Focality and Efficiency-Equality Tradeoffs in Bargaining: Experimental Evidence

Fabio Galeotti* Maria Montero† Anders Poulsen‡

June 10, 2015

Abstract

We collect experimental data from unstructured bargaining situations where we vary the set of feasible contracts, allowing us to assess the focality of three properties of bargaining outcomes: equality, Pareto efficiency, and total earnings maximization. Our main findings are that subjects avoid an equal earnings contract if it is Pareto inefficient; large proportions of bargaining pairs avoid equal and Pareto efficient contracts in favor of unequal and total earnings maximizing contracts, and these proportions increase when unequal contracts offer larger earnings to some players, even when this implies higher inequality. Finally, observed behavior violates the Independence of Irrelevant Alternatives axiom, a result we attribute to a 'compromise effect'.

Keywords: Bargaining, Pareto efficiency, equality, total earnings maximization, communication, Independence of Irrelevant Alternatives.

JEL Classification: C70; C72; C92.

*GATE Lyon Saint-Étienne, 93 Chemin des Mouilles, 69130 Écully, France.

†School of Economics, University of Nottingham, Nottingham NG7 2RD, United Kingdom.

‡Corresponding author. School of Economics and CBESS, University of East Anglia, Norwich NR4 7TJ, United Kingdom. We thank James M. Watson for helpful comments on the instructions. All errors are ours. Support from the Network for Integrated Behavioural Science (NIBS) is gratefully acknowledged.

1 Introduction

> The welfare properties of *equity* and *efficiency* may determine the focal equilibrium in any game, whether there is an arbitrator or not. (Myerson, 1991, p. 373)

Bargaining is ubiquitous in economic and social life. An employer negotiates with a union about wages and working conditions. A buyer and seller negotiate over price, product specifications, delivery, and warranty terms. A couple negotiate over which house to buy. Creditors negotiate over the division of the assets of a bankrupt company.

It is an important task for economists and other social scientists to predict which agreement, if any, the bargainers will reach. Will it be one that equates earnings? Will it be efficient, or maximize total earnings? In this paper we present the findings from an empirical investigation related to Myerson's conjecture quoted above, that a focal agreement possesses some combination of desirable welfare properties such as efficiency and equity. We carry out this investigation in a bargaining environment where subjects are free to make several offers in real time, they can communicate via written messages, and any agreement is binding.

Our central research questions are: Which agreements are focal when there is a *tradeoff* between different welfare properties? And how does the nature of the focal agreement depend on and vary with changes in the terms of the tradeoff? Although these questions seem quite basic and relevant, no systematic investigation has, as far as we know, taken place.

In this paper, we consider tradeoffs between three potentially salient payoff properties: equality, Pareto efficiency, and total earnings maximization. For simplicity, we will refer to the latter as 'total-earnings efficiency',[1] and to Pareto efficiency as 'efficiency'.

Suppose there is no feasible contract with all three payoff properties. An equal earnings agreement is then not total-earnings efficient, but it may or may not be efficient.[2] How focal is an equal earnings agreement in each case?

[1] In this paper, Pareto efficiency is defined in terms of money amounts. Pareto efficiency in terms of preferences does not necessarily coincide with Pareto efficiency in terms of money amounts, since subjects may care about the other subjects' payoffs.

[2] If a contract is total-earnings efficient, it must be efficient, but the converse is not true. As a simple example, consider an equal contract offering (Player 1, Player 2) earnings $(40, 40)$. If the only other feasible contract is $(30, 80)$, then $(40, 40)$ is total earnings

It seems to us that bargaining situations where an equal earnings contract is not total-earnings efficient are quite plausible and common. In principle, players could achieve equality and total-earnings efficiency simultaneously by agreeing on actions that maximize the size of the 'cake' and on any transfers needed in order to equate earnings. However, if there are constraints on the transfers that can be made, an equal earnings contract may not be total-earnings efficient, and may even fail to be efficient.[3]

As already mentioned, existing empirical bargaining research has, to the best of our knowledge, done very little work on characterizing focal agreements in the presence of tradeoffs between equality and efficiency. A consistent finding (see Camerer, 2003; Roth, 1995) is that equality of money earnings is a powerful focal point in bargaining, and many other interactive decision situations.[4] However, the equal earnings outcome was also total-earnings efficient (and hence efficient) in these experiments. We are only aware of two exceptions, Herreiner and Puppe (2010) and Isoni et al. (2014), described below.

We collect data from an experimental bargaining environment where pairs of subjects negotiate over a set of feasible contracts, and where a contract specifies an amount of money to each person. An example is where there are three contracts, (40,120), (50,50), and (120,40), with the first (second) number indicating the amount of money going to the first (second) person. The first and third contract offer unequal and total-earnings effi-

inefficient but still efficient. If the alternative contract is $(50, 80)$, then the equal contract is both inefficient and total earnings inefficient.

[3]An example is a household or business partnership where total earnings efficiency requires a degree of specialization (in the former case it may require that one person works outside and the other at home), and where there are financial, technological, informational, or legal constraints on the transfers that can be made. Other examples are the division of a deceased person's estate among the heirs, or creditors dividing the assets from a bankrupt company. In the estate example, suppose two siblings inherit two indivisible objects, A and B. Both siblings prefer object A to object B. They are liquidity constrained, so the sibling that gets A cannot compensate the sibling that got B. They may agree to get one object each, or they may sell the items and divide the proceeds equally. Depending on how marketable the items are, the proceeds from the sale may be quite low, and so the equal contract can be efficient but not total-earnings efficient, or even inefficient. A similar example is where two partners dissolve a firm, or a couple divorce.

[4]See, e.g., Camerer (2003), Feltovich and Swierzbinski (2011), Fouraker and Siegel (1963), Gächter and Riedl (2005), Isoni et al. (2013), Isoni et al. (2014), Karagözoglu and Riedl (2015), Karagözoglu and Bolton (2013), Nydegger and Owen (1975), Roth (1995), and Roth and Murnighan (1982).

cient earnings; the second gives equal and efficient but not total-earnings efficient earnings. If the subjects can agree on one of these contracts, they get the implied money; otherwise neither player gets any money. The bargainers are free to make as many proposals as they wish within a certain period of time. They can communicate via chat with each other, and any agreement is binding.[5] We also analyze the dynamics of bargaining, using data on contract proposals, agreement times, and the content of the chat messages that were exchanged.

We study several games differing in the set of available contracts, and use across-game comparisons to answer the following questions:

1. How does the focality of an equal contract vary with its efficiency and total earnings properties? For example, we compare the focality of the equal contract in games $\{(40,120),(80,80),(120,40)\}$ (where the equal contract maximizes total earnings), $\{(40,120),(50,50),(120,40)\}$ (where the equal contract is efficient but not total earnings efficient) and $\{(40,120),(30,30),(120,40)\}$ (where the equal contract is inefficient). Note that we keep contracts other than the equal contract unchanged.

2. Will an equal earnings contract become more or less focal if an alternative unequal earnings contract offers larger earnings to one of the players and the same to the other person, thus also making them more unequal? For example, we compare the focality of $(50,50)$ in games $\{(40,70),(50,50),(70,40)\}$ and $\{(40,240),(50,50),(240,40)\}$.

We also compare symmetric and asymmetric sets of contracts. In a *two-sided* game there is an equal earnings contract and two unequal earnings contracts, each of which favors one of the bargainers. An example is (40,120),(50,50),(120,40). In a *one-sided* game there is an equal but only one unequal contract, so that only one of the players can be favored over the other; an example is (40,120),(50,50). We collected data for both types of games since each seemed relevant for real world bargaining situations,[6]

[5] Due to the absence of a rigid bargaining protocol, these bargaining situations are often referred to as *unstructured* bargaining situations (see, for example, Anbarci and Feltovich, 2013; Feltovich and Swierzbinski, 2011; Gächter and Riedl, 2005; Isoni et al., 2014; Roth and Malouf, 1979; Roth and Murnighan, 1982; and the surveys in Camerer, 2003; Roth, 1995).

[6] As a stylized example, suppose two siblings inherit a house. They can either sell the house and divide the proceeds equally, or one of them can keep the house and pay the other a small (due to liquidity constraints) rent. If both siblings live in the same city, the situation is naturally two sided. However, if one sibling has moved overseas, the

and and comparing them allows us to investigate if the focality of an equal earnings contract differs depending on whether the game is one or two-sided.

Comparing one- and two-sided games also allows us to test the Independence of Irrelevant Alternatives (IIA) axiom (Nash, 1950). This important axiom from cooperative bargaining and social choice theory states that if a contract is agreed on when there is a large set of available contracts, then the same contract (assuming it is still feasible) is selected when the set of alternative contracts is smaller.[7] Consider a two and a one-sided game (40,120),(50,50),(120,40) and (40,120),(50,50). IIA states that if (50,50) is selected in the first game, then it is also selected in the latter game. The proportion of agreements on (50,50) in the one-sided game is therefore at least as large as in the two-sided game. Our data allows us to test this.

Our work is related to three existing contributions, Isoni et al. (2014), Herreiner and Puppe (2010) and Camerer et al. (2015), but differs from them in important ways.

Isoni et al. (2014) consider unstructured bargaining situations where players negotiate by claiming valuable 'discs' that are placed on a 'table', and each player is represented by a 'base' on the table (a similar representation is used in Isoni et al., 2013; see also Mehta et al., 1994). They measure the extent to which players use payoff irrelevant spatial cues as a means to select an agreement (see Schelling (1960)). In this type of environment there are only two possibilities: either the equal allocation is inefficient, or it is both efficient and total earnings efficient. The equality–efficiency tradeoff is thus either very severe, or there is no tradeoff at all. Our experimental design allows us to examine the set of possible tradeoffs more fully, including the, arguably empirically plausible, intermediate case where an equal allocation is efficient but does not maximize the sum of players' money earnings.

In the experiment by Herreiner and Puppe (2010) subjects negotiate for 10 minutes over how to divide four indivisible objects. Each bargainer has a monetary value associated with each bundle of goods, hence bargainers negotiate over which of sixteen payoff pairs to agree on (corresponding to

situation is one sided: either they sell the house and split the proceeds equally, or the sibling that lives locally keeps the house.

