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Power illusion in coalitional bargaining: An experimental analysis

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POWER ILLUSION IN COALITIONAL BARGAINING: AN EXPERIMENTAL ANALYSIS

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ABSTRACT

One feature of legislative bargaining in the real world is that the distribution of seats or voting weights often does not accurately reflect real bargaining power. Game-theoretic predictions about payoffs and coalition formation are insensitive to such purely nominal differences. The converse idea that nominal differences might matter is referred to as ‘power illusion’. We conduct an experimental test of the classical Baron-Ferejohn model with five player groups. We compare treatments with differences in nominal power holding real power constant. We find strong evidence for the existence of power illusion in almost all aspects of bargaining behavior even if subjects got familiar with the game. This implies that attention needs to be paid to nominal power differentials in the design of weighted voting systems.

Keywords: legislative bargaining, alternating offers, experiments, weighted voting, coalition formation

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1 Introduction

Collective decision-making frequently involves situations in which actors have different numbers of votes. Various political institutions assign heterogeneous voting weights explicitly. Examples include the Council of the European Union, the Board of Governors of the International Monetary Fund, and the U.S. Electoral College. Choice of weights sparks recurrent controversies in such bodies.¹ More generally, weighted voting arises when votes are combined and cast together. Important examples are the formation of coalition governments, voting in legislatures with cohesive factions, and shareholder voting in corporations.

The combinatorial nature of weighted voting implies that weights need to be distinguished from power. Seemingly different games can be strategically equivalent. For illustration, suppose that elections to a 100-seat simple-majority legislature result in five parties winning seats, and the seat distribution is (42, 33, 9, 9, 7). On closer examination, this seat distribution is isomorphic in terms of parties' possibilities to form winning coalitions to (2, 1, 1, 1, 0), with a simple majority threshold of 3. These *minimum integer weights* now readily expose that the largest party can form a winning coalition with any of the three middle parties, that all three of the middle parties must combine to exclude the largest, and that adding the seats of the smallest party never turns a losing coalition into a winning one. A remarkable real-world case where the distinction between weights and power was apparently overlooked is presented by the early European Economic Community: Under the voting rules in use between 1958 and 1973, Luxembourg's vote could not sway the outcome of any division.²

The present paper experimentally investigates how differences between *real* and *nominal* power affect bargaining behavior and outcomes, focusing on the influential multilateral bargaining model proposed by Baron and Ferejohn (1989). Two decision-making rules are equivalent (imply the same real power) if both generate identical sets of winning coalitions. Any differences between them that do not alter the set of winning coalitions are nominal. In the game under consideration, players hold equal real bargaining power while nominal power may differ. Nominal power differentials are, from the perspective of standard non-cooperative game theory, extraneous to the distribution of equilibrium payoffs and to coalition formation. We refer to the converse idea that nominal differences might matter as *power illusion*.

The effect of nominal power differentials is worth studying for at least two reasons: First, numerous empirical analyses of coalition governments find that payoffs to coalition members like, e.g., ministerial posts, bear an almost proportional relationship to the nominal votes each coalition partner contributes to the coalition (see Warwick and Druckman 2006). These studies seem to support "Gamson's Law", a conjecture named after William Gamson, who asserted that "Any participant will expect others to demand from a coalition

¹The EU's negotiations of the Treaty of Lisbon saw a particularly heated debate on voting rules. See, e.g., *The Economist* (2007, June 14th).

²In the parlance of cooperative game theory, Luxembourg and the smallest party in the above example were *dummy players*.

a share of the payoff proportional to the amount of resources which they contribute to a coalition” (Gamson 1961, p. 361). By contrast, non-cooperative theory predicts only *real* differences as captured by minimum integer weights to be relevant. This contradiction is discussed from an empirical perspective by Cutler et al. (2016). Second, most bargaining or voting situations in the real world are highly complex, and those involved may hence rely on nominal weights as cognitive short-cuts. Therefore, we should study whether this is already the case in environments that are relatively easy to understand.

The experiment reported on here compares two nominally different representations of simple majority rule in a Baron-Ferejohn game with five players. Our results show that, in contrast to the theory, voters significantly respond to the framing of the voting rule. Specifically, being nominally strong benefits proposers, whereas being nominally weak benefits responders. The reason is that strong proposers get higher claims more easily accepted, and weak responders are included into the proposer’s coalition more often. In the aggregate, these effects cancel each other out, so that the distribution of payoffs is not significantly different from the control treatment, where nominal and real power coincide. Surprisingly, we find that subjects continue to use nominal asymmetries as a ‘reference point’ even as they gain experience with the game.

To the best of our knowledge, the only other study of the question whether nominal power will matter is Fréchette et al. (2005a). It includes two treatments, which are nominally different variants of three-player simple majority rule in a Baron-Ferejohn game. They conclude that “There are minor differences in behavior between [...] treatments for inexperienced voters. These differences are, however, no longer present for experienced voters.” (p. 1507). However, the existence and extent of power illusion is likely to depend, among other factors, on the size of the decision-making body and the transparency of the voting rule. This paper provides a robustness check on their result in a richer setup with five players instead of three and greater variation in nominal weights. We compare the results from the two experiments in detail in order to determine the effect of increasing complexity, which is important in view of real world bargaining processes. Another difference to Fréchette et al. (2005a) is that we analyze behavior for each ‘type’ of bargainer (characterized by his nominal weight).

The remainder of the paper is organized as follows: Section 2 describes our experimental design and relates it to the previous experimental literature. The experimental results are reported in Section 3. We conclude in Section 4 and provide additional materials in two appendices.

2 Experimental design

2.1 The bargaining game

Consider a committee comprising n members who decide on how to split a fixed budget normalized to 1. The committee uses a weighted voting rule $[q; w_1, \dots, w_n]$ where w_i is legislator i ’s voting weight, and q is the quota. A coalition $S \subseteq N$ is *winning* iff

$\sum_{i \in S} w_i \geq q$; it is *minimal winning* if it is winning and no $T \subsetneq S$ is winning.

The Baron-Ferejohn model portrays this situation as a sequential non-cooperative bargaining game over multiple periods. It has been used in a wide range of applications, and it has been extended in several directions, e.g., special interest politics, the formation of coalition governments, and the geographic distribution of public expenditures.³ In all variants of the model, a proposer is selected randomly according to a known recognition *protocol*. The two most common assumptions are that recognition probabilities are proportional to players' voting weights, or that recognition probabilities are all equal.⁴ The proposer then puts forward an allocation of resources $(x_1, \dots, x_n) \in \mathbb{R}_+^n$ to the other players, subject to not exceeding the total budget constraint, i.e., $\sum_i x_i \leq 1$. Next, players simultaneously vote the proposal up or down, and the proposal is then either accepted or rejected according to the weighted voting rule. In the closed rule version of the game, no member of the committee can offer an amendment. If the proposal fails, then a new proposer is selected at random with the process being repeated until an allocation is determined.

We focus on the five-player Baron-Ferejohn game under simple majority rule where any coalition which includes at least three players can decide how to divide the pie. The canonical *representation* of this voting rule is $R = [3; 1, 1, 1, 1, 1]$, i.e., each agent has one vote, and three votes are needed to pass a proposal.⁵ R reflects the real bargaining power of the players in a particularly transparent way. We will therefore use R as a baseline in our experiment. Each weighted voting rule, however, has an infinite number of other representations, some of which can be obtained, for example, by multiplying weights and quota by the same positive constant. But in addition, non-homogenous representations exist that lead to the same possibilities for coalition formation and hence leave the theoretical analysis unaffected. In the experimental design, we chose the representation $R' = [18; 9, 8, 7, 6, 5]$ because it has the smallest integer weights such that the weights of all five players differ.

In order to allow for a clean assessment of the potential effect of such a non-transparent representation, our experimental implementation focuses, first, on the equal recognition protocol, i.e., in each round each player has probability $p_i = 1/5$ to be the proposer. By contrast, other protocols such as proportional recognition would have real power implications. Second, we assume that the budget does not shrink if the proposal does not pass. This ensures that the potential exclusion of some players from the winning coalition – and not discounting of future payoffs – is the key determinant of the equilibrium.

As is standard in the literature, we restrict attention to subgame perfect equilibria in stationary strategies (SSPE) as a benchmark prediction.⁶ In such an equilibrium, the

³A recent literature review can be found in Eraslan and McLennan (2013).

⁴The latter is also consistent with an institution such as the Council of the European Union, in which countries' votes count differently, but opportunities for making proposals are, at least formally, equal.

⁵In cooperative game theory, a weighted voting rule is called *homogenous* if it admits a representation $[q^h; w_1^h, \dots, w_n^h]$ such that all minimal winning coalitions have exactly the same total voting weight. Here, R is a homogeneous minimum integer representation. – Note that not all weighted voting rules allow homogeneous representations, nor are homogeneous representations necessarily unique. The problem how to apply the Baron-Ferejohn framework with non-homogenous weighted voting rules is so far unresolved.

⁶Stationarity rules out any dependence of the agents' strategies on the history of play. Baron and Ferejohn (1989) argue that, while any outcome can be supported as a subgame perfect equilibrium if

proposer’s offers to the other members of the committee are such that those voting for the proposal are exactly indifferent between voting for and against, and he keeps the residual for himself. Voters’ fear of being excluded in future proposals affords a great advantage to the proposer, even in the absence of time discounting. In our five-player game the outcome prediction of the unique SSPE is that the proposer allocates $3/5$ of the pie to himself, and the reservation value of $1/5$ to each of two other agents. Two agents are left out of the minimal winning coalition and receive nothing. The proposal is approved without delay. Ex ante, i.e., before a proposer has been selected, expected payoffs equal, of course, $1/5$ for each player.

2.2 Related literature

There have been a number of experiments that have aimed at testing the Baron-Ferejohn model, dating back to McKelvey (1991). Generally, several features of the model are qualitatively supported by the laboratory results, namely, infrequent delay and high frequency of minimal winning coalitions. Observed proposer power is not nearly as strong as theoretically predicted; proposers typically enjoy some advantage over other members of the coalition, but extract rents only partially.⁷ Generally, play in experimental bargaining games consistently deviates from subgame perfect equilibrium predictions in favor of a more equal distribution of benefits between players.⁸ Evidence of egalitarian behavior has been observed by Fréchet et al. (2003). Fréchet (2009) provides a re-analysis of these data in the light of a belief-based learning model. A recent study by Nunnari and Zapal (2016) reexamines data from several experiments on Baron-Ferejohn bargaining. It explains the observed deviations from the predictions of standard noncooperative game theory in terms of imperfect best responses and players’ incorrect beliefs about their chance to become proposer in the future. The effect of majority versus unanimity rule has been tested in Miller and Vanberg (2013). Miller and Vanberg (2015) additionally consider greater groups with five and seven players. They report significantly more costly delay under unanimity rule and in large groups.

