summing over all early agreement interactions in G19–G30, there were 64 LIE allocations and 350 LUE allocations (a ratio of 1 to 5.5). The corresponding numbers for deadline interactions were 32 and 454 (a ratio of 1 to 14.2).

D. The Advantage Conferred by Early Claims

One might conjecture that players who are quicker at making claims in the first few seconds gain an advantage over their opponents.[22] We investigate this possibility by looking at the value of the discs that had been claimed by each player 5s after the start of the game, excluding cases in which an agreement had already been sealed at that time. The value of the discs claimed by player L at 5s minus the

[22] This idea was suggested to us by an anonymous referee.

value of the discs claimed by R at 5s provides a measure of the *early claim advantage* of L relative to R. By comparing this with the *payoff difference* in favor of L (that is, the final payoff to L minus the final payoff to R), we can ask whether players with a positive early claim advantage tended to achieve higher payoffs than their co-players. However, in games with spatial cues, this comparison is subject to a potential confound. If players' first claims are of the discs closest to their bases, positive correlation between early claim advantage and final payoff asymmetry might indicate the effects of a common causal factor – the spatial layout of the discs. This problem can be avoided by confining the analysis to spatially neutral games. Regressing payoff difference in spatially neutral games against early claim advantage, we find a highly significant relationship ($p < 0.001$) with a coefficient of 0.19 (implying that an early claim advantage of £1 generates a payoff difference of £0.19).[23]

However, if data from early agreement interactions are excluded from this regression, the relationship between payoff difference and early claim advantage is much weaker, with a much smaller coefficient (0.08), which is only marginally significant ($p = 0.087$). The implication is that early claim advantage had relatively little effect in the final phase of play. This perhaps reflects the fact that, as noted in Section V.B, that phase typically began with both players maintaining claims with the same total value. In other words, any facts on the ground created by either player's early claim advantage had typically been erased before the final phase of play.

[23] We use random-effects GLS regressions, clustering at the level of the four-subject group. We pool the data from all spatially neutral games in Sets I and II. We exclude cases in which pairs failed to reach agreement. (In such cases, payoff difference is necessarily zero.) Further details of these regressions are provided in the Online Appendix.

E. Group Heterogeneity

In the light of the results we have reported so far, it would be interesting to know whether there was heterogeneity in players' *hawkishness* – that is, the strength of their inclination to hold out for larger payoffs – and in their *spatial awareness* – that is, their susceptibility to the effects of spatial cues. In principle, these are individual-level characteristics. However, the dynamic and interactive features of our bargaining table games make it very difficult to isolate characteristics of strategies at this level.

At first sight, it might seem that one could use some definition of players' 'initial' claims, and then treat those claims as if the two players had made them independently. But in our bargaining protocol, players could build up claims in a gradual fashion, so any notion of initial claims would be arbitrary. Many interactions ended in agreement within the first 10s, but in other cases players built up their claims gradually during the first phase of the game, presumably in awareness of their opponents' current claims. A further complication is that many pairs of players seemed to use the making and cancelling of claims as a flexible language for exchanging messages. Another possible approach would be to focus on players' claims in the middle phase of the game, when the rate of agreement was lowest. But, as explained in Section V.B, claims in this phase showed little heterogeneity.

We suggest that the cleanest approach to the investigation of heterogeneity is to use matching groups as the unit of observation. Because of sampling variation in the formation of groups, one might expect heterogeneity at the individual level to induce some degree of heterogeneity at the group level. For each matching group and for each of the eighteen games, we consider the number of agreements sealed by 60s (an index of *dovishness*, i.e. the converse of hawkishness), and the number of interactions ending with the (0, 0) allocation (an index of hawkishness). For games with spatial cues, we also consider NPA (an index of spatial awareness). Using Kruskal-Wallis tests, we find strongly significant differences for the first two variables in both Set I and Set II, indicating substantial heterogeneity in hawkishness, but we find no evidence of heterogeneity in spatial awareness.

F. Learning

So far, we have discussed our results as if each of the eighteen games were played in isolation. One might ask whether there were any systematic changes in behavior over the sequence of games played by each subject that could be relevant for the interpretation of our results. In fact, there were no strong effects of this kind.

Recall that, in each experimental session, games were presented in random order. Thus, for any given game, we can compare the behavior of pairs who faced it in the first half of the relevant session (that is, as one of the first nine games they played) with that of pairs who faced it in the second half. We make such comparisons in respect of average efficiency, NPA (for games in which spatial cues suggest an unequal allocation), and bargaining duration. We find few cases of significant differences in efficiency between the two halves of either Set I or Set II sessions, and no overall pattern of increase or decrease. There is some evidence that NPA increased over the course of Set I sessions, suggesting that subjects were learning to make use of spatial cues, but there seems to be no trend (in either direction) in Set II sessions. Cumulative distributions of bargaining duration are sometimes significantly different between the first and second halves of sessions, but there is no overall pattern of increasing or decreasing duration. However, there seems to be some tendency for bargaining duration to increase in games in which the LUE allocation is very unequal (particularly in G28–G30), suggesting that subjects were learning to be more hawkish in such games.

G. Summary

The key to many of the regularities that we have described in this Section is the distinction between early agreement and deadline interactions in games in which there are two unequal LUE allocations. We conjecture that two different modes of bargaining may have been at work in these games.

In the first 60s of play of these games, between 30 and 85 per cent of pairs reached agreement, and these agreements were concentrated in the first 30s. In most such agreements, one player accepted the lower LUE payoff and allowed the other player to take the higher payoff; in a small minority of cases, both players took the lower LUE payoff and some surplus was lost. This seems to indicate a *dovish* mode of bargaining – that is, a disinclination to contest for the larger share of the surplus – on the part of those players who accepted the lower LUE payoff early in the game. In contrast, the final 30s of play seem to be best characterized as a contest between two hawkish players, both of whom were holding out for the higher payoff, postponing concession as long as they dared. Since the proportion of early agreement interactions is inversely related to the inequality of the LUE allocation, we know that subjects cannot be partitioned into unconditional hawks and unconditional doves. But the heterogeneity of hawkishness at the group level suggests some underlying heterogeneity at the individual level. The fact that the main effects of payoff-irrelevant cues were on early agreement interactions suggests, contrary to Schelling's 'midnight bell' intuition, that those effects were mediated by the bargaining strategies of the more dovish players.

