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Communication in multilateral bargaining[☆]

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ABSTRACT

One of the most robust phenomena in the experimental literature on multilateral bargaining is the failure of proposers to extract equilibrium rents. However, all previous experiments have overlooked the fact that outside the lab committee members are allowed to – and do – engage in sometimes intense communication processes prior to voting on a proposal. We conduct an experimental test of the Baron–Ferejohn model in which we allow committee members to engage in unrestricted cheap-talk communication before a proposal is submitted. We find that proposers extract a significantly higher share of resources when communication is allowed. Communication increases proposer power through two channels. First, it mitigates the uncertainty surrounding the amount a coalition member is willing to accept. Second, it allows potential coalition members to compete for a place in the coalition by lowering this stated price.

1. Introduction

In many situations, individuals with conflicting preferences must reach an agreement on the division of fixed resources. This is the case, for example, for legislators at all levels of government, condominium boards or faculty committees. This classical multilateral bargaining problem has been extensively studied both in economics and in political science.

One aspect of real-life bargaining that has received little attention in the literature is that of communication,[1] despite the fact that communication between individuals is an integral part of bargaining processes. It is in fact difficult to find an example in which democratic decisions are made without people engaging in negotiations beforehand. In this paper, we explore both theoretically and experimentally how free-form communication affects bargaining outcomes and the bargaining process itself.

Baron and Ferejohn (1989) has emerged as the most popular formal model used to study multilateral bargaining.[2] According to the Baron-Ferejohn bargaining procedure, one member of the group is picked at random to propose a budget split, which is then voted up or down by all members using a majority voting rule. If the proposed split is rejected, a new proposer is chosen at random and the process is repeated until the proposed budget split receives the support of a majority of group members. The unique stationary subgame perfect equilibrium in this game specifies a budget split in which the proposer has bargaining power and appropriates a significantly larger share of resources than any other group member.

Proposer power has long been recognized as an important feature of the "divide-a-dollar game." For instance, in the context of legislative bargaining, the head of the appropriations committee, one of the most powerful committees in the Senate, has often been able to steer a disproportionate amount of funds to his district.[3] While the theoretical model of Baron and Ferejohn has long been able to capture the advantages of leading a bill to the floor, laboratory experiments have not

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[1] The notable exceptions are theoretical papers by Austen-Smith (1990) and Chen and Eraslan (2014) both of which study the effects of cheap-talk in the presence of asymmetric information between committee members. In addition, there are several experimental studies that incorporate communication in two player bargaining games. We survey this literature at the end of the introduction and highlight the important differences between bilateral and multilateral environments.

[2] Theoretical extensions of this model include (but are not limited to) Eraslan and Merlo (2002) who show uniqueness of the stationary equilibrium; Banks and Duggan (2000) who generalize this model to multidimensional choice space; Diermeier and Merlo (2000) who study a dynamic model of government formation in Parliamentary democracies; Battaglini and Coate (2007, 2008) who investigate the dynamic model of public spending; Diermeier and Feddersen (1998) who study cohesion in legislatures and the vote of confidence procedure; Tergiman (2013) who adds to that the possibility of offering a public good; and Battaglini et al. (2012) who study a version of the Baron–Ferejohn model with endogenously determined status quo.

[3] One of the recent examples is that of Ted Stevens from Alaska, see http://www.cepr.net/err/nytimesarticles/call_pork_11_29.htm.

http://dx.doi.org/10.1016/j.jpubeco.2014.06.006
0047-2727/

been able to match this stylized fact.[4] In fact, the under-exploitation of proposal power is a robust experimental finding.

We use the model of Baron and Ferejohn as a starting point and extend it to allow group members to engage in a cheap-talk communication. We show that while in theory the addition of cheap-talk communication does not change the distribution of resources in stationary equilibrium, experimentally, allowing individuals to communicate has a dramatic effect both on the bargaining process and the final allocation of resources. The proposer is able to extract a significantly *higher* share of resources when bargainers are able to communicate with each other. Further, these shares are very close to the theoretically predicted ones. The under-exploitation of proposal power is an outcome that has eluded experiments for almost two and a half decades[5] and we are the first to show that allowing committee members to communicate with each other prior to the bargaining stage largely reconciles theory and experiment.

We then explore the mechanism through which communication helps the proposer. We show that communication works via two channels: (1) communication serves as a tool for the proposer to learn the other members' reservation prices and (2) communication allows non-proposers to compete with each other by lowering their reservation prices. In other words, communication mitigates the uncertainty around the voting behavior of potential coalition partners and facilitates competition between coalition partners. This enhances the ability of the proposer to exploit his proposal power and receive shares close to those predicted by the theory.

Laboratory experiments provide a direct and powerful tool for investigating the effect communication on bargaining processes. Indeed, we are able to create a controlled environment in which the only difference between treatments is the ability of bargainers to communicate with one another. These two comparable treatments are necessary since our focus is to evaluate the effects of the communication per se, and how the presence of communication changes the strategic interaction between bargainers. While we often use the language of legislative bargaining to describe the environment and the results of the experiment, our setup is applicable to a wide variety of bargaining situations, such as the ones described at the beginning of this section.

Our paper contributes to the literature that investigates the effects of communication in various strategic environments (for a survey see Crawford, 1998). In the prisoner's dilemma game, pre-play communication has been shown to increase the rate of cooperation (for a meta-analysis see Sally, 1995). When the selection of equilibria is an issue, communication has been effective in facilitating coordination (Cooper et al., 1989, 1992). In both Bertrand price competition settings and Cournot quantity competition settings, communication has been shown to foster collusion (Friedman, 1967; Daughety and Forsythe, 1987; Waichman et al., 2010). In public good games, pre-play communication has consistently leads to increased provision of the public goods (Isaac et al., 1985; Isaac and Walker, 1988). Finally, most related to our current paper, in *bilateral* bargaining games, pre-play communication has been shown to promote more egalitarian outcomes and decrease the proposer power.[6,7]

Given these previous findings, one might expect that communication would negatively affect the ability of the proposer to extract rents when bargainers have free access to communication. Instead, the cheap talk stage in a multilateral bargaining setup effectively serves to create an auction in which non-proposers privately convey to a proposer their bids for a place in the coalition. The competition for a place in a coalition pushes the market price of a vote down, closer to the theoretically predicted one, and allows proposers to extract higher rents.

The remainder of the paper is organized as follows. We survey the literature that studies effects of communication in bargaining experiments below. We present the setup and theoretical predictions in Section 2. The experimental design is described in Section 3. Section 4 presents the results of the experiments and discusses the main driving forces. Concluding remarks are in Section 5.

1.1. Communication in bargaining experiments

While the literature on the effects of pre-play communication in various strategic setups is vast, there are surprisingly few studies that look at the effects of cheap-talk communication in a bargaining framework. Here we review the two most closely related to our paper, both of which focus on bilateral as opposed to multilateral bargaining.[8]

The first study is Roth (1995) who focuses on the ultimatum game and compares frequencies of disagreements and distribution of resources in three treatments: (1) a baseline treatment in which subjects could not communicate with each other, (2) a treatment in which subjects were asked to discuss the game with each other face-to-face prior to making their decisions and (3) a treatment in which subjects were asked to converse but were restricted to 'social' conversations.[9] In this experiment, communication has two significant results. First, communication decreases the frequency of disagreements, which are measured by the number of rejected proposals. Second, communication increases the share of the responder and overall leads to a more equal distribution of resources between the proposer and the responder.

