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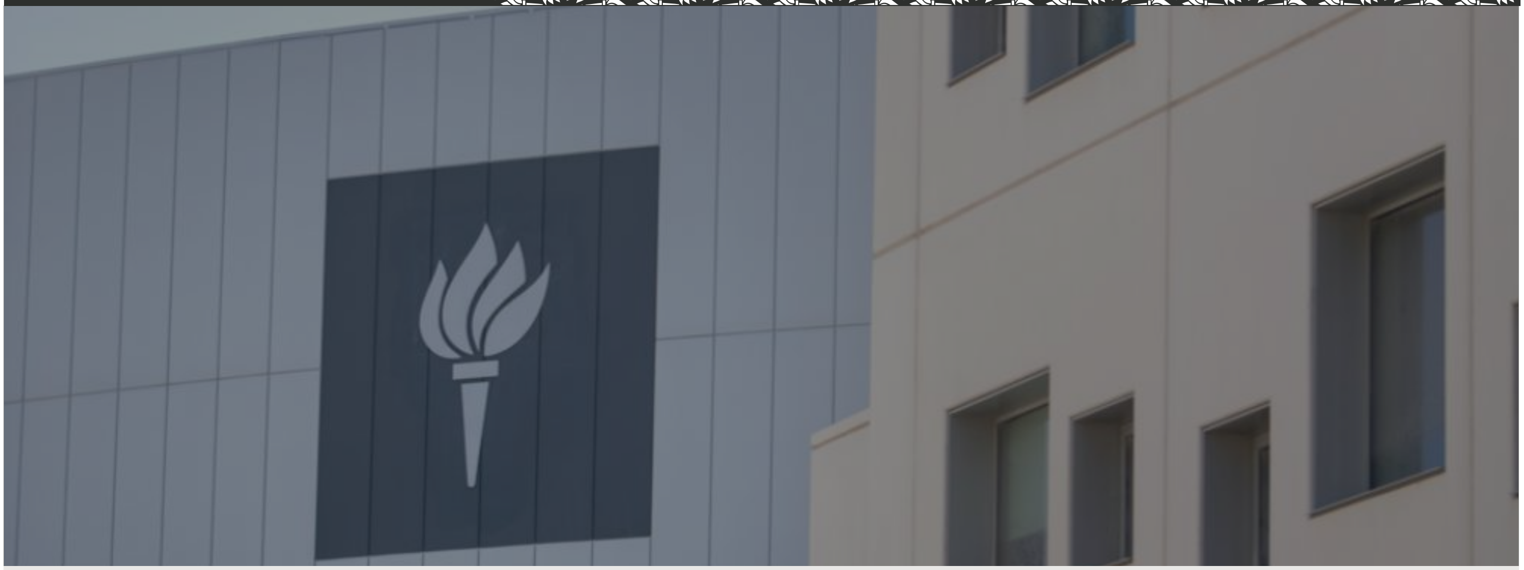

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The Determinants of Multilateral Bargaining: A Comprehensive Analysis of Baron and Ferejohn Majoritarian Bargaining Experiments

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Abstract

We collected and analyzed the data sets of all majoritarian Baron and Ferejohn (1989 Am. Pol. Sci. Rev.) experiments through 2018. By exploiting the variation of experimental parameters we are able to identify how group size, discount, voting weights, and institutional variables such as communication, affect the distribution of the surplus, proposer power, and agreement delay. We conduct the first structural estimation of the proposer's share equilibrium prediction and find little support for the theory. We also report on behavior following a disagreement and find strong evidence of history-dependent strategies in the form of punishment toward failed proposers and their supporters.

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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) and 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 the group proceeds to a sequential vote where a simple majority of members in favor are required for approval. In case of rejection, a new bargaining round takes place with every member having the same chance of being recognized. 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 patient enough players). By further restricting strategies to be history-independent, the authors characterize the stationary subgame perfect equilibrium (SSPE) which yields a unique distribution of payoffs. It predicts that only a simple majority of members will receive a positive share, i.e that minimal winning coalitions (MWCs) will form. Proposers will offer coalition partners a share which makes them indifferent between accepting and the continuation value of the game. The equilibrium continuation value of the game decreases the more impatient players are and also falls with the size of the committee. Given the

[1] In this article we focus on the closed-amendment rule which 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.

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 equilibrium.

The SSPE outcomes will serve as our testable theoretical predictions because all the literature has explicitly focused on these. However, given the richness of our dataset, 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 costly delay (measured by the experimentally induced discounting parameter) reduces chances of disagreement. Our regression analysis controls for player's voting weights, so that we can analyze if there exists an *illusion of power* which can account for variation in sharing of the pie. We also study learning patterns by explicitly controlling for experience in our regressions.

Most evaluations of the experimental evidence in the literature reach the conclusion that the evidence provides qualitative support for the model's key SSPE outcome predictions (see Palfrey (2016); Agranov (2020)). In general reviewing these results seem to suggest that proposer power exists but is moderate and substantially below theoretical predictions, MWCs are common but not universal, and most agreements are reached without delay. By exploiting the variation in experimental parameters (group size and discounting) across studies, we are the first to conduct a structural estimation of the model's predicted proposer share. Estimation results show little evidence of a good fit between theory and subject behavior. Moreover, reduced-form regressions testing the comparative statics rule out that the discount factor affects the proposer's demanded share which is rather flat. In support for the SSPE, MWCs and agreement without delay are modal.

One key feature regarding the bargaining process *per se* has been largely understudied in most of the existing literature: subject behavior when a disagreement has previously taken place. History independence of bargaining strategies (assumed for SSPE, which is the most widely accepted refinement in the theoretical literature) requires players to not be spiteful against a previous proposer who excluded them from the distribution of the fund. 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.[2] We find strong evidence for retaliation against previous proposers by subjects who did not agree with the share offered to them and moderate evidence of retaliation against those who supported a failed proposal. Based on our analysis we compute the empirical continuation values (i.e. what a subject would receive in expectation) following a rejection which can partially explain the low proposer power.

There is also wide variation in experimental design features such as the number of games played, subject payment scheme (i.e. random period payment or pay for all periods), strategy method versus direct response, and fund size, among others. Naturally, studies also use different metrics to report findings. Some focus on all proposals while others only on approved ones, and some further condition 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 fund. All these differences may obscure comparing 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.[3]

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 being asked in the

[2]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.

[3]We include in our analysis treatments that yield symmetric predictions and in which the fund to distribute is exogenous. We also incorporate treatments that allow for costless communication.

study, the parameters used (groups 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. 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 wining 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. Veto players as proposers receive larger shares compared to non-veto proposers and control. 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: 43531; 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. 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:

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. Subjects have not participated in previous BF experiments
6. All proposals and voting decisions are made by subjects and not by computers.

Studies marked with ** and * in Table 1 are included in our analysis. Treatments not meeting the conditions above in those studies are excluded.[4]

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.

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.

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

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

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[5], MWCs, and delay) in which we allow for multilevel random effects at the session and subject levels. Our first goal is to test the theoretical predictions of the SSPE and then to inspect alternative hypotheses of how experimental parameters and conditions may affect behavior.

Standard errors are clustered at the study level to account for the fact the experimental design details vary between researcher groups and these differences may impact bargaining behavior in ways we cannot control for explicitly.[6] One way in which we control for differences in incentives across studies is by including a measure of the show up fee relative to the money being divided.

