The Determinants of Multilateral Bargaining: A Comprehensive Analysis of Baron and Ferejohn Majoritarian Bargaining Experiments

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Abstract

We analyze the data sets of all majoritarian Baron and Ferejohn (1989 Am. Pol. Sci. Rev.) experiments through 2018. By exploiting the variation of the experimental parameters, we are able to identify how group size, discount factor (cost of agreement delay), voting weights, and communication affect bargaining outcomes and dynamics. The outcomes are qualitatively in line with the stationary subgame perfect equilibrium, i.e., minimum winning coalitions are modal; proposers demand larger shares

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than non-proposers; and most agreements are reached without delay. Experience and communication between players move outcomes closer to the equilibrium. However, bargaining dynamics are not stationary. Behavior following a disagreement is history-dependent in the form of retaliation towards failed proposers and their supporters, which, if rationally expected, may deter proposers from demanding high shares.

1 Introduction

In 1989 , David P. Baron and John A. Ferejohn published an influential article entitled "Bargaining in Legislatures". Their goal was to provide a game-theoretic foundation for the analysis of legislative bargaining in order to characterize the distribution of payoffs within legislatures. In its original form, the authors framed their model as an abstraction for problems of "distributive or expenditure policy in a unicameral, majority rule legislature not favoring any member of the legislature or any particular outcome". Since then, a plethora of applications and theoretical extensions have been set forth in and beyond political science (Eraslan and Evdokimov, 2019). Importantly, their model has been the subject of a vast number of experimental investigations, which are the focus of this paper.

Baron and Ferejohn proposed a very simple and intuitive bargaining protocol.[1] A group of three or more players bargain on how to divide one unit of wealth. Every member has the same probability of being *recognized* as the proposer. Once a proposal is submitted, it cannot be modified, and players proceed to a sequential vote where a simple majority of members in favor is required for approval. In case of rejection, a new bargaining round in which every member has the same chance of being recognized occurs. The payoffs for each subsequent round are discounted equally by all subjects.

In this game, any allocation is a Nash equilibrium and survives subgame perfection (for sufficiently patient players). By further restricting strategies to be history independent,

[1] In this article, we focus on the closed amendment rule that has received wide attention. A version of the model where another player must agree to move a proposal for a vote is not discussed here.

the authors characterize the stationary subgame perfect equilibrium (SSPE), which yields a unique distribution of payoffs. It predicts that minimum winning coalitions (MWCs) will form, meaning that only a simple majority of members will receive a positive share of the pie. Proposers will offer coalition partners a share that makes them indifferent between accepting and the continuation value of the game. The equilibrium continuation value of the game decreases with the cost of agreement delay and decreases with the size of the committee. Given the exclusion of redundant members and players' impatience, proposers are able to extract a larger share of the pie (known as proposer power). Finally, agreements are reached without delay in the equilibrium.

There is wide variation in experimental design features such as the number of games played, subject payment scheme (i.e., random period payment or payment for all periods), strategy method versus direct response, and fund size, among others. Naturally, studies also use different metrics to report findings or condition on different subsamples. For example, some studies focus on all proposals while others only on approved ones, and some studies further condition the analysis on proposals approved without delay. Certain studies report results for periods of play once subjects have gained experience while others pool all periods. Moreover, a few studies define a player as being excluded by the proposer when she receives a share of 0 while others use a more lenient measure such as receiving less than 5 percent of the funds. All these differences may affect comparisons across studies.

In this article, we uniformly analyze the data from all published studies through 2018. We focus on treatments that are comparable with each other in terms of their theoretical predictions and close in spirit to the symmetric nature of the original model.[2] Thus, our meta-analysis complements the thorough reviews offered by Palfrey (2016) and Agranov (2020), with which we share several findings.

The SSPE outcomes will serve as our testable theoretical predictions because all the

[2]We include treatments that yield symmetric predictions and in which the funds to be distributed are exogenous in our analysis. We also incorporate treatments that allow for costless communication.

literature has explicitly focused on them. However, given the richness of our dataset[3], we will also be able to investigate alternative *behavioral* hypotheses. For example, we verify whether there is a negative relationship between group size and the likelihood of reaching an agreement without delay and whether a costly delay (measured by the experimentally induced discounting parameter) reduces the probabilities of disagreement. Our regression analysis controls for player's voting weights so that we can analyze if there is an *illusion of power* that can account for the variation in the sharing of the pie. We also study learning patterns by explicitly controlling for experience in our regressions.

The data provide qualitative support for the SSPE; proposers demand and effectively keep larger shares, albeit smaller than predicted. MWCs are modal, representing 60 percent of all proposals, and immediate agreements are the norm (80 percent of all bargaining groups). With experience, the proportion of MWCs increases, which is accompanied by an increase in the proposer's demanded share. Nevertheless, conditional on proposals being MWCs, proposer power remains rather steady.

By exploiting the variation in experimental parameters (group size and discounting) across studies, we are the first to conduct a structural estimation of the behavioral relevance of factors determining the equilibrium proposer share. The estimation results show little evidence of a good fit between the theory and subject behavior. Moreover, reduced-form regressions testing the comparative statics rule out that the delay cost affects the proposer's demanded share, which is rather unresponsive to changes in discounting. Furthermore, we find that as the delay cost increases, the prevalence of MWCs decrease, which is likely due to the desire of proposers to reduce the probability of rejection. Effectively, immediate agreement is more likely as delay becomes more costly. While these findings are somewhat intuitive, they are not in line with the SSPE.

One key feature regarding the bargaining process *per se* that has been largely understud-

[3]We have three group sizes, communication treatments, and six discount factors from 0.5 to 1, for a total of 10 treatments. Not all combinations of discounting, group sizes, and communication have been investigated. We explain our sample and selection criteria in Section 3.

ied in most of the existing literature is the behavior of subjects after a disagreement has occurred. We believe that the lack of history analysis (off-equilibrium behavior) is mainly due to the relatively small size of the subsample in question given that delay is quite uncommon.[4] History independence of bargaining strategies (assumed in the SSPE) requires players to not be spiteful against a previous proposer who excluded them from the distribution of the fund. In a SSPE, the continuation value of the game following a rejection (i.e. off the equilibrium path) is equal for all players, yet this is not what the data show. We find strong evidence of retaliation against previous proposers by subjects who did not agree with the share offered to them previously. In addition, there is moderate evidence of retaliation against those who supported a failed proposal. The fact that the empirical continuation value is lower for previous proposers may partially explain why proposers fail to keep larger shares.

The paper proceeds as follows. First, we offer a summary of the literature in Section 2. Section 3 describes our data collection process and sample definition. The main analysis is presented in Section 4 and we summarize our results in a series of 8 concise findings throughout this section. Section 5 concludes the article.

2 Literature Review

In this review section we present a summary table with all known published studies in chronological order. In it, we include a brief description of the question(s) being asked in each study, the parameters used (group size, discount factor), and experimental design details such as total periods of play (i.e. number of bargaining games) and number of rounds within a game that are permitted until approval. Our goal is to provide researchers with a birds-eye view of the literature and its progression over the last three decades. We do not discuss in detail the results of each study because these are the focus of the analysis section.

[4] Exceptions are Fréchette et al. (2005b) who report no evidence of retaliation for previous proposers and Bradfield and Kagel (2015) who report that teams discuss retaliation and act on it.

We devote a section of the Online Appendix to review experiments with asymmetric players (and asymmetric predictions). These are excluded from our main analysis because data is too scarce to conduct a meaningful meta-analysis.

Table 1: Summary of Majoritarian Baron and Ferejohn Experiments in Chronological Order

Study and Brief Description	Parameters	Design Details
McKelvey (1991). Players negotiated on three possible outcomes. Each outcome defines a probability for each player of winning a fixed prize. It can be that all players win the prize (non-excludable). The predefined outcomes were chosen as to induce cycling of preferences in pairwise comparisons. The author concludes that the SSPE is not a good predictor: proposer power is lower than expected.	Group Size: 3; $\delta = 0.95$; Pie/Group Size: Varies; Sample size: n=36 N=4.	12 Periods. Infinite horizon. Single proposal. Direct response voting. Between-subject design. 3 USD show-up fee. Payment for all bargaining periods.
Fréchette et al. (2003).** The authors vary the bargaining protocol: open versus closed-amendment rule. Delays are longer, MWCs are less frequent, and egalitarian splits are more common with the open amendment rule.	Group Size: 5; $\delta = 0.8$; Pie/Group Size: 5 USD; Sample size: n=40 N=8.	10 or 15 periods (with partner matching but randomized IDs). Infinite horizon. All submit proposal. Direct response voting. Between subject design. 5 USD show-up fee. Payment for 4 random periods.
Diermeier and Morton (2005). By varying voting weights and recognition probabilities, different coalition compositions and distributions of the fund are predicted to occur. The authors conclude that SPE predictions are not useful in describing observed behavior and that simple rules of thumb may fare better.	Group Size: 3; $\delta = 1$; Pie/Group Size: 15 USD; Sample size: n=36 N=3.	18 Periods. Finite horizon (5 rounds). Single proposal. Direct response voting. Between-subject design. 7 USD Show up fee. Payment for 1 random period.
Fréchette et al. (2005a).** Coalition formation and proposer power is compared in BF and demand bargaining (Morelli, 1999) with and without an apex player. Apex players have more voting shares and higher recognition probability, which yields them a higher expected payoff but lower likelihood of being invited into a coalition when not proposing.	Group Size: 5; $\delta = 1$; Pie/Group Size: 12 USD; Sample size: n=60 N=6.	10 Periods. Infinite horizon. All submit proposal. Direct response voting. Between-subject design. 8 USD show-up fee. Payment for 1 random period.

Table 1 continued from previous page

Study and Brief Description	Parameters [1]	Design Details
Fréchette et al. (2005b).** Voting weights and recognition probabilities are varied in such a way that the equilibrium distribution of payoffs remains constant across treatments but coalition composition probabilities vary in equilibrium. Every player needs the support of one additional member to pass her proposal, thus changes in voting shares only affect nominal power, but no real power. The authors conclude that the SSPE is a better predictor than Gamson's law.	Group Size: 3; δ = 1 and 0.5; Pie/Group Size: 10 USD; Sample size: n=108 N=8.	10 Periods. Infinite horizon. All submit proposal. Direct response voting. Between-subject design. 8 USD show-up fee. Payment for 1 random period.
Diermeier and Gailmard (2006). Single round BF game with heterogeneous disagreement values (alternatively, an ultimatum game with majority rule voting). Contrary to rational behavior, proposers' shares are correlated with their disagreement value, the cheapest coalitions are not always formed (about 40%) and the all-way splits are common.	Group Size: 3; δ=0; Pie/Group Size: 1.25 USD; Sample size: n=99 N=12.	40 periods (varying disagreement values every 10). Finite horizon (1 round). Single proposal. Direct response voting. Within-subject design. 5 USD show-up fee. Payment accumulated for all periods.
Drouvelis et al. (2010).** Committee size is varied while keeping the voting shares and rule constant in order to identify if and how enlargement affects bargaining outcomes. According to theory, when a veto player loses veto power with enlargement, non-veto players benefit substantially. This finding is corroborated by the experimental evidence.	Group Size: 3; δ=1; Pie/Group Size: 1.2 GBP; Sample size: n=160 N=12.	10 Periods. Finite horizon (20 rounds). All submit proposal. Direct response voting. Between-subject design. No GBP show-up fee indicated. Payment accumulated for all periods.
Kagel et al. (2010).** The authors consider a treatment with one veto player in the committee. Delays are more frequent in the veto treatment. When veto players propose they receive larger shares compared to non-veto proposers and proposers in the baseline. However, they do not fully exploit their power.	Group Size: 3; δ=0.5, 0.95 ; Pie/Group Size: 10 USD; Sample size: n=150 N=12.	10 periods. Infinite horizon. All submit proposal. Direct response voting. Between-subject design. 8 USD show-up fee. Payment for 1 random period.