The second study is Andreoni and Rao (2011) who investigate the effects of pre-play communication in the dictator game. The authors alter who in a pair can speak and whether or not the person receiving a message can respond. The fraction of resources kept by the allocator crucially depends on whether the receiver is allowed to speak. The authors find that, in general, if the receiver sends a message to the allocator, he receives a higher share from him, so that communication allows for more egalitarian distributions.[10]

As we will see, our results show that the conclusions from bilateral bargaining setups do not extend to multilateral bargaining: while communication results in a more *equal* distribution of resources in the bilateral setups, it creates a more *unequal* distribution of resources in the multilateral setups.

[4] In most experiments, proposers do obtain higher shares than the coalition partners, but the difference is very far from what is predicted by the theory (see Frechette et al. (2003), Frechette et al. (2005a) and Frechette et al. (2005b), Drouvelis et al. (2010), Kagel, Sung and Winter (2010), Battaglini et al. (2012), Tergiman (2014) and Miller and Vanberg (2013)). We discuss these results in more detail in the Results section.

[5] Experimental tests of Baron and Ferejohn (1989) model date back to McKelvey (1991).

[6] We survey this literature in the next subsection.

[7] Xiao and Houser (2005) show that post-play communication can also have an effect as well. The authors modify the ultimatum in the following way: after receiving the offer from the proposer, responders were given the opportunity to send a message to proposers in addition to their decision to accept or reject. Proposers were aware of this and this resulted in slightly more egalitarian outcomes. The fraction of equal splits increases from 34% to 37%.

[8] See also Bolton et al. (2003) and Diermeier et al. (2008) who study effects of communication in three-person coalition negotiation experiments. Their setup is quite different from ours in various respects. In particular, subjects choose between pre-specified budget allocations in which the total amount of resources to be distributed depends on the number and identity of the coalition partners. Moreover, there is no proposer per se and, hence, no proposer power to investigate. Nonetheless, these experiments show that the type of communication (face-to-face, via a computer, with private rooms among others) affects the number of subjects included in the coalition.

[9] In this 'social' communication treatment, subjects were asked to learn each other's first name and year in school and were not allowed to discuss the bargaining game.

[10] In three out of four treatments in Experiment 1, in which the receiver had an opportunity to either send a verbal message to the allocator or respond to the message received from the allocator, the allocator eventually kept smaller fraction of resources than in the baseline treatment with no communication. In the baseline treatment, the allocator keeps on average \$8.5 while he keeps \$7.0–\$7.6 when the receiver can speak depending on the order of messages. However, in the treatment, in which the allocator sends a written verbal message along with the decision and the receiver remains silent, the allocator keeps on average \$9.4 which is significantly more than in the baseline treatment.

2. Theoretical model and predictions

As a base for our experiment, we use the classical model of multilateral bargaining of Baron and Ferejohn (1989). A legislature consists of N (odd) members (each representing a legislative district), a recognition rule that determines the standing proposal in each stage of the bargaining session, an amendment rule and a voting rule. The legislature allocates a fixed budget X of divisible benefits among its members using a majority rule with no side payments. Members of the legislature have preferences that depend only on the benefits allocated to their district.

The bargaining session consists of possibly several stages. At the beginning of the first stage, one member of the legislature is recognized to make a proposal. Each member i has the same probability of being recognized as a proposer $p_i = \frac{1}{N}$. The proposer i submits a proposal $x = (x_1^i, \ldots, x_N^i)$, where x_j^i is the share of member j in the proposal submitted by member i. To be valid, a proposal must satisfy the budget constraint $\sum_{j=1}^{N} x_j^i \leq X$.

This proposal is called the motion on the floor. Under the closed amendment rule, the rule we consider in this paper, the motion on the floor is immediately voted on by the members of the legislature.[11] If the proposal is approved by the majority of votes, then the legislature adjourns. If it is rejected, then the budget shrinks by a factor of $1 - \delta$, the legislature moves to the next bargaining stage, and the process repeats itself (a (possibly) new proposer is chosen at random etc.) until a proposed distribution receives a majority of votes. The discount factor δ can be thought of as representing the cost of delay in reaching an agreement.

Following the tradition in the literature, in order to avoid multiplicity of the subgame perfect Nash Equilibria, we will restrict our attention to the stationary subgame perfect Nash equilibria (SSPE). There exists a unique SSPE in this game, in which a proposer receives the highest share of resources and forms a minimum winning coalition that consists of randomly chosen $\frac{N-1}{2}$ members of the group and himself. The coalition partners each receive positive shares equal to the expected value of rejecting the proposed allocation, which takes into account several factors: the shrinking of the available resources, the possibility of becoming a proposer in the next bargaining session and the possibility of being excluded from the future coalition. The members excluded from the coalition receive nothing. This proposal is passed with no delay by $\frac{N+1}{2}$ "yes" votes: one from the proposer himself and the others from each of the coalition partners. The exact shares of the proposer and those of the coalition partners are given by the following equations:

$$x^{\text{Proposer}} = 1 - \frac{N-1}{2} \cdot \frac{\delta}{N} \quad \text{and} \quad x^{\text{Coalition Partner}} = \frac{\delta}{N}.$$

In this paper we also consider an extended version of Baron–Ferejohn model described above, in which the only modification is the addition of a cheap-talk communication stage between members of the legislature. More precisely, after one member is recognized as a proposer and before he/she submits motion to the floor, all members of legislature can engage in a negotiation process. Communication is unrestricted cheap-talk and any member can send any number of messages to any subset of the members in the legislature. In other words, members can exchange both private and public messages between each other.

The stationarity refinement of the sub game perfect Nash equilibrium guarantees that the extended version of the bargaining game has a unique SSPE which is identical to the SSPE in the bargaining game with no communication. To understand the intuition of this result further recall the definition of stationarity:

> "an equilibrium is said to be stationary if the continuation values for each structurally equivalent subgame are the same."
>
> [Baron and Ferejohn, 1989, page, 1191]

The stationarity restricts the equilibrium strategies to be time independent and anonymous. The continuation value of all members remains the same as in the game with no communication because the cheap-talk communication is non-binding. Consider, for instance, a non-proposer who during the negotiation stage indicates to a proposer that he is willing to accept an amount smaller than the one prescribed by the SSPE. This promise will not be followed through at the voting stage, since a non-proposer will always reject the motion that gives him less than the continuation value of the game. Therefore, there is no opportunity for a non-proposer to ensure a place in the coalition by indicating to a proposer that he is "cheaper" than other members. Thus, as before, the optimal allocation is the one in which the proposer forms a minimum winning coalition with two other random members and gives coalition partners their continuation value.

3. Experimental design

All the experiments were conducted at the California Social Sciences Experimental Laboratory (CASSEL) at UCLA. We ran three different treatments, in which a total of 235 subjects participated. The subjects were recruited from the general undergraduate population of UCLA and no subject participated in more than one experimental session. All the interactions between participants were performed through the computer terminals. Instructions for the Chat treatment can be found in our Online Appendix and all other treatment instructions are available upon request.

In all treatments we implemented the Baron–Ferejohn bargaining protocol described in Section 2 with parameters $N = 5$, $\delta = 0.8$ and $X = 250$ tokens. For this parameterization of the game, the unique SSPE prescribes that the proposer randomly chooses two members of the group, allocates 40 tokens to each of them, and appropriate the remainder of the resources, which corresponds to 170 tokens.