There are very few other experimental studies that have introduced nominal weights into the Baron-Ferejohn model. As mentioned above, only Fréchet et al. (2005a) test real versus nominal voting power. In particular, they compare $[50; 33, 33, 33]$ (“equal-weight-equal-selection” or EWES) to $[50; 45, 45, 9]$ (“unequal-weight-equal-selection” or UWES). Additionally, the authors consider the case in which players are selected as proposers in proportion to their voting weight, and the effect of discounting. The result that is of

players are sufficiently patient, the stationary equilibrium is a focal point.

⁷However, Agranov and Tergiman (2014) and Baranski and Kagel (2015) show that allowing subjects to engage in (cheap-talk) communication before a proposal is submitted brings experimental results much closer to the theoretical prediction. Kagel et al. (2010) find that adding veto rights substantially augments proposer power.

⁸These deviations are commonly explained in terms of moral motivations such as a desire to follow norms of fairness, which lead players to not focus exclusively on their individual monetary payoff. See, e.g., Fehr and Schmidt (1999). However, Montero (2007) shows that incorporating other-regarding preferences into the Baron-Ferejohn model makes the equilibrium division more *unequal*.

particular interest in our context is that the nominal change in the number of votes between EWES and UWES had no effect on the distribution of payoffs. Rather, payoffs continued to exhibit proportionality to the real weights, in line with the game theoretic bargaining models. Moreover, the composition of winning coalitions was not found to be biased in favor of players with weak nominal bargaining power. Fréchette et al. (2005b) compare the predictions of the Baron-Ferejohn model and a demand bargaining model (see Morelli 1999) with Gamson’s Law in a three-player majority setting. For the Baron-Ferejohn game, this study and Fréchette et al. (2005a) draw on the same data, so conclusions regarding nominal power do not differ.

Fréchette et al. (2005c) reports on Baron-Ferejohn and demand bargaining with five players in both an equal weights treatment, i.e., $[3; 1, 1, 1, 1, 1]$, and in the apex treatment $[4; 3, 1, 1, 1, 1]$. Here, one player has disproportionate real voting power so that this study is not a test of nominal versus real voting power. Diermeier and Morton (2005) investigated a finitely repeated variant with three-member groups and varied, one, the share of votes that each subject controlled, and, two, subjects’ recognition probabilities. In all treatments there was a total of 99 votes, and a threshold of 50 votes for approval. Vote assignments in their three treatments were (34, 33, 32), (49, 33, 17) and (46, 44, 9), respectively. All three are equivalent to the situation where each player has one vote, and a coalition of at least two players can pass a proposal. Still, in contrast to our paper, Diermeier and Morton (2005) do not study purely nominal power differentials. The reason is that they base the probability of being selected as a proposer on the subject’s percentage of votes, so that theoretical predictions differ across their treatments. The authors find little support for the predictions of the model as proposers allocate money to all players rather frequently and do not exploit their proposal power. But neither did a proportionality norm based on the vote share explain the experimental data well. Diermeier and Morton conclude that subjects’ behavior is best accounted for by an equal sharing rule, where the proposer chooses any winning coalition and then distributes payoffs equally among the coalition members. Drouvelis et al. (2010) conduct an experiment with the primary motivation to test the hypothesis that adding a new player to a weighted voting game could increase the voting power of an original player. Yet, one of their treatments (‘symmetric’) uses the three-person simple majority game $[4; 3, 2, 2]$, where one player is nominally stronger than the other two.⁹ Results from this treatment provide no evidence for the existence of power illusion. For example, differences in earnings or in the acceptance behavior between the ‘strong’ and the ‘weak’ players are insignificant.

Furthermore, this work is related to studies that have introduced other asymmetries into the Baron-Ferejohn framework. In theoretical work, Snyder et al. (2005) extend the Baron-Ferejohn framework to accommodate general weighted voting rules. Excluding situations where real and nominal power may diverge, they use large replicated games to show that voters’ expected payoffs are – in the limit – proportional to voting weights, at least under the assumption of proportional recognition probabilities. However, this result does

⁹This is compared to treatment $[5; 3, 2, 2]$, where one player can block any decision, and to the ‘enlarged’ treatment $[5; 3, 2, 2, 1]$.

not necessarily carry over to the small games usually found in applications (see Montero 2016). In a recent paper, Miller et al. (2015) study theoretically and experimentally the effects of exogenous heterogeneity in what players receive if negotiations break down. The equilibrium predictions differ starkly, depending on whether the decision is taken under unanimity or majority rule. Specifically, a player’s expected equilibrium payoff is increasing in her disagreement value under unanimity rule, but either decreasing or non-monotone under any less-than-unanimity rule. On the whole, experimental outcomes provide qualitative support for these model predictions. In a similar vein, Diermeier and Gailmard (2006) studied a take-it-or-leave-it setting where disagreement values differ between players.

We will compare our experimental results with these earlier studies, as well as with the broad experimental literature on the Baron-Ferejohn model, in the concluding section of the paper.

2.3 Experimental procedures

In each bargaining period of an experimental session groups of five subjects had to split 150 tokens among themselves by simple majority rule. In the *control treatment* (CT) subjects were each given a voting weight of 1. In the *power illusion treatment* (PIT), subjects were assigned to the voting weights $\{5, 6, 7, 8, 9\}$ at random. In each treatment, 50 participants took part in the experiment. Thus, 100 subjects participated in total. We had two sessions per treatment with 25 participants in one session, so that there would be five groups bargaining simultaneously in each session. Each participant played 10 periods. Table 1 summarizes information on the treatment conditions.

To minimize repeated game effects, subjects were randomly rematched into groups between periods (but not between the rounds within a given bargaining game) without having the possibility to learn others’ identity. Matching in PIT was subject to the restrictions (i) that each five member group contained one player of each weight type, and (ii) that each subject assumed each of the five weight types twice over the course of the ten periods.

Table 1.

Treatment conditions, sessions and subjects

Treatment	Representation $[q; w_1, \dots, w_n]$	# sessions	# subjects	# matching groups	# periods
CT	$[3; 1, 1, 1, 1, 1]$	2	50	10	10
PIT	$[18; 9, 8, 7, 6, 5]$	2	50	10	10

Within each game, the sequence of events was as follows. First, each subject was randomly assigned her voting weight. Subjects got to know their own voting weight as well as the voting weights of the other four group members and the quota. Each player was prompted to enter her proposal on how to allocate the 150 Tokens, i.e., to specify a vector (x_1, \dots, x_5) of integer token amounts, with no waste allowed. Others’ weight-types

were displayed in randomized order to avoid anchoring effects. One of the five proposals was randomly chosen (with probability $1/5$ for each) and displayed to all group members. The subject whose proposal was selected was not given the possibility to vote upon it; her approval was taken as granted. The remaining four players simultaneously voted in favor or against the standing proposal. If the proposal obtained a simple majority, the proposed distribution became binding, and that period was concluded for the five group members. If the proposal failed, a new round of the same game was initiated, where one player – possibly the same as before – was randomly selected (with probability $1/5$) to make a proposal. This was repeated until an allocation was achieved, with no shrinkage in the amount of tokens to be allocated.

Following each vote, detailed feedback was displayed to the subjects within each group. This consisted of the proposed distribution of benefits, the proposer’s weight type, the individual votes cast by each player together with their voting weight, and whether the distribution passed or failed. If the proposal passed, subjects were informed about their individual payoff in that game. To control for income effects, one period was randomly chosen at the end of the session to be paid off in private. The exchange rate was 1 token = 0.20 €. Subjects also received a 7 € show-up fee. Earnings averaged 13 € and ranged from a minimum of 7 € to a maximum of 20 €.

We conducted all four sessions in the Experimental Lab at the University of Hamburg, Germany, using z-tree (Fischbacher 2007). The experiment took place between March and April 2016. Participants were mostly undergraduate students from various disciplines, recruited via the administration software h-root (Bock et al. 2014) from an extensive subject pool. They were given copies of the instructions (see Appendix B), and the instructions were read aloud; all questions were answered in private. Participants were told that sessions would last approximately 1 - 1.5 h. All sessions lasted close to 1.5 h, including time for the instructions. A questionnaire completed the session. The questionnaire collected individual characteristics that included participants’ gender, age, field of study, and experience in laboratory experiments.

2.4 Conjectures

It is well-known that experimental outcomes typically differ from the theoretical point predictions of the alternating offers bargaining model, assuming that players maximize material payoffs. Here we will be interested instead in how the introduction of nominal voting weights affects players’ behavior as proposers and responders, and expected payoffs. Theory asserts that it should have no effect, irrespective of the presence or absence of social preferences. Therefore, we refer to our following expectations as conjectures rather than hypotheses. The design described above allows us to test the following conjectures about players’ strategic behavior:

CONJECTURE 1 (Proposers).

(a) *The share that a player allocates to himself increases in her voting weight.*

- (b) *A proposer allocates more to a member of the coalition the greater the latter’s voting weight.*

CONJECTURE 2 (Responders).

A players’ acceptance threshold is higher the larger her weight.

CONJECTURE 3 (Coalitions).

- (a) *Players are less often included in others’ coalitions the larger their weight.*
(b) *Coalitions have fewer members in PIT than in CT.*

Conjecture 3(a) follows as a corollary from Conjecture 2. The reasoning behind Conjecture 3(b) is that in CT absolutely no distinctions between players exist. We conjecture that perfect symmetry facilitates universal coalitions, whereas different numbers of votes in PIT might be used to motivate exclusion of some players.

In their experiment, Fréchette et al. (2005a) find minor differences between the “equal-weight-equal-selection” treatment (representation [50; 33, 33, 33]) and the “unequal-weight-equal-selection” treatment (representation [50; 45, 45, 9]) for inexperienced subjects. These differences vanished, however, for experienced subjects. This leads us to the last conjecture:

CONJECTURE 4 (Experience).