VI. An Alternative Implementation of Spatial Neutrality

In our main treatment, we have assessed the power of spatial cues using 'spatially neutral' games as controls. Recall that in these games, discs are located on a bargaining table on which each player has a 'base'; neutrality is implemented by making every disc equidistant from the two bases. An alternative way of implementing spatial neutrality would be to remove the bases altogether.[24] As a check on the robustness of our findings, we ran a *control treatment* in which bargaining tables were displayed without bases. This treatment is described in detail in the Online Appendix. Here we present only a brief summary.

In the control treatment, each of 92 subjects played 30 games in pairs formed using the same matching protocol as in the main treatment. The games were identical to G1–G30 in all respects except the following. First, the two bases on each bargaining table were removed. Second, the table was not rotated. Thus, both players saw the table in the orientation shown in Figure 1, rather than with the player's own base at the bottom. Third, since there were no bases, players' claims at each moment were shown only by the colors of the discs. Except for the fact that there were 30 games rather than eighteen, the control treatment was organized in exactly the same way as the main treatment. Subjects were recruited in the same way from the same pool. No individual participated in both treatments.

We found almost no significant differences in players' behavior between games with the same disc collection but different disc locations. The same is true for comparisons of average efficiency, of the percentage of LUE allocations, and of bargaining duration. In other words, in the absence of bases, players took no account of the spatial layout of the discs. The implication is that the 'no bases' displays lacked salient spatial cues that the players could use in bargaining.

To test whether the same was true of the spatially neutral games used in the main treatment, we compare behavior between those games and the corresponding games in the control treatment. On each of the three dimensions of comparison, we found

[24] This control was suggested by an anonymous referee who conjectured that the generally low disagreement rates for the Set I games in the main treatment might be explained by spatial properties of the bargaining table. In the light of the later Set II data, we suggest that the low disagreement rates in G1–G4 and G7–G18 result from the feasibility of EE or only slightly unequal LUE allocations in those games.

almost no significant differences.[25] Our interpretation is that spatially neutral displays with bases do indeed eliminate salient spatial cues that can be used in bargaining.

However, the fact that the two different methods of implementing spatial neutrality corroborate one another has useful implications for the design of bargaining experiments. The bargaining table design provides a simple, easily-understood and relatively unstructured representation of bargaining problems. It allows very quick exchanges of non-binding and standardized messages that participants can easily grasp and that are easy for the experimenter to code and analyze. It seems that if one is not specifically investigating the effect of spatial cues, the design can be made even simpler by removing the bases and positioning the discs randomly or arbitrarily.

VII. Trade-offs between Efficiency and Equality

Taken together, our results show a clear pattern in players' responses to trade-offs between efficiency and equality. In all the bargaining games we investigated, there was a strong tendency for players to settle on LUE allocations. This tendency was found even if equal but inefficient allocations were feasible, and even if spatial cues suggested non-LUE allocations. In this Section, we comment on this tendency. The role of spatial cues will be discussed in the Conclusion.

Our subjects' reluctance to settle on equal but inefficient allocations when efficient but unequal allocations were possible may seem surprising, given the large body of experimental evidence that suggests that people are typically inequality averse. Of course, our finding must be interpreted in relation to the constraints that the bargaining table design imposes on efficiency/equality trade-offs. Recall that

[25] Since there are ten spatially neutral games, ten comparisons can be made on each of the three dimensions. We found significant differences in only two of these comparisons. (G11 had lower average efficiency and a lower percentage of LUE agreements in the control treatment than in the main treatment.)

if the LUE allocations are denoted by (x, y) and (y, x) and the LIE allocation by (z, z), the design necessitates $z \leq \min(x, y)$. Nevertheless, the preponderance of LUE allocations in cases such as $x = 7$, $y = 4$, $z = 4$ (games G25–G27) and $x = 10$, $y = 5$, $z = 5$ (games G28–G30) suggests a lack of concern by subjects about inequality. If subjects were averse to disadvantageous inequality, as hypothesized by Fehr and Schmidt (1999) and Bolton and Ockenfels (2000), player L in such a game would prefer (z, z) to (y, x), and player R would prefer (z, z) to (x, y). Thus, (z, z) would be non-dominated *in utility*, and its equality of money payoffs might be expected to make it salient as a potential bargaining outcome.

It may be relevant that much of the evidence for inequality aversion comes from ultimatum games, dictator games, trust games and social dilemmas, which are quite different from our games. We conjecture that the framing and protocol of our experimental games may have prompted affective responses and modes of reasoning that are characteristic of real bargaining behavior and may have reduced the salience of considerations of fairness and equality.

Another important feature of our design is that, apart from the spatial locations of discs relative to bases, the positions of the two players are completely symmetrical. There is no predetermined order of moves, as in ultimatum games. Each disc has the same value to both players, and the rules of the game give each player the same opportunity to claim it. Thus, if the *outcome* of the game is unequal, its inequality may seem less objectionable because it has been reached by a *procedure* that respects equality of opportunity. This form of equality is particularly salient when, as was often the case in our experiment, the final stage of an interaction is a Chicken game in which the players maintain conflicting LUE claims as long as they individually dare. The hypothesis that people are more tolerant of ex post inequality if it is the result of a fair procedure has been proposed before, but fairness has usually been understood in terms of players' intentions (e.g. Armin Falk, Ernst Fehr, and Urs Fischbacher, 2003; Gary E. Bolton, Jordi Brandts and Axel Ockenfels, 2005). We suggest that *procedural fairness* in our bargaining games is a property, not of individuals' attitudes to one another's payoffs or intentions, but of the rules of the game they are playing.[26]

VIII. Conclusion

The main objective of our experiment was to answer a question that has remained open for over 50 years: Are the outcomes of explicit bargaining games influenced by payoff-irrelevant cues, as Schelling (1960) hypothesized? Our bargaining table design allowed us to set up explicit bargaining games into which payoff-irrelevant spatial cues could be introduced in an apparently natural way. The cues we used were known to influence behavior in pure coordination and tacit bargaining games. Our experiment tested the power of those cues relative to that of payoff-based principles of efficiency and equality, when players were able to communicate with one another over time and to propose and accept binding agreements.