[7] A more formal statement of IIA is: Supose a contract x is feasible both when the set of feasible contracts is S, and when it is T, where $T \subseteq S$. Then, if x is agreed on when the set is S, x is also agreed on when the set is T.

the sixteen different divisions of the four objects). Their games differ in terms of several payoff pairs (the entire Pareto frontier differs, as well as the interior, dominated, payoff pairs). This means that it is not straightforward to attribute any observed behavioral difference across games to the presence or absence of some specific property of the feasible payoffs. Our experiment varies the set of available contracts more systematically, often by changing just one contract at a time. We think this makes our experimental design more suited for an investigation of the effects of changes in the efficiency-equality trade off on bargaining behavior.[8]

Camerer et al. (2015) study unstructured bargaining with one-sided private information about the size of the available pie. Each pair bargains over the uninformed player's payoff. Subjects have two seconds to fix their initial offers, without seeing the offer of their partner, and then they have ten seconds to reach an agreement. At the end of a round, the size of the pie is revealed to the uninformed player. The main findings from this experiment are that disagreement rates are monotonically decreasing with the pie size, and small and medium pies are split equally. An important difference with our experiment is that an equal and total earnings efficient allocation always exists, though only the informed player knows which agreement achieves this allocation. Another way in which the experiments differ is that, besides making offers, our subjects can communicate via unstructured cheap-talk messages.

Our main findings are as follows. First, in the benchmark case where there is an equal contract that is also total earnings efficient, almost all bargainers settle on it as expected.

Second, we observe a strong tendency for bargainers to avoid the equal contract when it is inefficient. The percentage of bargaining pairs settling on an equal and inefficient contract never exceeds 10%. [9]

Third, when we consider the important intermediate territory where the equal earnings contract is efficient but not total-earnings efficient, we observe that the focality of the equal contract falls gradually as we lower

[8]Moreover, in Herreiner and Puppe (2010) when an equal contract is efficient it is close to being total earnings maximizing, so it is not possible to clearly assess the focality of equal earnings contracts that are efficient but far from total earnings maximizing.

[9]We should bear in mind that we measure Pareto efficiency in money terms, and our subjects may have additional concerns other than the maximization of money earnings. Still, our findings that most subjects disregard an equal and inefficient contract appear even stronger when allowing for this possibility.

its total earnings. In some of our games, more (less) than half of bargaining pairs settle on an unequal (equal) contract. For example, given the set of contracts $\{(50, 50), (40, 120)\}$, we find that only 32% of bargainers settle on the equal contract compared with 59% of bargainers settling on the unequal contract. We are, to the best of our knowledge, the first to find in a free-form unstructured bargaining situation that an equal and efficient but not total-earnings efficient contract can be less focal than an unequal and total-earnings efficient contract.

We also investigate the effect of changing the unequal allocations so that they offer higher total earnings but become more unequal. For example, will the equal contract $(50, 50)$ be more focal when the alternative unequal contracts are $(40, 70), (70, 40)$, or when they are $(40, 240), (240, 40)$? It is not a priori clear how the focality of the equal contract will differ between the two contract sets. A concern for own money earnings, possibly combined with a desire to maximize total earnings, can be expected to make the equal contract less focal in the second game; on the other hand, a sufficiently strong inequality aversion among subjects would generate the opposite effect. The data show that increasing the payoff to the favored player in the unequal contracts significantly *reduces* the focality of an equal and efficient contract. For example, we observe that in the bargaining game (40,70), (50,50), (70,40) about 71% (27%) of bargainers agree on the equal (an unequal) contract, while in (40,240), (50,50), (240,40) the percentages are about 36 % (51 %). We can interpret this as suggesting that bargainers tend to be more occupied with maximizing their own, and/or total, earnings than with ensuring equality of earnings.

The fifth main finding is that in most games, and on average across all games, more bargaining pairs settle on an equal contract in the two than in the corresponding one-sided game. This can be interpreted as a violation of the axiom of Independence of Irrelevant Alternatives (IIA), formulated in Nash (1950). We think there is a natural interpretation of this failure. In a two-sided game, such as {(40,120), (50,50), (120,40)}, the equal contract can be salient for two reasons: it offers equal earnings, and it can serve as a compromise that resolves the conflict of interest over which unequal contract the bargainers should agree to. In the one-sided game {(40,120),(50,50)}, the equal contract still possesses some inherent focality due to offering equal payoffs, but it is no longer a compromise between two other contracts. This makes the equal earnings contract less focal in one- than in two-sided games. To the best of our knowledge, we are the first to empirically observe a failure of the IIA axiom in bargaining due to a ”compromise effect”.[10]

The rest of the paper is organized as follows. In Section 2 we describe the experimental design, the procedures, and the bargaining games. The data are presented and analysed in Section 3. Some limitations of our study that suggest future research are discussed in Section 5. Section 6 concludes.

2 Experimental Design and Bargaining Games

In this section we present our experimental design, describe the bargaining games, outline the main research questions, and discuss some features of the experimental design.

2.1 Design and procedures

We ran the experiment in spring 2014, in the experimental lab of the Centre for Behavioural and Experimental Social Science, at the University of East Anglia (Norwich, United Kingdom). We ran 8 sessions with 14 participants each, making a total of 112 subjects.[11] The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007); recruitment was done using ORSEE (Greiner, 2004). Average earnings (including a £4 show-up fee) were £16.15. Each session lasted no more than one hour.

Subjects arrived to the lab and were allocated a desk. Instructions (see Appendix A) were circulated, and were read aloud by the experimenter.

[10]Nydegger and Owen (1975) test IIA by comparing a basic situation in which two subjects must agree on how to divide a dollar with a constrained situation in which player 1 cannot receive more than 60 cents whereas player 2 can still potentially receive the whole dollar. They observe that subjects divide the dollar equally in both cases, consistent with IIA. However, their experiment is a relatively weak test since there is a contract that is both equal and total earnings maximizing, which makes it strongly focal. To the best of our knowledge, our experiment is the first one to test IIA in a more demanding setting where no contract is both equal and maximizes total earnings. Bone et al. (2014) also observe a tendency to compromise in their data. However, their experiment is not a direct test of IIA since they do not compare larger choice sets with smaller ones.

[11]Background information on the participants is in Appendix A.

Subjects were informed that they would make decisions in 22 scenarios. In each scenario they would be randomly matched with one of the other participants (stranger matching), and would be presented with a set of (two or three) feasible contracts. Each contract specified a number of points to each paired subject, such as (50,50) or (40,240).

The two matched subjects were referred to as Persons 1 and 2.[12] A subject was informed that he or she would in some scenarios be referred to as Person 1, and in others as Person 2.[13]

In each scenario each pair of subjects had 120 seconds to negotiate over which contract to agree on. During this time they could make contract proposals, and write messages to each other. A subject could make a contract proposal by clicking with their mouse on one of the feasible contracts (a screenshot is provided in Figure 2.1). As long as an agreement was not reached a subject was free to change his or her contract proposal, or to retract it without replacing it with a new one, in real time and as frequently as desired. Subjects could also decide not to make any proposals at all. A binding agreement was reached if and only if the two players proposed (that is, clicked on) the same contract.[14] If the subjects did not reach an agreement before the 120 seconds expired, neither earned any points from the scenario.

The subjects could also write *cheap talk* messages to each other while making proposals. There were no constraints on the number and content of messages, except that subjects were told to avoid writing messages that revealed their identity, that physically threatened the other person, or that discussed what might or might not happen outside the lab. If it was detected that a participant wrote any such messages, the subject would not receive any money earnings. Subjects could make proposals without sending messages, and vice versa.

---

[12]The advantage of using labels for the two subjects is that it is easy to describe and refer to a contract and clear who gets how much. Moreover, each matched pair of subjects see the same representation of contracts on the screens (and this is common knowledge). The potential disadvantage is that labels may have an effect on behavior. We did not find any differences between the two players in the data (see Online Appendix).

[13]The alternative approach, that a participant was either Person 1 or 2 in all scenarios, has the disadvantage of reducing the number of possible matchings dramatically (a subject in the role of Person 1 could only be matched with those in the role of Person 2), and hence a subject would more frequently be matched with the same other participant.

[14]The same agreement technology is used in other papers, such as Roth and Murnighan (1982) and Feltovich and Swierzbinski (2011).

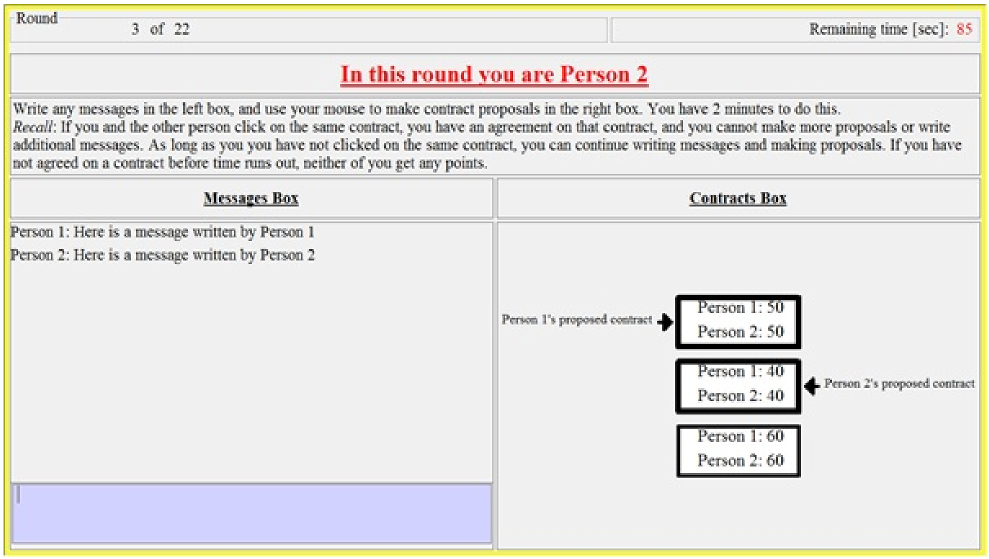

Figure 1: Screenshot.