The differences described above will no longer be present as bargainers become more experienced.

In view of this conjecture, we will distinguish between inexperienced and experienced subjects in the following presentation of experimental outcomes. We classify subjects as “experienced” during the last 5 periods, and as “inexperienced” during the first 5 periods.¹⁰

3 Results

We first present our experimental results regarding subjects’ behavior along the structure of Conjectures 1–3 above. Second, we describe payoffs earned. And third, we analyze how findings change when subjects gained experience (Conjecture 4).

3.1 Behavior

3.1.1 Proposers

Figure 1a shows the amount of tokens that proposers are predicted to allocate to themselves by treatment and subjects’ experience.¹¹ Clearly, proposers’ claims fall short throughout

¹⁰We tried several alternative classifications and obtained very similar results.

¹¹The figure and results in Subsection 4.1.1 use *all* proposals, selected or not, in the first round of each period. Predictions are based on GLS random effects models, see Table A1 in Appendix A. Coefficients on socio-demographic controls are reported in Table A2. We estimated all regressions without socio-demographic controls and obtained virtually identical results.

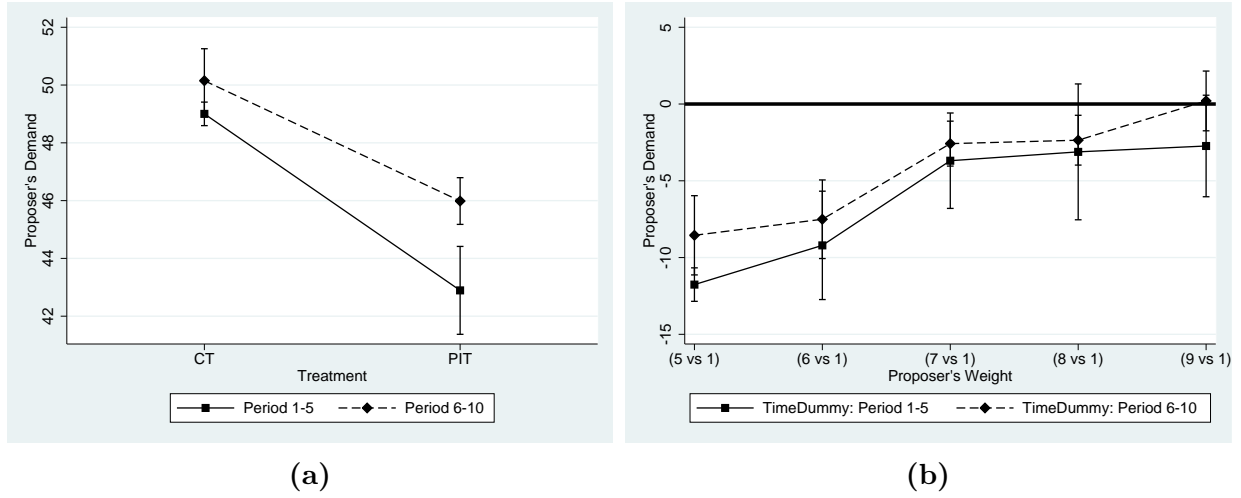


Fig. 1. Proposer offers to self. Solid lines refer to inexperienced subjects, broken lines to experienced subjects. Error bars represent 90% confidence intervals. Panel (a) compares CT and PIT. Panel (b) contrasts offers to self in PIT with CT by proposer’s weight-type.

of the model’s prediction. This will be explained shortly by responders’ behavior (see Sect. 3.1.2); as responders reject proposals close to the SSPE at a very high rate, modest self offers reflect expected utility maximization. Yet, proposers learn to keep more to themselves over time, a well-established finding in experimental alternating offers games. Compared to the control treatment both inexperienced and experienced proposers offer significantly less to themselves when weights nominally differ. The average treatment effect is about -5 tokens and highly significant (see Table A1, Panel A). As illustrated by Figure 1b, this difference is mostly due to the fact that weight-5, weight-6 and weight-7 players claim substantially less for themselves than the average proposer with voting weight 1 in the control treatment (thick solid reference line). Looking at weight-8 and weight-9 proposers only, we find almost no difference between the two treatments.

With respect to Conjecture 1(a) Figure 1b suggests that proposers tend to offer more to themselves when they were assigned larger weight. Table 2 reports the mean predicted self offers for proposers of different voting weight, averaged over all periods and splitted by experience. We find that small weight players allocate themselves substantially less than do proposers with larger weight. For example, the weight-6 player is predicted to claim on average 5.22 tokens less than the weight-7 proposer. Overall, mean self offers in PIT markedly deviate from the null hypothesis that they are uniform; corresponding χ^2 statistics are reported at the bottom of Table 2.

Result 1: *Proposers’ self offers depend on their nominal power. In particular, small weight players are significantly more modest in their claims.*

We next analyze the offers that proposers make to others. Averaging over all bargaining periods, and focusing on non-zero offers, Figure 2 illustrates that, in line with our Conjecture 1(b), the average amounts offered are larger the more weight a responder wields.

Table 2.

Linear predictions of proposers' self offer, by proposer type

	All Periods		Periods 1-5		Periods 6-10	
	Margin	Contrast	Margin	Contrast	Margin	Contrast
weight-1	49.57 (0.43)	—	49.00 (0.25)	—	50.15 (0.68)	—
weight-5	39.42 (0.57)	-1.80**	37.24 (0.44)	-2.56*	41.60 (1.51)	-1.04***
weight-6	41.22 (0.29)	(0.76)	39.80 (1.97)	(1.54)	42.64 (1.52)	(0.06)
		-5.22***		-5.51***		-4.93***
weight-7	46.44 (0.60)	(0.35)	45.31 (1.71)	(0.26)	47.57 (0.59)	(0.95)
		-0.4		-0.58		-0.22
weight-8	46.84 (1.49)	(0.91)	45.88 (2.52)	(0.81)	47.79 (0.48)	(1.01)
		-1.47		-0.38		-2.56***
weight-9	48.31 (0.72)	(2.17)	46.27 (2.13)	(4.63)	50.34 (0.75)	(0.29)
χ^2		43.85		23.81		19.33
p		0.000		0.001		0.001

Notes. Predicted mean amounts (Tokens). Standard errors in parentheses. Contrasts between two adjacent margins are marked with asterisks if significant (χ^2 -test): *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table 3 provides for a closer look on how proposer and responder type impact on proposals. Non-zero offers, shown in Panel A, increase virtually monotonically in respondent's weight for all types of proposer; as in Figure 2 effects are somewhat stronger during the first five bargaining rounds. The χ^2 -columns indicate that proposers with large voting weight are particularly discriminating, whereas offers by proposers with small weight are more egalitarian, especially in the beginning.

Expected offers, i.e., including zero-offers, are reported in Panel B of Table 3. We observe an interesting reversal when it comes to predictions of the expected offers that are made by small weight proposers. These change from initially being uniform across responders' types towards a strong bias against responders with large weight. This is due to differences in coalition formation between small weight and large weight proposers which will be explored in more detail in Sect. 3.1.3.

Result 2: *The asymmetry in PIT has distributional consequences at both experience levels as proposers condition their offers on responders' weight in line with Conjecture 1(b).*

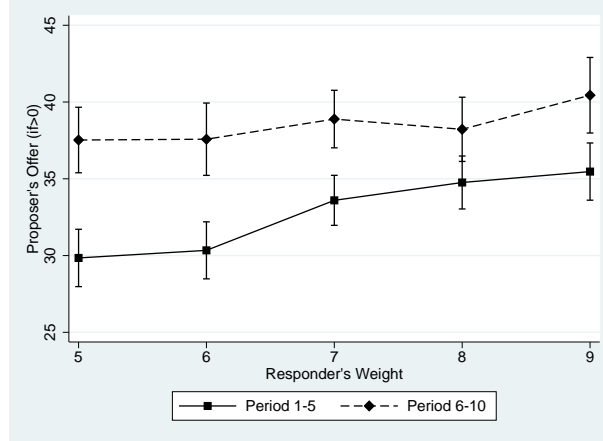


Fig. 2. Non-zero offers to others in PIT by responder’s weight. Solid lines refer to inexperienced subjects, broken lines to experienced subjects. Error bars represent 90% confidence intervals.

3.1.2 Responders

We expect responders to apply higher thresholds for accepting an offer the greater their weight. Yet, players’ acceptance thresholds are not observed in our experiment.¹² As an alternative approach, we estimate regression models in which the dependent variable is the decision to vote ‘yes’.

Predictions based on random effects probit models are given in Table 4.¹³ These allow us to contrast responders’ inclination to vote ‘yes’ across treatments, dependent on the amount offered. As one would expect, responders are always more likely to accept the greater the offer, irrespective of the treatment. Yet, we find that responders’ react significantly more strongly towards how much they are offered when power nominally differs.¹⁴ During the first five bargaining rounds, the average probability to vote ‘yes’ is identical across treatments (50.6% in CT vs. 51.4% in PIT, $\chi^2 = 0.05$, $p = 0.8199$), and the distribution of responders’ decision to vote ‘yes’ in PIT can be described as a mean-preserving spread of that in CT. During the last five rounds, the responders’ acceptance rate in CT is higher than in PIT at all levels of offer, resulting in a significantly greater mean (55.6% in CT vs. 48.4% in PIT, $\chi^2 = 3.54$, $p = 0.0598$). We interpret this as an indication of increased conflict in PIT.

¹²Alternatively, we could have asked subjects to state an acceptance threshold. However, this approach forces subjects to use a cutoff strategy that depends *only* on their own payoff.

¹³This table and Table A6 are based on *all* voting decisions, after first or later rounds.

¹⁴Regressions which we do not report here to avoid redundancy show that the increase in responsiveness goes back in particular to weight-5 and weight-9 players. Results are available from the authors upon request. – Unsurprisingly, responders are less likely to accept the more the proposer suggests to keep for himself. The effect of proposer’s share on responders’ probability to vote ‘yes’ does not vary between our treatments or across time. Proposer’s weight in PIT does not affect responders’ decision to accept, see Appendix A, Tables A6 and A7.

Table 3.