We found no evidence that spatial cues were effective *in opposition to* principles of efficiency and equality. There was a very consistent tendency for players to agree to allocations that were efficient and minimally unequal, even if spatial cues picked out other allocations – whether those other allocations were equal but inefficient, or efficient but less than minimally unequal. Nevertheless, spatial cues had some influence on players' payoffs. In games in which efficiency could be achieved only at the cost of an asymmetry in payoffs, players faced the problem of coordinating on which of them would take the larger share of the available surplus. In such games, payoff-irrelevant cues that favored one player relative to the other

[26] This conjecture about the effect of procedural fairness is consistent with a piece of evidence from the explicit bargaining experiment reported by Herreiner and Puppe (2010). In the game with the most severe efficiency/equality trade-off ('R3'), the least-unequal Pareto-efficient allocations were (66, 40) and (46, 75); the equal allocation with highest payoffs was (45, 45). Out of 48 pairs of co-players, 22 settled on (45, 45) and 19 on (46, 75), suggesting a greater willingness to sacrifice efficiency for equality than in our experiment. In Herreiner and Puppe's experiment, unlike ours, players' payoff opportunities were *not* symmetrical.

tended to skew the outcome of the game to the advantage of the former. In this respect, our results give qualified support to Schelling's hypothesis.

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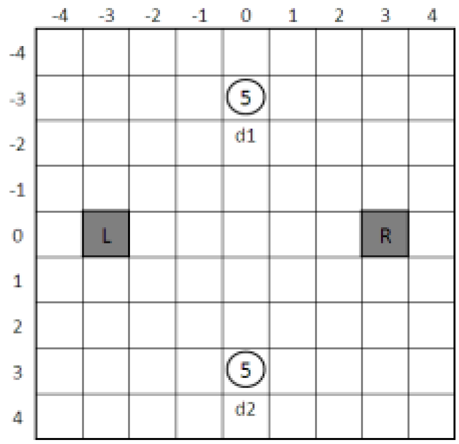
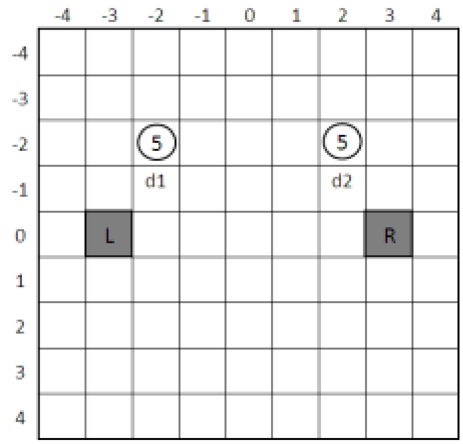
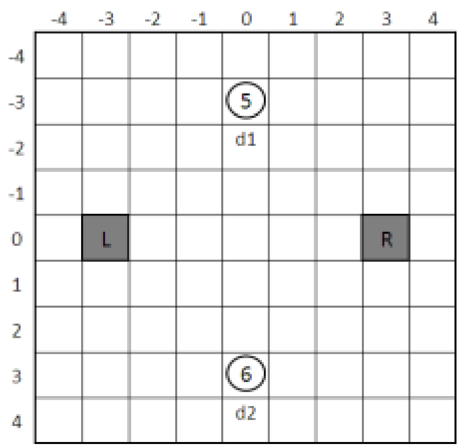