Before the beginning of each bargaining session, subjects were randomly divided into groups of five members and each was randomly assigned an ID number. Each of these groups represents a legislature with $N = 5$ members. A group's task is to divide a fixed budget of 250 tokens among its members. At the beginning of each bargaining session, one of the five members is randomly chosen to be the proposer. His assigned ID number is revealed to the entire group. The proposer proposes an allocation that is observed by all members of the group, with shares to each member clearly indicated. After that, all members of the group including the proposer are asked to vote to accept or reject the proposed allocation. If the allocation receives three or more votes then it passes and the bargaining session is over. If the allocation receives fewer than three votes, then the budget shrinks by 20% and the bargaining continues with a random selection of a (possibly) new proposer from the same group. This process repeats itself until a proposer's allocation gets the majority of votes and passes. After each bargaining session subjects are randomly re-matched to form new groups of 5 voters each and are assigned new ID numbers. We used random matching between bargaining sessions to implement the one-shot bargaining game we are interested in exploring in this paper. At the end of the experiment, we sum up all the tokens earned by each subject in all bargaining sessions and convert them to the US dollars using the rate 50 tokens = $1.

Our Baseline treatment followed the protocol described above. Subjects participated in a series of 15 bargaining sessions. This treatment is almost identical to the design of Experiment 1 in Frechette et al. (2003) (FKL hereafter). The main difference in the experimental procedures is

[11] See Frechette et al. (2003) for a comparison of the bargaining outcomes under closed and open amendment rules. Under the open amendment rule, after the proposer submits the motion to the floor, another member of the legislature is selected to either amend the proposal or move the previous question for the vote. If the proposed allocation is approved then the legislature adjourns, otherwise, the legislature moves to the next bargaining session and the process repeats itself until the proposed allocation is approved.

Table 1
Experimental design.

Treatment	Communication	# of experimental sessions	# of bargaining sessions	Total # of subjects
Baseline	No	3	15	95
Baseline Long	No	1	30	30
Chat	Yes	3	15	110

that FKL used the strategy method to elicit budget proposals from all bargainers before determining the identity of the proposer. This was done to maximize the number of observations one can obtain from the experiment. In our design, the identity of the proposer is determined before any allocation is submitted. This structure is necessary for the treatment with communication and so we kept it the same in all treatments for comparability.[12] As we will discuss in the results section, we don't detect significant qualitative differences in behavior observed in our Baseline treatment and that reported in FKL experiment.

The Chat treatment was similar to the Baseline treatment except for one feature. After the proposer was determined and his ID number revealed to the group members, but before the proposer submitted his proposal, members of the group could communicate with each other using a chat tool (see our Online Appendix for screenshots of proposers and non-proposers). This chat tool allowed subjects to send any message they wanted to any subset of members in their group. For instance, members could send private messages that would be delivered only to a particular member or to a subset of members, and they could also send public messages that would be observed by all members of the group. The duration of the communication was in the hands of the proposer: the chat tool was disabled when the proposer submitted his proposal for a vote. All the messages sent by subjects during the communication stage were recorded.

Our final treatment, Baseline Long treatment, was identical to the Baseline treatment except that subjects participated in a series of 30 bargaining sessions. We conducted this treatment to inspect whether communication serves simply to accelerate learning.

Table 1 summarizes the details of all our experiments.

4. Results

We report the results of our experiments in the following order. First, we explore whether introducing unrestricted communication changes bargaining outcomes. To answer this question we compare the Baseline and the Chat treatments. Second, we show that communication cannot be replaced by repetition of the game. We do this by comparing the results of the Chat and the Baseline Long treatments. Finally, we turn our attention to the communication specifically and study the mechanism through which communication affects the bargaining process.

For the majority of the analysis, we focus on the last five bargaining sessions of the experiment. The qualitative results are not sensitive to using more or fewer bargaining sessions in the analysis but we restrict our analysis to the last five so that the learning of the game has largely taken place.

4.1. The impact of unrestricted communication

One of the most robust phenomena observed in the experimental bargaining literature is the failure of the proposer to extract equilibrium rents. While proposers receive a larger share of the benefits than coalition members, these shares are often more than 40% smaller than the SSPE prediction.[13] Further, while experience and repetition slightly increase proposer shares, the gap between what had been observed in laboratory experiments and what is theoretically predicted remained large by the end of the experiment. This is a fact that has been noted in prior work and that we will revisit when we discuss the Baseline Long treatment.[14]

Fig. 1(a)–(b) present the histograms and the cumulative distribution of the proposer's share in the last 5 bargaining sessions in the Baseline and the Chat treatments for those proposals that passed without delay. Fig. 1 depicts the evolution of the proposer's share as subjects gain more experience with the game.[15]

The increase in the proposer's share when communication is allowed is significant both in magnitude and statistically. Indeed, in the last five bargaining sessions, in the Baseline treatment, proposers receive on average 110 tokens. In the Chat treatment, this average share is 144 tokens, more than 30% higher. The null hypothesis that proposers receive the same amount in the Baseline and the Chat treatments in the last 5 bargaining sessions is rejected at the 1% level with a Wilcoxon Ranksum test.[16] Fig. 1(d) shows that while in both Baseline and the Chat treatments proposers' shares grow with experience, in every single bargaining session the rents extracted by the proposers are higher when communication is allowed.[17]

We summarize the other characteristics of the bargaining process with and without communication in Table 2. In this table we report the frequency of delays as well as how the proposers distribute the resources between the voters in all three treatments. The first two columns show the outcomes of the Baseline and the Chat treatments.

Delays are rare with or without communication: more than 85% of the proposals in the last 5 bargaining sessions are accepted without delays in both treatments. The acceptance rate of proposals isn't significantly different between these two treatments.[18]

To compare the distribution of resources between treatments we use the convention introduced in FKL, which defines three mutually exclusive types of allocations: (1) the Double Zero strategy allocation, in which the sum of two lowest shares is less or equal to 20 tokens, (2) the Single Zero strategy allocation, in which four members receive

[12] There are also two other differences in the design of Experiment 1 in FKL and our Baseline treatment: the matching protocol and the payment scheme. FKL used fixed group matching and paid subjects for four randomly chosen bargaining sessions out of the 15 played. In our experiment, we implement random matching and pay subjects for all the bargaining sessions they played.

[13] FKL report that in the last 5 rounds of the experiment, proposers take between \$9 and \$10 out of total of \$25, while the equilibrium predicts that their share should be \$17.

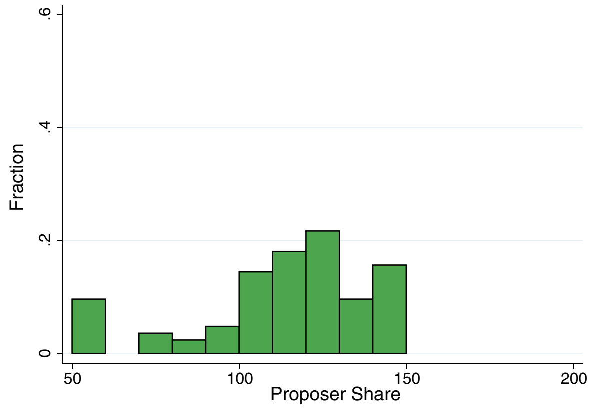
[14] For example, in Experiment 2 of FKL, the authors attempt to speed up the learning process by increasing the number of bargaining sessions from 15 to 25 and by introducing a sixth legislator, an economics graduate student, who was instructed to make proposals and to vote according to a pre-specified computer algorithm. This computer algorithm involved proposing higher shares to a proposer than the ones observed in the baseline experiments and approving only the proposals that gave her a share at least as large as the SSPE. Even in this modified version of the game, the authors observe that the proposers' share is still far below the SSPE predicted ones: in the last 3 bargaining sessions proposers receive on average of \$12 instead of \$17. This average of \$12 is in part driven by the computer allocating \$15 to a proposer and \$5 to two coalition members, and not just due to human proposers, who do not allocate such high shares to themselves. In the Equal Weights treatment in Frechette et al. (2003) the authors report that experienced subjects take on average 40.3% and inexperienced ones take 39.3% of the available resources for themselves, while the Baron–Ferejohn model predicts that this fraction should be 60%.