Table 1 continued from previous page

Study and Brief Description	**Parameters** [1]	**Design Details**
Miller and Vanberg (2013).* Length of bargaining is compared under majority and unanimity rules. Unanimity leads to an increase in bargaining duration.	Group Size: 3; δ=0.9; Pie/Group Size: 6.6 GBP; Sample size: n=48 N=4.	15 periods. Finite horizon (22 rounds). Direct response proposal. Direct response voting. Between-subject design. 4 GBP show-up fee. Payment for 1 random period.
Agranov and Tergiman (2014).** Free-form written communication during the proposal stage in open-door structure (any subset of players may communicate). Communication enables proposers to extract a higher share of resources close to SSPE predictions and mildly reduces delay.	Group Size: 5; δ=0.8; Pie/Group Size: 1 USD; Sample size: n=235 N=7.	15 or 30 periods. Infinite horizon. All submit proposal. Direct response voting. Between-subject design. 5 USD show-up fee. Payment for all bargaining periods.
Baranski and Kagel (2015).** Free-form written communication introduced during the proposal stage under two structures: closed door (voter and proposer only) or open door (as in AT2014). Regardless of structure, proposer power is close to SSPE, voters actively seek for the exclusion of other voters in MWC.	Group Size: 3; δ=1; Pie/Group Size: 10 USD; Sample size: n=126 N=8.	10 periods. Infinite horizon. Direct response proposal. Direct response voting. 8 USD show up fee. Payment for 1 random period. Free-form communication both structures.
Bradfield and Kagel (2015).** Individuals and teams (acting as one decision-maker) are compared. Teams play closer to the SSPE predictions: higher proposer power and higher MWCs.	Group Size: 3; δ=1; Pie/Group Size: 10 USD; Sample size: n=105 N=6.	10 periods. Infinite horizon. All submit proposal. Direct response voting. 8 USD show-up fee. Payment for 1 random period.

Table 1 continued from previous page

Study and Brief Description	Parameters [1]	Design Details
Miller and Vanberg (2015).* Similar to the 2013 article comparing groups of 3 with groups of 7. Probability of a proposal being accepted is found to be decreasing in group size.	Group Size: 3 and 7; δ=0.5; Pie/Group Size: 6.6, 7.1 GBP; Sample size: n=101 N=8.	15 periods. Finite horizon (length varies by treatment) . All submit proposal. Direct response voting. 4 GBP show-up fee. Payment for 1 random period.
Baranski (2016). All previous studies assume an exogenous fund but here, the total fund to distribute is jointly produced. All players simultaneously make investment decisions, which are scaled by an efficiency enhancing factor and added. Proportional sharing is prevalent and outcomes do not resemble SSPE.	Group Size: 5; δ=1; Pie/Group Size: Varies; Sample size: n=80 N=5.	10 periods. Infinite horizon. All submit proposal. Direct response voting. Between-subject design. 5 USD show-up fee. Payment for 1 random period. Initial investments are added and multiplied times 2. 4 USD endowment.
Fréchette and Vespa (2017). Previous studies show little variation in offers and proposer shares, complicating inferences on the determinants of voting. The authors consider a wide range of discount factors, leading to a wide range of theoretical voting thresholds, and allow for computer generated proposals to introduce heterogeneity. They conclude that SSPE predictions on voting fit the data better than simple rules of thumb.	Group Size: 3; δ=0, 0.2, 0.4, 0.6, 0.8, 1; Pie/Group Size: 10 USD; Sample size: n=72 N=5.	18 periods. Infinite horizon. All submit proposal. Direct response voting. Between-subject design. 15 USD show-up fee. Payment for 1 random period.

Table 1 continued from previous page

Study and Brief Description	**Parameters [1]**	**Design Details**
Miller et al. (2018).[**] The payoffs resulting from bargaining breakdown (disagreement values) are varied as in Diermeier and Gailmard (2006) but with more bargaining periods. Results show that the likelihood of voting in favor decreases as disagreement value increases. Under unanimity, players with higher disagreement are offered larger shares by proposers. Under majority, players with lower disagreement values are more often part of an MWC.	Group Size: 3; δ=0.66; Pie/Group Size: 10 EUR; Sample size: n=240 N=10.	30 periods. Infinite horizon (with probability of breakdown). Single proposal. Direct response voting. Within-subject design. 3 EUR show-up fee. Payment 1 random period.

** Data of relevant treatments used in all of the analysis.

* We only have data for round 1 proposals.

1 The letter n denotes the number of subjects and N denotes the number of sessions.

3 Data Collection and Sample Selection Procedure

Our data was collected from authors' websites when publicly available. If not, we contacted the corresponding authors directly with all providing their data. An exhaustive search was conducted on the main academic digital repositories searching for the keywords "multilateral bargaining experiments" and "Baron and Ferejohn (1989)" among others. Two research assistants were employed to aid in the search task and data compatibilization process.

In our comprehensive analysis we will delimit our sample of study to treatments in which the SSPE prediction is that all players have the same stationary value of the game. We refer to these treatments as symmetric. Note that this does not preclude asymmetric recognition probabilities when bargaining has an infinite horizon because symmetric stationary values may emerge. However, when there are a finite number of rounds to reach an agreement, we require equiprobable recognition.[5] In these cases, asymmetric recognition yields unequal continuation values.

Our restriction implies that we are choosing treatments with the following parameter configurations:

1. Equal real bargaining power: All members must have the same equilibrium probability of inclusion in a winning coalition. This further excludes treatments where some players have a disproportionate voting weight such as the Apex treatment in Fréchette et al. (2005a) and the veto treatments in Drouvelis et al. (2010) and Kagel et al. (2010).

2. Symmetric disagreement values: If bargaining reaches the final round, or if breakdown occurs as in Miller et al. (2018), we require that all players receive the same payoff.

In keeping as close as possible to the original form of the Baron and Ferejohn game, our included sample of treatments all share the following features:

[5] See Section 3.1 in Norman (2002).

1. There are at least two rounds of bargaining allowed in a given game.

2. The fund to distribute is exogenous.

3. Subjects have stable group identifiers within a bargaining group.

4. Subjects are not identifiable across games in the experiment.

5. All proposals and voting decisions are made by subjects and not by computers.

Finally, we exclude data from treatments in which subjects have participated in previous BF experiments. Studies marked with ** and * in Table 1 are included in our analysis. Treatments not meeting the conditions above in those studies are excluded.[6] The different combinations of group size, discount factor, and communication that are part of our analysis can be inferred from Table 2.

4 Analysis of Symmetric Treatments

Given the parameter configurations we have chosen to analyze, the model's point predictions and comparative statics under the SSPE are the following:

1. **Proposer's share:**

$$propshare = 1 - \frac{\delta}{group\,size}\left(\frac{group\,size - 1}{2}\right) \tag{1}$$

where $\frac{\delta}{group\,size}$ represents the discounted continuation value of the game (and the minimum amount any rational voter would accept) and $\left(\frac{group\,size-1}{2}\right)$ is the total number of votes needed for approval (excluding the proposer). The proposer's share falls with group size and δ.

[6] See Section 3 in the Online Appendix for a study-by-study explanation of which treatments were included.

2. **Minimum winning coalitions (MWCs):** only the minimum number of voters required for approval are offered δ/n, the rest are offered 0. Group size and discount factor have no effect on the prevalence of MWCs.

3. **Delay:** Agreements are reached without delay. Group size and discount factor have no effect on the timing of agreements.

These predictions hold for all rounds in infinite horizon games, and for all rounds except the last one in finite horizon games, where the prediction is that the proposer keeps the entire fund.

Before starting our analysis, a few definitions are necessary. A *game* naturally refers to a match between the same subjects until an agreement or the deadline are reached. Each game can last several *rounds*, which consist of a proposal and voting stage. We will refer to a player as being *included* in a proposal whenever she receives 5 percent or more of the total fund. A MWC is defined as a proposal where exactly the required majority of voters are included. An all-way split is a proposal in which all members receive shares greater than or equal to 5 percent of the total fund. Our analysis is robust to considering a strict definition (i.e. share greater than 0 counting as included), but we allow for some wiggle room as most studies do by considering *pittance shares* as equivalent to exclusion from the coalition. These are rarely voted in favor of. When we refer to the proposer's share, we are referring to the share that proposers demand for themselves. When the sample of analysis includes only accepted proposals, it is the share proposers effectively receive. The size of the pie has been normalized to 1 in all experiments and shares are expressed as proportions.

We will restrict our analysis to the first 10 games since this is the minimum number played in every study. Our main results will be presented in a series of regressions concerning the main variables (proposer's share, MWCs, and delay) in which we include subject-level and study-level random effects. The results presented here are robust clustering standard errors at the study level.[7] Our first goal is to test the theoretical predictions of the SSPE and then

[7]In the body of the paper we focus on multilevel random effects models. Our results are robust to

to inspect alternative hypotheses of how experimental parameters and conditions may affect behavior.

4.1 Proposer Power

Our first task is to investigate if proposers hold any power, that is, if they demand and receive larger shares than non-proposing members. Hence, we will pool our analysis over all values of δ for each group size. Soon after, we will focus on the accuracy of the point predictions. Panel A of Table 2 shows the mean share that proposers demand for each group size and discount factor, with and without communication. Focusing on games 6 to 10 for treatments without communication, proposers demand 52 percent of the total fund in groups of 3, 41 percent in groups of 5, and 26 percent in groups of 7. Meanwhile, non-proposers that are included in the coalition are offered 40, 25, and 18 percent, for each group size respectively (see Panel B of Table 2).

In treatments with communication, proposers demand larger shares: 61 percent in groups of 3 and 56 percent in groups of 5. While we have focused on demanded shares in round 1, restricting attention to accepted proposals leads to very similar results.

Finding 1. *Proposers demand and receive a larger share than non-proposers, in line with the stationary subgame perfect equilibrium. Proposer power is closer to equilibrium predictions in treatments with pre-proposal communication.*

We now turn to investigate if the proposers' demanded shares are in line with the theoretical benchmark as predicted by the parameters of the game. For this purpose we will conduct a structural estimation. Rearranging equation (1) and taking natural logarithms of both sides, we obtain

$$\ln(1 - propshare) = \ln(\delta) + \ln\left(\frac{group\,size - 1}{2 \times\ group\,size}\right).$$

clustering at the study level and having only subject random effects.