Before starting our analysis, a few definitions are necessary. We will refer to a player as being *included* in the 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 wiggle most studies do by considering *pittance shares* as equivalent to exclusion from the coalition.

4.1 Proposer Power

Our first task is to understand whether the proposals that subjects make hold any resemblance to those predicted under the SSPE. For this purpose will conduct a structural

[5]The size of the pie has been normalized to 1 in all experiments and shares are expressed as proportions.

[6]There are some exceptions where the number of clusters is so small relative to the regressors so that we cannot calculate the joint significance of our model (χ^2 statistic), hence we will include the study as a third nesting level in our random effects model.

estimation. Rearranging equation (1) and taking natural logarithms of both sides, we obtain

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

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

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

where η_i and ν_s are the subject and session-specific random effects and ϵ_{sit} is the error term.[7] Values of $\hat{\beta}_1$ and $\hat{\beta}_2$ equal to 1 would mean that the data approximates the theoretical predictions.

The results of our estimation are presented in Table 2. Our first regression is for the full data set, which includes all proposals (regardless of the game number or round within a given game) by every subject that was allowed to propose. However, there are two important aspects to highlight. First, we show in section 4.5 that history matters for bargaining behavior following a rejection so that proposals in further rounds may differ structurally from those in round 1. Hence, in our second estimation 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.[8] 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 (columns 3 and 4). In our last regression, we explore only proposals that received a majority vote in favor to investigate if accepted proposals conform closer to theoretical predictions (column 5).[9]

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

[8] 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).

[9] We are especially grateful to one anonymous referee who offered valuable insights regarding the sample

We can definitively reject that the SSPE is a good predictor of the observed proposer behavior in the data. The coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ are significantly different from 1 (Wald test p-value>0.6). Moreover, conditioning on round 1, or different experience levels has virtually no impact in the estimation results.[10]

Table 2: Structural Estimation of Proposer Behavior

	(1)	(2)	(3)	(4)	(5)
			Round 1		
	All	All	Games 1-5	Games 6-10	Accepted
Constant	-0.908***	-0.905***	-0.871***	-0.943***	-0.852***
	(0.062)	(0.065)	(0.060)	(0.069)	(0.057)
$\ln(\delta)$	-0.111	-0.117	-0.154*	-0.102	-0.076
	(0.099)	(0.101)	(0.086)	(0.119)	(0.096)
$\ln\left(\frac{group\,size-1}{2\times\,group\,size}\right)$	0.140***	0.134**	0.141***	0.142**	0.127***
	(0.050)	(0.052)	(0.042)	(0.056)	(0.048)
var(Session)	0.096***	0.099***	0.085***	0.109***	0.094***
	(0.017)	(0.017)	(0.011)	(0.019)	(0.018)
var(Subject)	0.134***	0.136***	0.142***	0.160***	0.127***
	(0.008)	(0.008)	(0.011)	(0.016)	(0.006)
var(Residual)	0.211***	0.203***	0.225***	0.160***	0.152***
	(0.021)	(0.018)	(0.024)	(0.019)	(0.015)
N	6580	5254	2254	2226	2957
χ^2	10.03	8.73	16.84	8.09	8.29

Standard errors in parentheses clustered by study.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We now proceed to investigate whether the comparative statics of the model hold regarding the discount factor. The SSPE predicts that the proposer's share falls as players become more patient, but we do not find supporting evidence. Table 3 displays the mean share for each group size and discount factor. We focus on games without communication because δ does not vary within group size for communication treatments, thus we cannot test the

selection for regression analysis.

[10]In Table 10 in the Online Appendix we further investigate other subsamples of interest such as games 11 and above and find similar results.

Table 3: Average Proposer's Share by Group Size and Discount Factor

	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)	

Only round 1 proposals in games 6-10. Standard errors of the mean in parentheses. SSPE predicted share in brackets next to the mean observed value.

prediction. We find that the mean proposer's share for groups of 3 is rather stable around 50 percent of the total surplus. In groups of 5, the mean share also appears to be unaffected by the experimentally-induced level of impatience.

Reduced form estimations presented in Table 4 yield a positive coefficient for the discount factor, except for groups of 5 in games 6-10 where the coefficient is in line with the comparative statics. It is unclear to us why learning may take place in groups of 5 but not in groups of 3. It should be noted, however, that none of these coefficients reach significance at conventional levels.[11]

Given our findings that the discount factor does not affect the proposer's mean share and the lack of fit between our structural estimation and the SSPE prediction, we will now turn to investigate alternative explanations for observed behavior. First, we seek to confirm (or not) the robustness of 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 an

[11]We are unable to estimate our models for groups of 7 as there is no variation in δ.

Table 4: Multi-level Random Effects Linear Regression for the Proposer's share as function of Discounting, by Group Size

	Groups of 3		Groups of 5	
	Games 1-5	Games 6-10	Games 1-5	Games 6-10
Constant	0.420***	0.498***	0.163	0.563***
	(0.055)	(0.067)	(0.226)	(0.210)
δ	0.067	0.023	0.245	-0.168
	(0.060)	(0.072)	(0.226)	(0.210)
Random Effects:				
var(Session)	0.037***	0.039***	0.041***	0.038***
	(0.006)	(0.009)	(0.012)	(0.010)
var(Subject)	0.080***	0.069***	0.082***	0.095***
	(0.006)	(0.006)	(0.020)	(0.011)
var(Residual)	0.100***	0.090***	0.102***	0.067***
	(0.005)	(0.007)	(0.008)	(0.001)
N	1599	1579	375	375
χ^2	1.27	0.10	1.18	0.64

Standard errors clustered at the study level.

Treatments without communication.

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

interaction between communication and experience and if the experience effect diminishes with each successive game.

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 with equal voting weights.

Finally, we wish to identify if the size of monetary incentives relative to the show up fee plays a role in how much proposer's demand. For this purpose we have coded from each paper (1) the show up fee (2) the monetary value of the pie to distribute (3) the probability that a given round is selected for payment. Based on this three variables we computed $\frac{\text{Show-up}}{\text{Pie}\times\text{Probability of Payment}}$ and included it as a control in our multilevel mixed-effects regressions. We have considered study-level random effects because we have very few clusters relative to the regressors so that we cannot calculate the joint significance of the estimated coefficients. In the Online Appendix we repeat the estimation exercise and find no meaningful differences in individual coefficient significance when clustering errors at the study level.

Our estimation results in Table 5 robustly confirm the significance of experience and communication.[13] The share demanded by proposers grows as subjects play the game and the coefficient is highly significant. Importantly, it grows at a decreasing rate as the negative coefficient for $Game^2$ reveals. Communication also increases the proposer's share, but the level effect is only significant at the 10% in groups of 5. The positive coefficient for the interaction between communication and game shows that each successive game of experience increases the proposer's share by close to 1 percentage point beyond the growth rate absent communication.

We find evidence supporting an illusion of power: proposers' demands increase in their

[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.

[13]We are unable to estimate our models for groups of 7 as there are is no variation in experimental parameters.

Table 5: Behavioral Determinants of the Proposer's Share by Group Size.