Linear predictions of proposer’s offers: impact of voting weights

Periods 1-5							Periods 6-10					
Panel A: Offer conditional on being non-zero												
5	—	31.75 (1.37)	31.85 (1.33)	31.68 (1.26)	32.57 (1.58)	2.38 0.498	—	38.71 (1.59)	38.42 (1.57)	36.11 (1.77)	39.73 (1.73)	7.52 0.057
6	30.89 (1.69)	—	31.54 (1.65)	31.57 (1.19)	31.64 (1.36)	1.24 0.743	38.69 (1.46)	—	39.09 (1.40)	36.20 (1.55)	37.56 (1.73)	7.07 0.070
7	28.63 (2.30)	28.50 (1.99)	—	37.73 (1.62)	37.18 (1.83)	21.05 0.000	38.48 (1.85)	37.81 (1.92)	—	39.90 (1.81)	41.06 (2.33)	1.50 0.683
8	30.88 (1.95)	30.61 (1.83)	36.28 (1.65)	—	40.36 (2.05)	14.39 0.002	34.86 (1.94)	35.89 (2.12)	40.05 (1.85)	—	42.80 (1.99)	8.77 0.033
9	28.85 (1.79)	30.22 (1.72)	35.02 (1.65)	38.08 (1.68)	—	27.16 0.000	37.46 (1.85)	37.19 (2.10)	37.98 (1.77)	40.52 (1.57)	—	11.55 0.009
χ^2	1.02	2.98	8.54	22.26	20.98		4.95	1.90	1.64	11.05	6.64	
p	0.796	0.395	0.036	0.000	0.000		0.176	0.594	0.650	0.012	0.084	
Panel B: Expected offer												
5	—	27.56 (3.43)	27.67 (3.43)	26.36 (3.44)	27.18 (3.44)	0.09 0.993	—	28.71 (3.48)	29.94 (3.47)	16.10 (3.59)	18.46 (3.58)	12.15 0.007
6	28.71 (3.43)	—	28.29 (3.43)	22.56 (3.48)	24.54 (3.47)	2.26 0.521	33.90 (3.44)	—	35.15 (3.43)	10.61 (3.66)	11.39 (3.66)	44.74 0.000
7	18.78 (3.51)	17.58 (3.52)	—	29.49 (3.46)	27.82 (3.47)	9.35 0.025	23.79 (3.51)	26.58 (3.48)	—	17.17 (3.59)	17.74 (3.59)	5.19 0.158
8	11.42 (3.62)	14.40 (3.57)	29.62 (3.45)	—	35.79 (3.43)	34.18 0.000	17.48 (3.56)	13.34 (3.63)	25.07 (3.51)	—	27.46 (3.50)	10.36 0.016
9	16.68 (3.53)	16.04 (3.55)	28.30 (3.46)	30.97 (3.45)	—	14.98 0.002	24.24 (3.50)	19.27 (3.55)	17.27 (3.58)	21.56 (3.54)	—	2.20 0.533
χ^2	13.03	8.84	0.17	3.50	6.05		11.47	11.88	14.24	4.73	10.45	
p	0.005	0.032	0.982	0.321	0.109		0.009	0.008	0.003	0.192	0.015	

Notes. The table refers to all first-round proposals in each period. Standard errors in parentheses.

Table 5 compares the predicted acceptance rates at an offer of 30 tokens between any two different types of responder within PIT. Controlling for the kind of offer being considered and sociodemographic characteristics, we find that responders with large voting weight are less likely to vote ‘yes’ than small weight responders. For example, the difference between weight-5 and weight-9 players is 23 percentage points in the earlier bargaining rounds. This pattern in individual voting behavior is largely persistent over time, but becomes less pronounced as players become more experienced. Again, weight-7 players are special in that they tend to accept proposals more easily than do other types. Table A5 in the appendix contains a full set of predictions when the amount offered to the responder varies.

Figure 3 compares the predicted distribution functions of accepted offers for the nominally weakest and strongest type of player. Clearly, accepted offers for both levels of experience are smaller for weight-5 responders than for weight-9 responders. Half of the acceptances by weight-5 players were for offers less than or equal to 29 (31) tokens for the first (last) five rounds, whereas for the weight-9 players half of acceptances were for offers less than or equal to 34 (36). Functions are located further to the right for both types during the later bargaining periods.

Result 3: *Responders are less likely to accept a proposal when they have large voting weight, other things being equal. This result lends support to our Conjecture 2. In addition,*

Table 4.

Predictions of responder's probability of voting 'yes': treatment effect

Offer to Responder	Period 1-5			Period 6-10		
	CT	PIT	Contrast	CT	PIT	Contrast
0	7.5 (2.9)	0.1 (0.2)	7.3** (2.9)	7.4 (3.1)	1.5 (1.1)	5.8* (3.3)
10	17.4 (4.0)	2.2 (1.7)	15.1*** (4.4)	19.3 (4.7)	7.3 (3.1)	12.0** (5.7)
20	33.0 (4.4)	15.8 (4.8)	17.2*** (6.5)	38.7 (5.1)	22.5 (5.0)	16.2** (7.3)
30	52.2 (4.2)	49.7 (5.4)	2.5 (6.9)	61.3 (4.6)	47.5 (5.2)	13.8** (7.0)
40	70.8 (4.0)	83.5 (4.4)	-12.7** (6.0)	80.5 (3.7)	73.4 (4.3)	7.1 (5.7)
50	84.9 (3.5)	97.2 (1.6)	-12.3*** (3.9)	92.2 (2.5)	90.3 (2.9)	1.9 (3.7)
χ^2	7088.82			57.54		
p	0.000			0.000		

Notes. The table reports mean probabilities (in %) of voting 'yes'. Standard errors in parentheses. Contrasts between two margins are marked with asterisks if significant (χ^2 -test): *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

we conclude that nominal power differentials lead to more conflict among players.

3.1.3 Coalition formation

Theory predicts that the probability of a non-zero offer to others is 50%, i.e., proposers seek to build minimum winning coalitions by making an offer to any two of the other four players. Yet, it is not unusual in experimental bargaining that proposers propose to include considerably more players into their coalition. Looking at all first-round proposals in each period, we find that players are even more inclined to make universal offers in PIT than in CT (80.9% and 73.3%, averaged over all periods). Accordingly, the amount offered to others, if greater than zero, is smaller in PIT compared to CT. Table 6 reveals that it is predominantly weight-5 and weight-6 proposers who account for the great number of non-zero offers.

The nominal asymmetry in PIT also affects the composition of coalitions: Table 6 shows that, for all types of proposer, responders of weight 7, 8 and 9 receive significantly more frequently an offer than responders of weight 5 and 6 during the earlier bargaining periods. In later bargaining periods, by contrast, weight-8 and weight-9 responders are offered

Table 5.

Probability to vote ‘yes’: pairwise comparison of voting weights

Responder’s weight	Period 1-5				Period 6-10			
	6	7	8	9	6	7	8	9
5	-3.1 (11.5)	0.7 (11.3)	-12.2 (11.4)	-23.0** (10.7)	6.8 (21.1)	2.4 (21.0)	-7.8 (20.5)	-17.1 (21.4)
6	—	3.8 (12.3)	-9.1 (12.2)	-19.9* (11.6)	—	-4.4 (14.5)	-14.6 (13.4)	-23.9 (15.1)
7		—	-12.9 (11.8)	-23.7** (11.0)		—	-10.2 (13.6)	-19.5 (14.9)
8			—	-10.8 (11.2)			—	-9.3 (13.9)

Notes. The table reports percentage point differences in responders’ probabilities to vote ‘yes’ at an offer of 30 tokens. Differences are marked with asterisks if significant (two-tailed t -test). Standard errors in parentheses: *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

coalition membership significantly less often compared to each lower-type responder.¹⁵ On the whole, the probability of weight-7 players to get an offer is about 10 percentage points greater than that of other types, and these differences are highly significant. Consistent with our Conjecture 3(a), the null hypothesis that different types of responders have equal probability to be receive an offer can be rejected ($\chi^2 = 20.88$, $p = 0.0003$).

During earlier bargaining periods, this divergence can be traced back to large weight proposers, who offer coalition membership more often to other large weight players. In later periods, we observe that small weight responders are preferred as coalition partners; this holds especially for proposers who have small weight themselves. This preference is fully rational in face of small weight players being less demanding responders compared to ‘large’ players.

Result 4: *Responder types vary with respect to receiving a non-zero offer. While inexperienced proposers include large weight responders too frequently, experienced proposers strongly rely on small weight responders as coalition partners.*

We next consider adopted proposals rather than first-round proposals. Figure 4 illustrates the absolute size of coalitions, including the proposer, by treatment and time. While still not consonant with the SSPE prediction, coalitions tend to have fewer members in PIT than in CT – 3.38 and 3.53 members, respectively, and the difference is marginally

¹⁵Over periods 1-5, differences in the probability to receive an offer are as follows: (7 vs. 5) 0.126, $p < 0.01$, (8 vs. 5) 0.091, $p < 0.01$, (9 vs. 5) 0.106, $p < 0.01$, (7 vs. 6) 0.128, $p < 0.01$, (8 vs. 6) 0.094, $p < 0.01$, (9 vs. 6) 0.109, $p < 0.01$. Over periods 6-10, we obtain: (8 vs. 5) -0.152, $p < 0.01$, (9 vs. 5) -0.128, $p < 0.01$, (7 vs. 6) 0.070, $p = 0.083$, (8 vs. 6) -0.108, $p = 0.021$, (9 vs. 6) -0.084, $p = 0.067$, (8 vs. 7) -0.177, $p < 0.01$, (9 vs. 7) -0.153, $p < 0.01$. Other differences are not significant at conventional levels under a two-tailed t -test.