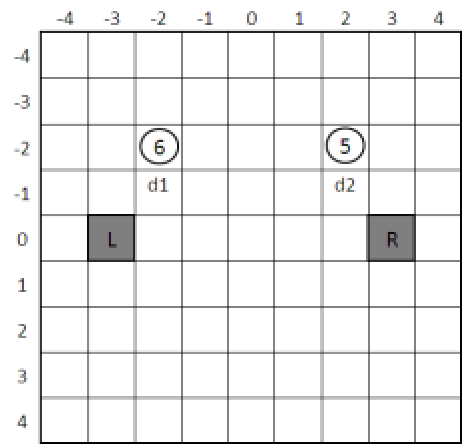
G1 = |5,5|



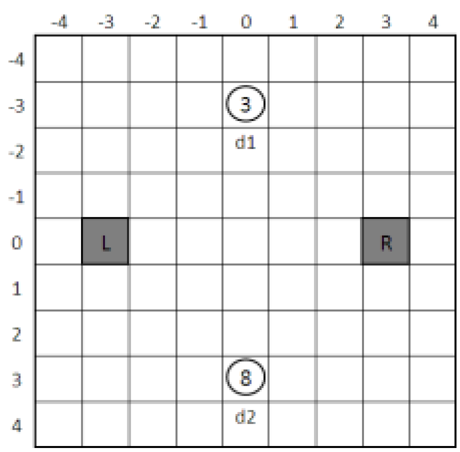
G2 = 5| |5



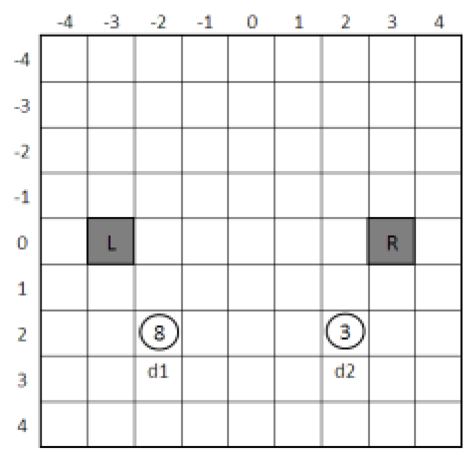
G3 = |5,6|



G4 = 6| |5

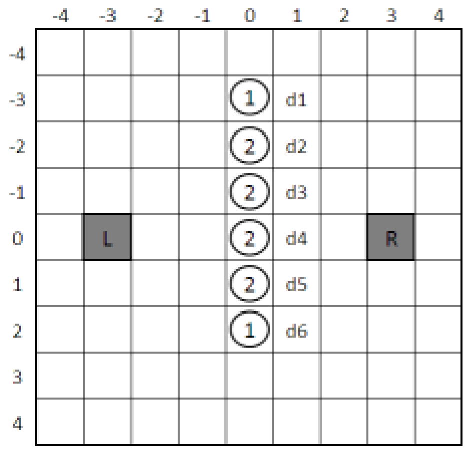


G5 = |3,8|

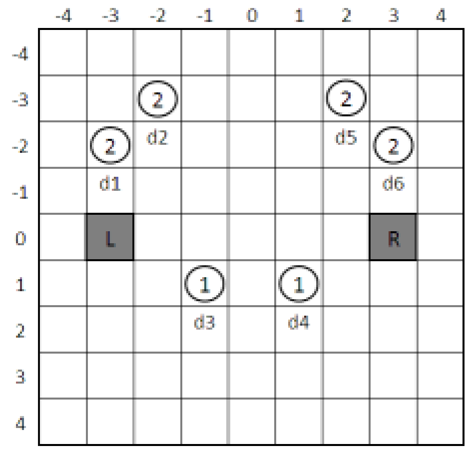


G6 = 8| |3

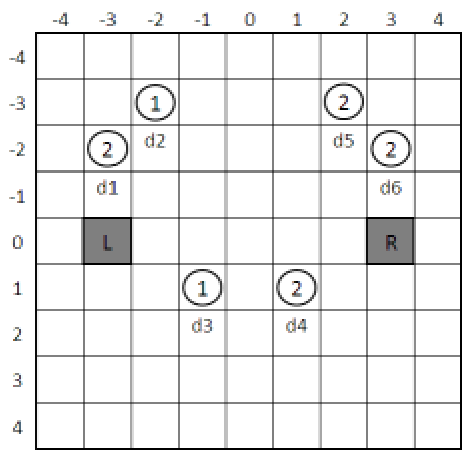
FIGURE 1 – THE GAMES USED IN THE MAIN TREATMENT