Table 2: Average Proposer Demanded Share and Voter Shares, by Proposal Type

Panel A: Proposer's Share					
	No Communication			Communication	
Group size:	3	5	7	3	5
$\delta = 0.5$	0.50 [0.83] (0.006)		0.26 [0.78] (0.010)		
$\delta = 0.67$	0.56 [0.78] (0.014)				
$\delta = 0.8$		0.42 [0.68] (0.009)			0.56 [0.68] (0.009)
$\delta = 0.9$	0.54 [0.7] (0.010)				
$\delta = 0.95$	0.49 [0.68] (0.012)				
$\delta = 1$	0.52 [0.67] (0.004)	0.40 [0.60] (0.008)		0.61 [0.67] (0.010)	
All δ	0.52 (0.003)	0.41 (0.006)	0.26 (0.010)	0.61 (0.010)	0.56 (0.009)
Panel B: Included Voter's Mean Share					
	No Communication			Communication	
Group size:	3	5	7	3	5
$\delta = 0.5$	0.38 [0.17] (0.005)		0.18 [0.07] (0.004)		
$\delta = 0.67$	0.39 [0.22] (0.010)				
$\delta = 0.8$		0.23 [0.16] (0.005)			0.20 [0.16] (0.002)
$\delta = 0.9$	0.41 [0.3] (0.010)				
$\delta = 0.95$	0.42 [0.32] (0.009)				
$\delta = 1$	0.41 [0.33] (0.004)	0.28 [0.20] (0.005)		0.38 [0.33] (0.008)	
All δ	0.40 0.003)	0.25 (0.004)	0.18 (0.004)	0.38 (0.007)	0.20 (0.002)

Only round 1 proposals in games 6-10. Standard errors of the mean in parentheses.
Mean voter's share is conditional on inclusion in the coalition.
SSPE predicted share in brackets next to the mean observed value.

To test whether the theory is useful in explaining the data we estimate the following econometric model:

$$\ln(1 - propshare_{sit}) = \beta_0 + \beta_1 \ln(\delta_s) + \beta_2 \ln\left(\frac{group\,size_s - 1}{2 \times\; group\,size_s}\right) + \eta_i + \epsilon_{sit} \tag{2}$$

where η_i is the subject random effect and ϵ_{sit} is the error term.[8] Values of $\hat{\beta}_1$ and $\hat{\beta}_2$ equal to 1 and $\hat{\beta}_0 = 0$ would mean that the data approximates the theoretical predictions.

The results of our estimation are presented in Table 3. Our first regression includes all proposals regardless of game number or the round within a given game. However, there are two important aspects to highlight. We show in Section 4.5 that history matters for bargaining behavior following a rejection, which implies that proposals in further rounds may differ structurally from those in round 1. Hence, in the estimations in columns 4 and 5 we restrict our sample to proposals made in the first round to avoid confounds. Second, it has been widely documented in the literature that, as subjects gain experience, their behavior moves closer to equilibrium play.[9] Thus, we further divide our samples into period 1-5 and 6-10 to examine patterns of learning (still restricting to round 1) in order to avoid any confounds that may arise by pooling data for all games since experience can be correlated with treatment variables.

Next, we explore only proposals that received a majority vote in favor in order to investigate if accepted proposals conform closer to theoretical predictions (columns 6 and 7). Finally, we restrict attention to MWCs (columns 8 and 9).[10]

In each of the subsamples under question we reject that the SSPE is a good point predictor of proposer behavior. The coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ are significantly different from 1 and β_0 is significantly different from 0.

[8] This only holds for $\delta \in (0, 1]$, not for $\delta = 0$.

[9] See, for example, how the proposer's share increases with experience in Figure 3 of Fréchette et al. (2003), Figure 1 in both Agranov and Tergiman (2014) and Baranski and Kagel (2015), and Table 6 in Miller and Vanberg (2015).

[10] We are especially grateful to the anonymous referees who offered valuable insights regarding the sample selection for regression analysis.

Table 3: Structural Estimation of Proposer Behavior

	All			Round 1		Accepted		MWCs	
	All	Games 1-5	Games 6-10	Games 1-5	Games 6-10	Games 1-5	Games 6-10	Games 1-5	Games 6-10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	-0.914***	-0.879***	-0.955***	-0.874***	-0.953***	-0.833***	-0.872***	-0.974***	-0.990***
	(0.062)	(0.061)	(0.064)	(0.061)	(0.069)	(0.052)	(0.057)	(0.057)	(0.064)
$\ln(\delta)$	-0.116	-0.150*	-0.101	-0.156*	-0.111	-0.139*	-0.049	-0.130**	0.013
	(0.100)	(0.085)	(0.115)	(0.087)	(0.120)	(0.076)	(0.123)	(0.065)	(0.093)
$\ln\left(\frac{group\,size-1}{2\times group\,size}\right)$	0.141***	0.146***	0.152***	0.142***	0.145***	0.134***	0.124***	0.134***	0.133**
	(0.050)	(0.043)	(0.050)	(0.043)	(0.055)	(0.039)	(0.043)	(0.043)	(0.053)
var(Session)	0.097***	0.083***	0.101***	0.086***	0.108***	0.076***	0.103***	0.065***	0.097***
	(0.017)	(0.011)	(0.018)	(0.011)	(0.019)	(0.014)	(0.016)	(0.010)	(0.019)
var(Subject)	0.134***	0.141***	0.157***	0.142***	0.160***	0.146***	0.142***	0.136***	0.124***
	(0.008)	(0.013)	(0.018)	(0.011)	(0.016)	(0.011)	(0.012)	(0.025)	(0.017)
var(Residual)	0.212***	0.230***	0.177***	0.226***	0.161***	0.132***	0.122***	0.134***	0.124***
	(0.021)	(0.026)	(0.017)	(0.025)	(0.019)	(0.009)	(0.012)	(0.004)	(0.010)
N	6481	2880	2803	2234	2206	1146	1121	1073	1447
χ^2	10.37	18.06	11.18	16.81	8.71	18.31	8.37	13.31	6.22

Standard errors in parentheses clustered by study.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2 further sheds light on why the SSPE does not predict well the proposer's mean share. Note that as the discount factor varies, the proposer's share for groups of 3 is rather stable around 52 percent of the total pie. In groups of 5, the mean share also appears to be unaffected by the experimentally-induced level of impatience. Thus, in the bottom row of each panel of Table 2 we compute the mean proposer's share pooling over all values of δ. Note that we have focused on games without communication because δ does not vary within group size for communication treatments, thus we cannot test the prediction.[11]

We now turn to investigate the role of experience on the proposer's demanded share. Figure 1 shows the evolution of the proposer share over ten games. The line connected by diamonds pools all proposals, while the line connected by x's is for MWC proposals. Note that when pooling all proposals in groups of 3 and 5, absent communication, the proposer's demanded share appears to grow with experience but not when restricting attention to MWCs. This is driven by the fact that MWCs are growing over time and the proposer's share is larger in such splits, an aspect we explore in Subsection 4.2. With communication, however, we find a rapid growth in the proposer's share also within MWCs.

In what follows, we will estimate the effect of different variables on the proposer's share. First, we seek to confirm (or not) the robustness of the previous results: that the proposer's share grows with experience and is higher when pre-proposal communication is allowed. We also investigate if there is a significant interaction between the discount factor and experience and between communication and experience.

Second, we wish to understand if subjects with higher voting weights make larger claims of the surplus, that is, if an *illusion of power* exists. For this purpose we have computed the voting weight for each subject in each bargaining game, defined as the proportion of votes held by a particular subject.[12] This variable is only included for groups of 3 because experiments with groups of 5 are all symmetric.

[11] In further regression analysis that we conduct below, we find that discounting starts to have the equilibrium predicted effect on behavior as subjects gain experience.

[12] Recall that, in our sample of analysis, some experiments have asymmetric weights yet all predict symmetric expected values, thus the proposer's share should be independent of them according to the SSPE.

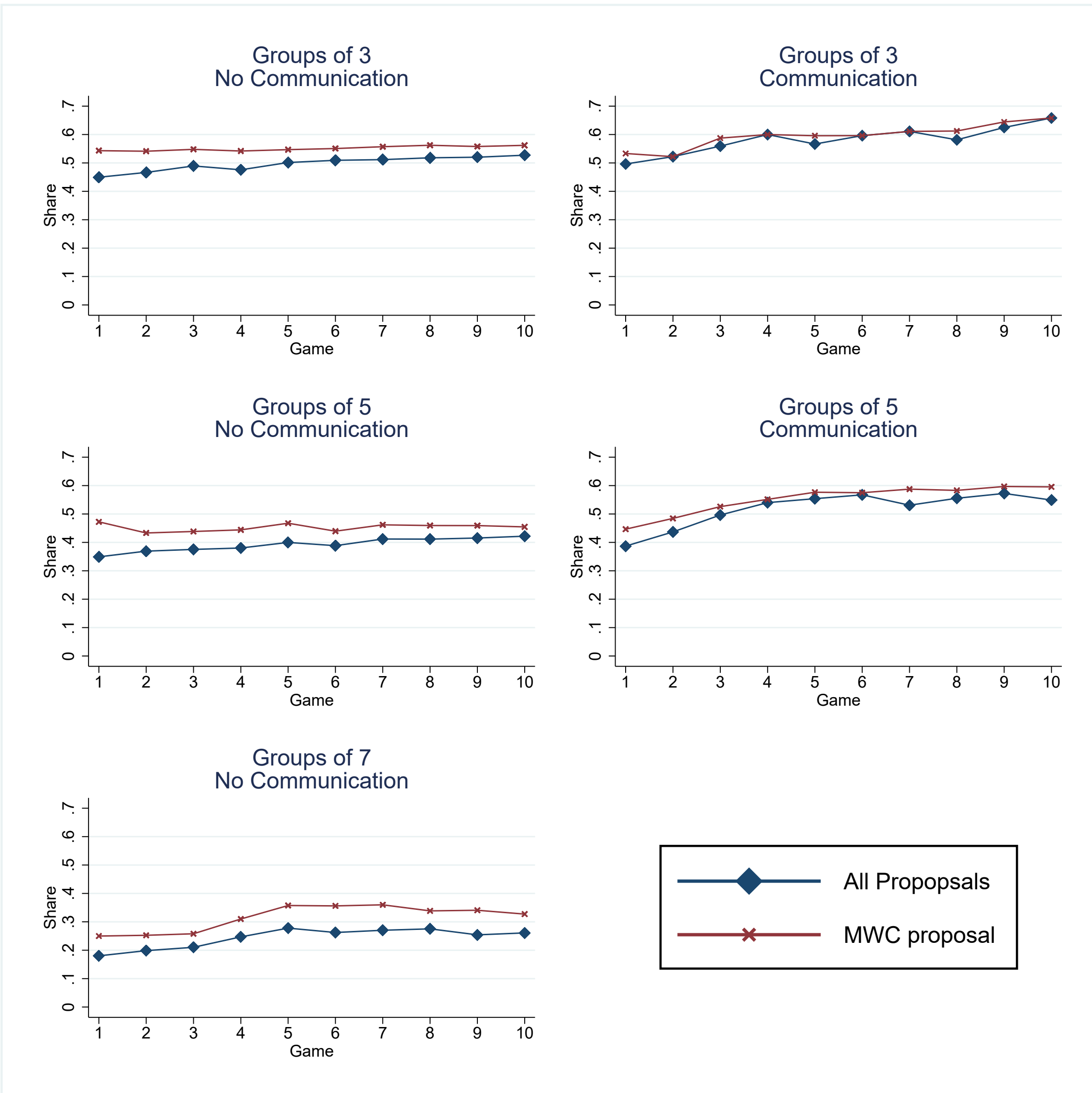


Figure 1: Evolution of the Proposer's Demanded Share, by Group Size and Communication

Table 4: Behavioral Determinants of the Proposer's Demanded Share, by Group Size.