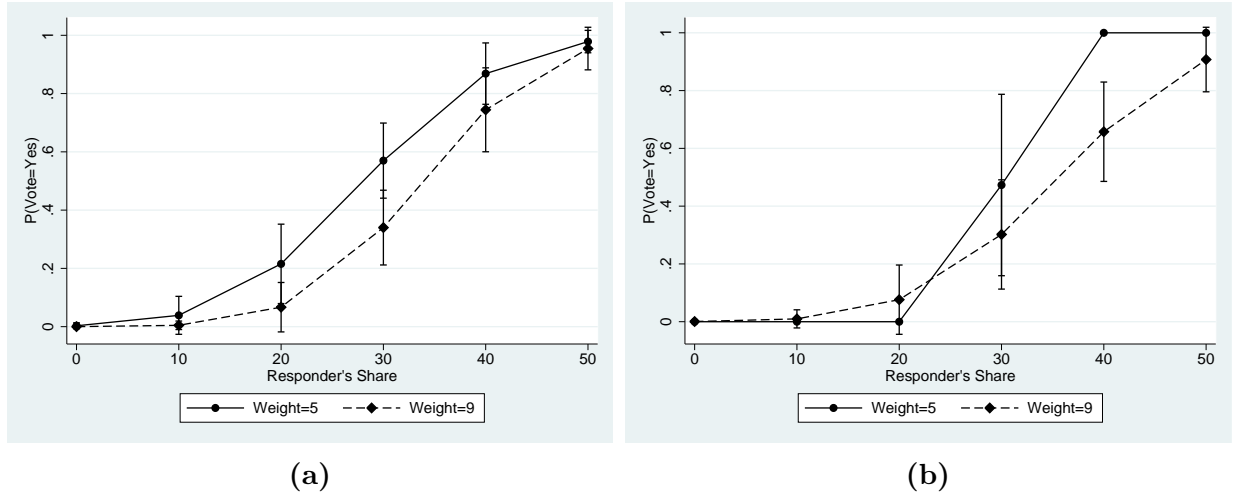


Fig. 3. Acceptance behavior of weight-5 and weight-9 players. Panel (a) and (b) show the probability of voting ‘yes’ given the offered allocation during the five earlier and five later periods, respectively.

significant ($p = 0.0691$, using a one-tailed t -test). Given that first-round proposals included more players in PIT than in CT, this points towards a more competitive behavior when perceived asymmetries exist. Moreover, in line with our observations on first-round proposals in PIT, the relative weight of the winning coalition, i.e., the combined voting weight of the coalition members as a share of the sum of all weights, is smaller in PIT than in CT. The averages are not yet distinct for inexperienced players (69.8% in PIT vs. 72.4% in CT, $p = 0.4080$ using a two-tailed t -test), but differ significantly for experienced players (64.1% vs. 68.8%, $p < 0.1$ using a two-tailed t -test).¹⁶

Result 5: *The size of coalitions is smaller under PIT, both absolutely and in terms of relative coalition weight. We interpret the decrease as reflecting a tendency towards more strategic behavior in face of the behavioral biases induced by nominal differences.*

3.2 Payoffs

Proposers’ average payoffs do not differ sharply between treatments, although the gap widens during the later bargaining periods (see Figure 5a). Figure 5b breaks proposers’ earnings in PIT down by weight. While these do not differ consistently from CT (thick solid reference line) for any type of proposer, the graph hints at a special and unexpected role of weight-7 players. The graphic impression is reflected in Table 7, which contains predicted mean payoffs for proposers of different weight. While not monotonic, earnings are mostly greater for proposers who wield larger weight, and the null hypothesis of a uniform distribution in proposers’ payoffs can be rejected. In particular, weight-5 and

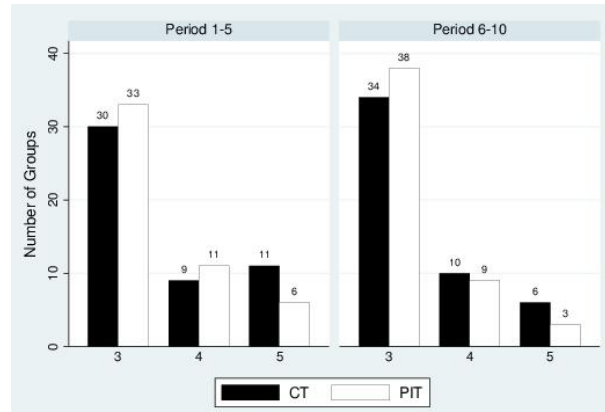
¹⁶These numbers still deviate from the theoretical prediction that proposals in both treatments should be passed by a coalition with a relative weight of 60%.

Table 6.

Linear predictions of probability of non-zero offer: impact of voting weights

Proposer's weight	Periods 1-5						Periods 6-10					
	Responder's weight					χ^2 p	Responder's weight					χ^2 p
	5	6	7	8	9		5	6	7	8	9	
5	—	95.5 (2.8)	95.5 (2.8)	94.1 (3.3)	94.1 (3.3)	0.26 0.968	—	86.7 (5.1)	88.1 (4.8)	69.0 (7.6)	69.0 (7.7)	10.06 0.018
6	97.1 (2.1)	—	95.9 (2.6)	85.9 (5.2)	89.9 (4.4)	5.91 0.116	93.6 (3.4)	—	95.0 (3.0)	57.0 (8.5)	57.0 (8.5)	35.26 0.000
7	83.0 (5.8)	81.2 (6.1)	—	90.3 (4.3)	88.7 (4.7)	2.58 0.460	76.9 (6.8)	82.8 (5.9)	—	63.9 (8.1)	63.9 (8.1)	7.02 0.072
8	62.2 (8.1)	68.8 (7.6)	92.1 (4.0)	—	95.0 (3.0)	23.90 0.000	72.0 (7.2)	63.0 (8.0)	80.2 (6.3)	—	79.3 (6.3)	4.63 0.201
9	79.0 (6.4)	77.4 (6.7)	91.8 (3.9)	91.8 (3.9)	—	7.46 0.059	78.9 (6.5)	70.7 (7.5)	66.2 (8.0)	70.6 (7.5)	—	2.23 0.527
χ^2	24.79	17.27	1.50	2.14	2.15		12.06	9.70	15.58	2.20	6.43	
p	0.000	0.001	0.682	0.544	0.542		0.005	0.021	0.001	0.532	0.093	

Notes. The table refers to all first-round proposals in each period. Standard errors in parentheses.

**Fig. 4.** Absolute size of winning coalitions in CT and PIT.

weight-6 proposers fare significantly worse than all other types at both levels of experience, with particularly poor outcomes for weight-6 players. We report corresponding regression results from GLS random effects models in Panel B of Table A1 in the appendix.

Result 6: *The more modest claims that small weight players make translate into lower payoffs. With respect to proposers, nominal power differences thus affect the distribution of final earnings.*

Similar to the above results on proposers' earnings, we do not find a significant difference in responders' payoffs between CT and PIT. This is illustrated in Figure 6. The left-hand graph, mirror-inverting Figure 5a, shows that responders' receive less in later rounds. The right-hand graph contrasts payoff in PIT for each type of responder with the average responder payoff in CT (thick solid reference line). While differences across responder

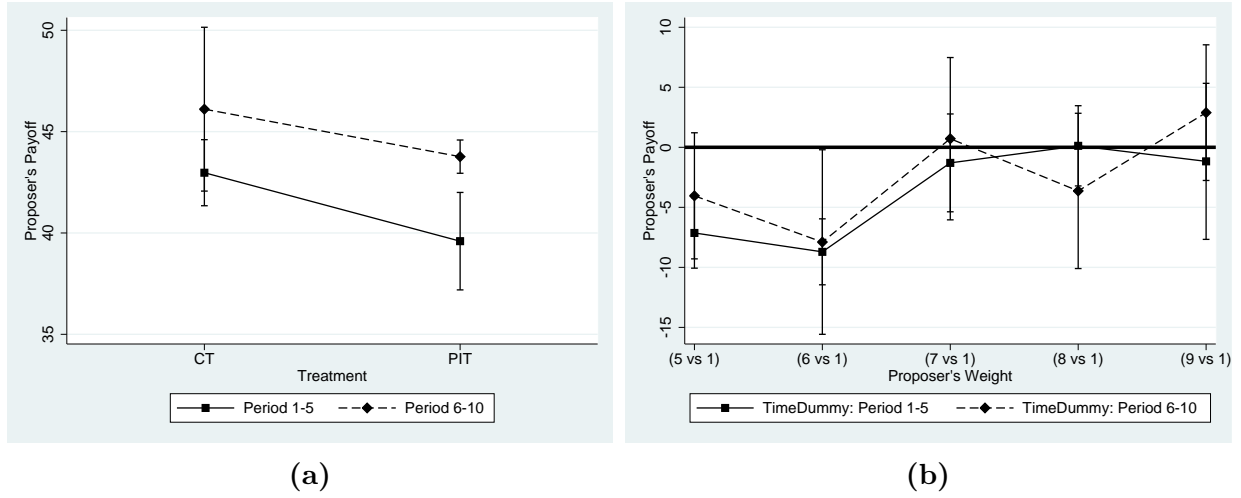


Fig. 5. Proposer's payoff. Solid lines refer to inexperienced subjects, broken lines to experienced subjects. Error bars represent 90% confidence intervals. Panel (a) compares CT and PIT. Panel (b) contrasts payoffs in PIT with CT by proposer's weight-type.

types are not statistically significant,¹⁷ it is still interesting to see that weight-7 responders achieve sustained high payoffs, whereas responders with larger weight earn markedly less in later bargaining periods.

We cannot reject the null hypothesis that responders' expected payoffs are uniform across weight types. Table 8 reveals that, in later bargaining rounds, weight-8 and weight-9 responders fare significantly worse than responders with lower weight. These low expected values can be traced back to the fact that high weight responders receive very few offers as players become more experienced, while at the same time remaining reluctant to accept offers (see Table 6).

Result 7: *Nominal power differentials impact on players' expected payoffs in their role as responders. In particular, 'strong' players are negatively affected.*

3.3 Experience

Finally, we explicitly study the role of experience in our experiment. Generally, we observe a number of major discrepancies between the theoretically predicted and observed bargaining behavior, and experience does little to diminish these, as has been noted in prior work (see, for example, Fr chet te et al. 2003). For example, the average self offer of experienced subjects is 33.4% (=50.15 tokens) and inexperienced ones take 32.7% in CT. The corresponding numbers in PIT are 30.7% and 28.6%, while the Baron-Ferejohn model predicts that this share should be 3/5.

The most substantial change within PIT as subjects gain experience is that they come to select coalition partners to whom they ascribe the smallest "reservation prices". And

¹⁷For further details, see also Table A8 in Appendix A.

Table 7.