[15] We focus here on the proposals that passed with no delay in order to remove any reputation concerns that would appear if the bargaining covered two more stages. Looking instead at all proposals submitted in the first bargaining stage or all passed proposal regardless of the delay does not change the results: proposers in the Chat treatment extract a significantly higher share than proposers in the Baseline treatment. These results are presented in our Online Appendix.

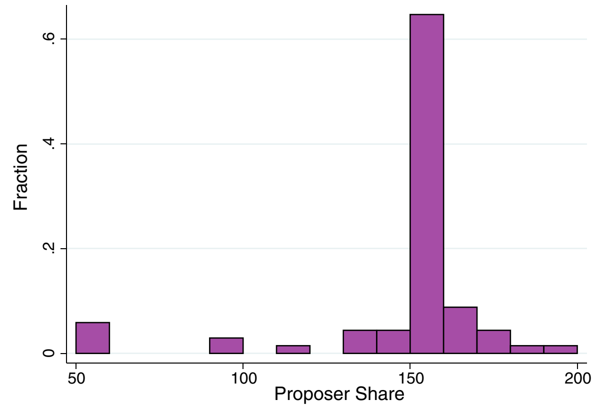
[16] The unit of observation is proposer's share (average per subject) in the last 5 bargaining sessions for those proposals that passed with no delay. Our conclusions are unchanged if we analyze the data at the session level, where the unit of observation is the average proposer share in each session. Indeed, the average proposer share in each session in the Chat treatment is higher than the average proposer share in any session in the Baseline treatment.

[17] A series of Wilcoxon Ranksum tests show that in twelve out of fifteen bargaining sessions (all but bargaining sessions 1, 2 and 7), the proposers' shares are significantly higher in the Chat than in the Baseline with a significance of at least 5%.

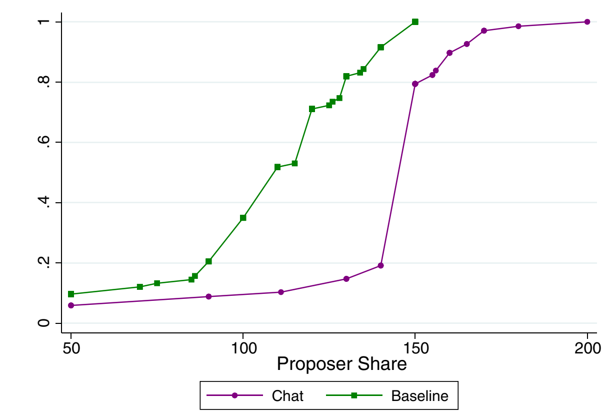
[18] The p-value of a 2-sided Test of Proportions is greater than 10% no matter whether we use session-level data or the acceptance rate of each proposal on the floor.



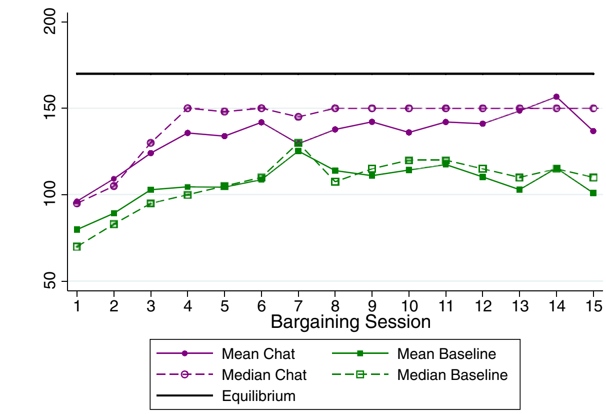
(a) Baseline: Histogram of Proposer shares in the last five bargaining sessions.



(b) Chat: Histogram of Proposer shares in the last five bargaining sessions.



(c) Cumulative distributions of Proposer shares in the last five bargaining sessions.



(d) Dynamics of Proposer share over the course of the game.

Fig. 1. Proposer shares. Notes: Proposals that passed without delay.

more than 10 tokens and the lowest share is less or equal to 10 tokens, and (3) the Equal Split strategy allocation, in which each share is at least 40 tokens. Interestingly, over 90% of all strategies can be categorized into one of these three strategies. Recall that according to the SSPE prediction, only three voters (including the proposer) receive positive shares and the two remaining voters get nothing. In other words, since the proposals are passed or rejected using a majority rule, we expect minimum wining coalitions of three voters to emerge and, thus, the

Table 2
Frequency of delays and distribution of resources in all treatments.

	Baseline	Chat	Baseline Long
Delays			
Proposals passed with no delay (all bargaining sessions)	81%	89%	80%
Proposals passed with no delay (first 5)	87%	90%	87%
Proposals passed with no delay (last 5)	87%	85%	73%
Distribution of resources			
Proposals that passed with no delay (all bargaining sessions)			
Double Zero strategy	72%	87%	72%
Single Zero strategy	7%	5%	6%
Equal Split strategy	17%	5%	18%
Proposals that passed with no delay (first 5)			
Double Zero strategy	66%	83%	62%
Single Zero strategy	7%	7%	15%
Equal Split strategy	22%	6%	19%
Proposals that passed with no delay (last 5)			
Double Zero strategy	78%	90%	73%
Single Zero strategy	2%	3%	5%
Equal Split strategy	14%	6%	18%

Double Zero strategy to be the most prevalent among the strategies described above.

This is precisely what we observe in the experimental data. In both treatments, a majority of the proposers form minimum winning coalitions by allocating resources to themselves and two other members of their group. This happens in 72% of all proposals submitted in the Baseline treatment and in 87% of all proposals submitted in the Chat treatment. By the last five bargaining sessions, these fractions are 78% and 90% and are not significantly different.[19] In the Chat treatment, among the proposals that passed with no delay in the last 5 bargaining sessions, 89% of subjects *always* use the Double Zero strategy when they are selected to be proposers and 5% *never* do it, compared with 63% and 22% in the Baseline treatment, respectively.

Supermajorities, that is situations in which proposers give positive shares to four out of five members of the group, or in other words Single Zero strategies, are rare in both treatments (7% and 5% in the Baseline and Chat treatments, respectively). We observe more equal-split distributions in the Baseline treatment (17% of all proposals) compared with the Chat treatment (5% of all proposals). Further, 21% of subjects have, at one point or another, used the Equal-Split strategy in the Baseline treatment, while this fraction is below 12% for the Chat treatment.[20]

Our results from the Baseline treatment are consistent with the prior literature. In particular, they are consistent with the findings reported in FKL, albeit small quantitative differences. Similarly to our results, FKL document that delays are rare.[21] Moreover, play converges toward minimal winning coalitions resulting in majority of proposers using the Double Zero rather than Single Zero or Equal Splits strategies.[22] Finally, while proposers obtain higher shares than any other member of the group, they under exploit their power and obtain on average 40% of the budget instead of the 68% predicted by the stationary equilibrium.[23]

Finally, we point out that in a later experiment, Baranski and Kagel (2014) report a similar increase in proposer power when communication is allowed. In their design, subjects are paired in groups of three and divide $30 and there is no discounting.[24] They find that when communication is allowed, as a fraction of SSPE prediction, proposers are able to extract between 15 and 22 percentage points more than without communication.[25] This is very much in line with our finding that proposers are able to extract 20 percentage points more when communication is allowed.[26] This shows that the increase in proposer power that we observe when people are allowed communicate in a multilateral bargaining setting is a robust result.