	Groups of 3			Groups of 5			Groups of 7
	No Comm. (1)	Comm. (2)	Pooled (3)	No Comm. (4)	Comm. (5)	Pooled (6)	(7)
Constant	0.275***	0.479***	0.275***	-0.073	0.337***	-0.093	0.140***
	(0.037)	(0.027)	(0.036)	(0.242)	(0.025)	(0.240)	(0.022)
δ	0.047		0.048	0.476*		0.480*	
	(0.030)		(0.030)	(0.277)		(0.275)	
Game	0.026***	0.025**	0.026***	0.075***	0.062***	0.084***	0.036***
	(0.005)	(0.011)	(0.005)	(0.012)	(0.010)	(0.012)	(0.006)
Game ^{2}	-0.001***	-0.001	-0.001***	-0.000	-0.004***	-0.001***	-0.002***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Communication			0.050			0.064***	
			(0.048)			(0.021)	
Voting Weight	0.339***		0.338***				
	(0.065)		(0.065)				
Game $\times\delta$	-0.007**		-0.007**	-0.073***		-0.073***	
	(0.003)		(0.003)	(0.013)		(0.012)	
Game $\times$ Communication			0.008*			0.003	
			(0.005)			(0.003)	
Game $\times$ Voting Weight	-0.015*		-0.015*				
	(0.009)		(0.009)				
N	3138	80	3218	750	220	970	280
χ^2	223.13	37.73	243.17	61.14	69.77	148.74	57.10

Standard errors in parentheses. Regression includes subject and study random effects. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$
See Online Appendix for estimation results with clustered standard errors the study level.

Our estimation results in Table 4 robustly confirm the significance of experience and communication. The share demanded by proposers grows as subjects play the game. Importantly, it grows at a decreasing rate as the negative coefficient for $Game^2$ reveals. Communication also significantly increases the proposer's share and the rate at which it grows with experience.

Previously, we had noted how the mean proposer's share appeared to be unaffected by the discount factor. While theory predicts the effect should be negative, our regression analysis yields a positive coefficient for δ (only significant at the 10% level in groups of 5). Importantly, given that the interaction between Game and δ is negative and significant in both groups of 3 and 5, we find that the discount factor starts to have the equilibrium predicted effect after seven repetitions of the game.[13]

We find evidence supporting an initial *illusion of power*: proposers' demands increase in their nominal voting weight. This is a reasonable expectation given our knowledge on anchoring effects (Tversky and Kahneman, 1974) and equity theory (Adams, 1965). In bargaining games, voting weights may well serve as a starting point for strategizing or reasoning about the game and can also create entitlements. Nonetheless, one should note that the illusion partially dissipates with experience in the game. Assuming that learning about the strategic irrelevance of nominal voting weights is linear, it would take subjects approximately 21 repetitions for the illusion to fade according to our estimates.

Finding 2. *The proposer's share fails to be determined by the discount factor and group size in the manner predicted by the stationary equilibrium. Proposers with higher voting weights demand larger shares, despite having no real differences in bargaining power. However, behavior moves closer to equilibrium as subjects gain experience in the game.*

[13] The net effect of δ on the proposer's share becomes significantly negative after period 13 in groups of 3 and period 11 in groups of 5 (Wald tests yield p=0.083 and p=0.078, respectively).

4.2 Minimum Winning Coalitions

According to the SSPE, only the minimum required number of members (including the proposer) should be offered a positive share and vote in favor. We find this to be the case for 62.1 percent of all proposals, offering moderate support for the theory.[14] Importantly, communication significantly increases the chances of MWCs to arise (see column 3 and 6 of the regression in Table 7) as these represent 86 percent of proposals with communication while only 64 percent without it.

There are no significant differences in the proportion of MWCs between groups of 3 and 5 (pooling all treatments). However, focusing on games with communication, the proportion of MWCs is slightly higher in groups of 3 than in groups of 5 (0.94 vs. 0.84, p=0.056 obtained from an OLS regression clustering at the subject level). Groups of 7 appear to have a lower proportion of MWCs. However, since the data comes from only one study in each case discussed in the paragraph, we do not believe this is strong evidence of a violation of the SSPE.

Finding 3. *Minimum winning coalitions are the modal proposed split of the fund, in line with the stationary equilibrium. Communication increases their likelihood.*

Figure 2 shows the evolution of MWCs, all-way splits, and equal splits over the course of ten games.[15] The first two panels clearly show that MWCs increase as subjects gain experience. Pooling all treatments, MWCs represent only 33 percent of proposals in game 1, but more than double by game 10, reaching 69.3 percent.

To investigate the effect of discounting and voting weights, and further corroborate the statistical significance of experience and communication on MWCs, we proceed to estimate a random effects probit model. The marginal effects are reported in Appendix Table 7.

Our first finding is that the discount factor affects the prevalence of MWCs, contrary to

[14] It is 60.1 percent of all round 1 proposals, 60.2 of accepted proposals.

[15] An *equal split* is defined as a division of the pie in which each member's share is within 5 percentage points of $\frac{1}{\text{Group Size}}$.

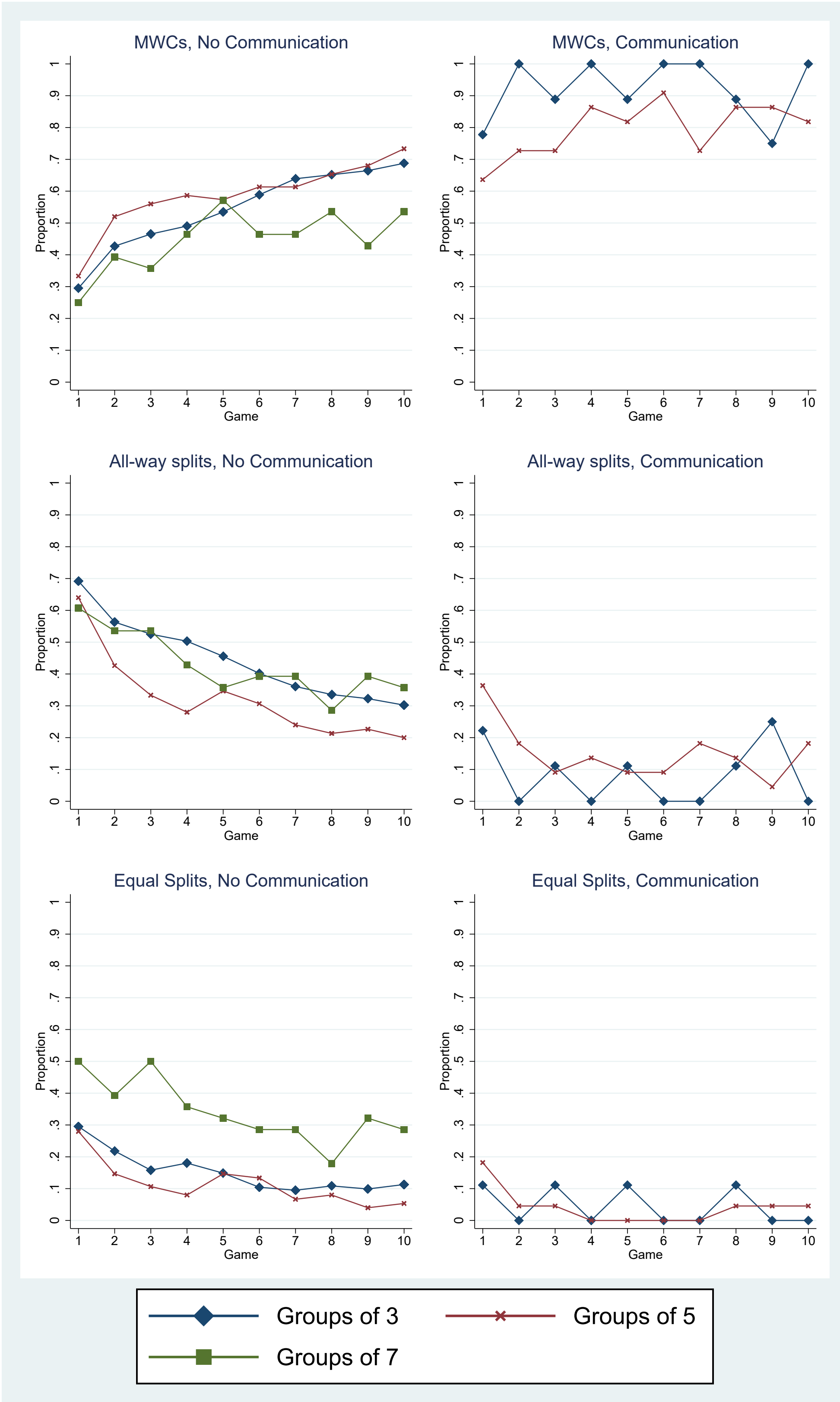


Figure 2: Minimum Winning Coalitions, All-way Splits, and Equal Splits

the equilibrium prediction that it should have no effect. In groups of 3, a 10 percentage point increase in δ is associated with a 4.3 percentage point increase in the likelihood of subjects proposing a MWC. The effect is larger in groups of 5, 16.8 percentage points. This finding is robust to focusing on games 6 to 10 only. One reason this may happen is because proposers could fear rejection more as δ falls since the efficiency loss is higher. As such, proposers would be willing to offer more inclusive splits in order to increase odds of approval.

Turning to the effect of voting weights, we find that subjects whose voting weights are less than 1/3 (in groups of 3) are less likely to propose MWCs than those whose weight is above 1/3 (71 vs 82 percent). However, the estimated marginal effect is not significant.

Finding 4. *Minimum winning coalitions increase with experience and are more likely as the cost of delay decreases.*

4.3 Delay

How likely are groups to delay reaching an agreement beyond the first round of bargaining? According to the SSPE, all proposals should pass in round 1 regardless of group size and discount factor. In line with the prediction, we find that 79.6 percent of all bargaining games end in round 1 pooling over all ten games.