Linear predictions of proposers' payoff, by proposer type

	All Periods		Periods 1-5		Periods 6-10	
	Margin	Contrast	Margin	Contrast	Margin	Contrast
weight-1	44.52 (1.75)	—	42.90 (1.00)	—	46.14 (2.49)	—
weight-5	38.94 (0.58)	2.71 (1.66)	35.77 (1.87)	1.57 (2.19)	42.10 (0.90)	3.85 (5.20)
weight-6	36.22 (2.19)	-8.01***	34.20 (0.67)	-7.41***	38.25 (4.44)	-8.61***
weight-7	44.23 (2.53)	(0.56) 1.46	41.61 (1.88)	(1.69) -1.42	46.86 (3.11)	(1.30) 4.35
weight-8	42.77 (1.96)	(4.40) -2.61	43.03 (1.55)	(3.17) 1.29	42.51 (2.53)	(5.79) -6.52**
weight-9	45.38 (2.23)	(4.05)	41.73 (3.81)	(5.19)	49.03 (1.05)	(3.01)
χ^2		15.98		9.05		9.02
p		0.003		0.060		0.061

Notes. Predicted mean amounts (Tokens). Standard errors in parentheses. Contrasts between two adjacent margins are marked with asterisks if significant (χ^2 -Test): *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

although different responder types become slightly more similar over time with respect to their acceptance rate, a great amount of heterogeneity remains present (see Table 5). When we compare PIT to CT, a difference in the average probability to vote ‘yes’ only emerges during the last periods.

Result 8: *Contrary to our conjecture, we find that nominal weight differentials continue to affect bargaining behavior even when subjects become more experienced with the game. Nominal weights appear to provide a focal point that players use increasingly in order to decide to how much to allocate to themselves and to whom to make an offer.*

4 Summary and conclusion

We conducted an experiment designed to investigate whether and how legislative bargaining is influenced by “illusions” about voting power. To this end, we compared two treatments where real bargaining power and the selection protocol were identical, but voting weights differed. The main result of our paper is that we find evidence that differences in nominal voting power persistently influence behavior and outcomes. This presents a novel finding that contrasts with the very few previous experiments involving nominal voting weights variation, where only transitory effects were observed. Specifically, we observe that proposers’ self-offers are increasing in voting weight, which probably reflects induced

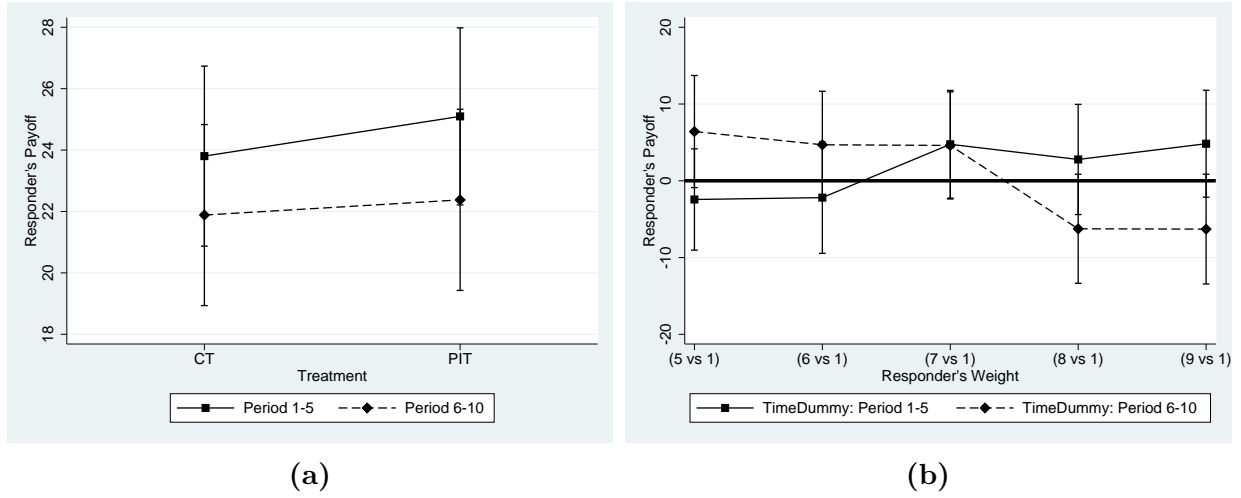


Fig. 6. Responder's expected payoff. Solid lines refer to inexperienced subjects, broken lines to experienced subjects. Error bars represent 90% confidence intervals. Panel (a) compares CT and PIT. Panel (b) contrasts expected payoffs in PIT with CT by responder's weight-type.

aspiration levels. This result is similar to the finding by Fréchette et al. (2005a) that nominally more powerful proposers make significantly larger requests, albeit only when inexperienced.

Moreover, we find that offers to others depend on both proposer and responder type, but these relationships are non-monotone and do not translate into significant differences in responders' final payoffs. Looking at responders' acceptance behavior, we observe that nominally strong players are more reluctant to accept any given amount offered to them. This suggests that players with small voting weight come to think of themselves as less important. Apparently, subjects learn from the experience of earlier periods not only to keep more for themselves in the role of proposer (but nowhere near the very large share predicted), but also to exclude demanding high-weight types from the coalition. This is in contrast to Drouvelis et al. (2010), who report that the nominally strong player's probability to be included in the winning coalition is fully consistent with equal treatment. Finally, small-weight players in our experiment earned less than large-weight players if they are the proposer; however, earnings were not smaller than the average under the control treatment. In view of Gamson's Law, our results are mixed: we do not see the predicted sharp increase in strong players' payoffs, but a tendency in later rounds to make small players part of the coalition.

Most strikingly, behavioral biases evoked by nominal asymmetry did not fade away in our experiment as players became more experienced. Recent empirical work on the allocation of cabinet portfolios in European parliamentary democracies has revealed that real (minimum integer) weights are important in explaining coalition outcomes when there are only very few parties. If, however, the number of parties increases, then nominal weights ('raw' seats) are better at predicting outcomes (see Cutler et al. 2016). Falcó-Gimeno and

Table 8.

Linear predictions of responder’s expected payoff, by responder type

	All Periods		Periods 1-5		Periods 6-10	
	Margin	Contrast	Margin	Contrast	Margin	Contrast
weight-1	22.85 (1.29)	—	23.73 (1.77)	—	21.86 (1.78)	—
weight-5	24.13 (2.71)	0.41 (3.86)	21.29 (3.60)	-0.24 (5.40)	28.28 (4.04)	1.72 (5.53)
weight-6	24.09 (2.80)	-3.27 (3.89)	21.54 (4.03)	-6.94 (5.60)	26.56 (3.83)	-0.09 (5.39)
weight-7	27.36 (2.74)	6.46* (3.89)	28.48 (3.87)	1.97 (5.51)	26.47 (3.83)	10.86** (5.48)
weight-8	20.90 (2.81)	-1.25 (3.91)	26.50 (3.95)	-2.05 (5.47)	15.61 (3.93)	0.41 (5.57)
weight-9	22.15 (2.77)		28.56 (3.82)		15.57 (3.97)	
χ^2		3.18		3.62		10.36
p		0.078		0.060		0.005

Notes. Predicted mean amounts (Tokens). Standard errors in parentheses. Contrasts between two adjacent margins are marked with asterisks if significant (χ^2 -Test): *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Indridason (2013) argue that nominal weights serve as natural ‘focal points’ in difficult and complex bargaining environments. In line with these empirical finding, our results support the reasoning that players in a complex situation rely more strongly on cognitively less demanding assessments of bargaining power than on the accurate theoretical assessment.

Our results have some interesting general implications for the design of weighted voting systems. First, they offer a basis for the claim that discrepancies between the distribution of voting weights or seats and the distribution of power ought to avoided.¹⁸ Second, the purely nominal gradation of power was sufficient to let subjects preferably self-select into coalitions with players that had similar voting weight. The reason behind the emergence of coalitions that are as homogeneous as possible might be homophily (see, e.g., Currarini et al. 2009). This might also explain the privileged role of the weight-7 player who is similar to both the low- and high-weight players, and who could thus act a kind of median voter. Future work should analyze how homophily or group identification interacts with power illusion.

In line with previous experimental studies of the Baron-Ferejohn model, we find substantial deviations from the theoretical point predictions (for purely self-interested players) also in our control treatment. In particular, proposers use their power far less than predicted under the SSPE, and make offers to too many other players. Immediate agreements

¹⁸As a rather radical way to eliminate such distortions, Berg and Holler (1986) propose to use a randomized qualified majority threshold.

occurred in 72% (73%) of our games in CT (PIT), which is very similar to the results of comparable treatments in, e.g., Fréchette et al. (2005a) and Drouvelis et al. (2010). The same is true for the frequency of minimal winning coalitions, which resulted in 64% and 71% of games in CT and PIT, respectively. These patterns broadly support the corresponding implications of the Baron-Ferejohn model. Our experiment used the Baron-Ferejohn model because it is the leading legislative bargaining game and widely used in political science. There is relatively little experimental work on other interesting bargaining procedures, e.g., models of the demand bargaining type. The question whether power illusion is contingent upon the bargaining protocol remains to be investigated in future research. Another interesting issue is whether the decision-making quorum has an effect on power illusion. Unanimity rule would be a particularly prominent alternative; yet, we then expect entitlement effects to blur any results.

One explanation for our findings could be that we considered bargaining groups of five players rather than three, in contrast to previous literature. What is more, our experimental design involved more variation in the distribution of voting weights. The fact that determining equilibrium continuation values is cognitively quite demanding poses a common problem to all experimental investigations of the Baron-Ferejohn model. Still, results in the control treatment are very similar to those observed elsewhere; this makes us confident that the power illusion features of our experiment are not driven by our subjects being special with respect to their cognitive limitations. Although subjects certainly do not compute the game-theoretic equilibrium, we believe that the fundamental strategic forces, and the basic intuition underlying the theory, are sufficiently transparent for subjects to recognize and to respond to. But, on top of this, subjects apply their own reference points for fairness and appropriateness, which are shaped in part by the game's outward appearance.