Conclusion 1. *Proposers extract a significantly higher share of resources when unrestricted communication is allowed. Overall, the sessions with unrestricted communication show much closer conformity to the SSPE predictions than the sessions without communication.*

4.2. Learning through repetition versus communication

We have seen that introducing unrestricted communication has a significant impact on the bargaining process. Indeed, in the Chat treatment we observe bargaining outcomes that are close to the SSPE predictions. In this section, we will investigate whether learning through repetition (the standard way in which most of the experimental literature asserts learning) has similar effects on bargaining outcomes. In order to do that, we will compare the Baseline Long treatment with the Baseline treatment and the Chat treatment.

Table 2 shows that the Baseline Long treatment is similar to the Baseline treatment in terms of strategies used by the proposers and the frequencies of delays. Indeed, there is no statistical difference in the amount of delays, the fraction of minimum winning coalitions and fraction of equal splits between the Baseline and Baseline Long treatments (p-value > 0.10). However, the bargaining outcomes in the Baseline Long treatment are starkly different from those in the Chat treatment. The fraction of delays, as well as the fraction of minimum winning coalitions and equal split strategies are significantly different between the Chat and Baseline Long treatments.[27]

Perhaps more surprisingly, allowing subjects to play more repetitions of the game does not increase the power of the proposer. Fig. 2 presents the cumulative distributions of the shares of the proposer in the Baseline, Chat and Baseline Long treatments. In fact, the mean (median) share of the proposer in the proposals that passed without delay in the Baseline Long treatment in the last 15 bargaining sessions is 105 (110) tokens, which is similar to 106 (110) tokens in the Baseline treatment and significantly smaller than 133 (150) tokens in the Chat treatment when looking at all bargaining sessions. These results are confirmed by statistical analyses.[28,29]

Conclusion 2. *Increasing the number of repetitions of the game does not help subjects converge to the SSPE predictions even after they play the game many (30) times: repetition is not a substitute for communication.*

In the remainder of the analysis we will focus on the Chat treatment specifically. We will analyze the chat messages that the members of the legislature send to each other to study the mechanism through which communication affects bargaining outcomes.

4.3. General features of the conversations

We start by noting that in the Chat treatment, subjects indeed used the messaging system in almost every bargaining session (in 94% overall and in 97% of the last 5 bargaining sessions). Furthermore, a vast majority of the conversations included discussions about the game being played (92% of the conversations in all bargaining sessions and 99% in the last 5 bargaining sessions).[30]

Table 3 illustrates examples of the two most common types of conversations we observe. In both conversations non-proposers discuss with the proposer the amount they are willing to accept in exchange for their vote. In the first example, non-proposers initiate this conversation,

[19] Given that some categories have very few observations, we perform a Fischer Exact Test where the unit of observation is, for each subject, the fraction of times he employed Double Zero strategy over the time period considered. We find that $p = 0.089$ if we consider all bargaining sessions, and $p > .10$ if we consider only the last five bargaining sessions.

[20] The p-value of a Fisher Exact Test where the unit of observation is, for each subject, the fraction of proposals that are Equal-Split is $p = 0.063$.

[21] FKL report no delays in the last five bargaining sessions in their experiment (see Fig. 1 in their paper).

[22] About 75% of proposals use the Double Zero strategy in the last two periods (see Fig. 4, panel 1 in their paper).

[23] Recall that in our Baseline treatment, proposers keep 110 tokens on average, which corresponds to 44% of the budget.

[24] In addition, there are other minor differences in the experimental procedures: Baranski and Kagel (2013) pay one out of 10 bargaining rounds and in some treatments subjects are not given IDs.

[25] Without communication proposers receive 14.6 dollars out of 30 (the SSPE predicts 20 dollars) and with communication they can extract between 17.6 and 19 dollars on average.

[26] They go from receiving on average 110 tokens without communication to receiving 144 tokens with communication (the SSPE predicts 170).

[27] The p-values for Fisher Exact Tests using the fraction of time that each individual uses those strategies as a unit of observation are all strictly less than 0.01.

[28] Wilcoxon Ranksum and Kolmogorov–Smirnoff tests show that the median and distribution of proposers' shares in the Baseline and Baseline Long treatments are not statistically different (p-value > 0.10). The statistical comparison between the Baseline Long and the Chat treatment, however, shows that these two treatments result in significantly different outcomes for proposers (p-value < 0.01).

[29] The findings concerning the Baseline Long and Baseline treatments are in line with the previous literature (see FKL (2003) and Footnote 14).

[30] We used a broad definition of what it means to talk about the game, which includes conversations about various strategies, the discussion of possible shrinking of the budget if the proposal is rejected, pleading to be in the coalition, requests for splits. There were, of course, over the course of the conversations, also messages that were not explicitly about the game, but that may have served to ingratiate oneself to the proposer. Many of these types of messages included jokes, such as "There are two cupcakes in an oven. The first says 'Boy its hot in here'. And the second says 'Hey look a talking muffin!'".

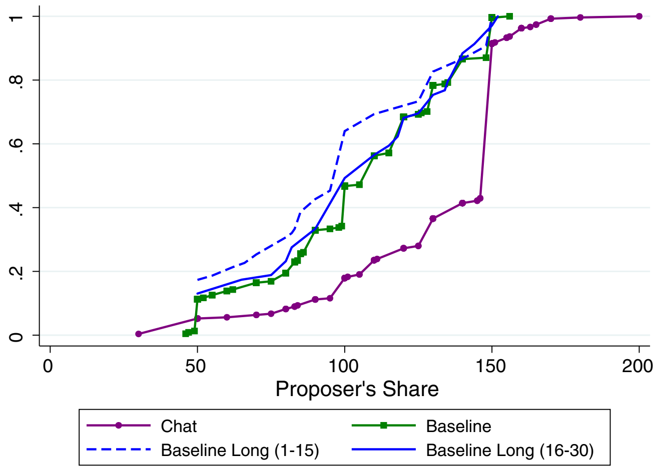


Fig. 2. Cumulative distribution of proposer shares in the Baseline, Chat and Baseline Long treatments. Notes: Proposals that passed without delay in all bargaining sessions.

while in the second example it is the proposer who solicits this information from one of the group members and then bargains it down.

In order to analyze the communication between bargainers, we will define the notion of "reservation price" and explore how frequently these are stated in conversations and how they evolve over the course of our experiment.

We call "reservation price" the amount that a non-proposer declares acceptable in return for a vote. If a non-proposer announced two or more reservation prices following some discussion with the proposer (such as in example 2), we take the lower number. If a non-proposer did not declare an amount, the stated reservation price is assigned a missing value in this particular bargaining session. This definition is used in the remainder of the analysis.

In most of the conversations, at least one bargainer announced a reservation price to the proposer. In fact, this was done in 82% of conversations in all bargaining sessions and in 93% in the last 5 bargaining sessions. Second, 74 of all conversations about the reservation prices were initiated by non-proposers, while 26% were initiated by the proposers.

As the game progresses, more and more voters reveal their reservation prices. The solid line in Fig. 3 shows the fraction of subjects who have sent messages with their reservation prices at least once up until that point in time over the course of the different bargaining sessions. By bargaining session 4, over 50% of subjects had announced their reservation price at least once, and by the end of the game this fraction rises to over 90%. The dashed line in Fig. 3 shows the fraction of non-proposers who have submitted reservation prices in each bargaining session. The trend is clearly increasing over time, which means that a higher fraction of non-proposers are submitting their reservation price to the proposer as they gain experience in the game.