Finding 5. *Delay in reaching an agreement is uncommon. 80 percent of proposals are accepted in the first round, in line with the stationary equilibrium predictions.*

As we have conjectured earlier based on previous evidence (Miller and Vanberg, 2015), larger groups may be more likely to experience delay. Also, the likelihood of delay may be correlated negatively with how costly it is to turn down a proposal. To study the determinants of bargaining disagreement, we conduct a probit regression for delay as a function of group size and δ.[16] Furthermore, we control for experience, communication possibilities,

[16]For consistency with our previous estimations, we initially conducted a mixed effects probit regression with study, but we reject this specification in favor of the OLS model (L.R. test, p-value>0.1). The variance estimates were not significant at conventional levels and the intra-class correlation coefficients were below 5 percent in each cluster level.

and interact each variable with the game of play. In our regression, the unit of observation is a bargaining group in round 1 for games 1-10. Standard errors are clustered at the study level (results are robust to clustering at the session level). The average marginal effects are reported in Appendix Table 8.

As we can see from the marginal predictions in panel A of Figure 3, groups of 7 have significantly higher delays than groups of 3 and 5, yet we find no differences in delay between the latter two. While we confirm the significant difference between groups of 3 and 7 reported in Miller and Vanberg (2015), this result should be interpreted with caution as there is only one study with groups of 7 and the observations come from 28 subjects only.

While this difference can be thought of as a treatment effect, it may have a more "statistical" explanation in the sense that a single "no" vote suffices to impede immediate agreement when the allocation is a MWC. Thus, the larger the group size, the larger the number of members within a MWC, which entails a larger probability of facing at least one vote against. A similar argument has been set forth by Miller and Vanberg (2015) to partially account for the increased failure rates when unanimous voting is required.

Does discounting affect the probability of delay? We find that the lower the costs of delay are (Panel B of Figure 3), the more likely it is that bargaining will extend beyond the first round. While this finding matches one's intuition, it is not predicted by the SSPE.

Agranov and Tergiman (2014) and Baranski and Kagel (2015) have reported that communication reduces the likelihood of delay, although both studies failed to achieve significance at the 5 percent, level or better. In our pooled data, we are able to confirm the significance of this effect ($p < 0.01$) which accounts for an 11.9 percentage point difference. In panels A and B of figure 3 we have graphed the marginal effects for group size and δ with and without communication.

Finally, we find weak evidence that delay rates increase with experience (marginal effect of 0.005). One would have expected a larger effect since the proposer's mean share increases, leaving less funds to *buy* the necessary votes. However, minimal winning coalitions also

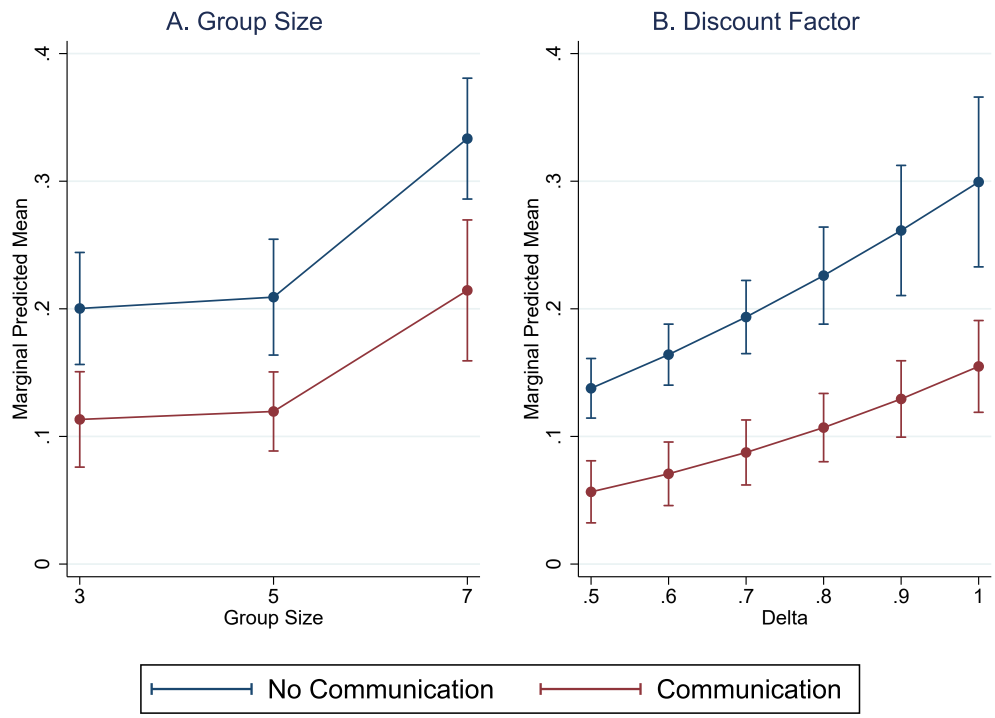


Figure 3: Marginal Probability of Delay, Games 6 to 10

become more prevalent, which leaves more pie to divide per included member and this effect may counteract the proposer's enhanced demands. In our upcoming voting section we explore how voters value their own share and how the proposer's demand affects their willingness to support a proposal.

Finding 6. *Delay in reaching an agreement increases in group size, decreases in the cost of delay and when communication is possible, and is unaffected by experience in the game.*

4.4 Voting

It has been reported throughout the different studies that the main determinant of voting is a player's own share (which we confirm). However, there is lack of clarity concerning other factors that may play a role in voting decisions such as the proposer's share and whether or not the allocation is a MWC. Differences in results may be due to the fact studies do not report comparable voting regressions and/or because studies can be underpowered to identify true effects. Some studies control for experience, others restrict attention to members included in the proposal, and estimation techniques to control for correlation of observations tend to differ. Importantly, most studies do not vary the discount factor or the group size, hence cannot report on the effect of these parameters on voting.

Table 5: The determinants of voting: Linear probability regressions

	Groups of 3		Groups of 5		Groups of 7
	No Comm. (1)	Comm. (2)	No Comm. (3)	Comm. (4)	(5)
Constant	0.527***	0.553	0.775***	-0.036	0.434***
	(0.083)	(0.376)	(0.198)	(0.066)	(0.062)
Own Share	0.018***	0.020***	0.028***	0.044***	0.001
	(0.000)	(0.002)	(0.001)	(0.001)	(0.002)
Proposer's Share'	-0.004***	-0.007	-0.000	0.001	0.005***
	(0.001)	(0.009)	(0.001)	(0.002)	(0.002)
δ	-0.293***		-0.722***		
	(0.052)		(0.219)		
Voting Weight	-0.217				
	(0.160)				
MWC (=1 if yes)	0.004	-0.121	-0.083**	0.001	0.003
	(0.021)	(0.329)	(0.042)	(0.056)	(0.045)
N	1466	70	700	440	840
χ^2	2061.12	127.69	830.24	2029.73	13.24

Standard errors in parentheses. Study level and subject level random effects included. Only games 6 to 10. Similar results hold when pooling all games.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In Table 5 we present the results from a linear probability regression of the voting decision on own share (as a percentage of the total fund), the proposer's share, the discount factor, the voting weight of the subject in question, and we control for whether the proposal is a MWC or not.[17] Our analysis is separated by group size and communication possibilities.

[17] The reason we estimate OLS regressions is because we were unable to estimate probit regression models for groups of 5 due to lack of convergence in the maximization of the likelihood function. We attempted several integration methods and parameters, but could not obtain convergence. Probit model estimations for all other cases lead to similar conclusions to those obtained from the linear probability models shown here.

First and foremost, a subject's main determinant of voting is her own share, which is positive in all samples and significant for groups of 3 and 5. In groups of 3 without communication, an extra percentage point of the total fund offered to a member increases her probability of voting in favor by 1.8 percentage points, and by 2.8 percentage points in groups of 5. In both group sizes, the effect of own share appears to be stronger when communication is possible.

The effect of the proposer's share on voting has been either unreported or mixed in the current literature. In groups of 3, we find that subjects are less likely to vote in favor as the proposer's share increases in treatments without communication, but when subjects can communicate, the effect is no longer significant. In groups of 5, the proposer's share does not appear to influence the voting decision. Finally, in groups of 7, the coefficient is positive and significant. Thus, our results are inconclusive as well, but we should note that the estimates in columns (2), (4), and (5) are derived from a single study in each subsample. As such, focusing on groups of 3 and 5 without communication (columns 1 and 3), which encompass 10 studies, we can conclude that the effect is negative, but only significant in groups of 3. In these, an additional percentage point of the total fund that the proposer keeps reduces the probability of voting in favor by 0.4 percentage points.

The discount factor has a negative impact on voting: the more patient subjects are, the less likely they are to vote in favor. Note that in a SSPE, players should vote in favor for any share greater than or equal to $\frac{\delta}{\text{group size}}$. As such, the negative coefficient for δ provides evidence in favor of the SSPE and is in line with the findings by Fréchette and Vespa (2017). Recall that, proposals are made by computers and human subjects in their study. Thus, we confirm that the effect of discounting on voting is not driven by the presence of computer generated proposals. An important observation is that the relationship between the cost of delay and voting in favor explains at the individual decision level our previous finding that delay is more likely to occur as the discount factor increases.

In the estimations in Table 5, we find no evidence that nominal voting weights affect voting decisions when subjects have gained experience. Conducting the same regression for games 1-5 to investigate if subjects suffer from an *illusion of power* early in the experimental sessions, we find that the effect is significant (coef. -0.329 for groups of 3 without communication, $p<0.05$). Thus, our finding that the effect of nominal voting weights vanishes with experience is also reported in the recent work by Maaser et al. (2019) who implement asymmetric weights in groups of 5.

Concerning communication, we repeated the estimations in Table 5 for each group size pooling all treatments and find that it has a positive effect on voting. Subjects are unconditionally more willing to vote in favor when they can communicate: 13 percent more likely in groups of 3 and 9.4 percent more likely in groups of 5 ($p< 0.01$). Such effect may be the result of the culmination of a verbal negotiation process in which respondents expressed their agreement. The evidence from chat transcripts described in Agranov and Tergiman (2014) and Baranski and Kagel (2015), is consistent with proposers being able to extract larger shares because they either pit voters against each other or voters explicitly request that other members be excluded from the coalition. While this seems to be in line with the strategic explanation, one may be tempted to conclude that communication leads to behavior that is closer to the SSPE. As we show in subsection 4.5 on off-equilibrium behavior, retaliation is largely at play in treatments with communication as well. Hence, while proposals may be closer to the SSPE prediction, the strategies by which subjects abide are not.

Our results regarding the effect of whether or not the allocation is a MWC remain unclear. Excluding this dummy variable from our analysis does not affect the significance of our estimations and previously reported results. We repeated the estimations presented in Table 5 conditioning on the sample of MWCs, and our qualitative results remain unaffected.