	(1) Groups of 3		(2) Groups of 5	
Constant	0.329***	(0.043)	0.120	(0.083)
Game	0.019***	(0.004)	0.045***	(0.007)
Game ^{2}	-0.001***	(0.000)	-0.001***	(0.000)
Communication	0.058	(0.047)	0.044*	(0.025)
Voting Weight	0.334***	(0.064)		
$\frac{\text{show up fee}}{\text{Pie}}$	-0.005	(0.014)	0.203***	(0.079)
Game × Communication	0.007	(0.004)	0.008***	(0.003)
Game × Voting Weight	-0.015*	(0.009)		
Game $\times \frac{\text{show up fee}}{\text{Pie}}$	0.000	(0.001)	-0.025***	(0.004)
Random Effects:				
var(Study)	0.033***	(0.010)	0.032***	(0.018)
var(Session)	0.017***	(0.007)	0.018***	(0.009)
var(Subject)	0.069***	(0.003)	0.071***	(0.005)
var(Residual)	0.097***	(0.001)	0.094***	(0.002)
N	3258		970	
χ^2	234.50		130.25	

Standard errors in parentheses.

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

nominal voting weight. This is a sensible 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. Voting weights may also create entitlements too. 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.

The show-up fee relative to the size of the pie (in expectation) appears to be negative but not significant for groups of 3, even with experience. For groups of 5 the higher show-up fees are relative to the pie, the more proposers demand but this fades with each successive game. We have not hypothesized about the expected direction or magnitude of this variable. It has been included as a control in our regressions. A specific experiment would be required to test for wealth effects in multilateral bargaining and to identify if there are differences in behavior that correlate with the experimental choice of compensation (i.e. random period(s) vs. all periods).

Conclusion 1. *Evidence from a structural estimation shows that the SSPE is not a good predictor for the observed proposer's share. Discounting has no significant effect on the proposer's share. The proposer's share is increasing in experience, communication, and nominal voting weights, with the effect of communication growing with experience and voting weights fading.*

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, in support for the theory.[14] Panel A in Figure 1 shows the distribution of proposal types by group size.

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

In order to test the SSPE predictions it should be clear to the reader that a structural estimation is trivial as it entails regressing the MWC dichotomous variable on a constant and testing if the constant equals 1.[15] Thus, we proceed to test the comparative statics for the experimental parameters δ and group size for which there should be no effect. The average marginal effects from a multilevel probit regression are reported in Table 6 (coefficients reported in Table 3 of Online Appendix). Results hold if we include treatments with communication as well.

Table 6: Marginal Effects for MWC (Treatments without Communication)

	(1) Games 1-5		(2) Games 6-10	
δ	0.417***	(0.132)	0.204	(0.162)
Groups of 5	0.062	(0.161)	0.006	(0.081)
Groups of 7	0.046	(0.059)	-0.153*	(0.089)
N	2114		2094	

Standard errors in parentheses clustered at study level. * p<0.1, ** p<0.05, *** p<0.01 .

Group size has mostly no significant effect on the probability of an MWC being proposed, with the exception of groups of 7 in games 6-10. Note that there is only one study with groups of 7 and the significance of the coefficient is low, hence we do not believe this to be indicative of a failure of the SSPE. In the initial games (1-5) the discount factor positively affects the likelihood of MWCs, but this effect vanishes with experience (games 6-10).

We also conducted a multi-level random effects probit model to account for the role of experience, communication, voting weights, and the show up fee relative to the pie, and interacted each variable with experience (i.e. same regressors as we did for the proposer's share in Table 5). The results, reported in appendix Table 9 show that communication significantly increases the chances of MWCs to arise, and so does experience.

We also find that subjects whose voting weight is less than 1/3 (in groups of 3) are less

[15]For completeness, we conducted such regression and can reject that the constant term is equal to 1 (p-value>0.5).

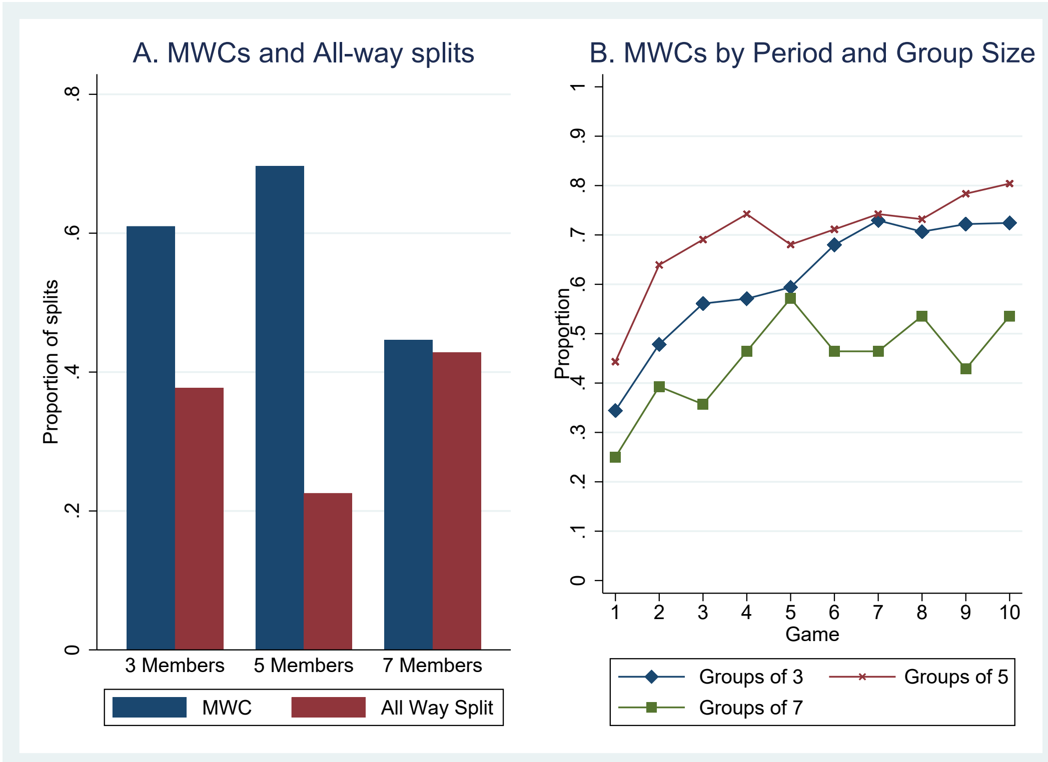


Figure 1: Minimum Winning Coalitions and All-Way Splits

likely to propose MWCs than those whose weight is above 1/3 (71 vs 82 percent). This may be another manifestation of the illusion of power because in more inclusive allocations, the proposer keeps less than in MWCs. Our regression results confirm a positive marginal effect for voting weights.

Conclusion 2. *Minimum winning coalitions are the modal allocation (62 percent of all proposals). The evidence suggests that, with experience, the parameters of the game do not affect the prevalence of MWCs, in accordance with SSPE predictions. Evidence shows that learning occurs after a few games of experience and that communication has a positive effect. Subjects with smaller voting weights propose all-way splits more often than those with higher voting weights.*

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, but this relies on proposers making the *right* offers and voters following through. 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 answer our question we conduct a probit regression for delay as a function of group size and delta.[16] Furthermore, we control for experience (game of play), communication possibilities, and show-up fee relative to the pie. We interact our controls 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. The average marginal effects are reported in Appendix Table 10 and the estimated coefficients are reported in the Online Appendix.