As we discussed in the introduction, communication in *bilateral* bargaining setups has been known to focus subjects on fairness norms, and promote conversations on fairness and equality. This ultimately leads to more egalitarian outcomes and less proposer power. We used our data from the communication stage to identify the conversations that contain phrases about equality and fairness.[31] In Fig. 3 we show the fraction of subjects who have used the chat tool to lobby for fairness at least once up to each point in time over the course of the difference bargaining sessions. We also show the fraction of subjects who are using the chat tool to lobby for fairness at each point in time. The fraction of subjects who have used the messages to lobby for fairness, both cumulatively and per bargaining session is strikingly low. Indeed, by the end of the game fewer than 5% of subjects are doing that per bargaining session, and the overall fraction of subjects who have done that at least once over the course of the game does not go beyond 30%. The comparison between Fig. 3(a) and (b) confirms that the majority of conversations pertain to reservation prices.

Finally, a noteworthy aspect of the data is that reservation prices are transmitted mostly through private rather than public chats. When voters chat about prices, fewer than 10% of these messages are sent to the entire group; in fact, almost 90 resources through private (bilateral) conversations. So, even though public communication was allowed in this experiment, subjects opted not to use it.

Table 3
Typical conversations in the chat treatment.

Example 1	
2 → proposer:	I'll vote for you if you give me 50
1 → proposer:	Hi shoot me 40 for an auto yes
3 → proposer:	I am good with 50
Example 2	
Proposer → 2:	How much will it take for your vote?
2 → proposer:	50
Proposer 2:	What about 40
2 → proposer:	Sure

4.4. Communication as a tool to reduce uncertainty for the proposers

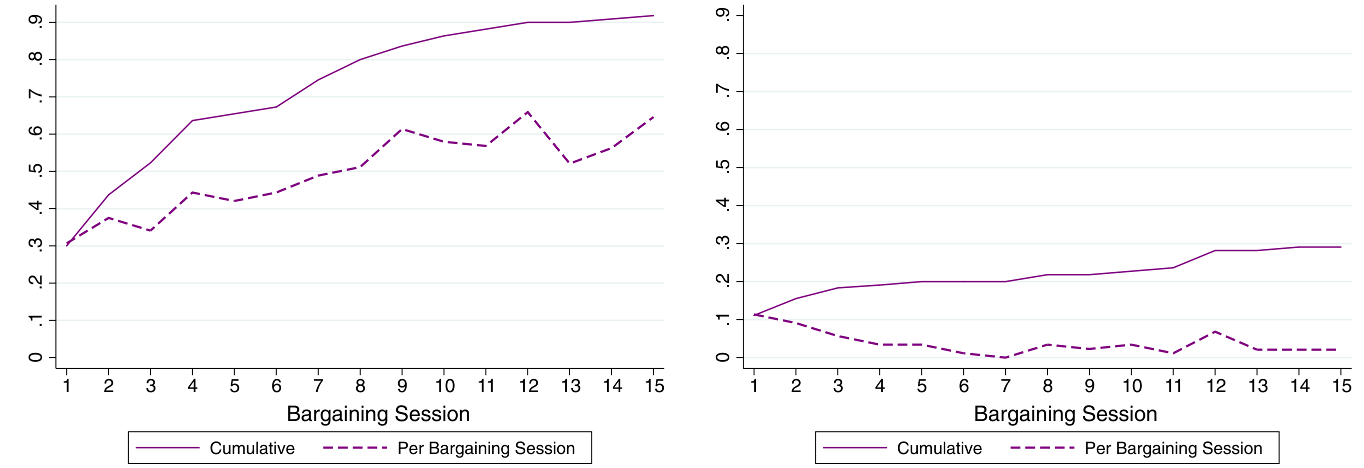
The experimental literature on bargaining games with no communication attributes the failure of the proposer to obtain the large shares predicted by the theory to the reluctance of at least some coalition partners to accept small shares. Indeed, when voters are offered shares close to the predicted ones, they often reject them. This forces the proposers to reduce their own shares in order to offer higher amounts to the votes and secure enough "yes" votes for the proposal to pass. McKelvey (1991), Frechette et al. (2003), Diermeier and Morton (2005), Frechette et al. (2005a), Frechette et al. (2005b), and Frechette et al. (2005a) document this phenomenon in multilateral bargaining environments, and Roth (1995) surveys a large body of experimental data from alternating-offer bilateral bargaining experiments that exhibit similar patterns.[32] Frechette et al. (2005a) suggest that what drives the proposers to offer coalition partners higher shares is the heterogeneity between subjects rather than the average behavior:

> *… in this case it is not so much what the average base player is willing to accept that is responsible (because the average willingness to accept is reasonably close to the SSPE prediction). Rather, it is the between subject variation in what base players are willing to accept that is responsible, so that to maximize expected income, formateurs need to offer substantially more than the SSPE share or else face very high rejection rates.*
>
> [Frechette et al., 2005a]

By providing a channel for the non-proposer to signal to the proposer the amount he/she is willing to accept, communication may solve the uncertainty problem that the proposer faces and allows proposers to extract larger share of resources in the game with communication.

[31] The most common examples of these types of messages are similar to "let's just split equally," "Equal is nice," "50, 50, 50, 50, 50."

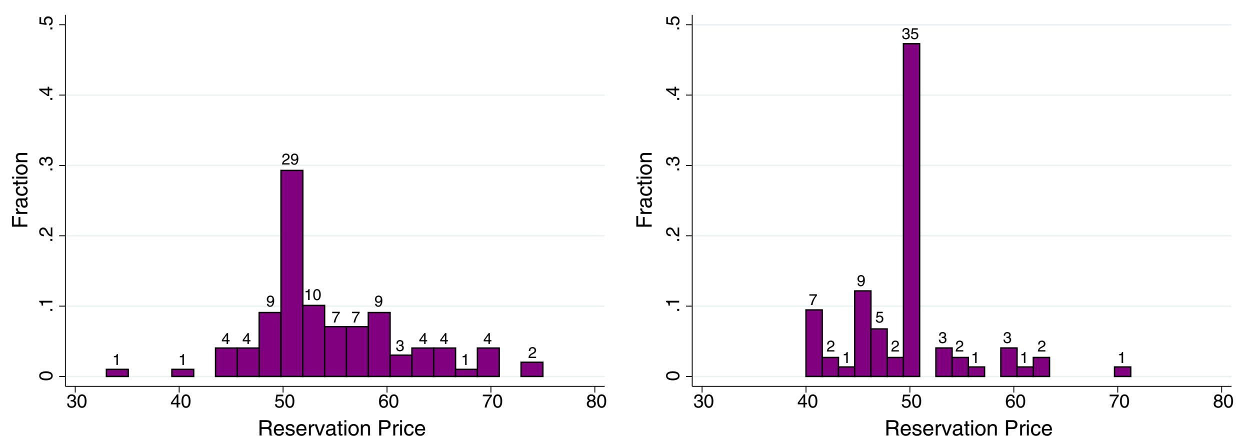
[32] See also Embrey, Frechette and Lehrer (2012) who study the role of posturing and reputation in bargaining games. In particular, Embrey et al. conduct an experimental investigation of the Abreu and Gul (2000) model, which can be thought of the two-stage bargaining protocol between two parties. In the first stage, each bargainer announces his demanded share of resources. If both demands are compatible with the budget, the game ends. If not, bargainers enter the second stage which is a concession game, which ends when one player concedes to the demands of others. While posturing is possible in our setup as well, since bargainers can use the cheap-talk communication stage to announce their demands, the success of such posturing behavior is significantly lessened by the competition that non-proposers face from other non-proposers. In the next section we provide evidence that this competition is one of the important forces that drives demands down and reduces the value of establishing "tough" reputation.