Finding 7. *The probability of voting in favor of a proposal is positively correlated with own share, the cost of delay, and the possibility to communicate. It is negatively correlated with the proposer's share.*

4.5 Disagreement paths: History-dependent behavior

For the most part, the literature has not studied in detail how previous bargaining rounds affect future bargaining behavior within a committee. Given that the stationary equilibrium prediction hinges on history independence, it becomes crucial to investigate whether subjects actually behave in such way.

In this section we will first calculate the *empirical continuation value* in a similar fashion as Bradfield and Kagel (2015) do in their study. Next, we compute a measure of retaliation against the previous proposer by weighing the offered shares by previous non-proposers and comparing what non-proposers share among themselves. Third, we further explore how one's previous voting decision affects next-round proposal behavior. Finally, we look into whether or not supporters of a failed proposal are punished by those who voted against and if the first proposer is loyal to those who supported her in round 1.

In what follows, we focus on games 6 to 10 in treatments without communication.[18] We did not find significant differences between games 1 to 5 and 6 to 10. If anything, our results are stronger in later games.

4.5.1 Empirical Continuation Values

We will compute the empirical continuation value for members who proposed a failed allocation and for non-proposers too. To do so, we weigh all round 2 proposals within a committee by their probability of being up for a vote, and then calculate the mean share offered to the previous proposer and to non-proposers. The results, presented in Table 6, are quite clear: round 1 proposers face a lower continuation value than non-proposers. In groups of 3, the difference is 8 percentage points ($p= 0.005$), and in groups of 5, it is 5 percentage points ($p= 0.108$).[19]

[18] In treatments with communication, there are only 6 observations in groups of 3 and 9 observations in groups of 5 thus we cannot conduct a separate analysis.

[19] The p-values are obtained from linear regressions clustering at the study level controlling for experience.

Table 6: Empirical Continuation Values as Proportion of Total Fund

	Groups of 3	Groups of 5
Round 1 Proposer	0.28	0.16
	(0.010)	(0.008)
Round 1 Non-Proposer	0.36	0.21
	(0.005)	(0.002)

Std. err. in parentheses are clustered at study level.

4.5.2 Round 1 non-proposers' offers

Note that in the preceding exercise we have included in the calculation of the continuation value what round 1 proposers offer themselves in round 2. We now focus only on round 2 proposals emanating from players that were not selected as proposers in round 1. For each subject we identify the share offered to the previous proposer and the mean share offered to members that did not propose in round 1. We exclude the share that subjects demand for themselves in order to not exaggerate the differences that will mechanically arise from proposer advantage. As shown in Figure 4, the mean share offered to round 1 proposers by previous non-proposers is 15 percent of the fund in groups of 3, which is less than half of what previous non-proposers share among themselves. A similar but less pronounced pattern is observed in groups of 5. We next discuss a possible reason behind the reduced effect in groups of 5.

4.5.3 Retaliation by those who did not support the proposal

There is a key distinction between groups of 3 and 5 in how a rejection may have occurred. In groups of 3, both non-proposers in the group must have voted against the proposal in round 1, because one favorable vote suffices for approval (besides the proposer). But this is not the case in groups of 5 where one member may have voted in favor but three others did not. Hence, it is reasonable to expect that those who supported a previous proposal need not *punish* the proposer, which we corroborate in the data. Figure 5 shows that supporters and

Figure 4: Shares offered in Round 2 by Subjects that did not Propose in Round 1 (by recipient and group size)

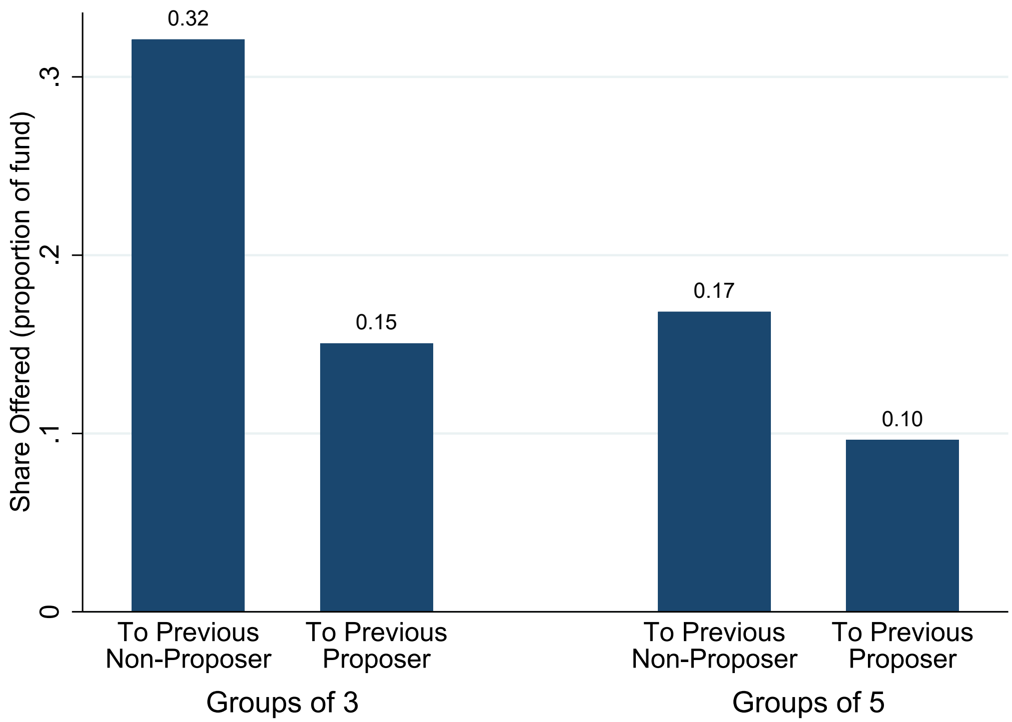
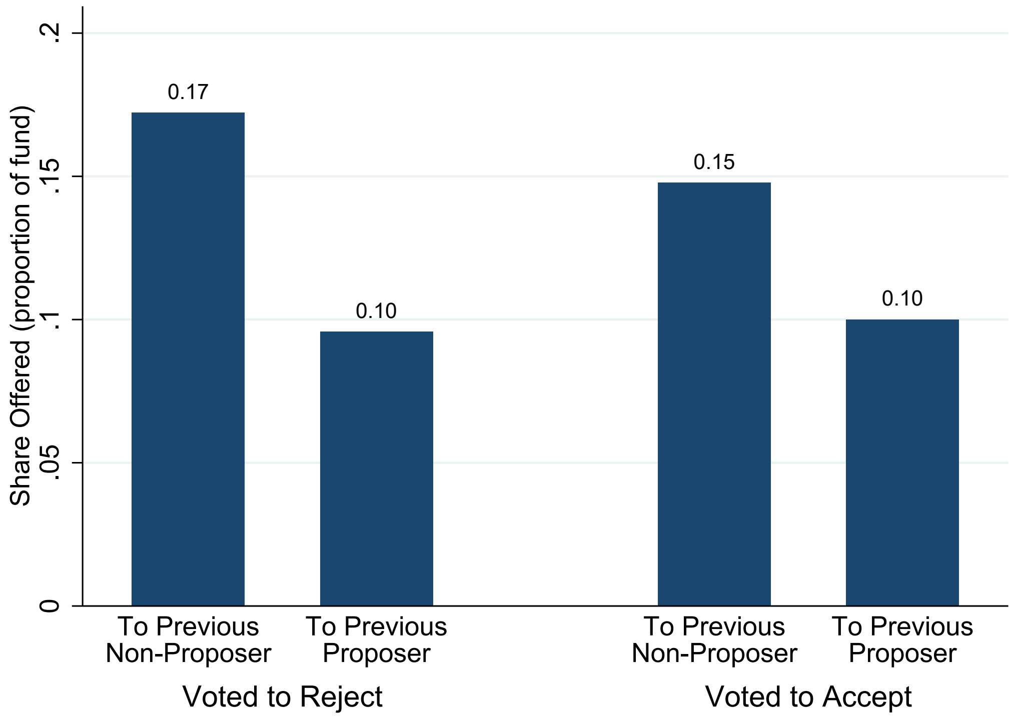


Figure 5: Shares offered in Round 2 by Subjects that did not Propose in Round 1 for Groups of 5, by Voting Decision



non-supporters offer higher shares to non-proposers than to round 1 proposers (p= 0.051). Hence, we do not find evidence of favorable treatment towards previous proposers by their round 1 supporters.[20]

Finding 8. *Proposing behavior following a disagreement displays strong history-dependence and, as such, is inconsistent with stationary strategies. Subjects typically offer lower amounts to the previous proposer than what they offer to other members. This violation of stationarity does not fade with experience.*

[20]A similar result holds when focusing on whether a member is included or excluded from the coalition. See Figure 1 in the Online Appendix.

5 Discussion and Concluding Remarks

The results from our meta-analysis provide qualitative support for the main equilibrium outcome predictions, especially as subjects gain experience. As such, the SSPE is useful in organizing the data, i.e., moderate proposer power exists, most proposals assign positive shares of the funds to an MWC, and the vast majority of groups reach an agreement in the first round.

Subjects' main determinant of voting in favor is their own share, and they are more willing to vote in favor as the delay cost increases. As a corollary, we document a second behavioral regularity unpredicted by the SSPE. Namely, a round-one agreement becomes more likely as the cost of delay increases.

A widely documented finding is that experience in the game has an important effect on outcomes; the proposer's share increases and so does the prevalence of MWCs. An issue that we examined is if the proposer's share within MWCs was also growing with experience, but we find that it remains rather flat. This means that the previously reported growth in proposer power is mainly driven by the increase in MWCs and corresponding decrease in all-way splits. Importantly, allowing for pre-proposal communication leads to an increase in the proposer's mean share within MWCs as subjects gain experience.

Several attempts have been made to reconcile the gap between the theory and experimental results with respect to the modest proposer power observed. For example, Montero (2007) keenly solves the SSPE with other-regarding preferences such as those specified by Fehr and Schmidt (1999) or Bolton and Ockenfels (2000). Under reasonable parameters, the SSPE predicts even a larger share for the proposer, which paradoxically increases inequality compared to the standard case. A similar result is obtained when risk aversion is incorporated into players' utility functions (Harrington, 1990). As such, this reasonable assumption about preferences cannot alone explain the low proposer power.

The result in Montero (2007) relies on the fact that inequality-averse voters would be willing to accept a share lower than the discounted continuation value of the game with standard preferences (under the SSPE). This happens because they dislike the disadvantageous inequality that would arise from the possibility of being left out of the MWC in a future round. We have clear evidence that subjects generally reject offers close to the theoretical prediction (which are not common), so they would certainly reject even lower offers. However, the result in Montero does not imply that subjects do not have other-regarding preferences because it can be that they are either solving a simpler game in their minds or not relying on stationary strategies. In the first case, if subjects are myopic and only focus on the current round of play, equal splits within an MWC would be supported under reasonable parameters of the Fehr and Schmidt (1999) inequality averse utility function (see Online Appendix Section 4). In the second case, if subjects abide by non-stationary strategies, we know that the set of equilibria is large and the question arises regarding which proposals will be coordinated on. Thus, it is possible that social preferences dictate which allocation is selected as a subgame perfect equilibrium.