As we can see from the marginal predictions in panel A of Figure 2, 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 and the observations come from 28 subjects only.

While this difference can thought of as a treatment effect, it may have a more "statistical" explanation in the sense that as coalitions become less inclusive (which we know happens with experience) a single "no" vote suffices to impede immediate agreement. 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.

[16]For consistency with our previous estimations, we initially conducted a mixed effects probit regression with study and session random effects, 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.

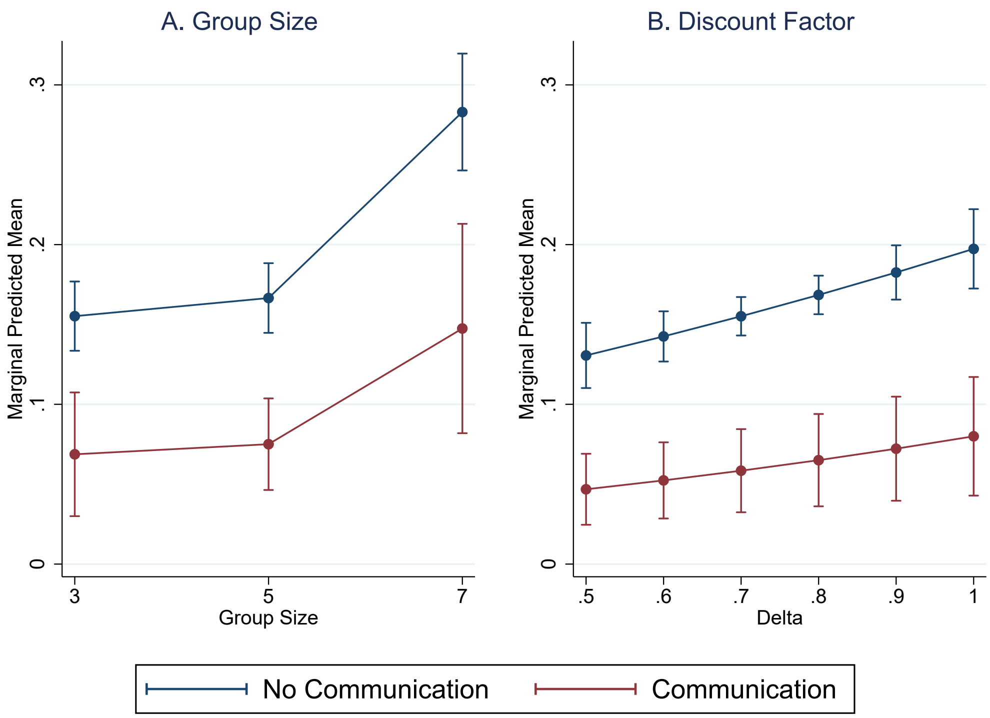


Figure 2: Marginal Probability of Delay

Does discounting affect the probability of delay? We find that the lower the costs of delay are (Panel B of Figure 2), 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 a 9 percentage point difference. In panels A and B of figure 2 we have graphed the marginal effects for group size and δ with and without communication.

Finally, we do not find that delay rates evolve with experience. This is rather surprising since the proposer's mean share increases, leaving less funds to *buy* the necessary votes. However minimal winning coalitions are also becoming 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, how the proposer's take affects their willingness to support a proposal, and the effect of experience.

Conclusion 3. *Delay is uncommon with 85 percent of proposals being accepted in the first round. The probability of delay in reaching an agreement is (1) lower when subjects may communicate; (2) negatively correlated with the cost of disagreement; (3) and we find modest evidence supporting that it is positively correlated with group size.*

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, other factors which may play a role in the voting decision have been set forth such as the proposer's share and whether or not the allocation is a MWC, but the results thus far are ambiguous. 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.

We build on the econometric models of Fréchette et al. (2005a) and Fréchette and Vespa (2017) by estimating a random effects voting probit (two-level model) in which the the probability of voting in favor is a function of own share (as a proportion of the total fund), the proposer's share (as a proportion of the total fund), the discount factor, and whether the proposal is an MWC or not. To test for an illusion of power, we include a player's voting weight and interact it with experience. We include the usual controls to account for experience and the show up fee relative to the pie. We also include an interaction for the communication dummy with own share. We estimate individually for each group size and

Table 7: Multilevel Random Effects Voting Probit (Marginal Effects)

	(1) Groups of 3		(2) Groups of 5		(3) Groups of 7	
Own Share	2.386***	(0.109)	1.888***	(0.175)	0.134	(0.194)
Communication	0.199***	(0.057)	0.107***	(0.022)		
Prop. Share	-0.319***	(0.094)	-0.171*	(0.094)	-0.007	(0.138)
δ	-0.483***	(0.067)	-1.117***	(0.189)		
Voting Weight	-0.497***	(0.139)				
Game	-0.002	(0.002)	-0.003	(0.003)	-0.003	(0.004)
MWC (1=yes)	-0.080***	(0.029)	0.018	(0.032)	0.051	(0.032)
Show Up Fee / Pie	0.001	(0.010)	0.123*	(0.067)		
N	2676		1668		1289	

Standard errors in parentheses. Coefficients reported in online appendix.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

restrict our sample to included voters (i.e. their share exceeds 5 percent of the fund).[17]

The average marginal effects predicted by our model are reported in Table 7. Confirming the previous evidence, the marginal effect for own share is positive for all group sizes and significant at the 1% level for groups of 3 and 5, but not significant at conventional levels for groups of 7. The likelihood of voting in favor increases by 20 percentage points when players may communicate in groups of 3 and by 10 percentage points in groups of 5.

Previous studies investigating the role of the proposer's share on voting typically showed that subjects are less likely to vote in favor of a proposal as the proposer's share increases (controlling for own share). However, the instances where such result was statistically significant were rare, and for the most part, such variable was excluded from regression analyses in the literature.[18] We find that the probability of voting in favor falls as the proposer's demanded share increases, which is significant for groups of 3 (p<0.01), groups of 5 (p<0.1) but not significant in groups of 7. Give we have reached significance for our large sample in groups of 3 and 5, we believe that for groups of 7 the effect also exists but we are

[17]The analysis with clustered standard errors is in the Online Appendix, confirming our results.

[18]For example, Fréchette et al. (2005b) only find a significant effect for their symmetric treatment while Fréchette et al. (2005a) only find a significant effect for their apex treatment, and Fréchette and Vespa (2017) report significance in only some econometric specifications.

underpowered to detect it.

Voters react to the discount factor in the direction predicted by the SSPE: as the cost of delay increases, the likelihood of rejecting falls. We do not find a significant effect of experience, yet the game marginal effect is negative in all cases.

Fréchette and Vespa (2017) report a negative relationship between the discount factor and voting, which we verify in our pooled analysis. Thus, their finding is not driven by the fact that subjects cannot tell if a proposal is made by a human or computer. This relationship between the cost of delay and supporting a proposal explains at the individual decision level why delay is more likely to occur as the discount factor increases which was previously reported.