(a) Fraction of non-proposers sending a message with their reservation price.

(b) Fraction of non-proposers sending a message related to fairness.

Fig. 3. Conversation topics. Notes: The solid line shows the fraction of subjects who sent such a message at least once up to that point in time. The dashed line shows the fraction of subjects who sent such a message at that point in time. Data from both public and private chats are included.



(a) Chat: all bargaining sessions

(b) Chat: last five bargaining sessions

Fig. 4. Histogram of reservation prices. Notes: Fraction and number of observations in each bin in all bargaining sessions and in the last five.

Fig. 4 shows the histogram of stated reservation prices reported by the non-proposers at the communication stage in all bargaining sessions (Fig. 4) and last five bargaining sessions (Fig. 4).[33] These figures show that reservation prices among voters are heterogeneous. While there is a noticeable peak at 50 tokens, some voters are willing to accept shares as low as 30 tokens, while others demand 75 tokens. This heterogeneity is reduced as subjects gain experience with the game but remains present even in the last 5 bargaining sessions.

The importance of the communication channel becomes clear: in the Chat treatment, the proposers can learn the amounts that voters are willing to accept. The only strategic consideration that is left for the proposers is to decide whether to trust the information offered by the voters (the question we turn to next). In the Baseline treatment the task of the proposer is significantly more complicated. Indeed, he has to form beliefs about the amounts that other members are willing to accept, and then determine the allocation that maximize his expected payoff given the risk that his proposal might be rejected.

One possible problem the proposers face is whether to trust the information they receive in the communication stage. Indeed, this is a cheap-talk communication stage in which no promises are binding. Our data indicates that voters follow through on the promises they make in the communication stage. Overall, coalition partners accepted offers that are at least as high as their stated reservation price 96% of the time in all bargaining sessions.[34] Moreover, voters reject shares that are lower than their stated reservation price 72% of the time.[35] As we will show in the next section, this happens predominantly in situations where the proposer is selecting two other ('cheaper') members to be part of the coalition.

[33] The unit of observation is the reported reservation price of a subject in a particular bargaining session.

[34] Half of the remaining cases (which represent 4% of the cases) in which voters voted against a proposal that offered them at least as much as their reservation price were in situations where the other non-proposer in the coalition received more than they did.

[35] Interestingly, proposers do not lie to individual responders, by, for example, suggesting that other responders are willing to accept less.

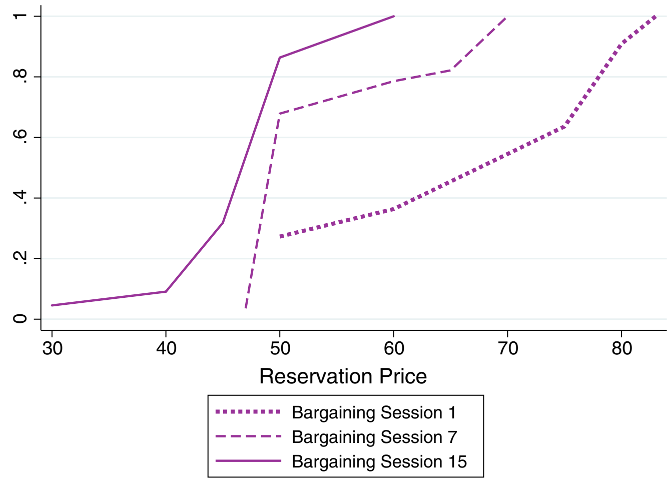


Fig. 5. Cumulative distribution function of reservation prices in bargaining sessions 1, 7 and 15.

Table 4
Random effects probit regression with clustering at the experimental session level.

	Regression I	Regression III
	Coefficient (p-Value)	Coefficient (p-Value)
Reservation price	−0.031***	−0.013
	(0.009)	(0.104)
Two cheapest		1.091***
		(0.000)
Time dummies	Yes	Yes
Constant	2.240***	.162
	(0.002)	(0.824)
# of subjects	97	97
# of observations	506	506

Notes: These probits were run using data from the 94% of proposals that were not equal splits. The dependent variable is $InCoalition_{i,t}$ takes the value of 1 if non-proposer is offered at least 20 tokens. Proposers are excluded from these regressions.
*** Significant at 1%.

Conclusion 3. *By communicating their reservation prices to the proposer, voters reduce the uncertainty the proposer faces.*

5. Communication as a tool to compete for a place in the coalition

As we have seen, the fraction of subjects who announce their reservation prices increases over time. In this section, we will present evidence that suggests that competition between non-proposers for a place in the coalition is a strong force that drives reservation prices down and allows proposers to extract a large share of resources.

Fig. 5 shows the cumulative distribution functions (CDFs) of the reservation prices stated by non-proposers in the first, seventh and last bargaining sessions. As subjects gain experience in the game, their demands shift downwards as can be seen with the leftward shift of the CDFs. A pairwise series of Ranksum tests show that the reservation prices in bargaining session 1, 7 and 15 are significantly different.[36]

Thus, not only is the fraction of subjects who express a reservation price increasing, but those prices are decreasing over the course of the game. The second example in Table 3 is one example of how proposers bargain with non-proposers and lower their reservation prices. While observed in some conversations, this behavior is not, however, the predominant feature of the data. We document that non-proposers lower their reservation prices within a bargaining session (in response to a proposer or from their own volition) in just over 5% of the bargaining sessions and by an average of just above 12 tokens and a median of 10 tokens. Most of the decrease in reservation prices occurs across the bargaining sessions.

We next turn to showing how declaring a reservation price impacts outcomes. Table 4 shows the results of two random effects Probit regressions with clustering at the experimental session level. The dependent variable y_{it} is equal to 1 if one is invited into the coalition (i.e. offered more than 20 tokens). The independent variables always include time dummies (which here correspond to the different bargaining sessions), as well as the subjects' reservation price.[37] Regression II adds to Regression I a dummy equal to 1 if one's bid is among the two lowest.[38] Finally, proposers are excluded from these regressions and we focus on the 94% of proposals that were not equal splits.[39] The specifications for the two regressions can be seen in Eqs. (1) and (2) below.[40]

$$InCoalition_{i,t} = \mathbb{1}\left[\beta_0 + \beta_1 P_{i,t} + \sum_{\tau=2}^{\tau=15}\left(\gamma_\tau D_{i,\tau}\right) + \alpha_i + \epsilon_{i,t} \geq 0\right] \quad (1)$$

$$InCoalition_{i,t} = \mathbb{1}\left[\beta_0 + \beta_1 P_{i,t} + \beta_2 L_{i,t} + \sum_{\tau=2}^{\tau=15}\left(\gamma_\tau D_{i,\tau}\right) + \alpha_i + \epsilon_{i,t} \geq 0\right] \quad (2)$$

Regression I shows that announcing a low reservation price increases the chances of being accepted in the coalition. However, once the ranking of the reservation prices are taken into account, we can see that the amount itself no longer matters. Rather it is the ranking of the reservation prices that determines whether one is part of the coalition: proposers invite the two cheapest members to obtain the required majority of votes. This is the case whether one looks at all bargaining sessions or only at the last five. Indeed, as Regression II shows, in terms of being invited in the coalition, what matters is whether or not one's reservation price was among the two cheapest in the group.[41]

We next turn to explain what may factor in a subject's reasons for stating a particular reservation price. In Table 5 we report the results of a random effects regression with clustering at the experimental session level. The dependent variable $y_{i,t}$ is the stated reservation price. The independent variables include a series of time dummies (where time is the recording of the number of bargaining sessions where one was a non-proposer) as well as a dummy for whether a subject was in the coalition in the previous bargaining session. This is of particular relevance since proposers invite the cheapest members into the coalition. Thus, someone who was left out of the coalition in bargaining session

[36] The p-values are 0.000, 0.0023 and 0.0153 for the comparison of reservation prices between bargaining sessions 1 and 15, 7 and 15 and 1 and 7, respectively.