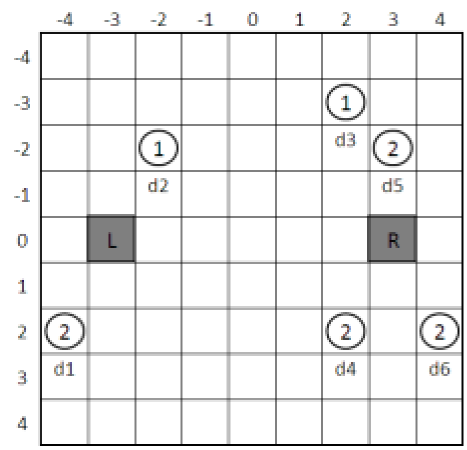
G7 = |1,2,2,2,2,1|



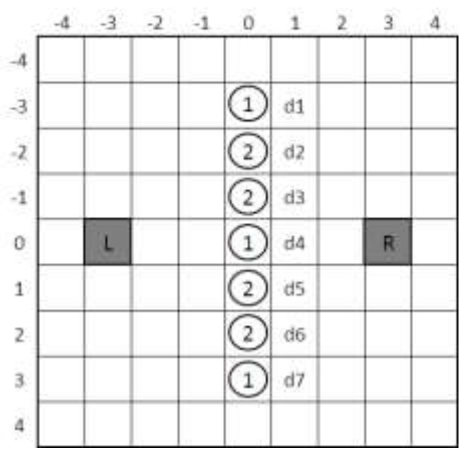
G8 = 2,2,1| |1,2,2



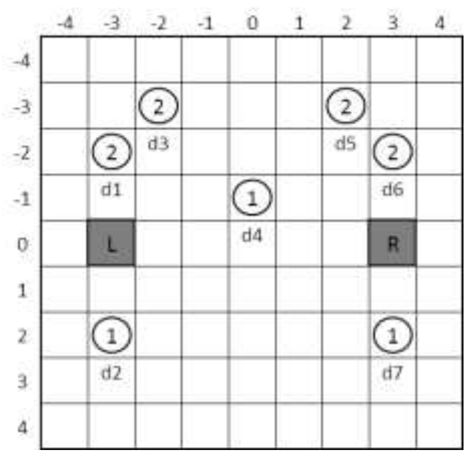
G9 = 2,1,1| |2,2,2



G10 = 2,1| |1,2,2,2

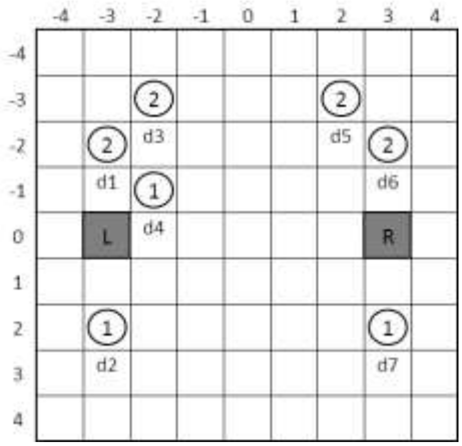


G11 = |1,2,2,1,2,2,1|

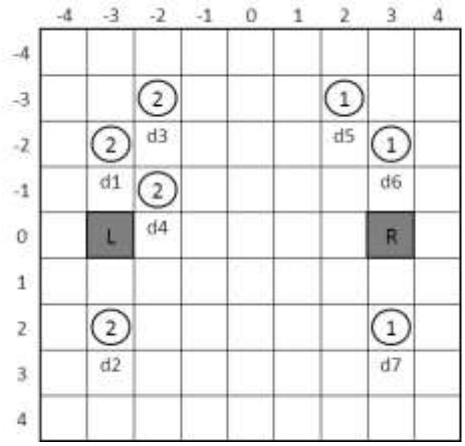


G12 = 2,1,2|1|2,2,1

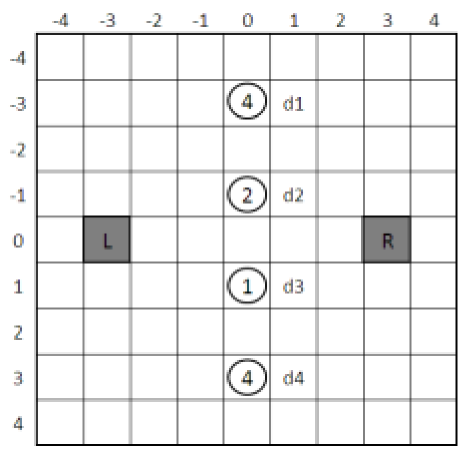
FIGURE 1 – *CONTINUED*



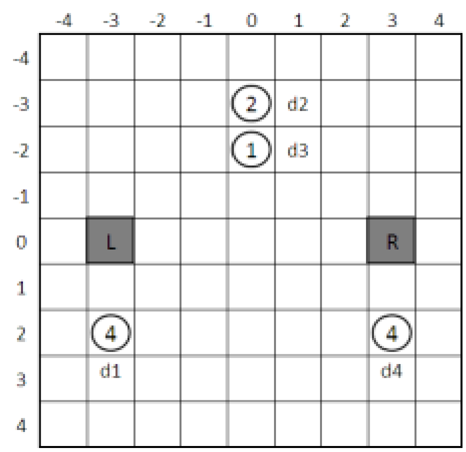
G13 = 2,1,2,1| |2,2,1



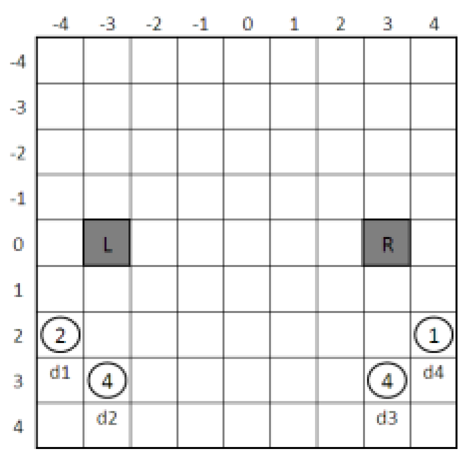
G14 = 2,2,2,2| |1,1,1



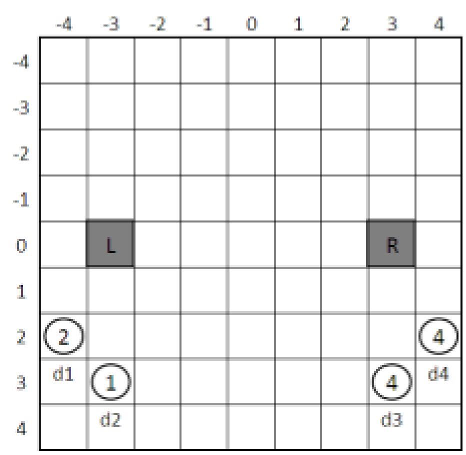
G15 = |4,2,1,4|



G16 = 4|2,1|4

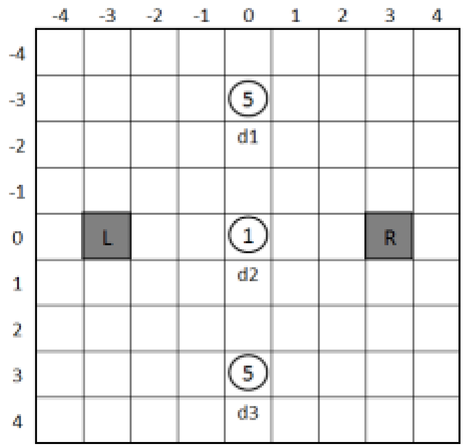


G17 = 2,4| |4,1

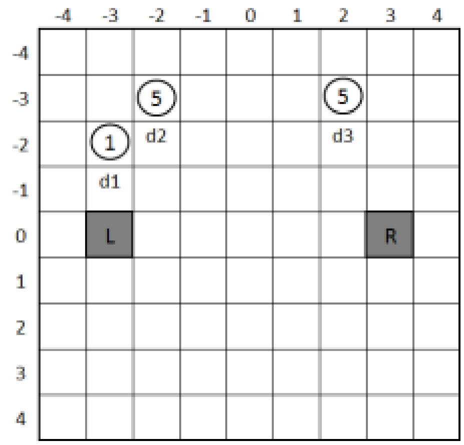


G18 = 2,1| |4,4

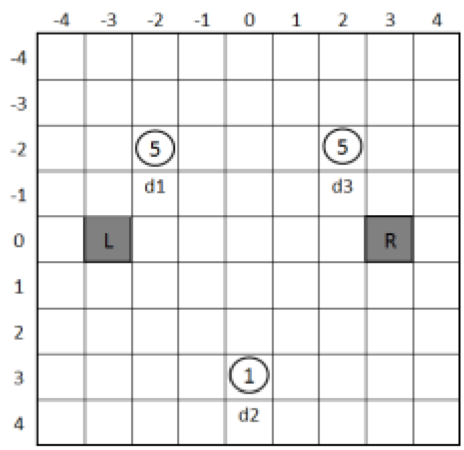
FIGURE 1 – *CONTINUED*



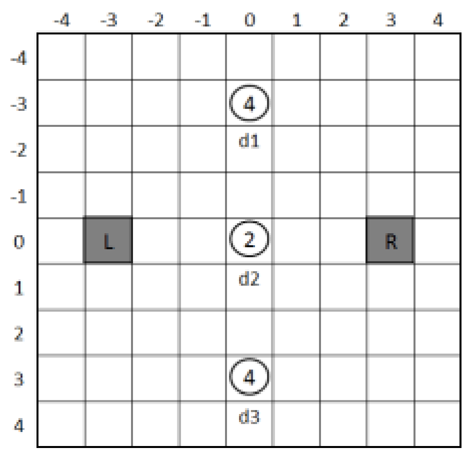
G19 = |5,1,5|



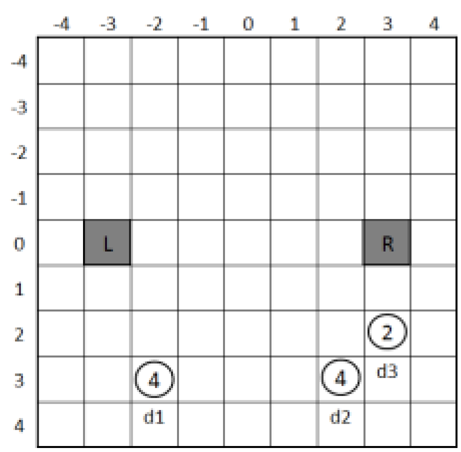