The 22 bargaining scenarios were presented to different subjects in a different order, not known to them in advance. Also, subjects were informed that they would play the scenarios against different co-participants. All this was common knowledge.[15]

When everyone had completed the 22 scenarios the computer randomly selected three rounds for payment. The conversion rate from points to pounds was 20 points = £1.

2.2 The Bargaining Games

We refer to an unstructured bargaining game with a certain set of feasible contracts as a *bargaining game* (this is equivalent to a scenario in the experiment). We collected data for 22 different bargaining games, shown in Table 1 below. The numbers are measured in experimental points.

[15]There were some unavoidable constraints on the matching protocol, due to the real-time nature of the bargaining: if a participant plays a given scenario X as the, say 7th in his game sequence, then another subject in the room must also be playing X as her seventh scenario. The matching protocol that we designed maximized the dispersion of matchings subject to these constraints. See Appendix B for details.

Games 1–11 are of the form $\{(z,z),(x,y),(y,x)\}$, where $z > 0$ and $0 < x < y$. Since there are two alternatives to the equal contract, each of which favors a different player, we call them *two-sided* games. Games 12-22 are of the form $\{(z,z),(x,y)\}$, and are *one-sided* games. Each two-sided game has a corresponsing one-sided game.

When $z \leq x$, the equal earnings contract (z,z) is inefficient. These are Games 5-6, 9-11, and their corresponding one-sided versions 16-17, 20-22. When $z > x$, the equal earnings outcome is efficient. This applies to games 1-4, 7-8, 12-15, and 18-19. If $2z \geq x + y$, the equal contract is total earnings efficient, and thus also efficient. There are two such games, 1 and 12.[16]

2.3 Game Comparisons

Comparing behavior across bargaining games allows us to answer the following questions:

Question 1: How Does the Focality of the Equal Contract Depend on its Efficiency and Total Earnings Properties?

We compare bargaining behavior in Games 1–6, and Games 12–17. Here we fix two unequal and payoff efficient contracts, $(x,y) = (40,120)$ and $(y,x) = (120,40)$ (only $(x,y) = (40,120)$ for one-sided games), and vary the payoff z, where $z \in \{30,40,50,60,70,80\}$.

Question 2: How Does the Focality of an Equal and Efficient (but not total-earnings efficient) Contract Vary with the Earnings and Inequality Properties of the Unequal and Total-earnings efficient Contracts?

We fix an equal contract $(z,z) = (50,50)$, and consider different unequal contracts, $(40,y)$ and $(y,40)$, with $y \in \{70,120,240\}$. These are Games 4, 7, and 8 (two-sided) and Games 15, 18, and 19 (one-sided).

When y increases, self-interested subjects would find the contract that favors them more attractive, and may try harder to get an agreement on that contract. We may then expect agreements on the equal contract to fall

[16] We only collected data for two games with an equal and total earnings efficient contract, since we anticipated that the equal earnings outcome would be strongly salient. The data confirmed this.

Game	Feasible contracts	Equal contract efficient?	Equal contract total-earnings efficient?
1	(80,80),(40,120),(120,40)	Y	Y
2	(70,70),(40,120),(120,40)	Y	N
3	(60,60),(40,120),(120,40)	Y	N
4	(50,50),(40,120),(120,40)	Y	N
5	(40,40),(40,120),(120,40)	N	N
6	(30,30),(40,120),(120,40)	N	N
7	(50,50),(40,70),(70,40)	Y	N
8	(50,50),(40,240),(240,40)	Y	N
9	(50,50),(60,70),(70,60)	N	N
10	(50,50),(60,120),(120,60)	N	N
11	(50,50),(60,240),(240,60)	N	N
12	(80,80),(40,120)	Y	Y
13	(70,70),(40,120)	Y	N
14	(60,60),(40,120)	Y	N
15	(50,50),(40,120)	Y	N
16	(40,40),(40,120)	N	N
17	(30,30),(40,120)	N	N
18	(50,50),(40,70)	Y	N
19	(50,50),(40,240)	Y	N
20	(50,50),(60,70)	N	N
21	(50,50),(60,120)	N	N
22	(50,50),(60,240)	N	N

Table 1: The 22 bargaining games.

Notes: Y = Yes; N = No.

and agreements on an unequal contract to increase (of course, disagreement may go up as well). Subjects may also care positively about the other person's earnings (see Charness and Rabin, 2002; Engelmann and Strobel, 2004), and since an increase in y makes it cheaper for a subject to increase the earnings of the other subject, we may expect this to have the same impact on the focal agreements.[17] On the other hand, when y increases, the unequal contracts become more unequal, and subjects may dislike settling on a contract that gives them less, or more, than the other subject (see Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). This can make the equal earnings contract more focal. Thus it is *a priori* unclear what the overall effect of changing the y parameter is, and our experimental data will shed light on this.

Question 3: Does the Focality of Equality Differ in One and Two-Sided Games?

We considered both one and two-sided games because in real world bargaining the set of feasible contracts may or may not be symmetric. We also wished to test the hypothesis that the equal contract may be more salient in the two than in the one-sided case, since the one-sided setting eliminates the potential coordination problem of deciding which of the two unequal contracts to agree on.

As mentioned in the Introduction, comparing one and two-sided games allows for an empirical test of the Independence of Irrelevant Alternatives (IIA) axiom (Nash, 1950). A finding that more people agree to equality in the two- than the one-sided game can be interpreted as a violation of IIA.

[17]Suppose $y = 120$. If players agree on the unequal contract that gives 120 to one player and 40 to the other instead of on the equal contract (50,50), one player is in effect sacrificing $50 - 40 = 10$ points in order to benefit the other player by $120 - 50 = 70$ points. Thus the 'benefit–sacrifice ratio' is 7. Equivalently, the price of transferring one point to the other player is $1/7$ points. When y is larger, 240, the sacrifice-benefit ratio increases to 19, and the price falls to $1/19$. Similarly, the opportunity cost of agreeing to the equal contract in terms of total earnings is 110-100=10 when $y = 70$, $170 - 100 = 70$ when $y = 120$, and $280 - 100 = 180$ when $y = 240$.

2.4 Discussion of Some Experimental Design Features

Since we collect data using a within–subject design, there are several potential effects that can introduce dependencies between games. [18] In what follows we describe how our design aims to minimize these effects and argue that the behavioral patterns we would expect if these effects were significant are not found in the data.

2.4.1 Learning Effects

Subjects can naturally be expected to learn as they move through the sequence of twenty-two scenarios, and to transfer their experience from early scenarios to later ones. However, since subjects do not encounter the scenarios in the same order, these effects should not lead to systematic aggregate effects that bias comparisons across scenarios.

If learning effects were significant, we would expect that the agreement rate and the proportions of agreements on the various contracts would change over time. There is however no such evidence in the data – see the Online Appendix.

2.4.2 Repeated Game Effects

By repeated game effects we mean any behavior in a current scenario that is intended to influence the behavior of co-players in future scenarios. For example, a subject might believe that he or she, by punishing a 'greedy' co-player in the current scenario, can induce future co-players to behave more co-operatively. Or that he or she, by being 'generous' now can induce generosity from future co-players. We believe our design minimises such reasoning and behavior. As already mentioned, subjects played different scenarios against different co-participants, making it difficult to reward or punish a specific individual for their behavior in past scenarios.

One way to look for repeated game effects in the data is to consider the subjects' chat conversations; it would appear natural for subjects to use the chat messages to reinforce and make any repeated game reasoning more salient. We analysed the chat messages and find only very few messages

[18] Of course, an ideal method would be to use a between subjects design, where each subject only makes a decision in a single scenario. This would require a very large number of subjects, sessions, and research budget.

where subjects express or suggest repeated game reasoning. Furthermore, the other subject almost always either did not react to or discounted such suggestions (see the Online Appendix).

2.4.3 Super Game Heuristics

It is conceivable that the subjects could coordinate on a normative 'super game rule' or heuristic for the overall game, composed of the 22 scenarios. For example, subjects could use a rule whereby in each scenario they select a total-earnings efficient outcome, to ensure that the overall outcome will be also total-earnings efficient. Alternatively, they could select an equal earnings outcome in all scenarios, to make sure that the overall outcome is also equal. If such a rule was used on a large scale by the subject population, we would expect to see approximately the same rate of agreement on inequality in all our 22 games. We do not observe this in the data – rather, as we shall describe below, behavior varies in a systematic way across games, suggesting that observed behavior is mostly specific to the game that is currently played rather than reflecting some rule or heuristic that is being applied to all the games.

3 Experimental Findings: Aggregate Bargaining Outcomes

Table 2 shows descriptive statistics for each of the 22 games (the *Game* column).[19] The number of bargaining pairs that played each game is given in the *Obs* column.[20] The table reports for each game the proportions of each of the three possible outcomes of bargaining, namely a disagreement, an agreement on equality, or an agreement on inequality (columns *Rate of disagreement*, *Rate of agreement on equality*, and *Rate of agreement on inequality*). These proportions sum to 100, and the rate of agreement equals 100−*Rate*

[19] We do not find any statistically significant effects of subject labels (1 or 2) on behavior. Similarly, the way in which a given set of contracts was ordered on the screen has no effect. Details are in the Appendix. We therefore pool all data.

[20] A technical problem at the end of one of our sessions resulted in the loss of some data; as a result we have a different number of observations for some games.

of disagreement.[21] The table also shows the average time to agree for the pairs that did reach an agreement (column *Average time to agree*), the average total earnings of a bargaining pair (*Average total-earnings efficiency*, both in points and as a percentage of the maximum achievable value), the number of proposals made per subject (*Average number of proposals*), and the average number of messages sent per subject (*Average number of messages*).