Nunnari and Zapal (2016) present an elegant model in which players have biased assessments of their chance of proposing. Conditional on not being the current proposer, players incorrectly believe their odds of proposing the next round are higher (i.e. experience the gambler's fallacy). Theoretically, this predicts higher continuation values for non-proposers, which leads to reduced proposer power. Additionally, Nunnari and Zapal compute quantal response equilibrium voting strategies and find that, combined with the gambler's fallacy in the proposer recognition process, one may simulate behavior that generates a fraction of proposals having more than minimal coalition partners (in addition to moderate proposer power). According to our calculated empirical continuation value in groups of 3, we would require the proposer in round 1 to believe that her odds of being selected again are close to 0 in order to justify equal sharing within the coalition.

Our findings suggest that a primary source of the divergence between data and SSPE theoretical predictions is the assumption that players are using history-independent strategies.

We find that retaliation towards failed proposers is largely at play, as they face lower continuation values and lower odds of being included in a coalition compared to non-proposers. Thus, the stationarity refinement is inaccurate and should be disposed of when theorizing if one seeks to realistically capture bargaining dynamics.

Retaliatory behavior may help explain the lower-than-predicted proposer power. If subjects rationally anticipate it, they may demand lower shares when proposing in order to disburse larger shares to voters, and thus, increase approval odds. One way to test this conjecture is by designing an experimental treatment in which subjects' identities are not known, such as in the study by Fréchette and Vespa (2017). We have excluded this article from our analysis because proposals are also be made by the computer in their treatments, which may obscure the comparison with the standard treatments included here. Exploring how the availability of information on which to condition punishment strategies affects bargaining outcomes is a topic we seek to address in future work.

The model of Baron and Ferejohn is one of complete information and there is no rationale for disagreements to arise (unless these are mistakes (Nunnari and Zapal, 2016)). Nonetheless, there is indirect evidence that information asymmetries may be present. For example, the results from treatments where communication is possible show that disagreement rates are reduced compared to treatments absent communication. Subjects tend to communicate their willingness to accept and there are differences between subjects in this regard. Importantly, we have interpreted behavior following a rejection as retaliatory, but it may also be that subjects have *learned* that failed proposers have a very high acceptance threshold and, thus, choose to partner with *cheaper* members (in expectation). We are unaware of a model with preference uncertainty in multilateral bargaining that could aid in organizing the data. Moreover, differences in stated reservation shares from voters may also arise from variation in cognitive abilities or levels of reasoning. Future theoretical work in this area may enhance our understanding of bargaining behavior.

While the present comprehensive analysis is valuable for consolidating our knowledge on the determinants of bargaining, it also has some weaknesses. For example, we have restricted our attention to the first ten games of play because this is the minimum number of games played in all studies of our sample. However, behavior appears to continue evolving beyond the tenth game. Conclusion 9 of Fréchette et al. (2005b) reports that with more experience, there starts to be a more noticeable effect of discounting on the proposer's share. That result is based on 20 games of experience, two times as many games as are considered here. Evidence from Maaser et al. (2019) reveals that voting weights also cease to affect voting and proposing behavior in later games. Thus, one practical application for future experimental design is to allow for ample repetition for learning to occur.

It is our hope that future theoretical and experimental work can help us further our understanding of the stylized results and open questions reported in this meta-analysis.

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A Additional Tables

Table 7: Multi-level Random Effects Probit for Minimum Winning Coalitions, Marginal Effects

	Groups of 3			Groups of 5			Groups of 7
	No Comm. (1)	Comm. (2)	Pooled (3)	No Comm. (4)	Comm. (5)	Pooled (6)	(7)
δ	0.453***		0.446***	1.642**		1.680**	
	(0.088)		(0.086)	(0.722)		(0.801)	
Game	0.039***	0.013	0.039***	0.030***	0.018*	0.028***	0.018**
	(0.002)	(0.013)	(0.002)	(0.006)	(0.010)	(0.005)	(0.008)
Voting Weight	0.184		0.181				
	(0.170)		(0.168)				
Communication		0.000	0.336***		0.000	0.145***	
		(.)	(0.065)		(.)	(0.050)	
N	3138	80	3218	750	220	970	280

Marginal effects, estimated coefficients reported in Online Appendix.
* p<0.1, ** p<0.05, *** p<0.01

Table 8: Probit for Agreement Delay

	Marginal Effect	Std. Error
δ	0.295***	(0.070)
Group size=5	0.008	(0.028)
Group size=7	0.125***	(0.021)
Communication	-0.119***	(0.018)
Game	0.005*	(0.003)
N	2402	

Coefficients reported in Online Appendix.
Standard errors clustered by study.
* p<0.1, ** p<0.05, *** p<0.01

Online Appendix for *The Determinants of Multilateral Bargaining: A Comprehensive Analysis of Baron and Ferejohn Majoritarian Bargaining Experiments* by Andrzej Baranski and Rebecca Morton.

1 Supporting Figures and Tables

Table 1: Structural Estimation of Proposer Behavior for Treatments without Communication

		Round 1			Accepted		MWCs	
	All	All	Games 1-5	Games 6-10	Games 1-5	Games 6-10	Games 1-5	Games 6-10
Constant	-0.929***	-0.927***	-0.888***	-0.969***	-0.839***	-0.886***	-0.998***	-1.011***
	(0.051)	(0.053)	(0.058)	(0.056)	(0.043)	(0.036)	(0.050)	(0.050)
$\ln(\delta)$	-0.082	-0.088	-0.128*	-0.070	-0.104*	-0.003	-0.113**	0.054
	(0.082)	(0.084)	(0.074)	(0.100)	(0.061)	(0.100)	(0.053)	(0.071)
$\ln\left(\frac{group\,size-1}{2\times group\,size}\right)$	0.172***	0.168***	0.168***	0.179***	0.158***	0.159***	0.162***	0.170***
	(0.033)	(0.034)	(0.031)	(0.036)	(0.024)	(0.023)	(0.032)	(0.037)
var(Session)	0.068***	0.071***	0.068***	0.079***	0.042***	0.070***	0.050***	0.068***
	(0.009)	(0.008)	(0.007)	(0.013)	(0.009)	(0.011)	(0.008)	(0.015)
var(Subject)	0.139***	0.141***	0.146***	0.162***	0.148***	0.139***	0.141***	0.130***
	(0.008)	(0.008)	(0.012)	(0.018)	(0.012)	(0.010)	(0.028)	(0.017)
var(Residual)	0.212***	0.205***	0.228***	0.163***	0.127***	0.122***	0.134***	0.126***
	(0.023)	(0.020)	(0.025)	(0.020)	(0.009)	(0.013)	(0.005)	(0.010)
N	6062	4802	2079	2061	991	977	949	1322
χ^2	31.95	27.86	39.87	28.27	56.76	49.52	27.74	21.19

Standard errors in parentheses clustered by study.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Behavioral Determinants of the Proposer's Share by Group Size, with Clustered Standard Errors by study

	Groups of 3		Groups of 5	
	No Comm. (1)	Pooled (2)	No Comm. (3)	Pooled (4)
Constant	0.262***	0.262***	0.072	0.050
	(0.055)	(0.054)	(0.156)	(0.150)
δ	0.074	0.074	0.330*	0.337*
	(0.054)	(0.053)	(0.170)	(0.172)
Game	0.026***	0.026***	0.075***	0.083***
	(0.004)	(0.004)	(0.006)	(0.011)
Game ^{2}	-0.001***	-0.001***	-0.000	-0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Communication		0.032		0.084**
		(0.020)		(0.034)
Voting Weight	0.351***	0.350***		
	(0.105)	(0.104)		
Game $\times \delta$	-0.007**	-0.007**	-0.072***	-0.073***
	(0.003)	(0.003)	(0.003)	(0.003)
Game $\times$ Communication		0.008***		0.003***
		(0.001)		(0.001)
Game $\times$ Voting Weight	-0.015***	-0.015***		
	(0.001)	(0.001)		
N	3138	3218	750	970
χ^2	1.37e+10	.	.	.

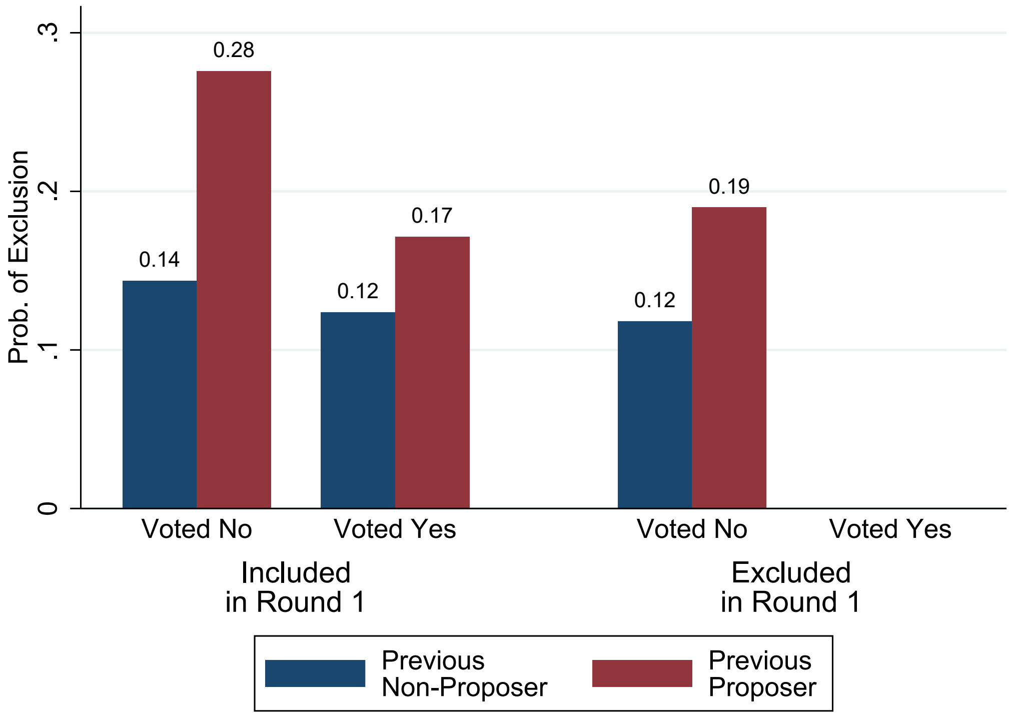
Standard errors in parentheses clustered at the study level. Regression includes subject random effects.
* p< 0.1, ** p< 0.05, *** p< 0.01
Notes: We do not estimate the model with clustering standard errors for communication in groups of 3 and 5, or for groups of 7 without communication because there is only one study in each case.