G20 = 1,5| |5



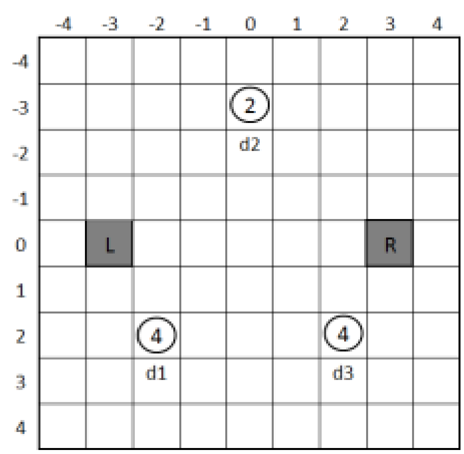
G21 = 5|1|5



G22 = |4,2,4|

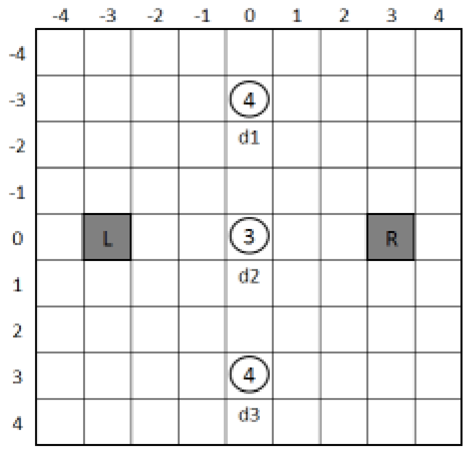


G23 = 4| |4,2

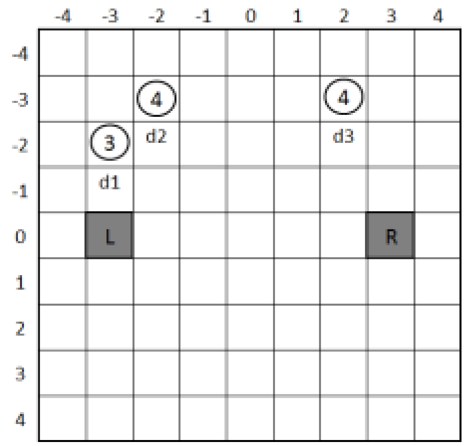


G24 = 4|2|4

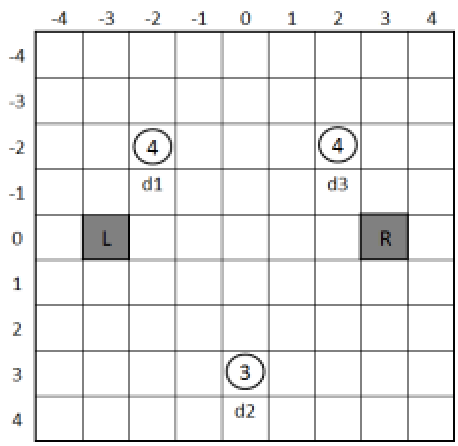
FIGURE 1 – *CONTINUED*



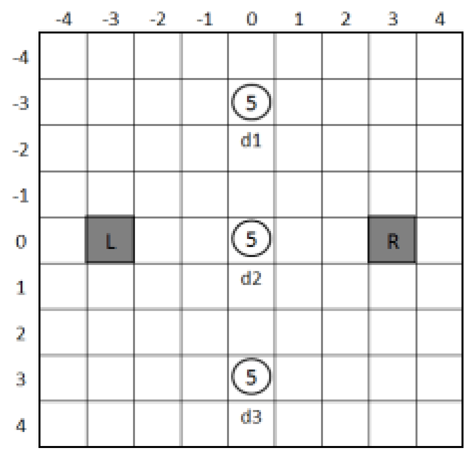
G25 = |4,3,4|



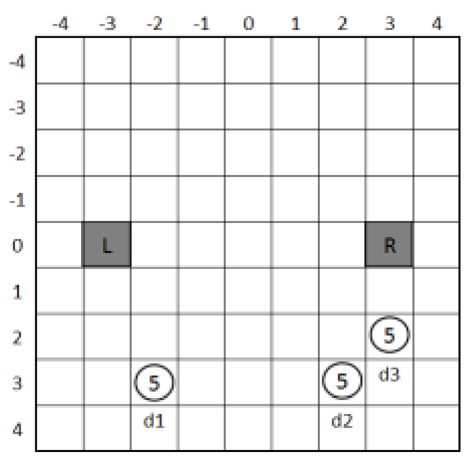
G26 = 3,4| |4



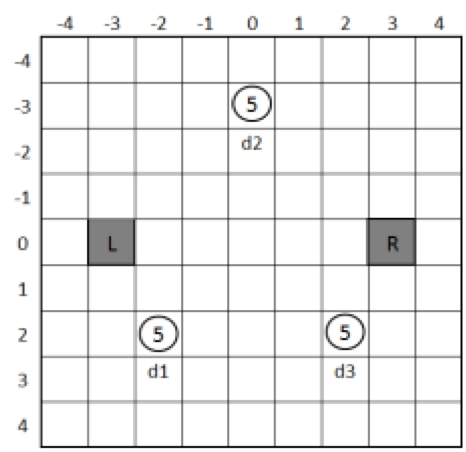
G27 = 4|3|4



G28 = |5,5,5|



G29 = 5| |5,5



G30 = 5|5|5

FIGURE 1 – *CONTINUED*

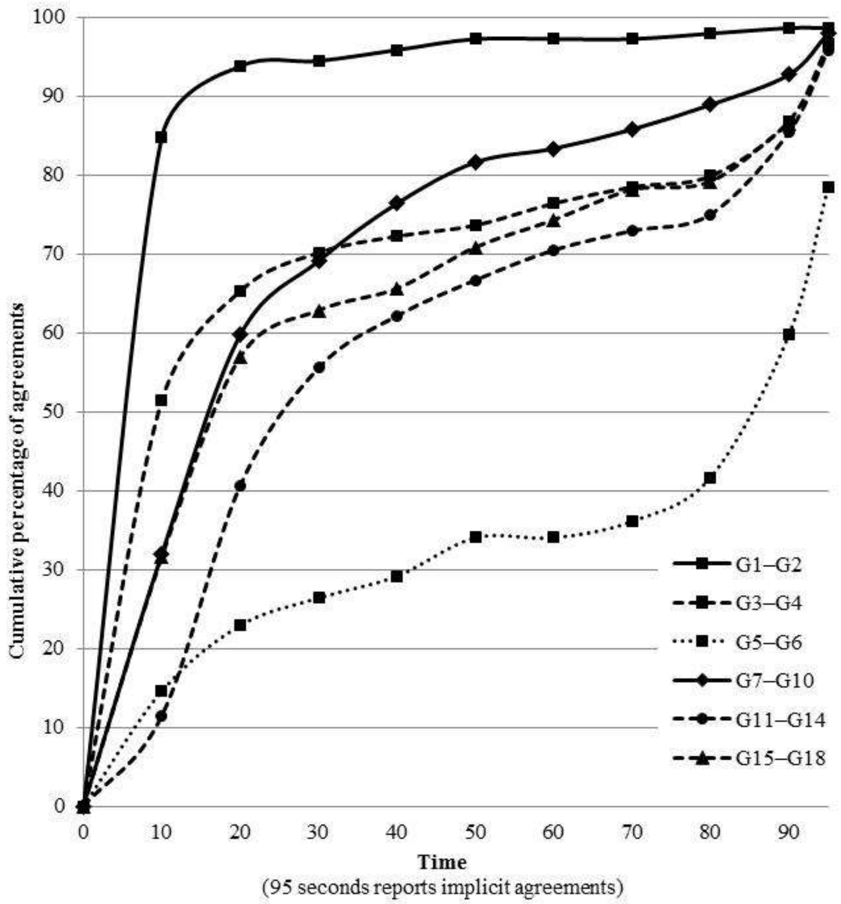


FIGURE 2A – CUMULATIVE DISTRIBUTIONS OF AGREEMENT TIMES FOR SET I IN MAIN TREATMENT (GAMES WITH COMMON DISC COLLECTIONS POOLED)

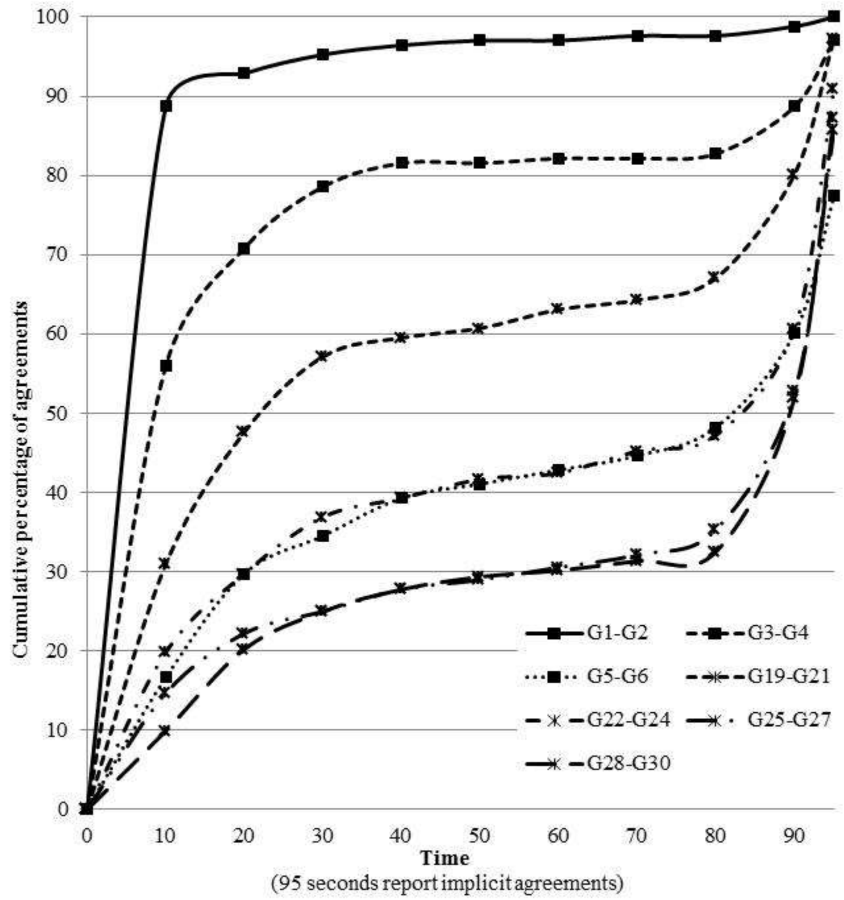


FIGURE 2B – CUMULATIVE DISTRIBUTIONS OF AGREEMENT TIMES FOR SET II IN MAIN TREATMENT (GAMES WITH COMMON DISC COLLECTIONS POOLED)

TABLE 1 – SUMMARY OF THE GAMES USED IN THE EXPERIMENT