Whenever we make pairwise comparisons of games, we use session averages as the units of observation, in order to control for the non-independence of the observations at the individual level. All statistical tests are two-tailed, and, unless otherwise mentioned, significance refers to the 5 % level. The Appendix reports the *p* values of all the pairwise comparisons of the games.

[21] A different measure is to report the equal and unequal agreements as proportions out of the total number of agreements (that is, the proportions of each type of agreement, conditional on an agreement); this does not change any of the results reported below. See the Online Appendix.

Game	Obs	Feasible Contracts	Rate of disagree-ment	Rate of agreement on equality	Rate of agreement on inequality	Average time to agree (in sec.)[a]	Average total-earnings efficiency (points)	Average total-earnings efficiency (%)	Average number of proposals[a]	Average number of messages[a]
1	54	(80,80), (40, 120), (120,40)	1.82%	94.55%	3.63%	39.3	157.09	98.18%	1.25	2.06
2	53	(70,70), (40, 120), (120,40)	0%	83.02%	16.98%	48.74	143.4	89.62%	1.24	3.05
3	51	(60,60), (40, 120), (120,40)	8.93%	78.57%	12.5%	50.02	114.29	71.43%	1.46	3.41
4	50	(50,50), (40, 120), (120,40)	10.71%	46.43%	42.86%	68.86	115	71.88%	1.45	4.61
5	49	(40,40), (40, 120), (120,40)	10.91%	3.64%	85.45%	77.67	139.64	87.27%	1.42	5.09
6	49	(30,30), (40, 120), (120,40)	12.50%	7.14%	80.36%	68.76	132.86	83.04%	1.48	4.87
7	55	(50,50), (40, 70), (70,40)	1.79%	71.43%	26.78%	57.31	100.89	91.72%	1.48	3.19
8	48	(50,50), (40, 240), (240,40)	12.73%	36.36%	50.91%	70.98	178.91	63.90%	1.43	4.98
9	55	(50,50), (60,70), (70,60)	1.79%	3.57%	94.64%	62.18	126.61	97.39%	1.38	3.94
10	49	(50,50), (60, 120), (120,60)	10.91%	3.64%	85.45%	75.57	157.45	87.47%	1.38	4.67
11	48	(50,50), (60, 240), (240,60)	14.29%	8.93%	76.78%	81.29	239.29	79.76%	1.42	5.15
Tot. 2-sided	609	-	7.88%	39.57%	52.55%	63.20	145.88	83.76%	1.40	4.10
12	53	(80,80), (40, 120)	5.36%	92.86%	1.78%	35.91	151.43	94.64%	1.33	2.04
13	56	(70,70), (40, 120)	0%	91.07%	8.93%	47.89	141.79	88.62%	1.13	2.87
14	53	(60,60), (40, 120)	5.36%	64.29%	30.35%	55.49	125.71	78.57%	1.27	2.78
15	51	(50,50), (40, 120)	8.93%	32.14%	58.93%	61.41	126.43	79.02%	1.28	3.38
16	55	(40,40), (40, 120)	1.79%	5.36%	92.85%	33.98	152.86	95.54%	1.06	2.01
17	56	(30,30), (40, 120)	0%	0.00%	100%	30.32	160	100.00%	1.02	1.74
18	54	(50,50), (40, 70)	3.57%	60.71%	35.72%	47.8	100	90.91%	1.15	2.65
19	51	(50,50), (40, 240)	8.93%	30.36%	60.71%	52.96	200.36	71.56%	1.23	3.28
20	55	(50,50), (60, 70)	0%	1.82%	98.18%	28.35	129.45	99.58%	1.07	1.41
21	55	(50,50), (60, 120)	1.79%	5.36%	92.85%	30.47	172.5	95.83%	1.15	1.63
22	55	(50,50), (60, 240)	1.79%	0.00%	98.21%	43.02	294.64	98.21%	1.22	2.5
Tot. 1-sided	615	-	3.41%	34.96%	61.63%	42.27	159.61	90.21%	1.17	2.39
Tot.	**1224**	**-**	**5.64%**	**37.25%**	**57.11%**	**52.44**	**152.78**	**87.00%**	**1.29**	**3.24**

Table 2: Average aggregate bargaining outcomes. Note: The average number of proposals and average number of messages are per subject.

3.1 Agreement Rates

The rate of disagreement is in general quite low (the average is below 6%). In several games there are no disagreements at all, and the disagreement rate never exceeds 15%. The disagreement rates tend to be lower than in other unstructured bargaining experiments, such as Gächter and Riedl (2005) (where the average rate of disagreement is 16.7 %) and Roth and Murnighan (1982) (17%), but quite close to those found in other studies, such as Herreiner and Puppe (2010) (4.7%) and Isoni et al. (2014) (5.3 %).

In spite of the generally high agreement rate, there are some noteworthy differences between the games. In two-sided games the agreement rate tends to decrease as the equal contract becomes less efficient (Games 1-6). In particular, the agreement rates of games 1 and 2 are significantly larger than those of Games 3-6 (Wilcoxon signed-rank tests, $p < 0.05$).[22] There is no significant pattern for the corresponding one-sided Games 12–17. We summarize these findings as:

Finding 1. *In two-sided games 1–6, the agreement rate tends to fall as the equal contract offers lower and lower total earnings and eventually ceases to be efficient. In one-sided games (12–17) there is no significant pattern.*

A natural explanation is that the focality of the equal contract falls when its total payouts drop, and players are then more likely to consider alternative contracts. Since two-sided games present subjects with two such contracts, each of which is preferred by a different player, this increases the conflict of interest and makes it more difficult for subjects to agree. In one-sided games there is only one unequal allocation, so that the conflict of interest is less pronounced.

A comparison of two- and one-sided games shows that there tends to be more disagreement in the two- than in the corresponding one-sided game. Pooling the data of all two-sided and all one-sided games, the agreement rates are significantly higher for one-sided games ($p = 0.022$).

Finding 2. *The average rate of disagreement in two-sided games is significantly higher than in one-sided games.*

We finally consider how the agreement rate is affected when, keeping the equal contract fixed, we make the unequal contract(s) more unequal.

[22] There is no statistically significant difference between Games 1 and 2, and between Games 3, 4, 5, and 6 respectively ($p > 0.1$).

In two-sided games where the equal contract is efficient (games 7, 4, and 8), increasing the amount of money the favored player receives results in an increasing disagreement rate (the drop is significant from Game 7 to 4 (p = 0.048)). Something similar happens when the equal contract is inefficient (Games 9-11), where the agreement rate in Game 9 is significantly larger than in Games 10 and 11 ($p < 0.05$). In the corresponding one-sided games (Games 18, 15 and 19 and 20-22) the disagreement rate is weakly increasing as the unequal allocation offers higher payouts but becomes more unequal; this effect however is not statistically significant.

We summarise these findings in:

Finding 3. *In two-sided games, making the unequal contracts more unequal by increasing the payout to the favored player while keeping the payoff to the other player constant tends to increase the disagreement rate. No significant effect is found in one-sided games.*

Once more, a natural explanation for these findings is that making the unequal contracts more unequal increases the conflict of interest in two-sided games, and this makes it harder to agree. In one-sided games, there is by design no potential for disagreement over which of the unequal contracts to settle on, and thus there is less disagreement.

3.2 Agreements

3.2.1 Question 1: How Does the Focality of the Equal Contract Depend on its Efficiency and Total Earnings Properties?

In Games 1–6 and 12–17 more bargaining pairs settle on the equal and efficient contract than on an unequal and total earnings maximizing contract (with the exception of Game 15 where the opposite is observed). As we lower the total payouts of the equal contract there is in both two and one-sided games a monotonic and statistically significant drop in the rate of agreement on the equal contract[23]. We also observe that as soon as the equal contract becomes inefficient (Games 5 and 16), there is a dramatic

[23] The relationship between the rates of agreement on equality for two-sided games is: Game 1 = Game 2 > Game 3 > Game 4 > Game 5 = Game 6 ($p < 0.05$); while for one-sided games it is: Game 12 = Game 13 > Game 14 > Game 15 > Game 16 > Game 17 ($p < 0.05$).

drop in the focality of equality; in Game 5 (16), only 2 out of 55 (3 out of 56) bargaining pairs agree on the equal contract.

Finding 4. ***(Question 1)*** *In two and one-sided games 1–6 and 12–17, almost all bargaining pairs agree on the equal contract when it is total-earnings efficient, and the proportions agreeing on the equal and efficient contract fall gradually as its total payouts decrease. A majority of agreements remain on the equal contract as long as it remains efficient (with the exception of Game 15). However, the proportion of agreements on the equal contract drops dramatically as soon as the equal contract ceases to be efficient.*

These findings show that the focality of an equal and efficient contract decreases only gradually as its total earnings are lowered; if an equal and efficient contract ceases being total-earnings efficient, there is no sharp decline in its focality. Hence, total-earnings efficiency is a sufficient, but not a necessary condition for subjects to accept an equal agreement. On the other hand, the equal allocation being efficient does not rule out a substantial proportion of bargainers (a majority in Game 15) settling on an unequal allocation.[24]

The second finding, that an equal but inefficient outcome lacks any salience, is, we think, equally important. It is a clear affirmation of the fundamental and well-known claim made by most economists, that Pareto efficiency is a necessary condition for an outcome to be socially acceptable. We are, to the best of our knowledge, the first study that demonstrates this.[25]

3.2.2 Question 2: What is the Impact on the Focality of the Equal Contract When The Unequal Contract(s) Offer More Money to One of the Players?

Comparing Games 7,4,8 and 18,15,19 show that when the unequal and total earnings maximizing allocations become more attractive to one of the

[24]Our data thus fail to lend support to the 'principle' stated in (Herreiner and Puppe, 2010, p. 230): "First, determine the most equal distribution of rewards. If this contract is Pareto optimal, then choose it."