The overall effect of communication is positive: subjects are unconditionally more willing to vote in favor when they can communicate. Such effect may be the result of the culmination of a verbal negotiation process in which respondents expressed their agreement. Importantly, the marginal effect of the interaction with own share is negative, -0.8 for groups of 3 ($p<0.01$) and -.14 for groups of 5 ($p<0.05$). This means that verbal negotiation makes voters more willing to accept lower shares.[19] It may be evidence of an underlying psychological effect of communication since it implies that players value the same offered share differently simply because they were able to exchange messages with the proposer. Alternatively, it can be the result of subjects learning to reason strategically about the game, thus, voting in favor of equilibrium prescribed shares.

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

[19] In calculating the marginal effect of the interaction term we have used the method by Ai and Norton (2003), which takes the discrete difference (in communication) over the partial derivative with respect to own share.

SSPE. As we show in subsection 4.5 on off-equilibrium behavior, retaliation and reciprocity as largely at play with communication as well. Hence, while proposals may be closer to the SSPE prediction, the strategies by which subjects abide are not.

We find evidence that nominal voting weights affect voting decisions, despite theory predicting that only real bargaining power differences should do so. Specifically, players with higher voting weights are less likely to vote in favor, controlling for the share offered. The study by Fréchette et al. (2005b) in which nominal shares are varied, does not estimate the effect of voting weights on voting behavior. Thus, we uncover an *illusion of power* derived from voting shares, which is in line with the psychological theory of inequity (Adams, 1965) and the principle of proportionality in payoff distribution and coalition formation according to Gamson (1961). Diermeier and Morton (2005) have previously set forth this as a potential explanation of behavior in bargaining games. The marginal effect of the interaction between experience and voting shares is positive (0.05) but not significant ($p>0.1$). Thus in our sample, the illusion of power at the voting stage does not appear to fade with experience, in contrast with the findings by Maaser et al. (2019).

Results regarding the effect of whether or not the allocation is a MWC remain unclear. For example, Baranski and Kagel (2015) report a negative and significant effect, while Fréchette and Vespa (2017) finds both negative and positive (non-significant) effects. Excluding this dummy variable from our analysis does not affect the significance of our estimations and previously reported results.

Conclusion 4. *The probability of voting in favor is (1) positively correlated with own share; (2) positively correlated with the cost of delay; (3) positively correlated with the possibility to communicate; and (4) 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 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, 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 supporter 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.

4.5.1 Empirical Continuation Values

We will compute this 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 8, and are quite clear: round 1 proposers face a lower continuation value than non-proposers. In groups of 3, the difference is 8 percentage points, and in groups of 5 it is 4 percentage points. Both of these differences are significant at the 5 percent level or better.[20]

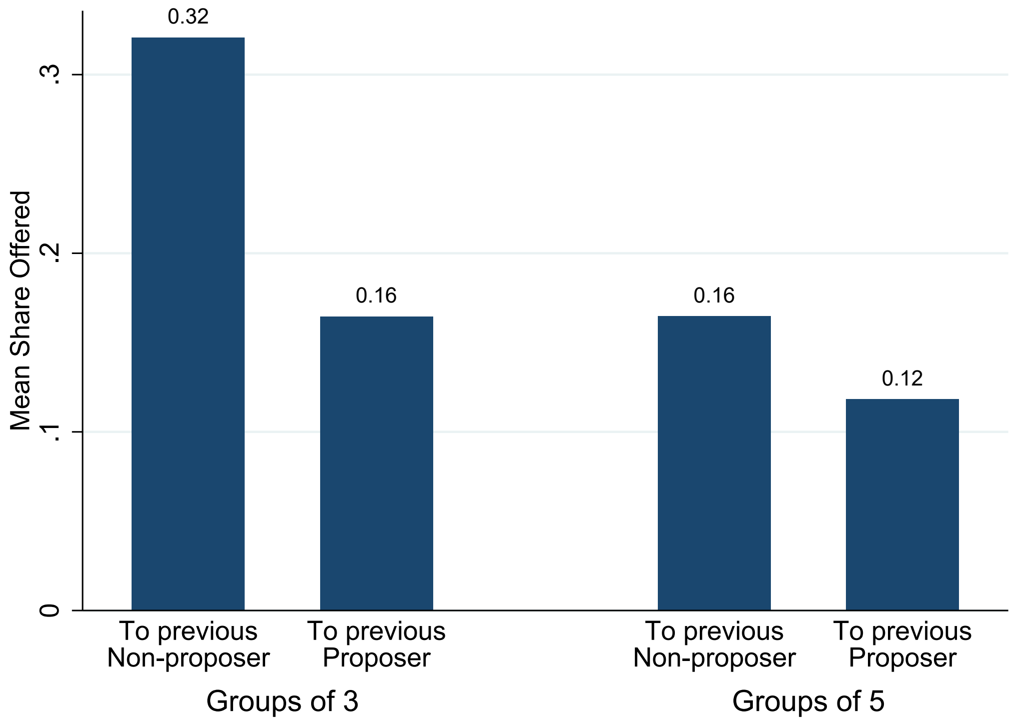
Table 8: Empirical Continuation Values as Proportion of Total Fund

	Groups of 3	Groups of 5
Round 1 Proposer	0.28	0.17
	(0.010)	(0.007)
Round 1 Non-Proposer	0.36	0.21
	(0.005)	(0.002)

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

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

Figure 3: Shares offered in Round 2 by Subjects that did not Propose in Round 1 (by recipient and group size)



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 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 3, the mean share offered to round 1 proposers by previous non-proposers is 17 percent of the fund in groups of 3, which is 15 percentage points lower than what previous non-proposers share among themselves.

4.5.3 Retaliation by those who did not support the proposal

In groups of 5 a similar, but less pronounced, pattern arises. However, note that there is a key distinction in how a rejection may have occurred between groups of 3 and 5. 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 4 shows that those who reject a proposal in round 1 offer shares that are 54 percent larger to non-proposers than to round 1 proposers (17 versus 11 percent of the fund respectively, p-value$<$0.05). We do not find evidence of positive reciprocity towards previous proposers by their round 1 supporters.

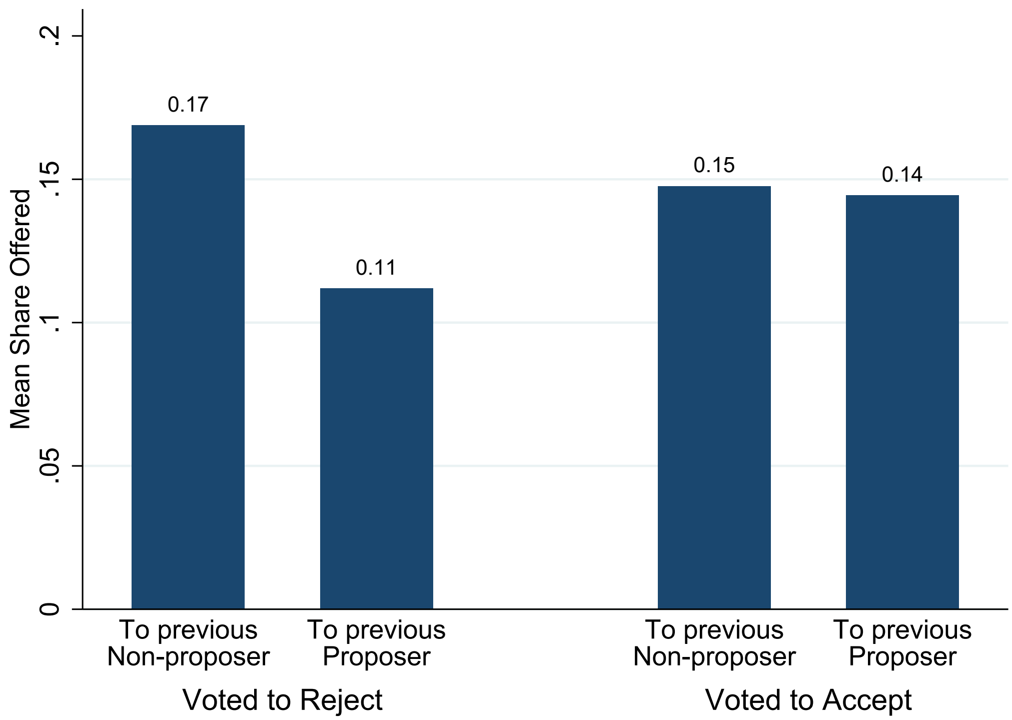
We further conditioned on whether a subject had been included or not by the proposer in round 1 and find that those who vote against, regardless of inclusion status, are more likely to exclude the proposer in their round 2 proposals. See Online Appendix Figure 2.