[37] Subjects who did not state a reservation price are assigned a missing value for the reservation price.

[38] In Regression II those individuals who did not state their reservation prices are counted as not part of the two cheapest members and have a dummy equal to 0. Excluding them from the regression does not change our results. In that regression the coefficient on reservation price remains insignificant and the coefficient on the dummy is 1.33 (significant at the 1% level).

[39] Using all the proposals, including the equal ones does not change the results, which are available from the authors upon request. The reason that we exclude them here is that in this result we focus on competition for a place in the coalition. If distributions are equal, there is no such competition.

[40] Here $\mathbb{1}$[.] is an indicator function for a right hand side greater than zero. Here the time dummy variable $D_{i,\tau}$ has the value 1 if the observation comes from bargaining session $t = \tau$ and $P_{i,t}$ is subject i's reservation price in bargaining session t. Finally, $L_{i,t}$ is a dummy variable equal to one if subject i's reservation price is among the two lowest at bargaining session t.

[41] Running a regression in which the ranking of one's reservation price is included instead of a dummy recording whether the rank is among the two lowest leads to similar results: the two cheaper members are more likely to be invited in the coalition.

Table 5
Random effects regression (*ReservationPrice*$_{i,t}$ is one's declared reservation prices) with clustering at the experimental session level.

	Coefficient	Robust std. err.	p value
Bargaining session 2	10.17***	25.20	0.000
Bargaining session 3	10.13***	3.44	0.001
Bargaining session 4	8.913***	2.87	0.004
Bargaining session 5	6.548***	9.63	0.000
Bargaining session 6	5.721**	2.53	0.011
Bargaining session 7	4.478***	4.29	0.000
Bargaining session 8	2.805**	2.37	0.018
Bargaining session 9	1.590	1.40	0.161
Bargaining session 10	0.152	0.08	0.939
Bargaining session 11	0.665	0.45	0.655
Bargaining session 12	−1.906	−1.10	0.273
Bargaining session 13	0.976	1.21	0.227
Bargaining session 14	−0.292	0.24	0.809
Not prev. in coalition	−0.892**	−2.19	0.028
Constant	50.18***	20.17	0.000

# of subjects: 99.
# of observations: 538.
Notes: These probits were run using data from the 94% of proposals that were not equal splits. Proposers are excluded from these regressions.
** Significant at 5%.
*** Significant at 1%.

$t-1$ may be pushed to lower his price in bargaining session t. The specification for this regression can be seen in Eq. (3) below.[42]

$$ReservationPrice_{i,t} = \beta_0 + \beta_1 E_{i,t-1} + \sum_{\tau=2}^{\tau=15}\left(\gamma_\tau D_{i,\tau}\right) + \alpha_i + \epsilon_{i,t} \tag{3}$$

The results of the regression show that relative to the last bargaining session (bargaining session number 15), stated reservation prices in the earlier bargaining sessions are significantly higher. The effect of time, however, decelerates by the 9th bargaining session. The effect of not being included in the previous coalition is negative and significant with a p-value of 0.028. In other words, a voter who was excluded from the coalition in the previous bargaining session responds by lowering the amount he is willing to accept in this bargaining session to increase his chances of getting into the coalition.

Finally, we run a random effects probit regression with clustering at the experimental session level in which the dependent variable is equal to 1 if a subject lowered his reservation price relative to his previous stated price. The independent variables are a series of time dummies as well as whether or not a subject was part of the coalition in the previous bargaining session. As before, proposers are dropped from the regressions. The marginal effect of not being in the previous bargaining session on the probability of lowering one's bid is significant at the 1% level with a magnitude of 21%. That is, not being in the previous bargaining session's coalition implies a 21% average decrease in the probability of lowering one's reservation price.[43]

To summarize, the results of the regression analysis support the hypothesis that communication increases competition between non-proposers for a place in the coalition: (1) those who state a smaller reservation price are more likely to be included in the coalition, (2) the two cheapest members are more likely to be included in the coalition, and (3) non-proposers lower their reservation price when they were previously excluded from a coalition. It is this drop in reservation prices that allows a proposer to extract a higher share of the resources.

Conclusion 4. *Non-proposers compete with each other for a place in the coalition by lowering their reservation price. The proposers select the cheapest members and invite them into the coalition.*

6. Conclusion

In this paper we extend the classical multilateral bargaining framework of Baron and Ferejohn (1989) to allow members of the legislature to communicate with one another through chat boxes after the proposer is selected and before he/she submits her proposal for allocation of resources. The communication is unrestricted and members of legislature are free to send any message to any subset of members of their group.

We show that adding a cheap-talk communication stage leads to outcomes that are significantly closer to the SSPE predictions, relative to any other experiment in this literature, where even experienced proposers fail to extract close to equilibrium rents. Indeed, communication has a significant and large effect on the distribution of resources and allows proposers to extract a significantly higher share of resources, approaching the theoretically predicted one.

In analyzing the content of the chat messages, we study the mechanism through which communication affects bargaining outcomes. Non-proposers use the chat messages to communicate their reservations prices, that is, the amount it will take for them to vote in favor of a proposal. These prices are trust-worthy: coalition partners accept offers that are least of equal values as ones they state during negotiations, while they reject offers that are lower. Proposers select the coalition partners with the smallest stated reservation prices. This leads to a decrease in stated reservation prices as subjects gain experience with the game. Our paper shows that the emerging consensus that communication allows subjects to appeal to fairness considerations does not necessarily happen in multilateral bargaining. In the present case, communication facilitated competition between non-proposers by essentially creating a double auction between them for a place in the coalition.

Many features that are present outside the laboratory are impossible to replicate in an experiment, while others are easily implemented. Communication is one such feature. Our experiment has shown that letting subjects communicate in a Baron–Ferejohn multilateral bargaining game is sufficient to largely reconcile theory and experiment. This modification to an experimental protocol may prove useful in future experimental work.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2014.06.006.

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[42] Here the time dummy variable $D_{i,\tau}$ has the value 1 if the observation comes from bargaining session $t=\tau$ and $E_{i,t-1}$ is a dummy indicating whether subject i was excluded from the coalition in bargaining session $t-1$. Finally, α_i is a subject specific error term and $\epsilon_{i,t}$ is the idiosyncratic error term.

[43] The marginal effect analysis is in our Online Appendix. The specification for this regression is:

$$LoweredReservationPrice_{i,t} = \mathbb{1}\left[\beta_0 + \beta_1 E_{i,t} - 1 + \sum_{\tau=2}^{\tau=15}(\gamma_\tau D_{i,\tau}) + \alpha_i + \epsilon_{i,t} \geq 0\right]. \tag{4}$$

Here $\mathbb{1}$ [.] is an indicator function for a right hand side greater than zero, the time dummy variable $D_{i,\tau}$ has the value 1 if the observation comes from bargaining session $t=\tau$ and $E_{i,t-1}$ is a dummy equal to one if subject i was excluded from the coalition in bargaining session $t-1$. Finally, α_i is a subject specific error term and $\epsilon_{i,t}$ is the idiosyncratic error term.

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