[25]These findings differ from those reported in Herreiner and Puppe (2010) (cf. Experiment R3), where there are two Pareto-efficient contracts, (66,40) and (46,75), and an equal and inefficient contract, (45,45). They observe that out of 48 bargaining pairs, 22 agree on (45, 45) and 19 on (46, 75). This suggests a greater willingness to sacrifice efficiency for equality than in our experiment.

players, a significantly smaller proportion of bargaining pairs agree on an equal contract, *and* a significantly larger proportion agree on an unequal contract (Game 7 > Game 4 > Game 8 ($p < 0.05$), and Game 18 > Game 15 > Game 19 ($p < 0.05$)). If we consider the games where the equal contract is inefficient (Games 9–11 and 20–22), the drop in the agreement rate on equality is not significantly different. This can be attributed to the fact that very few people agree to inefficient equality in any case.

Finding 5. ***(Question 2)*** *Consider the two and one-sided games with a given efficient and equal contract (Games 7, 4, 8 and 18, 15, 19). In both groups of games, when the unequal contracts offer more money to one subject and the same to the other, significantly fewer subjects agree on an equal and efficient contract, and significantly more agree on an unequal contract. When the unequal contracts offer sufficiently high money to one of the subjects, a majority of agreements are on unequal contracts (Games 8 and 15 and 19). The same happens when the equal contract is inefficient (Games 9–11 and 20–22), but the changes are not significant.*

We can offer the following account of the findings. When we go from Game 7 to 4, and from 4 to 8, three things happen: first, each unequal contract offers a higher reward to one of the players; second, the total earnings in the unequal contracts increase; third, the unequal contracts become more unequal. As discussed earlier, we expect the first to lead a self-interested subject to bargain harder in favor of his or her preferred unequal contract; the second factor makes unequal contracts more attractive to subjects who in addition to their own care about total earnings; the last factor, to the contrary, makes the equal contract relatively more attractive to subjects who sufficiently strongly dislike inequality. Interpreted this way the data show that in the population as a whole the first and second effects dominate the third.

We think these findings are interesting for another reason. One might have conjectured that subjects would have responded to the increased conflict of interest over which unequal contract to agree on by being more likely to agree on an equal compromise, as a means to avoid conflict. This is not what the data show. Although the disagreement rate does increase somewhat as we go from Game 7 to 4 and to 8, it remains quite low, and players' total earnings increase (cf. the absolute total-earnings efficiency column in the data table). The subjects are quite able to deal with more intense conflict and become more, not less, willing to settle on unequal outcomes.

3.2.3 Question 3: Does the Focality of the Equal Contract Differ in Two and One-Sided Games?

Finally, we compare the focality of equality in two and the corresponding one-sided game. Recall that Independence of Irrelevant Alternatives (IIA) states that the rate of agreement on an equal contract in a one-sided game is at least as large as in the corresponding two-sided game.

Finding 6. ***(Question 3)*** *In two-sided games the average rate of agreement on the equal contract is significantly higher than in one-sided games ($p = 0.022$). The opposite is true for agreements on an unequal contract. It follows that the data violate Independence of Irrelevant Alternatives, as defined in Nash (1950).*

An equal outcome naturally gets some focality from its unique and absolute property of offering equality of earnings[26] – a property which holds regardless of which other contracts are available. The equal contract can also be focal not because it is equal because it can serve as a compromise between more extreme contracts, and this context-dependent property is relevant in two-sided but, by design, not in one-sided games.

3.3 Total Earnings Efficiency

There are two reasons why in a game the average total-earnings efficiency (TEE) can be significantly below 100%: either many bargaining pairs disagreed, or they agreed on an outcome that did not maximize total earnings.

Consider first how TEE varies across the games that differ in the equal contract, Games 1–6 and 11–17. As the equal contract becomes less efficient, TEE drops from almost 100% (Games 1 and 12) to around 70% – 80% of efficiency (Games 3, 4, and 14, 15), but then increases again when the equal earnings contract becomes weakly or strongly dominated (Games 5, 6 and 16,17).[27]

[26] Or, more generally, of minimizing earnings differences.

[27] TEE is significantly larger in Game 1 than Games 2-6 (Wilcoxon signed-rank tests, $p < 0.05$), in Game 2 than 3 and 4 ($p < 0.05$), in Game 5 than 3 and 4 ($p < 0.1$), in Game 6

Finding 7. *There is, in both one and two-sided games 1–6 and 11–17, a u-shaped relationship between the rate of total-earnings efficiency (TEE) and the properties of the equal earnings contract: TEE initially decreases as the equal contract becomes less than total-earnings efficient, but picks up again once the equal contract becomes inefficient.*

This u-shaped pattern can be explained straightforwardly by observing that when the equal contract is total earnings maximizing, everybody agrees on it, and disagreements are very low, so TEE is close to 100%. As the total earnings offered by the equal contract fall but the latter remains efficient there is more disagreement, and a tendency to agree on unequal contracts more often. However, since a majority of bargainers still settle on the equal contract, the net effect is that TEE falls. Nevertheless, as soon as the equal contract ceases to be efficient, almost all agreements are on an unequal and total earnings maximizing contract, and this raises TEE again.

TEE also drops if the inequality of the alternative contract increases when the equal contract is efficient but not surplus maximizing (two-sided Games 7, 4, 8, and one-sided Games 18, 15, and 19)[28]. The same is true for two-sided games with an inefficient equal contract (Games 9-11).[29]

Finding 8. *TEE decreases both in one and two-sided games (except one-sided games where the equal contract is inefficient) when the unequal contract(s) offer more money to the favored player while keeping the payoff to the other player the same.*

If we pool together the data of two-sided and one-sided games respectively, the percentage of total-earnings efficiency is statistically significantly higher for one-sided games ($p = 0.018$).

Finding 9. *The average TEE in one-sided games is significantly higher than in two-sided games.*

This can be attributed to the fact that bargainers tend to settle on the equal contract more often in the two than in the one-sided games.

---

than 4 ($p = 0.028$), in Game 12 than 14 and 15 (($p < 0.05$), in Game 13 than 14 and 15 ($p < 0.05$), in Game 16 than 13-15 ($p < 0.05$), and in Game 17 than 12-16 ($p < 0.05$).

[28] Game 7 > Game 4 = Game 8 ($p < 0.05$), and Game 18 > Game 15 > Game 19 ($p < 0.05$).

[29] Game 9 > Game 10 = Game 11 ($p < 0.05$), and Game 18 > Game 15 > Game 19 ($p < 0.05$). For one-sided games (Games 20-22), there is no significant difference in the rate of total-earnings efficiency between the games.

3.4 Agreement Times

The agreement time is the number of seconds elapsed before an agreement is reached. Although in our bargaining environment it does not matter when any agreement is reached ('time is not money', except of course if the deadline is exceeded), there are significant differences in agreement times across our games.

3.4.1 Average Agreement Times

Table 1 shows that as the total earnings of an equal and efficient contract fall (Games 1–4, and 12–15), it takes longer to reach an agreement.[30] Intuitively, as the focality of the equal contract is diminished, players need to spend more time on reaching an agreement on one of the unequal contracts.

When the unequal contracts offer higher total payouts but also become more unequal (Games 7, 4, 8 and 18, 15, 19), the agreement times in the two-sided games increase significantly (Game 7 $<$ Game 4 $=$ Game 8, $p < 0.05$). For one-sided games there is no clear pattern. When the equal contract is inefficient, agreement times increase in both one-sided and two-sided games as the unequal contract becomes more unequal (Game 9 $<$ Game 10 $=$ Game 11, and Game 20 $=$ Game 21 $<$ Game 22, $p < 0.1$). These findings appear intuitive – a more pronounced conflict over which unequal contract the players should settle on should make negotiations last longer. For one-sided games, increased agreement times for the game (50,50),(60,240) suggest that at least some subjects care about equality.

Comparing one and two-sided games reveals that the agreement time is always longer for two than for one-sided games, and the difference between the averages is significant ($p = 0.018$). Indeed, in many games, such as (40,40),(40,120),(120,40) and (40,40),(40,120), it takes on average more than twice as long to reach an agreement in the two than the one-sided game.

It is also interesting to investigate if, conditional on reaching an agreement, it takes more time to agree on an equal than on an unequal contract. If we compare averages across all games, we find that it takes significantly

[30]For two-sided games, Game 1 $=$ Game 2 $=$ Game 3 $<$ Game 4 $=$ Game 5, Game 4 $=$ Game 6, and Game 5 $>$ Game 6 ($p < 0.05$). For one-sided games, Game 12 $=$ Game 16 $=$ Game 17 $<$ Game 13 $=$ Game 14 $=$ Game 15 ($p < 0.05$).

less time to agree on an equal than on an unequal contract (44.23 on an equal and 57.79 seconds on an unequal contract, p = 0.018). If we consider two and one-sided game games separately, the averages are 48.34 and 74.40 (p=0.018) for two-sided, and 43.77 and 57.79 (p=0.398) for one-sided games. The (in)significant difference for (one) two-sided games supports the hypothesis that it is especially the conflict of interest over the two unequal contracts that prolongs the negotiations.

We can summarize the above in

Finding 10. *i) Agreement times increase significantly in two-sided games whenever an equal and efficient contract offers lower total earnings (Games 1–6, 12–17), but not in one-sided games; ii) the same is observed when the unequal contracts offer more to the favored player and the same to the other player (Games 7, 4, 8 and 9, 10, 11), but there is no significant pattern in one-sided games; iii) it takes significantly more time to reach an agreement in two than in one-sided games. iv) it takes significantly more time to agree on an unequal than on an equal contract in two but not in one-sided games.*

3.4.2 Cumulative Distributions of Agreement Times

We can get additional insights from considering the cumulative distributions of agreement times. These are shown in Figure 2. In Games 1-6 there seems to be two main patterns at work. In Games 1-3 most of the agreements are sealed quickly within the first 60 seconds (between 64% and 72%), while, in games 4-6, most of the agreements are sealed later in time (only between 31% and 40% of the agreements are reached within the first 60 seconds). In the one-sided games 12-17, the different patterns are less evident. Subjects seem to be quicker in reaching an agreement in Games 12, 16, and 17 (between 77% and 84% of the agreements are sealed within the first 60 seconds) than in Games 13, 14, and 15 (where between 55% and 68% of the agreements are sealed before 60 seconds).