Disc collection	LUE	LIE	Spatially neutral	Spatial cues pick out:			
				EE	LUE, not equal	Efficient, not LUE	LIE, not efficient
{5,5}	(5,5) (5,5)	(5,5)	G1 = |5,5|	G2 = 5| |5	-	-	-
{1,2,2,2,2,1}	(5,5) (5,5)	(5,5)	G7 = |1,2,2,2,2,1|	G8 = 2,2,1| |1,2,2	-	G9 = 2,1,1| |2,2,2 G10 = 2,1| |1,2,2,2	-
{5,6}	(5,6) (6,5)	(0,0)	G3 = |5,6|	-	G4 = 6| |5	-	-
{5,1,5}	(5,6) (6,5)	(5,5)	G19 = |5,1,5|	-	G20 = 1,5| |5	-	G21 = 5|1|5
{4,2,1,4}	(5,6) (6,5)	(4,4)	G15 = |4,2,1,4|	-	G17 = 2,4| |4,1	G18 = 2,1| |4,4	G16 = 4|2,1|4
{1,2,2,2,2,2,1}	(5,6) (6,5)	(5,5)	G11 = |1,2,2,1,2,2,1|	-	G13 = 2,1,2,1| |1,2,2	G14 = 2,2,2,2| |1,1,1	G12 = 2,1,2|1|2,2,1
{4,2,4}	(4,6) (6,4)	(4,4)	G22 = |4,2,4|	-	G23 = 4| |4,2	-	G24 = 4|2|4
{4,3,4}	(4,7) (7,4)	(4,4)	G25 = |4,3,4|	-	G26 = 3,4| |4	-	G27 = 4|3|4
{3,8}	(3,8) (8,3)	(0,0)	G5 = |3,8|	-	G6 = 8| |3	-	-
{5,5,5}	(5,10) (10,5)	(5,5)	G28 = |5,5,5|	-	G29 = 5| |5,5	-	G30 = 5|5|5