Appendix A Additional Tables

Table A1.
Determinants of proposer's self offer and payoff

Dependent variable:	Panel A		Panel B	
	Proposer's self offer		Proposer's payoff	
Treatment	-5.14*** (0.67)	-6.11** (1.13)	-2.88 (2.22)	-3.38* (1.90)
Proposer's weight=5	—	-10.16*** (0.65)	—	-4.78** (2.20)
Proposer's weight=6	—	-8.36*** (0.68)	—	-8.23*** (2.27)
Proposer's weight=7	—	-3.14*** (0.92)	—	-1.30 (3.27)
Proposer's weight=8	—	-2.74 (1.74)	—	-0.32 (2.48)
Proposer's weight=9	—	-1.27* (0.75)	—	-1.52 (2.60)
Time	2.12** (0.93)	1.15** (0.54)	3.65*** (0.97)	-0.97 (2.98)
(Treatment \times Time)	—	1.94 (1.47)	—	3.66*** (0.89)
(Proposer's weight=5 \times Time)	—	—	—	1.04 (1.71)
(Proposer's weight=6 \times Time)	—	—	—	3.09 (3.66)
(Proposer's weight=7 \times Time)	—	—	—	0.81 (4.37)
(Proposer's weight=8 \times Time)	—	—	—	2.02 (1.76)
(Proposer's weight=9 \times Time)	—	—	—	-3.76* (2.15)
Sociodemographic controls	yes	yes	yes	yes
Observations	1000	1000	200	200
groups	100	100	93	93
σ_ε	11.98	11.98	7.85	7.55
R^2	0.089	0.090	0.197	0.246

Notes. GLS random effects panel models. For sociodemographic controls see the following Table (A2). Standard errors (in parentheses) are clustered at the session level: *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table A2.

Determinants of proposer's self-offer and payoff, cont'd: sociodemographic controls

Dependent variable:	Panel A		Panel B	
	Proposer's self offer		Proposer's payoff	
Age	-0.49*** (0.18)	-0.49*** (0.18)	-0.49*** (0.16)	-0.52*** (0.20)
Sex	2.63 (3.57)	2.63 (3.58)	4.60 (4.34)	4.69 (4.18)
Siblings	-0.65 (0.98)	-0.65 (0.98)	-0.93 (0.64)	-0.92* (0.55)
Labexperience	0.32 (2.25)	0.32 (2.25)	2.83* (1.69)	2.77 (1.92)
Law	-0.62 (2.18)	-0.62 (2.18)	2.73 (6.32)	3.92 (6.30)
Mathematics	-3.32* (1.85)	-3.32* (1.85)	-3.49 (2.67)	-3.65 (2.81)
Political Science	-0.42 (5.25)	-0.42 (5.25)	0.39 (4.03)	1.12 (4.08)
Cultural studies	10.88 (8.50)	10.88 (8.52)	5.69** (2.59)	5.75** (2.34)
Philosophy	-1.01 (2.18)	-1.01 (2.18)	-1.84 (3.99)	-3.61 (3.81)
Other Fields	3.40 (2.48)	3.40 (2.48)	3.99 (4.18)	2.86 (4.92)
Educational Sciences	-2.50 (3.24)	-2.50 (3.25)	-2.69 (4.88)	-2.28 (5.06)
Works	-2.97 (2.17)	-2.97 (2.18)	-0.75 (1.13)	-0.89 (1.29)

Notes. The table continues Table A1. Independent variables: Age, Sex $\{female = 0, male = 1\}$; number of Siblings; Labexperience $\{[0 - 2] = 0, [3 - 5] = 1, more = 2\}$ previous experiments; dummies for various fields of study (benchmark category is Economics); student Works $\{no = 0, yes = 1\}$. Standard errors in parentheses: *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table A3.

Determinants of proposer's offers

Dependent variable Model	I		II		III		IV		V		VI	
	P(Offer > 0) Probit-RE	P(Offer > 0) Probit-RE	P(Offer > 0) Probit-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer Tobit-RE	Offer Tobit-RE	Offer Tobit-RE	Offer Tobit-RE
Treatment	0.268* (0.156)	0.439*** (0.165)	-1.92* (1.12)	-2.92** (1.20)	2.42** (0.99)	3.11** (1.26)						
Time	-0.356*** (0.046)	-0.213*** (0.063)	4.68*** (0.77)	3.53*** (1.07)	-2.01** (0.80)	-1.31 (1.13)						
(Treatment × Time)	—	-0.309*** (0.093)	—	2.20 (1.52)	—	-1.41 (1.59)						
Sociodemographic Controls	yes	yes	yes	yes	yes	yes						
Constant	0.194 (0.485)	0.116 (0.488)	37.37*** (3.49)	37.88*** (3.51)	16.48*** (3.09)	16.13*** (3.11)						
<i>n</i>	4000	4000	2903	2903	4000	4000						
<i>groups</i>	100	100	100	100	100	100						
σ_u	0.690	0.693	5.87	5.90	2.51	2.51						
R^2	—	—	0.111	0.114	—	—						
Wald χ^2	78.39***	88.08***	105.38***	109.47***	27.23**	28.01**						

Notes. Regressions I&II: random-effects Probit panel model (P(Offer > 0)); III&IV: random-effects GLS panel model (offer in Tokens if > 0) with session clustered standard errors; V&VI: random-effects Tobit panel model (offer in Tokens). Each proposer offers [0, 150] Tokens to four responders in the first round of each period of ten periods altogether. Independent variables: Treatment {CT = 0; P/T = 1}; Time {Periods 1-5 = 0, Periods 6-10 = 1}; interactions. For sociodemographic controls see the following Table (A4). Standard errors in parentheses: *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table A4.

Determinants of proposer's offers, cont'd: sociodemographic controls

Dependent variable Model	I		II		III		IV		V		VI	
	P(Offer > 0) Probit-RE	P(Offer > 0) Probit-RE	P(Offer > 0) Probit-RE	P(Offer > 0) Probit-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer (if > 0) GLS-RE	Offer Tobit-RE	Offer Tobit-RE	Offer Tobit-RE	Offer Tobit-RE
Age	0.046*** (0.018)	0.046*** (0.018)	0.046*** (0.018)	0.046*** (0.018)	-0.31*** (0.12)	-0.31*** (0.12)	-0.31*** (0.12)	-0.31*** (0.12)	0.27** (0.11)	0.27** (0.11)	0.27** (0.11)	0.27** (0.11)
Sex	-0.444*** (0.170)	-0.444*** (0.170)	-0.448*** (0.170)	-0.448*** (0.170)	4.75*** (1.37)	4.75*** (1.37)	4.77*** (1.37)	4.77*** (1.37)	-2.22** (1.08)	-2.22** (1.08)	-2.23** (1.08)	-2.23** (1.08)
Siblings	-0.029 (0.070)	-0.029 (0.070)	-0.028 (0.070)	-0.028 (0.070)	0.43 (0.52)	0.43 (0.52)	0.43 (0.52)	0.43 (0.52)	0.12 (0.45)	0.12 (0.45)	0.11 (0.44)	0.11 (0.44)
Labexperience	-0.142 (0.125)	-0.142 (0.125)	-0.143 (0.126)	-0.143 (0.126)	1.27 (0.98)	1.27 (0.98)	1.29 (0.97)	1.29 (0.97)	-0.49 (0.79)	-0.49 (0.79)	-0.49 (0.79)	-0.49 (0.79)
Law	-0.423 (0.313)	-0.423 (0.313)	-0.424 (0.314)	-0.424 (0.314)	4.84** (2.32)	4.84** (2.32)	4.82** (2.31)	4.82** (2.31)	-1.05 (2.04)	-1.05 (2.04)	-1.05 (2.04)	-1.05 (2.04)
Mathematics	0.017 (0.215)	0.017 (0.215)	0.015 (0.216)	0.015 (0.216)	0.46 (1.79)	0.46 (1.79)	0.44 (1.79)	0.44 (1.79)	1.18 (1.38)	1.18 (1.38)	1.18 (1.38)	1.18 (1.38)
Political Science	0.047 (0.266)	0.047 (0.266)	0.049 (0.267)	0.049 (0.267)	1.66 (2.41)	1.66 (2.41)	1.62 (2.41)	1.62 (2.41)	-0.15 (1.64)	-0.15 (1.64)	-0.14 (1.64)	-0.14 (1.64)
Cultural studies	-0.396 (0.321)	-0.396 (0.321)	-0.398 (0.322)	-0.398 (0.322)	1.28 (2.46)	1.28 (2.46)	1.23 (2.46)	1.23 (2.46)	-4.24** (2.03)	-4.24** (2.03)	-4.24** (2.03)	-4.24** (2.03)
Philosophy	-0.031 (0.517)	-0.031 (0.517)	-0.035 (0.519)	-0.035 (0.519)	0.08 (3.38)	0.08 (3.38)	-0.01 (3.35)	-0.01 (3.35)	0.20 (3.18)	0.20 (3.18)	0.21 (3.18)	0.21 (3.18)
Other Fields	-0.118 (0.347)	-0.118 (0.347)	-0.121 (0.349)	-0.121 (0.349)	1.23 (1.82)	1.23 (1.82)	1.16 (1.82)	1.16 (1.82)	-1.53 (2.20)	-1.53 (2.20)	-1.52 (2.20)	-1.52 (2.20)
Educational Sciences	-0.202 (0.283)	-0.202 (0.283)	-0.204 (0.284)	-0.204 (0.284)	1.81 (1.71)	1.81 (1.71)	1.78 (1.71)	1.78 (1.71)	0.34 (1.80)	0.34 (1.80)	0.34 (1.80)	0.34 (1.80)
Works	0.007 (0.182)	0.007 (0.182)	0.008 (0.183)	0.008 (0.183)	1.21 (1.34)	1.21 (1.34)	1.21 (1.33)	1.21 (1.33)	0.80 (1.15)	0.80 (1.15)	0.80 (1.15)	0.80 (1.15)

Notes. The table continues Table A3. Independent variables: Age, Sex ($\{female = 0, male = 1\}$; number of Siblings; Labexperience $\{[0-2] = 0, [3-5] = 1, more = 2\}$ previous experiments; dummies for various fields of study (benchmark category is Economics); student Works ($\{no = 0, yes = 1\}$). Standard errors in parentheses: *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table A5.