Compared to the two-sided version, Games 18, 14, and 19 display a slightly horizontally inverted s-shaped curve, with more agreements sealed at the beginning and the end of the interaction, and a standstill in the middle. If we look at the games where we vary the inequality of the unequal contract when the equal contract is inefficient (two-sided Games 9-11, and one-sided Games 20-22), the cumulative distribution of the agreement time is convex in two-sided games, and concave in one-sided games.

In particular, in two-sided games most of the agreements are sealed at the end of the bargaining with a stasis at the beginning, while, in one-sided games, the majority of the agreements are reached at the very beginning of the interaction (after 60 seconds, in two-sided games, only 21-47% of the agreements are sealed, against the 73-84% of one-sided games). These differences between two and one-sided games can again be attributed to the presence or absence of a coordination problem over unequal and total earnings maximizing contracts.

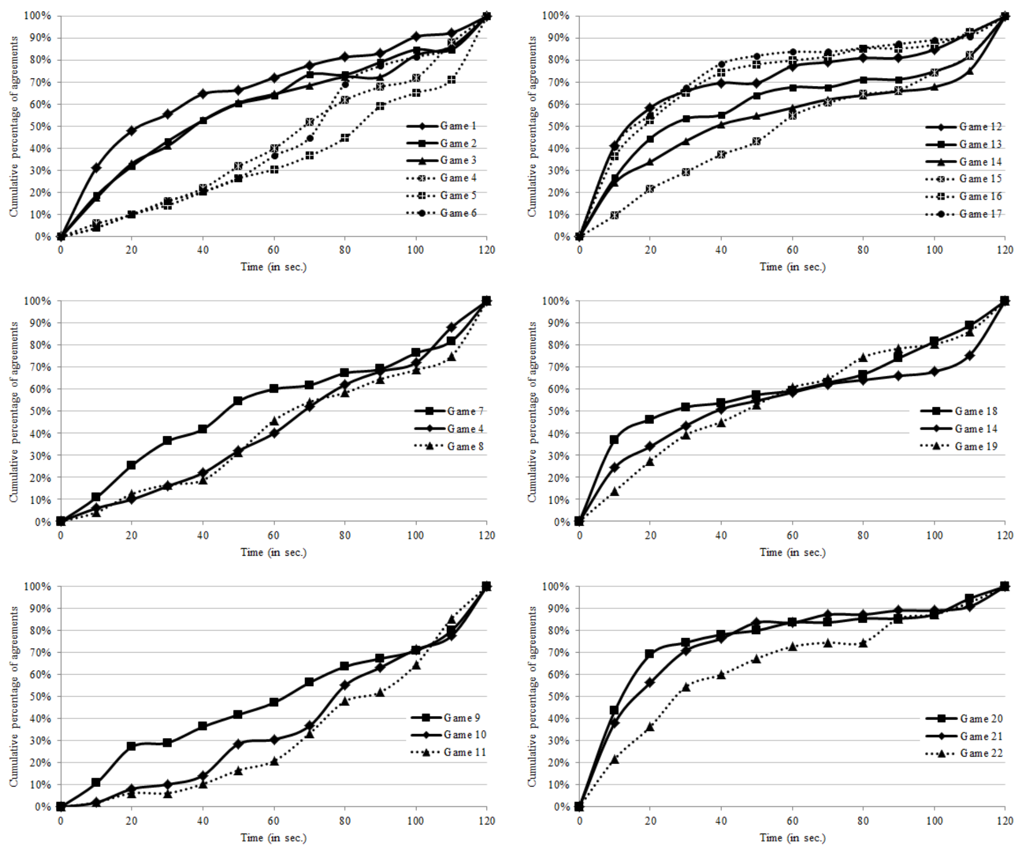

Figure 2: Cumulative distributions of agreement times in each of the 22 bargaining games.

We can also consider the cumulative distribution of agreement times in the one and two-sided games, and for agreements on equal and unequal contracts. These are shown in Figure 3. Finally, we can compare the distributions on equal and unequal contracts, separately for one and two-sided games. These are shown in Figure 4. These figures confirm the previously stated findings for average agreement times: it takes more time to reach an agreement in the two than in the one-sided game; people who agree on an equal contract do it so faster than those who agree on an unequal contract. Finally, the difference between the cumulative equal and unequal contract agreement times is much bigger in two than in one-sided games.

There is a significant difference between agreement times on an unequal contract in two and one-sided games ($p = 0.018$). This confirms what has already been found above, that it naturally takes longer to agree on an unequal contract in a two than in a one-sided game. We also find that on average it takes significantly longer to agree on equality in two compared to one-sided games ($p = 0.063$).

3.4.3 Deadline Effects

The 'deadline effect' refers to the finding that in many bargaining experiments there is a surge in the number of agreements as the deadline approaches. For example, in Roth and Murnighan (1982), where subjects negotiate for twelve minutes, typically one-third of agreements are sealed during the last thirty seconds. See Roth et al. (1988) for a description of the findings from other experiments.

The table below lists for each game the percentage of agreements reached in the last 30, 20, 10, 5, and 1 seconds before the deadline.

If we consider agreements reached within the last thirty seconds, it is apparent that the most pronounced deadline effects are observed in Games 5, 7–11, and 14–15, where more than 30% of agreements are reached during the last thirty seconds. These are games where there is either a quite high conflict over the two unequal contracts (Games 5, 7–11), or where players disagree on whether they should settle on an efficient equal or on an unequal and total earnings maximizing contract (Game 14 and 15). This supports the hypothesis that any deadline effect, and, more generally, late agreements, are primarily due to players engaging in a 'chicken type' of bargaining over which of two contracts the players should agree on.

It is also interesting to compare our deadline effects data with those from other bargaining experiments, such as Isoni et al. (2014) and of Roth

Game	Number (and %) of agreements						
	Total	Last 30 secs	Last 20 secs	Last 10 secs	Last 5 secs	Last 2 secs	Last 1 sec
1	54 (98.18%)	9 (16.67%)	5 (9.26%)	4 (7.41%)	3 (5.56%)	1 (1.85%)	1 (1.85%)
2	53 (100%)	11 (20.75%)	8 (15.09%)	8 (15.09%)	4 (7.55%)	1 (1.89%)	1 (1.89%)
3	51 (91.07%)	14 (27.45%)	9 (17.65%)	7 (13.73%)	4 (7.84%)	1 (1.96%)	0 (0%)
4	50 (89.29%)	16 (32%)	14 (28%)	6 (12%)	4 (8%)	2 (4%)	0 (0%)
5	49 (89.09%)	22 (44.9%)	18 (36.73%)	15 (30.61%)	11 (22.45%)	6 (12.24%)	6 (12.24%)
6	49 (87.5%)	11 (22.45%)	9 (18.37%)	7 (14.29%)	6 (12.24%)	4 (8.16%)	3 (6.12%)
7	55 (98.21%)	17 (30.91%)	13 (23.64%)	10 (18.18%)	6 (10.91%)	4 (7.27%)	2 (3.64%)
8	48 (87.27%)	17 (35.42%)	15 (31.25%)	12 (25%)	9 (18.75%)	2 (4.17%)	2 (4.17%)
9	55 (98.21%)	18 (32.73%)	16 (29.09%)	11 (20%)	6 (10.91%)	3 (5.45%)	2 (3.64%)
10	49 (89.09%)	18 (36.73%)	14 (28.57%)	12 (24.49%)	8 (16.33%)	7 (14.29%)	6 (12.24%)
11	48 (85.71%)	23 (47.92%)	17 (35.42%)	10 (20.83%)	5 (10.42%)	4 (8.33%)	3 (6.25%)
Tot. 2-sided	561 (92.12%)	176 (31.37%)	138 (24.6%)	102 (18.18%)	66 (11.76%)	35 (6.24%)	26 (4.63%)
12	53 (94.64%)	10 (18.87%)	9 (16.98%)	6 (11.32%)	2 (3.77%)	0 (0%)	0 (0%)
13	56 (100%)	16 (28.57%)	14 (25%)	10 (17.86%)	9 (16.07%)	3 (5.36%)	1 (1.79%)
14	53 (94.64%)	18 (33.96%)	17 (32.08%)	13 (24.53%)	8 (15.09%)	6 (11.32%)	2 (3.77%)
15	51 (91.07%)	17 (33.33%)	13 (25.49%)	9 (17.65%)	8 (15.69%)	5 (9.8%)	3 (5.88%)
16	55 (98.21%)	8 (14.55%)	7 (12.73%)	4 (7.27%)	3 (5.45%)	1 (1.82%)	1 (1.82%)
17	56 (100%)	7 (12.5%)	6 (10.71%)	6 (10.71%)	2 (3.57%)	1 (1.79%)	0 (0%)
18	54 (96.43%)	14 (25.93%)	10 (18.52%)	6 (11.11%)	4 (7.41%)	4 (7.41%)	3 (5.56%)
19	51 (91.07%)	11 (21.57%)	10 (19.61%)	7 (13.73%)	5 (9.8%)	2 (3.92%)	2 (3.92%)
20	55 (100%)	8 (14.55%)	7 (12.73%)	3 (5.45%)	0 (0%)	0 (0%)	0 (0%)
21	55 (98.21%)	6 (10.91%)	6 (10.91%)	5 (9.09%)	3 (5.45%)	1 (1.82%)	0 (0%)
22	55 (98.21%)	10 (18.18%)	7 (12.73%)	4 (7.27%)	3 (5.45%)	2 (3.64%)	2 (3.64%)
Tot. 1-sided	594 (96.59%)	125 (21.04%)	106 (17.85%)	73 (12.29%)	47 (7.91%)	25 (4.21%)	14 (2.36%)
Tot.	1155 (94.36%)	301 (26.06%)	244 (21.13%)	175 (15.15%)	113 (9.78%)	60 (5.19%)	40 (3.46%)

Table 3: Number and percentage of agreements reached in each game during the last 30, 20, 10, 5, 2, and 1 seconds.