Table 3: Multi-level Random Effects Probit for Minimum Winning Coalitions

	Groups of 3			Groups of 5			Groups of 7
	No Comm. (1)	Comm. (2)	Pooled (3)	No Comm. (4)	Comm. (5)	Pooled (6)	(7)
Constant	-3.775***	1.234	-3.775***	-8.251**	0.243	-9.313**	-1.899***
	(0.633)	(1.397)	(0.630)	(4.064)	(0.459)	(4.210)	(0.604)
δ	2.786***		2.781***	8.525*		9.636**	
	(0.559)		(0.556)	(4.628)		(4.786)	
Game	0.438***	0.639	0.442***	0.276***	0.339*	0.518**	0.455***
	(0.086)	(0.508)	(0.085)	(0.103)	(0.197)	(0.243)	(0.175)
Game ^{2}	-0.016***	-0.053	-0.016***	-0.011	-0.024	-0.014*	-0.030**
	(0.004)	(0.048)	(0.004)	(0.009)	(0.018)	(0.008)	(0.015)
Communication			2.546***			1.227***	
			(0.741)			(0.397)	
Voting Weight	0.717		0.717				
	(1.124)		(1.125)				
Game $\times\delta$	-0.101*		-0.101*			-0.233	
	(0.054)		(0.054)			(0.243)	
Game $\times$ Communication			-0.093			-0.088	
			(0.105)			(0.057)	
Game $\times$ Voting Weight	0.035		0.035				
	(0.148)		(0.148)				
N	3138	80	3218	750	220	970	280
χ^2	298.00	1.86	311.15	43.45	5.20	54.25	13.23

Marginal effects reported in text.
* p<0.1, ** p<0.05, *** p<0.01.

Figure 1: Probability of Excluding in Round 2



Notes: Red bars indicate the probability that round 1 proposers are excluded in proposals made in round 2 by those who were not selected as proposers in round 1. Blue bars indicate the probability that round 1 non-proposers are excluded from proposals made in round 2 by those who were not selected to propose, excluding themselves. It is clear that round 1 proposers face higher odds of exclusion.

2 Asymmetric players: Shifting the balance of bargaining power

The first experiments in which subjects bargain to divide a fixed amount of wealth were Diermeier and Morton (2005) and Fréchette et al. (2005b) in finite and infinite bargaining horizons, respectively. Both studies sought to understand how changes in voting shares and proposer recognition probabilities affect bargaining outcomes and considered three-person committees. In the finite horizon case, players with higher recognition probabilities have higher expected payoffs, thus are always excluded from the winning coalition when not proposing. For the infinite case, all players have the same continuation values regardless of differences in recognition (this only holds in three-person games). These studies also varied voting shares making sure that no player could unilaterally implement a division of the fund and that all players could, at some point, be part of a winning coalition. In other words, care was taken to vary *nominal* bargaining power, but not *real* power.[1]

The articles report some shared findings: proposers keep larger shares on average, but quite below the equilibrium predictions and MWCs are modal while all-way splits quite rare.[2] Nevertheless, the studies differ in their findings regarding equilibrium mixing behavior of coalition partner choice. In a treatment with unequal recognition

[1]In the finite game, by varying the recognition probabilities, players had different continuation values.

[2]Diermeier and Morton (2005) report that in 42 percent of allocations in which all members receive a positive share are actually pittance coalitions since two members receive $22 each and give $1 to the their member. This seems to be an effect of the impossibility to divide the pie equally between coalition partners.

probabilities and unequal voting weights, Fréchette et al. (2005b) report that the proportion of observed coalitions matches the SSPE prediction quite close. On the other hand, Diermeier and Morton (2005) report for comparable treatments that "[t]he best account of the subjects' behavior is provided by a simple sharing rule where the proposer chooses any winning coalition and then distributes the pay-off equally among the coalition members. (pg. 224)".

Fréchette et al. (2005a) introduce a treatment in which one player (called apex player) holds a substantial voting weight such that real bargaining power shifts in her favor.[3] Equilibrium specifies that base players form a coalition with the Apex player less often than with other base players, but the opposite holds true in the data. Base players seek a to form coalitions with the apex player around 70 percent of the time, when theory predicts only 25 percent. This anomaly appears to happen because base players are able to keep 46.9 percent of the fund when they form an MWC with the apex player, but only 31.9 percent with other base players.

We now turn to studies where one player has veto power, meaning every coalition must include them to pass. Note, however, that veto players cannot unilaterally impose a division of the total fund. Theory predicts that veto players earn more than base players both as proposers and voters.[4] Two concurrent studies Drouvelis et al. (2010) (finite bargaining horizon no discounting) and Kagel et al. (2010) (infinite bargaining horizon with discounting) set out to identify how a veto player affects bargaining outcomes, not only for veto players, but also for base players. Both studies reach qualitatively similar conclusions: veto players receive larger shares than non-

[3]This paper compares the model of demand bargaining with BF.

[4]For the theoretical framework, see Winter (1996).

veto players but these are substantially below the theoretical predictions. Drouvelis et al. (2010) further report on a treatment where a fourth player with inferior voting shares to all existing players is introduced to the group such that the former veto player is not essential in every coalition (i.e. the weak players may pass a proposal by joining forces). In accordance with the theory, the authors find that former veto players earn lower payoffs on average, and former weak players earn higher shares.

Table 4: Predicted and Observed Percentage of the Total Fund[a], Delay, and Minimum Winning Coalitions in Treatments with Veto Players

	Drouvelis et al. 2010		Kagel et al. 2010 [b]	
	Predicted	Observed	Predicted	Observed
Shares (% of fund)				
Veto Player is Proposer	98	67	92.4	62
Veto Player not Proposer	98	61	79.8	52
MWC (% of Approved Proposals)	100	72	100	59
Delay (% of Approved Proposals)	0	49	0	46

[a] Conditional on approved allocation being a MWC.
[b] We report only the treatment with low cost of delay ($\delta = 0.95$).

3 Included and Excluded Studies and Treatments

We proceed study by study as mentioned in Table 1 in the main body to explain which treatments and sessions we have used in our data analysis.

1. McKelvey (1991): The experiments by McKelvey do not fit the description of the Baron and Ferejohn divide-the-dollar game. (Also, we do not have this data.)

2. Fréchette et al. (2003): We only use data from the treatment with the closed-amendment rule. We exclude the treatment in which the experimenters had one subject per group propose according to an algorithm in order to see if they could speed learning (Experiment 2 in their paper). Link to data.

3. Diermeier and Morton (2005): Treatments in this article are omitted because they all have asymmetric continuation values.

4. Fréchette et al. (2005a): We only use data from the treatment with equal weights and inexperienced subjects. The Apex treatment, and experienced subjects (those who had previous participated in the same experiment) are not included. Clearly, we do not use data from the demand bargaining game because it is another model. Link to data.

5. Fréchette et al. (2005b): we use the data for treatments with equal weights and equal proposer selection (EWES), Unequal Weights equal selection (UWES), and unequal weights and unequal selection. We do not use the data from sessions with experienced subjects. Link to data.

6. Diermeier and Gailmard (2006): We do not use any data because it is a single round bargaining game.

7. Drouvelis et al. (2010): We only use data from the symmetric treatment. We do not use the data from the *enlarged* or *veto* treatments. Data available from the authors.

8. Kagel et al. (2010): We use the data for the low cost ($\delta = 0.95$) and high cost ($\delta = 0.5$) of delay for the control treatment. Data from the veto treatments is not used. Data available from the authors.

9. Miller and Vanberg (2013): We only use the data for the majority rule treatments and do not use the data for unanimity. Data available from the authors.

10. Agranov and Tergiman (2014): We use the data for all treatments in this paper: chat, baseline, and baseline long. Data available from the authors.

11. Baranski and Kagel (2015): We use the data only for the treatment with *open door* communication because subject IDs remain fixed within a given bargaining game. In the other two treatments, ID's are shuffled so that subjects within a game cannot be identified. Link to data.

12. Bradfield and Kagel (2015): We use the data for the control treatment and do not use the data for teams. Data available from the authors.

13. Miller and Vanberg (2015): We use all the data in this article. We only have data for round 1 proposals. Data available from the authors.

14. Baranski (2016): All treatments are with endogenous production, hence we do not use the data from this paper.

15. Fréchette and Vespa (2017): In this paper some proposals are made by the computer and subjects cannot tell which ones. Thus we have not included the data in our analysis.

16. Miller et al. (2018): We only use the data from treatment 1 where all players have a symmetric disagreement value of 20. Furthermore, we only use the data for such treatment when it occurred in the first 10 games. Note that this experiment is as within subject design, so subject play with different disagreement values. We wanted to only use data from subjects with no experience, thus we only focus on those sessions in which the symmetric treatment was played in games 1-10. We do not use data from the unanimity treatments. Data available from the authors.

4 Fehr and Schmidt (1999) preferences with Myopic Players.

Consider a committee with n members and the majority voting rule $\frac{n+1}{2}$ and let $\mathbf{s} = (s_1, ..., s_n)$ denote a distribution of the fund where $s_i \geq 0$ and $\sum s_i = 1$ Assume that all players have the following preferences:

$$(1) \qquad u_i(\mathbf{s}) = s_i - \frac{\alpha}{n-1} \sum_{j \neq i} \max\{s_j - s_i, 0\} - \frac{\beta}{n-1} \sum_{j \neq i} \max\{s_i - s_j, 0\}$$

where $\beta \in [0, 1]$ and $\alpha > \beta$ per the assumptions in Fehr and Schmidt (1999). Recall that α is the parameter that captures distaste for advantageous inequality and β for disadvantageous inequality.

We now compare the utility levels of two different allocations:

(a) The equal split: $\mathbf{s^E} = (1/n, ..., 1/n)$

(b) The equal split within a MWC: $\mathbf{s^{EM}} = \left(\underbrace{\frac{2}{n+1}, ..., \frac{2}{n+1}}_{\frac{n+1}{2}\text{shares (majority)}}, \underbrace{0, ...0}_{\text{excluded}} \right)$

Lemma 1. $u(\mathbf{s^E}) < u(\mathbf{s^{ME}}) \iff \beta < \frac{n-1}{n}$.

Proof. Plugging in the allocations into the utility function, we obtain:

$$\begin{aligned} u(\mathbf{s^{EM}}) &= \frac{2}{n+1} - \frac{\alpha}{n-1} \cdot 0 - \frac{\beta}{n-1} \cdot \frac{2}{n+1} \cdot \frac{n-1}{2} \\ &= \frac{2}{n+1} \left(1 - \frac{\beta}{2} \right) \ . \end{aligned}$$

and

$$\begin{aligned} u(\mathbf{s^E}) &= \frac{1}{n} - \frac{\alpha}{n-1} \cdot 0 - \frac{\beta}{n-1} \cdot \frac{2}{n+1} \cdot 0 \\ &= \frac{1}{n} \ . \end{aligned}$$

Hence we have that:

$$
\begin{aligned}
\frac{2}{n+1}\left(1-\frac{\beta}{2}\right) > \frac{1}{n} &\iff \\
2-\beta > \frac{n+1}{n} &\iff \\
2-\frac{n+1}{n} > \beta &\iff \\
\frac{2n}{n}-\frac{n+1}{n} > \beta& \\
\frac{n-1}{n} > \beta& \;.
\end{aligned}
$$

$\square$

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