4.5.4 Retaliation against supporters of failed proposals

Is there evidence of retaliation against supporters of a failed proposal? Do failed proposers reciprocate positively to those who supported them in round 1? Regression analysis shows that proposers share 4.5 percentage points more with those who supported them (p=0.09) than with those who voted against. There is also evidence of retaliation against supporters of a failed proposal who receive on average 3 percentage points less of the total fund than those who voted against the round 1 proposal (p=0.06). For details and the regression output see Online Appendix Table 8).

In the appendix we have further investigated other relevant questions. For example, does it pay to vote against a proposal? We find that those who were included by the proposer in round 1 and rejected the offers are better off (in round 2) 69 percent of the time in groups of 3 and only 33 percent of the time in groups of 5. Those who had been excluded in round

Figure 4: Shares offered in Round 2 by Subjects that did not Propose in Round 1 for Groups of 5, by Voting Decision



Notes: Those who rejected a previous proposal offer 6 percentage points less to the previous proposer than to non-proposers (p-value<0.05). There is no difference for those that voted in favor (p-value>0.5).

1, are typically better off in round 2, with approximately 80 percent of the time getting a larger share taking discounting into consideration. (see Table 9 in Online Appendix).

Conclusion 5. *Proposing behavior following a disagreement displays strong history-dependence and, as such, is inconsistent with stationary strategies. Subjects who rejected a previous proposal offer lower amounts to the previous proposer than what they offer to other members. We find moderate evidence for retaliation against supporters of a failed proposal and gratitude from the proposer to those who supported her in the previous round.*

5 Discussion and Concluding Remarks

Our meta-analysis has uncovered several important aspects about bargaining behavior. The proposer's demanded share remains largely flat no matter the discount factor which is the main experimentally induced strategic parameter of the model. This is not only contrary to the SSPE predictions but also to one's intuition. MWCs are modal and increase with experience, but all-way splits are also proposed often and rarely rejected. In line with a reasonable behavioral conjecture, delay increases as subjects face lower costs of rejection which is explained by the lower likelihood of voting in favor. As such, it appears that voting decisions are affected by the strategic parameters of the game but that proposer's offers are not.

We find evidence of an illusion of power. Voting weights are positively correlated with the proposer's demanded share and the likelihood of proposing a MWC. We also find that those with higher voting weights are less likely to vote in favor, controlling for the offered share.

In our analysis, we have uncovered the presence of negative reciprocity in counter-proposals upon a disagreement. Subjects strongly retaliate against previous proposers by offering them less than what they would receive at random. Supporter of a failed proposal are also a target of retaliation albeit in a weaker fashion.

Several attempts have been made to reconcile theory with experimental results in the Baron and Ferejohn (1989) game. 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 obtains when risk aversion is incorporated into players' utility functions (Harrington, 1990).

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) because they dislike the disadvantageous inequality that would arise from the possibility of being left out from the MWC in a future round. We have clear evidence that subjects by and large reject offers close to the theoretical prediction (which are not common), so they would certainly reject even lower offers. However, this observation does not imply that subjects have no 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 a MWC would be supported under reasonable parameters of the Fehr and Schmidt (1999) inequality averse utility function (see Online Appendix Section 7). In the second case, if subjects abide by non-stationary strategies, we know that the set of equilibria is large and the question arises of which proposals will be coordinated on. It can be that social preferences dictate which allocations to select 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 next round are higher (i.e. fall prey of 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). For example, using 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 empirical 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. Retaliation and reciprocity are largely at play, thus the stationarity refinement is inaccurate and should be disposed of when theorizing if one seeks to realistically capture behavior. Importantly, such reciprocal strategies are also present in the treatments with communication. This means that communication may lead to outcomes that resemble those predicted by the SSPE but not because of the strategies players are using.

The model of Baron and Ferejohn is one of complete information and there is no space for disagreement to arise (unless these are mistakes (Nunnari and Zapal, 2016)). The results from treatments where communication is possible show that disagreement rates are reduced. subjects tend to state their willingness to accept in their communications and there are differences between subjects in this regard. We are unaware of a model with preference uncertainty in multilateral bargaining that could aid in organizing the data. Importantly, differences in stated reservation shares from voters may also arise from variation in cognitive abilities or levels of reasoning.

In our comprehensive analysis we have uncovered several effects previously unreported in the literature regarding the impact of group size and discount factor on proposal and voting strategies. It is our hope that future theoretical and experimental work can help us further our understanding of the stylized results and open questions reported here.

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A Additional Tables

Table 9: Multi-level Random Effects Probit for Minimum Winning Coalitions

	(1) Groups of 3		(2) Groups of 5	
Game	0.039***	(0.004)	0.028***	(0.006)
Communication	0.332***	(0.044)	0.181**	(0.081)
Voting Weight	0.209***	(0.059)		
$\frac{\text{show up fee}}{\text{Pie}}$	0.010	(0.021)	0.563***	(0.124)
N	3258		970	

Marginal effects; Standard errors in parentheses clustered at study level.
Round 1 proposals in games 1-10.
For estimated coefficients see online appendix.
* p<0.1, ** p<0.05, *** p<0.01

Table 10: Probit for Agreement Delay

	Marginal Effect	Std. Error
Constant		
δ	0.369***	(0.062)
Group size=5	0.010	(0.051)
Group size=7	0.162***	(0.030)
Communication	-0.182***	(0.020)
Game	-0.002	(0.003)
Show Up Fee / Pie	0.005	(0.011)
N	1514	

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: Behavioral Determinants of the Proposer's Share by Group Size with Clustered Standard Errors by Study

	(1) Groups of 3		(2) Groups of 5	
Constant	0.329***	(0.045)	0.087	(0.056)
Game	0.019***	(0.003)	0.045**	(0.018)
Game ^{2}	-0.001***	(0.000)	-0.001	(0.001)
Communication	0.043**	(0.020)	0.077***	(0.016)
Voting Weight	0.334***	(0.102)		
$\frac{\text{show up fee}}{\text{Pie}}$	-0.000	(0.009)	0.242***	(0.050)
Game × Communication	0.007***	(0.001)	0.008***	(0.002)
Game × Voting Weight	-0.015***	(0.001)		
Game $\times \frac{\text{show up fee}}{\text{Pie}}$	0.000	(0.001)	-0.025***	(0.005)
var(Session)	0.037***	(0.010)	0.033***	(0.001)
var(Subject)	0.069***	(0.005)	0.071***	(0.006)
var(Residual)	0.097***	(0.005)	0.094***	(0.003)
N	3258		970	
χ^2	.		.	

Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

χ^2 could not be calculated since we have too few clusters.

This regression reproduces that in Table 5 of text.

Table 2: Probit for MWC by Group Size For Treatments without Communication

	(1) Games 1-5		(2) Games 6-10	
δ	1.883***	(0.668)	1.339	(1.194)
Groups of 5	0.279	(0.736)	0.068	(0.595)
Groups of 7	0.212	(0.280)	-0.967**	(0.492)
var(Session)	0.370*	(0.218)	0.544	(0.411)
var(Subject)	1.739***	(0.295)	3.971***	(1.068)
N	1984		1964	
χ^2	16.11		50.31	

Marginal effects reported in text (Table 6).
Standard errors clustered at study level in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

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

	(1) Groups of 3		(2) Groups of 5	
Constant	-1.765***	(0.157)	-5.411**	(2.742)
Game	0.476***	(0.043)	0.656***	(0.223)
Game ^{2}	-0.021***	(0.004)	-0.021***	(0.005)
Communication	2.128***	(0.365)	1.545*	(0.924)
Voting Weight	0.139	(0.589)		
$\frac{\text{show up fee}}{\text{Pie}}$	0.312***	(0.119)	4.677**	(2.230)
Game × Communication	0.053*	(0.029)	-0.076	(0.060)
Game × Voting Weight	0.167***	(0.060)		
Game $\times \frac{\text{show up fee}}{\text{Pie}}$	-0.050***	(0.013)	-0.256	(0.179)
var(Session)	0.342	(0.325)	0.263	(0.426)
var(Subject)	1.760***	(0.257)	1.666**	(0.678)
N	3258		970	

Marginal effects reported in text.
Standard errors clustered at study level in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Table 4: Probit for Delay

Constant	-2.005***	(0.219)
δ	1.421***	(0.198)
Group size=5	0.032	(0.182)
Group size=7	0.533***	(0.084)
Communication	-1.894***	(0.120)
Game	-0.008	(0.019)
Game^2	0.000	(0.001)
Game $\times$ Communication	0.108***	(0.020)
Show Up Fee / Pie	0.030	(0.043)
N	1432	

Standard errors clustered at the study level.

Marginal effects reported in text.

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

Table 5: Multilevel Random Effects Voting Probit (Coefficients) with Clustering

	(1) Groups of 3	(2) Groups of 5	(3) Groups of 7
Constant	0.339	4.316***	4.316***
	(0.588)	(0.299)	(0.299)
Own Share	12.507***	10.041***	10.041***
	(1.560)	(3.752)	(3.752)
Communication	0.960*	-0.956	-0.956
	(0.562)	(0.758)	(0.758)
Communication × Own Share	1.326	8.738**	8.738**
	(1.313)	(3.514)	(3.514)
Prop. Share	-1.674	-1.065**	-1.065**
	(1.079)	(0.502)	(0.502)
δ	-2.538***	-6.950***	-6.950***
	(0.301)	(0.246)	(0.246)
Voting Weight	-3.230***		
	(0.501)		
Game	-0.047*	-0.016	-0.016
	(0.026)	(0.017)	(0.017)
Game × Voting Weight	0.118		
	(0.074)		
MWC (1=yes)	-0.430	0.108	0.108
	(0.320)	(0.113)	(0.113)
Show Up Fee / Pie	0.006	0.763	0.763
	(0.023)	(0.523)	(0.523)
Random Effects:			
var(Session)	0.047	0.000	0.000
	(0.038)	(0.000)	(0.000)
var(subject)	0.526***	0.583**	0.583**
	(0.124)	(0.235)	(0.235)
N	2676	1668	1668
χ^2	.	.	.
χ^2 (LR test Probit vs. RE)	189.15	78.98	78.98

Standard errors in parentheses clustered at study level.

Marginal effects reported in Table 6.

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

Table 6: Multilevel Random Effects Voting Probit (Marginal Effects) with Clustering

	(1) Groups of 3		(2) Groups of 5	
Constant				
Own Share	2.386***	(0.229)	1.888***	(0.333)
Communication	0.199***	(0.020)	0.107***	(0.005)
Prop. Share	-0.319	(0.210)	-0.171***	(0.063)
δ	-0.483***	(0.045)	-1.117***	(0.113)
Voting Weight	-0.497***	(0.029)		
Game	-0.002	(0.004)	-0.003	(0.003)
MWC (1=yes)	-0.080	(0.057)	0.018	(0.019)
Show Up Fee / Pie	0.001	(0.004)	0.123	(0.097)
N	2676		1668	
χ^2 (LR test Probit vs. RE)				

Standard errors in parentheses.

Coefficients reported in Table 5.

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

Figure 1: Shares offered in Round 2 by subjects who did not propose in Round 1, by Vote in Round 1 and Recipient

Notes: This figure is for groups of 5 only. Those who voted against a proposal in round 1 (histograms on the left), retaliate against the proposer by offering a 0 share 50% of the time, yet other non-proposers are excluded only 30% of the time. Those who voted in favor (histograms on the right), exclude the proposer 35% of the time, at the same rate they exclude other non-proposers. Thus we find evidence for negative reciprocity, but no sign of positive reciprocity.

Table 7: Shares offered in round 2 by subjects that did not propose in round 1

Constant	0.228***	(0.007)
To previous proposer (=1 if yes)	-0.121***	(0.019)
Current Proposer Voted Yes in Round 1 (=1 if yes)	-0.009	(0.006)
To previous prop. $\times$ Current prop Voted Yes	0.050*	(0.029)
Recipient Voted Yes in Round 1 (=1 if yes)	-0.010	(0.020)
δ	-0.001	(0.002)
Communication	0.000	(0.000)
N	960	
R^2	0.09	

Standard errors clustered at the subject level in parentheses.

Only for groups of 5 since in groups of 3 the non-proposer recipients must have voted no.

Variable "show up fee / Pie" dropped because of collinearity.