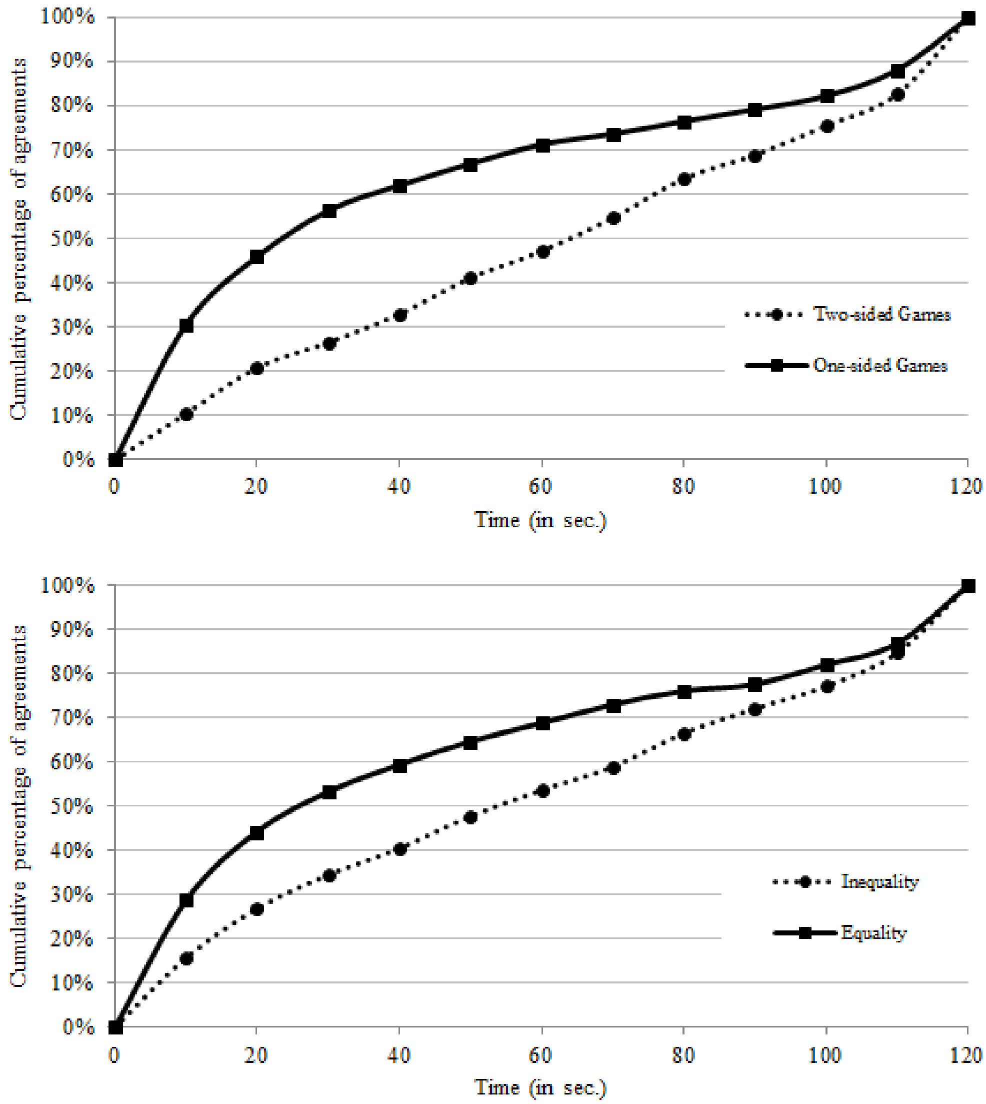

Figure 3: Cumulative distributions of agreement times. Top panel: two versus one-sided games. Bottom panel: equal versus unequal agreements.

and co-authors (see again Roth et al., 1988). In Isoni et al. (2014) subjects have ninety seconds to negotiate. While the framing and other aspects dif-

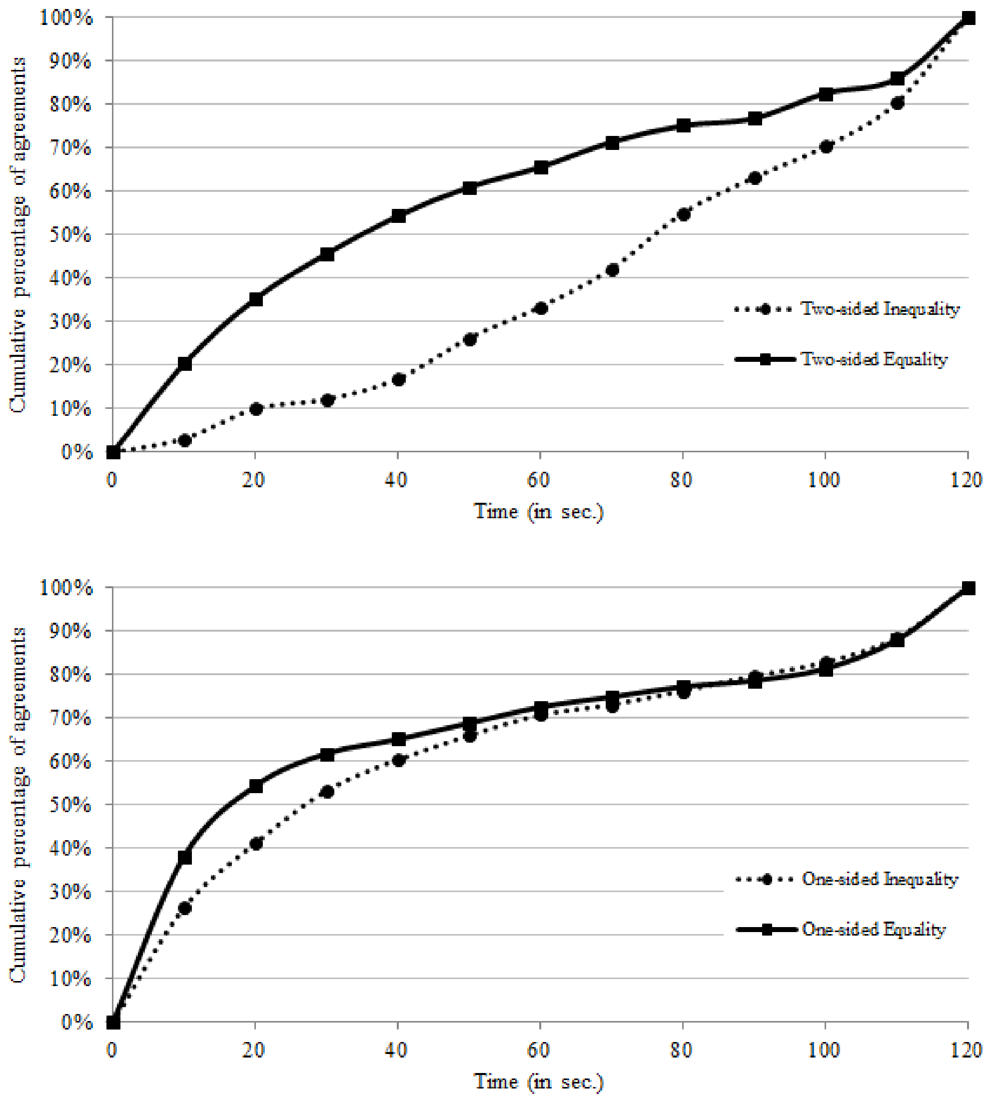

Figure 4: Cumulative distributions of agreement times: equal and unequal agreement times, in two (top panel) and one-sided games (lower panel).

fer from our bargaining environment[31], some of their games have payoff

[31]One difference has to do with the agreement technology. In Isoni et al. (2014)'s exper-

structures that can be compared to ours. Their Games G7–G10 have an equal and total earnings maximizing contract, and several unequal contracts over which players have conflicting preferences. This is qualitatively similar to our Game 1. The agreement time data are quite similar for Game 1 and their Games G7–G10, and there is no deadline effect. Agreements are reached at a more or less constant rate through the bargaining period, and the cumulative distributions have the same gradually increasing concave shape. For example, in G7 about 80% of bargaining pairs have reached an agreement during the first half of the bargaining period; in G1, the percentage is about 70%.

Some of the other games studies in Isoni et. al.'s experiments display a much stronger deadline effect than in our data. We attribute this primarily to these games being of the Battle of the Sexes type, where there is no outcome that gives players equal and strictly positive payoffs. For example, their games G5 and G6 have feasible contracts offering payoffs (3,8),(8,3). The data show that with ten seconds left, only slightly more than 40% of bargaining pairs have reached an agreement. Their data reveal that most interaction is of the 'Chicken type' where subjects hold out hoping that the other subject will concede before they do. A concession happens shortly before the deadline. We do not have games in our experiment that are directly comparable. Among our games the closest analogue is Game 11, (50, 50),(60,240),(240,60), where there is an inefficient equal contract. Here the deadline effect is much less pronounced than in Isoni et al.'s games G5–G6; with ten seconds left, almost 20% of bargaining pairs have not reached an agreement. We offer the following speculative hypothesis for why there is this difference. The presence of an inefficient equal payoff contract may reduce the perceived conflict through a 'reference point effect', where the equal and inefficient contract, although very itself unlikely to be agreed on, replaces the zero payoff disagreement point and serves to make an agreement on an unequal contract appear less extreme and hence more palatable to players.[32]

It is also relevant to compare our data and those in Roth and co-workers (see Roth et al., 1988). In their common knowledge 'both players know' treatments in Roth and Murnighan (1982) the two subjects negotiate over

iment, players do not need to formally seal the deal before the deadline. As long as their claims are compatible, they will get the payoffs. Thus the 'effective' rate of agreement in their experiment may exceed the notional agreement rate.

[32]Future experimental work could investigate the plausibility of this hypothesis.

point divisions using the binary lottery procedure (see also Roth, 1995). There are two salient agreements (an equal point division giving unequal expected money earnings, and an unequal point division giving equal expected money earnings), each of which is preferred by a different subject. As in Isoni et al. (2014)'s experiment, this can again be seen as a Chicken type of bargaining environment, and the authors observe a pronounced spike in the frequency of agreements very near, or at, the deadline.