TABLE 2A – RESULTS FOR SET I IN MAIN TREATMENT

Game description	Average Efficiency	Percent Efficient	Percent Equal	Percent LUE	Percent SC (SC claims)	Percent Disag. (expl.)	NPA[a,b]
G1 = |5,5|	0.986	98.6	98.6	98.6	-	1.4 (0)	-
G2 = 5| |5	0.986	98.6	98.6	98.6	98.6 (84.7)	1.4 (1.4)	-
G3 = |5,6|	0.958	95.8	-	94.4	-	4.2 (1.4)	-
G4 = 6| |5	0.972	97.2	-	97.2	58.3 (58.3)	2.8 (1.4)	0.194*
G5 = |3,8|	0.802	79.2	-	79.2	-	19.4 (2.8)	-
G6 = 8| |3	0.760	75.0	-	73.6	38.9 (38.9)	23.6 (6.9)	0.033
G7 = |1,2,2,2,2,1|	0.983	97.2	87.5	87.5	-	1.4 (0)	-
G8 = 2,2,1| |1,2,2	0.981	94.4	87.5	87.5	87.5 (70.8)	1.4 (0)	-
G9 = 2,1,1| |2,2,2	0.958	95.8	79.2	79.2	12.5 (9.7)	4.2 (0)	0.125**
G10 = 2,1| |1,2,2,2	0.983	95.8	81.9	81.9	0 (0)	1.4 (0)	0.035
G11 = |1,2,2,1,2,2,1|	0.984	97.2	0.0	95.8	-	1.4 (0)	-
G12 = 2,1,2|1|2,2,1	0.986	98.6	0.0	94.4	0 (0)	1.4 (0)	-
G13 = 2,1,2,1| |2,2,1	0.943	93.1	1.4	88.9	51.4 (38.9)	5.6 (0)	0.097
G14 = 2,2,2,2| |1,1,1	0.915	90.3	1.4	87.5	0 (0)	8.3 (1.4)	0.036*
G15 = |4,2,1,4|	0.971	95.8	0.0	94.4	-	2.8 (2.8)	-
G16 = 4|2,1|4	0.944	94.4	0.0	87.5	0 (0)	5.6 (2.8)	-
G17 = 2,4| |4,1	0.957	94.4	0.0	93.1	68.1 (63.9)	4.2 (0)	0.528***
G18 = 2,1| |4,4	0.956	94.4	0.0	94.4	0 (0)	4.2 (0)	0.047**

Notes:

a – Normalized payoff asymmetry, NPA = $(w_F - w_U) / (w_F^* - w_U^*)$; where w_F and w_U are means of actual earnings of favored and unfavored players respectively, w_F^* and w_U^* are earnings of favored and unfavored players in the SC allocation.

b - Significance level in 1-tail Wilcoxon signed-rank test of comparison of payoffs to favored and to unfavored players averaging within matching groups: * = 10 percent, ** = 5 percent, *** = 1 percent.

TABLE 2B – RESULTS FOR SET II IN MAIN TREATMENT

Game description	Average Efficiency	Percent Efficient	Percent Equal	Percent LUE	Percent SC (SC claims)	Percent Disag. (expl.)	NPA[a,b]
G1 = |5,5|	1.000	100.0	100.0	100.0	-	0 (0)	-
G2 = 5| |5	1.000	100.0	100.0	100.0	100 (88.1)	0 (0)	-
G3 = |5,6|	0.976	97.6	-	97.6	-	2.4 (0)	-
G4 = 6| |5	0.964	96.4	-	96.4	69 (69)	3.6 (1.2)	0.417***
G5 = |3,8|	0.782	77.4	-	77.4	-	21.4 (3.6)	-
G6 = 8| |3	0.759	75.0	-	75.0	42.9 (42.9)	23.8 (6)	0.088
G19 = |5,1,5|	0.968	88.1	9.5	86.9	-	2.4 (1.2)	-
G20 = 1,5| |5	0.958	89.3	7.1	86.9	48.8 (41.7)	3.6 (2.4)	-0.107
G21 = 5|1|5	0.962	86.9	9.5	86.9	9.5 (7.1)	2.4 (1.2)	-
G22 = |4,2,4|	0.886	76.2	15.5	76.2	-	8.3 (2.4)	-
G23 = 4| |4,2	0.888	77.4	14.3	76.2	44 (38.1)	8.3 (1.2)	0.083
G24 = 4|2|4	0.867	76.2	13.1	76.2	13.1 (11.9)	10.7 (4.8)	-
G25 = |4,3,4|	0.838	78.6	7.1	78.6	-	14.3 (1.2)	-
G26 = 3,4| |4	0.872	82.1	6.0	78.6	51.2 (46.4)	10.7 (4.8)	0.214**
G27 = 4|3|4	0.805	76.2	6.0	75.0	6 (6)	17.9 (1.2)	-
G28 = |5,5,5|	0.802	76.2	6.0	76.2	-	17.9 (4.8)	-
G29 = 5| |5,5	0.885	82.1	9.5	82.1	34.5 (32.1)	8.3 (2.4)	-0.131
G30 = 5|5|5	0.845	77.4	10.7	77.4	10.7 (4.8)	11.9 (4.8)	-

Notes:

a – Normalized payoff asymmetry, NPA = $(w_F - w_U) / (w_F^* - w_U^*)$; where w_F and w_U are means of actual earnings of favored and unfavored players respectively, w_F^* and w_U^* are earnings of favored and unfavored players in the SC allocation.

b - Significance level in 1-tail Wilcoxon signed-rank test of comparison of payoffs to favored and to unfavored players averaging within matching groups: * = 10 percent, ** = 5 percent, *** = 1 percent.