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

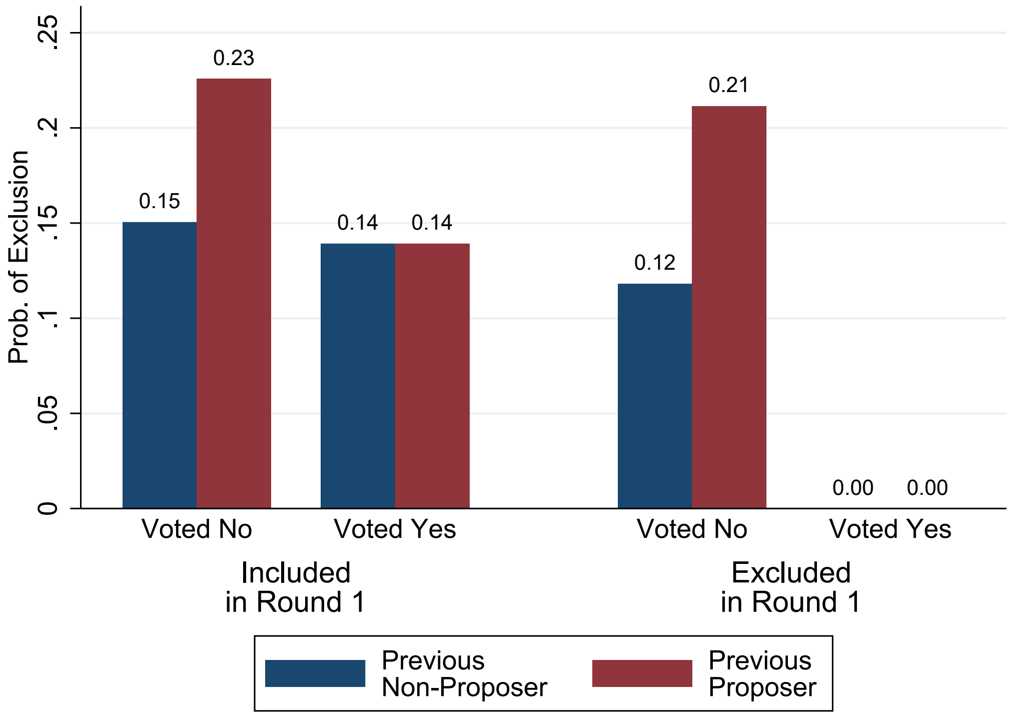
2 History Dependence

2.1 Probability of Exclusion in Round 2 of Previous Proposer

We investigate how likely it is that the previous proposer is excluded from the allocation in comparison with the probability of exclusion that a non-proposer faces. Recall exclusion means receiving a share less than or equal to 5 percent of the fund. We analyze behavior depending on whether the subject had been excluded or not in round 1 by the proposer and by how she voted. Also, we focus on groups of 5 because in groups of 3 both non-proposers have voted no, regardless of inclusion status.

The first pair of bars is for subjects that voted against the round 1 proposal but were included. We see that those who vote against are 50 percent more likely

Figure 2: Probability of Excluding in Round 2



Notes: Red bard 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.

to exclude the round 1 proposer from their round 2 proposal. Those who vote yes and were included in the round 1 proposal, tend to exclude round 1 proposers and non-proposers at equally likely rates (second pair of bars). Note that those who were excluded and voted no in round 1, tend to exclude the proposer 75 percent more often than they exclude non-proposers (third pair of bars). We have no single subject that voted yes when excluded.

2.2 Retaliation and Reciprocity towards Supporters of failed proposals

In Table 8 we investigate in supporters of a previous proposal are punished. To this effect, we regress the offered share in round 2 to other group members excluding the previous proposer. Our variable of interest is how the recipient of the share voted in round 1. Standard errors have been clustered at the subject level and study fixed effects were included (none were significant at conventional levels). In the first estimation (column 1) we regress the share offered by non-proposers. We can see a negative effect of having supported a failed proposal. The magnitude is not large , but reveals that retaliation is not directed exclusively to the round 1 proposer. In column 2 we estimate the same regression for proposals in round 2 made by the same subject that proposed in round 1. We find a positive effect for having supported the previous proposal.

Table 8: Linear Regression of Share Offered In Round 2

	(1)		(2)	
		Share Offered in Round 2:		
	by Round 1 Non-Proposer		by Round 1 Proposer	
Constant	0.365**	(0.175)	-0.314	(0.203)
To Member that Supported Round 1	-0.029*	(0.015)	0.045*	(0.026)
Game	0.001	(0.001)	-0.002	(0.002)
Communication	0.013	(0.018)	-0.003	(0.013)
Show up / pie	-0.158	(0.134)	0.361**	(0.153)
N	477		144	
R^2	0.02		0.03	

Standard errors in parentheses, clustered at subject level.
Study level dummies included but not shown, non were significant (p>0.5).
* p<0.1, ** p<0.05, *** p<0.01

2.3 Round 2 Outcomes With Respect to Round 1

How often does it pay to vote against a proposal? Clearly, excluded members have nothing to lose from rejecting, but included members are not guaranteed a better deal in the coming round of bargaining. Table 9 shows how often round 2 shares are lower, equal, or higher than round 1 offers for non-proposers that rejected despite being included in the proposal. We find that in groups of 3 almost 70 percent of the time they are met with a higher payoff. This is not so in groups of 5, where more than half of the time subjects end up worse off. Subjects that were excluded in the first round (not shown in table) , have an 88 percent chance of improving their share in groups of 3 and a 78 percent in groups of 5.

Table 9: Round 2 Share Relative to Round 1 for voters that Rejected conditional on being included in the proposal, by Group Size

	Groups of 3	Groups of 5
Worse off in Round 2	37.7	54.7
Same Share in Round 2	2.5	11.6
Better off in Round 2	69.8	33.7

For subjects included in round 1. Proposals accepted in round 2.

3 Structural Estimation of Proposer's Share: Alternative Samples

Table 10: Structural Estimation of Proposer Behavior

	(1) Games 11 or Above	(2) Games 6-10 No Chat	(3) Games 1-3	(4) Games 8-10
Constant	-0.960***	-0.983***	-0.871***	-0.943***
	(0.056)	(0.091)	(0.060)	(0.069)
$\ln(\delta)$	-0.062	-0.422***	-0.154*	-0.102
	(0.099)	(0.131)	(0.086)	(0.119)
$\ln\left(\frac{group\,size-1}{2\times group\,size}\right)$	0.177***	0.103*	0.141***	0.142**
	(0.036)	(0.053)	(0.042)	(0.056)
var(Session)	0.080***	0.132***	0.085***	0.109***
	(0.012)	(0.024)	(0.011)	(0.019)
var(Subject)	0.161***	0.160***	0.142***	0.160***
	(0.017)	(0.005)	(0.011)	(0.016)
var(Residual)	0.162***	0.124***	0.225***	0.160***
	(0.019)	(0.015)	(0.024)	(0.019)
N	2081	774	2254	2226
χ^2	26.88	24.17	16.84	8.09

Standard errors in parentheses, clustered at study level.

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

4 The first experiment

McKelvey (1991) is the first reported experimental test of a BF bargaining protocol (equiprobable recognition with majority rule) with an important distinction: proposals did not directly constitute a split of the pie, instead, they consisted of a vector or probabilities of winning a fixed prize of \$2 for each player. This difference makes it hard to interpret and compare his results to the rest of the literature. For example, the proposal $(0.9, 0.5, 0.1)$ is one in which player 1 has a 90% chance of winning \$2, player 2 has a 50% chance and player 3 has a 10% chance. Each draw was independent, all players could be winners. McKelvey allowed for only three possible proposals which were chosen as to induce cycling of preferences in pairwise comparisons. This game is quite far from the standard BF model where players divide a fixed sum of money. Nonetheless, the results obtained already foreshadowed what would be a constant in almost all experiments to follow: proposers did not take full advantage of their power as predicted by the SSPE. McKelvey concluded that "[t]he stationary solution ... does not do a terribly good job of explaining observed experimental data", and hinted at the possibility of a non-stationary equilibrium "because of the fear of retaliation in successive rounds".

5 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 11: 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 ($\delta = 0.95$).

6 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).

3. Diermeier and Morton (2005): We only use the treatment in which player's voting weights are 32, 33, and 34.

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.

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.

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.

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.

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

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

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.

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

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

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.

7 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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