4 Experimental Findings: Contract Proposals and Messages

In order to better understand the factors that shape the bargaining outcome, we now consider the contract proposals and messages that were exchanged. Recall that while a contract proposal was either payoff-relevant (if the other subject had already made the same proposal) or potentially so (the other subject could make the same proposal afterwards), the messages were cheap talk. Of course, the messages can significantly affect the subjects' beliefs and aid coordination on a contract. As we show below this is systematically the case, and the nature of the messages vary predictably with the game played.

To begin, we can look at how many contract proposals subjects make and how many messages they exchange. We will then turn to the analysis of the content of the communication and its dynamics in order to provide a classification of the bargaining processes. Recall that a subject makes a binding contract proposal when he or she clicks on one of the available contracts displayed in the computer screen. In the majority of cases (78.88%), subjects make only one binding contract proposal during a bargaining period. In very small number of cases (0.93%) subjects make more than 3 binding contract proposals.[33] If we look at the number of messages exchanged, subjects mostly send at least one message – typically between 1 and 4 messages – and in only 32.03% of cases they do not send any messages at all. This suggests that subjects seems to rely more on the chat to bargain, and make binding proposals only to propose, at the very beginning of the bargaining, their preferred contract or, at the end of the bar-

[33] Note that a subject can switch between the same contracts so a binding proposal is not necessarily a new one.

gaining, the contract agreed in the chat. There is also evidence of a positive correlation between agreement time and number of messages exchanged and binding proposals made respectively.[34] We also find a positive and significant correlation between number of messages sent and number of contract proposals made (ρ = 0.164, p = 0.016), suggesting that chat messages and contract proposals are "complements" and not "substitutes".

We can also investigate whether subjects start the bargaining by sending a message or by making a binding proposal. In 34.84% of cases, subjects first send a message via chat, while in 33.13% of cases, they first send a binding proposal. In the remaining 32.03% of cases, they do not use the chat at all but simply submit proposals by clicking with the mouse on the contracts. Within each pair of bargainers, there is a strong and significant correlation between the number of messages sent by one bargainer and the other (Spearman ρ = 0.864, p = 0.016). In contrast, there is no correlation between the number of binding proposals made by one subject and those made by his/her matching partner (ρ = 0.038, $p > 0.999$).

5 Future Research

There are some obvious limitations of our study that can be relaxed in future expeirments.

5.1 Other Structural variables

In order to make game comparisons simple we deliberately chose to only work with few (two or three) feasible contracts. This could be restrictive. For example, a larger set of feasible contracts, which could require more time to consider and bargain over, might make an equal contract more salient as a way to identify an agreement. This could be investigated.[35]

Another restriction is that all games had a contract that offered equal earnings; it is relevant to investigate if the results would change signifi-

[34]Between agreement time and messages exchanged, Spearman ρ = 0.312 (p = 0.016), between agreement time and binding proposals made, ρ = 0.784 (p = 0.016).

[35]It is also possible that endogenous randomization devices a la Rock-Papers-Scissors would be less likely to be used, since it might be less salient or obvious which contract domain the randomization should apply to.

cantly if no contract offered exactly equal earnings (see Güth et al. (2001) for an ultimatum game experiment investigating this issue).[36]

We also assumed there is no time pressure, due to discounting or risk of exogenous breakdown of bargaining (see for example Muthoo (1999)). This is clearly unrealistic in some bargaining contexts. We conjecture that time pressure will make bargainers more anxious to reach an early agreement, and this might affect the focality of contracts.

We believe that the role of all these and other structural features of the bargaining environment and their impact on the focal bargaining outcome can be fruitfully investigated using a similar basic framework as the one used in this paper.

5.2 Obtaining Measures of Individual Preferences

We did not attempt to measure (or control for) individual bargainers' preferences. One could imagine a future experiment where subjects are asked to make dictator choices facing the same set of feasible contracts that they bargain over, or asked to rank the contracts in an incentive compatible manner. Possessing such information could allow us to get a deper understanding of the mechanisms driving the agreements.[37]

6 Conclusions

People often need to bargain in order to reach a joint decision, but it may not be possible to find an agreement that is both equal and efficient – in such situations, how do bargainers jointly trade off these properties? Do

[36]For example, behavior in the game $(40, 120), (50, 50), (120, 40)$ could be compared with $(40, 120), (55, 45), (120, 40)$. Contract $(55, 45)$ is a compromise, as is $(50, 50)$, but no longer exactly equal.

[37]For example, consider an agreement on $(50, 50)$ in game $(50, 50), (40, 240)$. Having data on preferences would enable us to distinguish between the case where the agreement emerges as a compromise between self-interested subjects, and the situation where both players prefer the equal allocation. Of course, this assumes that subjects' preferences as elicitated in the dictator task actually apply to the bargaining task. This is not an innocent assumption: it seems quite plausible that subjects' preferences over contracts can be affected by the process of bargaining, such as the sequence of proposals and the content of messages.

they tend to settle on equality even if other, unequal, agreements are preferred by some bargainers and they may offer larger total earnings? Or do they instead settle on unequal but total earnings maximizing agreements?

We report on the findings from experiments where subjects are free to make proposals, can communicate, and sign binding agreements, and we systematically vary the nature of the tradeoff between efficiency and equality, thereby allowing us to understand how the typical focal agreement varies with the efficiency-equality tradeoff.

Our results suggest that the focal agreement varies with the tradeoff in a quite plausible manner: First, an inefficient agreement is almost never agreed on, but efficiency is not a sufficient condition for focality; second, equality and efficiency together ensure strong focality only if the total earnings are also sufficiently high; if not, most bargainers settle on an unequal and total earnings maximizing contracts. Our findings also reveal that equality gets its focality from two sources, namely its absolute property of offering equal earnings, and from being a compromise between unequal contracts over which there is a conflict of interest.

References

Anbarci, N. and N. Feltovich, 2013, How sensitive are bargaining outcomes to changes in disagreement payoffs?, working paper.

Bolton, G. E. and A. Ockenfels, 2000, Erc: A theory of equity, reciprocity and competition, The American Economic Review 90, pp. 166–193.

Bone, J., J. D. Hey, and J. Suckling, 2014, What price compromise?, Theory and Decision 77, 359–376.

Camerer, C., 2003, Behavioral game theory: experiments in strategic interaction, The Roundtable series in behavioral economics (Russell Sage Foundation).

Camerer, C., G. Nave, and A. Smith, 2015, Dynamic unstructured bargaining with private information and deadlines: Theory and experiment, working Paper.

Charness, G. and M. Rabin, 2002, Understanding social preferences with simple tests, Quarterly Journal of Economics 117, 817–869.

Engelmann, D. and M. Strobel, 2004, Inequality aversion, efficiency, and maximin preferences in simple distribution experiments, The American Economic Review 94, pp. 857–869.

Fehr, E. and K. M. Schmidt, 1999, A theory of fairness, competition, and cooperation, Quarterly Journal of Economics 114, 817–868.

Feltovich, N. and J. Swierzbinski, 2011, The role of strategic uncertainty in games: An experimental study of cheap talk and contracts in the nash demand game, European Economic Review 55, 554 – 574.

Fischbacher, U., 2007, z-tree: Zurich toolbox for ready-made economic experiments, Experimental Economics 10, 171–178.

Fouraker, L. E. and S. Siegel, 1963, Bargaining Behavior (McGraw-Hill).

Gächter, S. and A. Riedl, 2005, Moral property rights in bargaining with infeasible claims, Management Science 51, 249–263.

Greiner, B., 2004, The online recruitment system orsee 2.0 - a guide for the organization of experiments in economics, Working Paper Series in Economics 10, University of Cologne, Department of Economics.

Güth, W., S. Huck, and P. Ockenfels, 2001, The relevance of equal splits in ultimatum games, Games and Economic Behavior 37, 161–169.

Herreiner, D. K. and C. Puppe, 2010, Inequality aversion and efficiency with ordinal and cardinal social preferences – an experimental study, Journal of Economic Behavior and Organization 76, 238 – 253.

Isoni, A., A. Poulsen, R. Sugden, and K. Tsutsui, 2013, Focal points in tacit bargaining situations: Experimental evidence, The European Economic Review 59, 167–188.

Isoni, A., A. Poulsen, R. Sugden, and K. Tsutsui, 2014, Efficiency, equality, and labels: Experimental evidence on focal points in explicit bargaining situations, The American Economic Review 104, 3256–3287.

Karagözoglu, E. and G. Bolton, 2013, On the interaction of hard and soft bargaining leverage: A test of schelling's hypothesis and a modified zeuthen-harsanyi model, working paper.

Karagözoglu, E. and A. Riedl, 2015, Performance information, production uncertainty, and subjective entitlements in bargaining.

Mehta, J., C. Starmer, and R. Sugden, 1994, The nature of salience: An experimental investigation of pure coordination games, The American Economic Review 84, 658–673.

Muthoo, A., 1999, Bargaining Theory with Applications (Cambridge University Press).

Myerson, R. B., 1991, Game Theory – Analysis of Conflict (Harvard University Press).

Nash, J., 1950, The bargaining problem, Econometrica 18, 155–162.

Nydegger, R. and G. Owen, 1975, Two person bargaining: An experimental test of the nash axioms, International Journal of Game Theory 3, 239–249.

Roth, A. E., 1995, Bargaining experiments, in: J. H. Kagel and A. E. Roth, eds., Handbook of Experimental Economics (Princeton University Press), 253–348.

Roth, A. E. and M. W. K. Malouf, 1979, Game-theoretic models and the role of information in bargaining, Psychological Review 86, 574–594.

Roth, A. E. and J. K. Murnighan, 1982, The role of information in bargaining: An experimental study, Econometrica 50, 1123–1142.

Roth, A. E., J. K. Murnighan, and F. Schoumaker, 1988, The deadline effect in bargaining: Some experimental evidence, The American Economic Review 78, 806–823.

Schelling, T. C., 1960, The Strategy of Conflict (Harvard).