Predictions of responder's probability of voting 'yes': voting weight

Periods 1-5							
Offer to responder	Responder's voting weight					χ^2	p
	5	6	7	8	9		
0	0.3 (0.6)	0.1 (0.1)	0.1 (0.3)	0.1 (0.4)	0.0 (0.0)	0.36	0.986
10	3.9 (4.0)	1.7 (2.4)	1.9 (4.3)	2.0 (3.6)	0.5 (0.9)	1.10	0.895
20	21.5 (8.3)	15.7 (8.4)	17.4 (13.4)	13.9 (9.8)	6.7 (5.2)	2.97	0.563
30	57.0 (7.8)	53.9 (9.0)	57.7 (8.7)	44.8 (8.9)	34.0 (7.8)	6.85	0.144
40	86.9 (6.4)	88.1 (6.6)	90.2 (7.2)	78.9 (8.3)	74.4 (8.8)	2.99	0.560
50	97.8 (2.3)	98.6 (1.8)	99.0 (1.9)	95.6 (4.7)	95.5 (4.4)	0.95	0.918
Periods 6-10							
Offer to responder	Responder's voting weight					χ^2	p
	5	6	7	8	9		
0	—	2.4 (3.4)	2.1 (3.6)	2.7 (3.0)	0.0 (0.2)	1.49	0.828
10	0.0	9.8 (8.1)	8.7 (8.6)	8.4 (5.8)	1.0 (1.9)	4.24	0.375
20	0.0 (0.0)	27.4 (11.8)	24.6 (12.5)	20.5 (8.1)	7.6 (7.3)	14.19	0.007
30	47.3 (19.1)	54.1 (11.0)	49.7 (11.3)	39.5 (9.1)	30.2 (11.5)	3.13	0.537
40	100.0	79.1 (8.0)	75.1 (7.9)	61.8 (10.0)	65.8 (10.5)	27.86	0.000
50	100.0	93.3 (4.4)	91.2 (5.1)	80.7 (9.5)	90.7 (6.8)	8.09	0.088

Notes. Mean probabilities in %. Standard errors in parentheses.

Table A6.

Responder's probability of voting 'yes': treatment effect

Dependent variable Model	(I) Vote Probit-RE	(II) Vote Probit-RE	(III) Vote Probit-RE
Offer to responder	0.071*** (0.004)	0.062*** (0.005)	0.059*** (0.007)
Proposer's self-offer	-0.040*** (0.005)	-0.043*** (0.006)	-0.049*** (0.008)
Treatment	-0.200 (0.149)	-1.206*** (0.451)	-2.156*** (0.657)
Offer to responder \times Treatment	—	0.025*** (0.009)	0.053*** (0.017)
Proposer's keep \times Treatment	—	0.005 (0.009)	0.011 (0.013)
Time	0.092 (0.108)	0.065 (0.110)	-0.412 (0.602)
Offer to responder \times Time	—	—	0.006 (0.010)
Proposer's keep \times Time	—	—	0.011 (0.012)
Treatment \times Time	—	—	1.372 (0.923)
Offer to responder \times Treatment \times Time	—	—	-0.040** (0.020)
Proposer's keep \times Treatment \times Time	—	—	-0.011 (0.019)
Sociodemographic controls	yes	yes	yes
n	1100	1100	1100
$groups$	100	100	100
Wald χ^2	309.78***	294.74***	289.29***
σ_u	0.458	0.480	0.483

Notes. Marginal effects of a random effects Probit model. Dependent variable: *Vote* (no=0, yes=1) is a responder's voting decision (n). Independent variables: *Offer to responder* is the amount of tokens offered to a responder ($\in [0, 150]$); *Proposer's Keep* is the amount of tokens allocated by the proposer to himself ($\in [0, 150]$); *Treatment* is a dummy variable (CT= 0, PIT= 1); *Time* is a dummy variable (Periods 1–5 = 0, Periods 6 – 10 = 1); interactions of these variables. Sociodemographic controls and constant not reported. Standard errors in parentheses: *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table A7.

Predictions of responder's probability of voting 'yes': treatment effect

Proposer's keep	Period 1-5			Period 6-10		
	CT	PIT	Contrast	CT	PIT	Contrast
30	65.0 (3.8)	55.8 (2.1)	9.2** (4.4)	65.6 (4.1)	56.2 (3.6)	9.4* (5.5)
45	47.9 (3.0)	47.1 (2.4)	0.8 (4.0)	53.6 (3.1)	45.0 (2.9)	8.6*** (4.2)
60	30.2 (4.2)	37.7 (0.4)	-8.5 (5.9)	40.5 (4.3)	33.2 (4.7)	7.4 (6.4)
75	15.3 (4.8)	28.7 (5.7)	-13.4* (7.5)	27.7 (6.3)	22.0 (6.6)	5.6 (9.2)
90	5.9 (3.6)	20.4 (7.0)	-14.5* (7.9)	16.6 (7.2)	12.9 (7.1)	3.7 (10.1)
χ^2	50.53			5.48		
p	0.000			0.360		

Notes. First row: means (Probabilities in %); second row: standard errors. Contrasts between two margins are marked with asterisks if significant (χ^2 -Test). *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Table A8.

Determinants of responder's expected payoff

Regression Dependent Model	I Payoff Tobit-RE	II Payoff Tobit-RE	III Payoff Tobit-RE	IV Payoff Tobit-RE
Treatment	0.90 (1.85)	1.29 (2.53)	—	—
Responder's Weight=5	—	—	1.28 (3.02)	-2.43 (4.01)
Responder's Weight=6	—	—	1.24 (3.20)	-2.19 (4.42)
Responder's Weight=7	—	—	4.51 (3.05)	4.75 (4.27)
Responder's Weight=8	—	—	-1.95 (3.11)	2.78 (4.36)
Responder's Weight=9	—	—	-0.70 (3.06)	4.83 (4.23)
Time	-2.32 (1.74)	-1.92 (2.47)	-2.32 (1.74)	-1.86 (2.44)
Treatment×Time	—	-0.80 (3.48)	—	—
Responder's Weight=5×Time	—	—	—	8.85 (5.93)
Responder's Weight=6×Time	—	—	—	6.88 (6.07)
Responder's Weight=7×Time	—	—	—	-0.15 (5.96)
Responder's Weight=8×Time	—	—	—	-9.03 (6.08)
Responder's Weight=9×Time	—	—	—	-11.13* (6.03)
Sociodemographic controls	yes	yes	yes	yes
<i>n</i>	800	800	800	800
<i>n</i> (Offer > 0)	587	587	587	587
<i>groups</i>	100	100	100	100
σ_ε	23.91	23.91	23.85	23.65
Wald χ^2	9.71	9.76	12.88	23.59

Notes. Random effects Tobit panel model. Each responder receives a payoff of [0, 150] Tokens in the last round of each period of ten periods altogether. Independent variables: Treatment {CT = 0; PIT = 1}; Responder's Weight {5, 6, 7, 8, 9}; Time {Periods 1-5 = 0, Periods 6-10 = 1}; interactions. Sociodemographic Controls are all insignificant and therefore omitted. First row: coefficients; second row: standard errors. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

Appendix B Instructions

Welcome to the experiment and thank you very much for your participation!

Please read the instructions carefully. Do not talk to your neighbor during the experiment. Should you have any questions, please make sure to read the instructions again. Should you still have questions after that, please notify us. We will then come to you and answer your questions. It is of vital importance that you follow this rule. Otherwise the results of this experiment are scientifically useless to us.

Please take your time reading the explanations and making your decisions. You will not be able to influence the duration of the experiment by deciding quickly, as we will always wait for the rest of the participants to finish. The experiment is completely anonymous. You will not find out who you played with, not during the experiment or afterwards. The other participants will not find out, not during the experiment or afterwards, which role you played, what decisions you made, and how much you earned.

During the experiment, we will not talk about Euros, but about Tokens. The following exchange rate is used:

100 Tokens equal 20 Euros.

For your participation, you will receive 7 Euros as a fixed payout plus the amount of Tokens you earned during the experiment based on your and the other participant's decisions. You will be paid in cash individually and privately after the end of the experiment. The estimated duration of the experiment is 60-90 minutes. Below you will find a detailed description of the experiment proceedings.

Instructions:

In the experiment we will ask you to decide how a specific number of Tokens will be distributed among your group of five players. This will be repeated in 10 periods. During each period, suggestions are made regarding the distribution. A distribution proposal has to distribute the available Tokens completely among the suggesting player and the other players. This proposal is presented to the other 4 players and they can either accept or reject it. A suggestion is accepted if it receives at least half of the votes. *(In PIT: However, the respective players each have a different number of votes. At the beginning of each period, the players are informed how many votes they have and how many votes the other players have.) (In Control: In each Period, all players have one vote.)*

Detailed Instructions:

In each negotiation period, you will form a group of five players with the other participants. You will play 10 periods. In each period, you will be placed in a group of 5 new players.

Additionally, no information from previous periods will be communicated to the next period. Each negotiation period follows the exact same procedure. First, all 5 group members anonymously and at the same time make a proposal regarding the distribution of the available Tokens. One of these five distribution proposals will be randomly selected and put to a vote.

- If this first proposal receives the majority of votes, it will be implemented. Every player receives the Tokens allocated to him during this round.
- If the proposal is rejected, one player from the group of five is randomly selected and can propose a new distribution proposal to be voted on. This can also be the player whose suggestion was just rejected.
- This procedure is repeated until a distribution proposal is accepted. Once a distribution proposal is accepted, the negotiation period ends.

(In PIT: At the beginning of each negotiation period, every player is allocated a number of votes. The total number of votes is constant across all negotiation periods. At the beginning of each negotiation period, the players are informed how many votes they themselves have and how many votes each other player has.) (In Control: At the beginning of each negotiation period, every player is allocated with one vote.) A distribution proposal is accepted if at least half of the votes are cast in favor of the proposal. *(In PIT: During a negotiation period, each player keeps his number of votes until an agreement is reached. However, the number of votes for each player can vary from one negotiation period to the next.)* A negotiation period only ends when a distribution proposal receives the majority of the votes.

Calculation of your payout:

During the negotiation periods your payout is directly determined by the distribution proposal: You will receive the number of Tokens determined by the accepted distribution. Should no Tokens have been allocated to you, you do not receive anything. Of the 10 negotiation periods, one will be randomly selected at the end of the experiment. Thus, each negotiation period could be the one relevant for the payout. The payouts for all participants are determined by this randomly selected period.

Calculation of your final payout for participating in the experiment:

Your final payout consists of two parts:

- (i) Every participant receives a participation payment of 7 Euros. This is independent of your decisions or the decisions of other players.
- (ii) The payout for the experiment is determined by the voting behavior of all group members and by the randomly selected period. This period determines the payout for all players in the experiment.

After the last period, you will receive a brief questionnaire. Following that, you will receive your payout in cash.

The experiment will start shortly. Should you have any more questions, please raise your hand and wait quietly until somebody approaches you. Please do not talk to the other participants during the whole experiment. Thank you very much for your participation.

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